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What's in a frame? Goal framing, trust and reciprocity

Ananish Chaudhuri [a,*], Yaxiong Li [a], Tirnud Paichayontvijit [b]

[a] Department of Economics, University of Auckland, New Zealand.
[b] Thailand Development Research Institute, Thailand

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ABSTRACT

Economists often rely on the Berg et al. (1995) trust game, or variants thereof, to identify levels of trust and reciprocity, which are fundamental to discussions of social capital. But to what extent is behavior in this game sensitive to the way the instructions are framed? We use the Berg et al. trust game played for ten rounds with random re-matching to study this. We implement a number of variations in the way the game is presented to subjects. We show that levels of trust, reciprocity and returns to trust are significantly higher under "goal framing", which highlights the conflict inherent in the game, between self-interest and maximizing social surplus. Furthermore, with such framing, trust measured via the experimental game exhibits significant positive correlation with trust measured via the Social Values Orientation questionnaire.

1. Introduction

The decision to trust strangers, or reciprocate others' trust, is ubiquitous in a variety of economic transactions, especially where contracts are incomplete. For instance, trust and reciprocity are central to agency problems, which lie at the heart of most, if not all, employment relationships. Trust and reciprocity are essential components of social capital. Consequently, social scientists have long been interested in understanding their role and implications for a variety of economic phenomena, including issues of economic growth and well-being. The literature is voluminous. Selected references include Arrow (1974), Fukuyama (1995), Knack and Keefer (1997), La Porta, Lopez-de-Silanez, Shleifer, and Vishny (1997) and Putnam (2000).

Given the difficulty of studying some of these questions using naturally occurring data, one immediate question is: how do we measure trust? Social scientists, other than economists, have typically tended to rely on survey responses in order to gauge trust in strangers. Such include the GSS trust question, the World Values Survey, the Social Values Orientation (SVO) questionnaire, as well as other types of questions. See for instance, Kuhlman, Camac, and Cunha (1986), Liebrand (1986), Messick and McClintock (1968), Parks (1994), Parks and Hulbert (1995), Parks, Henager, and Scamahorn (1996), Rotter

* Corresponding author at: Department of Economics, University of Auckland, Private Bag 92019, Auckland 1142, New Zealand.
E-mail addresses: a.chaudhuri@auckland.ac.nz (A. Chaudhuri), sherry.li@auckland.ac.nz (Y. Li), tirnud@tdri.or.th (T. Paichayontvijit).

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(1967), Yamagishi (1986), Yamagishi and Sato (1986) and Yamagishi and Yamagishi (1994). Cook and Cooper (2003, chap. 8) and Kramer (1999) provide reviews of this line of work.

Economists, given their emphasis on extrinsic motivation, have traditionally looked to study responses elicited via monetarily incentivized decision-making experiments. In doing so, this literature has relied heavily on the "investment game" (also often referred to as the "trust game") first introduced by Berg et al. (1995) or variants thereof.[1] The Berg et al. (1995) version is a paired bargaining game, with each pair member starting the game with the same endowment. The first mover (henceforth, sender) can transfer any part of this endowment, or all of it, to the second mover (henceforth, receiver). Any such transfer, by the former, is tripled by the experimenter before it reaches the latter. The receiver then has a choice, whether to keep the entire tripled amount received, or to send some of it back to the sender. Any amount returned by the receiver is not tripled. The game ends following the receiver's decision.

This game is essentially a sequential prisoner's dilemma, with a clear conflict between self-interest and the social optimum. Self-interest predicts a Nash equilibrium involving mutual defection by both players. The receiver has no incentive to send any money back, regardless of the size of the transfer made by the sender. Anticipating that, the sender should not transfer any money. Of course, if players are influenced by motivations such as trust and reciprocity, then the outcome may well be different, with both players potentially better off than they would be if they simply hung on to their initial endowments.[2] Amount sent by the sender is typically used to measure trust, while the proportion returned (amount returned as a fraction of the amount received, with the latter being three times the amount sent) serves as the measure of reciprocity.

Berg et al. start from the premise that trust is a "primitive" in human interactions and designed their game as a way to measure it. Ortmann, Fitzgerald, and Boeing (2000) undertake a comprehensive replication of the Berg et al. experiments, and reinforce the view regarding the primacy of trust. But, such prior experimental studies document that, on average, acts of trust do not pay off. Sender earnings, following an act of trust, are typically less than what would have been the case had he not extended trust in the first place.[3] So much so that in his survey of this literature Camerer (2003, p. 86) comments: "*The fact that the return to trust is around zero seems fairly robust.*" Similar arguments have been made by others, such as Ashraf, Bohnet, and Piankov (2006) and Bolle (1995).[4]

Should we then conclude that the Berg et al. trust game is ill-equipped to measure trust? And in that context, is trust a bad investment? Will reposing trust in unknown strangers typically lead to losses? Or, could it be that the absence of extra returns to trust is an artefact of the way the instructions are framed?

Cookson (2000, p. 56–57) writes:

"Experimental economists typically devote a great deal of effort into investigating complex variations in strategy sets. . .in order to test competing theories. By contrast, rather less time and effort is spent on investigating simple variations in how those strategy sets are described to subjects".

Cookson goes on to suggest that experimental results may be less robust than is commonly presumed and that presentational details matter, particularly when discussing generalizability of results from one experiment to another or to non-experimental settings. Interpretation of experimental results may depend crucially on how participants perceive the game and whether they and the experimenter attach the same meanings to the relevant actions in the strategy sets. See Samuelson (2005) and Levati, Miettinen, and Rai (2011) for further arguments along these lines.

Cookson (2000) defines a "framing effect" as one, where different ways of describing the same choice problem leads to changes in behavior, even though the underlying information and choices remain essentially the same. Dufwenberg, Gächter, and Hennig-Schmidt (2011) comment that framing may play a pre-eminent role in psychological games (as in Geanakoplos, Pearce, & Stacchetti, 1989 or Battigalli and Dufwenberg, 2009), where payoffs depend on both actions and beliefs; if and when framing changes beliefs, actions may change as well. The literature on framing effects is large, starting with Tversky and Kahneman (1981) who demonstrated the influence of reference points and how framing the same choice as

[1] Such variants include gift exchange games as in Fehr, Gächter, and Kirchsteiger (1997) or binary trust games as in Clark and Sefton (2001), Eckel and Wilson (2004) and Snijders and Keren (2001).

[2] Chaudhuri and Gangadharan (2007, p. 959) define trust and reciprocity in the following way."*In a one-shot game, an action taken by an agent is "trusting" if (1) it leads to the creation of a surplus that can be shared with another agent but (2) leaves the first agent vulnerable to the possibility of exploitation if the second agent expropriates the entire surplus, which makes the first agent worse off than she would have been had she not taken the trusting action. An action by the second agent is "reciprocal" if the second agent foregoes the opportunity to expropriate said surplus (even though he can do so with impunity in a one-shot game) and shares any such surplus created with the first agent.*"

[3] In the original Berg et al. (1995) study, senders (as well as receivers) had an initial endowment of US $10. On average the senders in that study transferred US $5.16 to the receivers, leaving the sender on average with US $4.84. On average, receivers received US $15.48 (i.e., three times US $5.16) and returned US $4.66 implying that on average senders ended up with US $9.50 implying that they would have been better off by holding on to their initial US $10 endowment.

[4] A set of papers, including Ashraf et al. (2006), Chaudhuri and Gangadharan (2007) and Cox (2002, chap. 14, 2004, 2009), suggest that, at least a part of the transfers made in this game are motivated by altruistic motives. However, all three papers also conclude that the larger proportion of transfers are motivated by trust and reciprocity. Glaeser, Laibson, Scheinkman, and Soutter (2000), using a slightly modified version of the Berg et al. game, show that traditional attitudinal survey responses (such as responses to the GSS trust question) about trust predict reciprocal behavior better than they predict trusting behavior. Karlan (2005) examines whether behavior in the trust game predicts loan repayments to a Peruvian group-lending microfinance program. He finds that individuals who are "trustworthy", in the sense of reciprocating a trusting gesture by the paired sender, are indeed less likely to default on their loans. But Karlan finds no such correlation for "trusting" individuals, those who send money as the sender. A number of papers including Bohnet and Zeckhauser (2004), Dohmen et al. (2005), Eckel and Wilson (2004) and Kosfeld, Heinrichs, Zak, Fischbacher, and Fehr (2005) clearly demonstrate that the decision to trust in this game is significantly different from simply making a decision under risk.

a "gain" or a "loss" can lead to dramatic changes in behavior. Similarly, Ross and Ward (1996) and Liberman, Samuels, and Ross (2004) report how cooperation rates are substantially higher when the same prisoner's dilemma game is referred to as a "Community game" as opposed to the "Wall Street" game.

In work related closely to this study, Cronk (2007) looks at framing effects in the Berg et al. (1995) trust game among the Maa speaking pastoralist Maasai in Kenya. In a control treatment, subjects play the game with neutral instructions. The experimental treatment utilizes a framing that invokes *"osotua"*, which literally means "umbilical cord" but refers to "*gift-giving relationships based on obligation, need, respect, and restraint*." (Cronk, 2007, p. 352). The results suggest that compared to games with neutral framing, in the "osotua" frame there is reduced trust (measured by average amount sent by senders) and reciprocity (measured by average proportion returned by receivers). Senders also expect lower returns in the "osotua" frame. Cronk suggests that the "osotua" framing seems to have shifted game play away from the logic of investing and towards mutual obligations to respond to genuine needs. Since the perceived need is assessed to be less in the framed game, transfers are also lower.

Also of interest is the meta-analysis of trust games undertaken by Johnson and Mislin (2011), who collect data from 162 replications of the Berg et al. (1995) game to identify factors affecting behavior. However, in keeping with the caveat noted by Cookson earlier, most of these studies manipulate what would be considered *structural features* of the game such as whether (i) the receiver receives an endowment or not (amount sent is lower with receiver endowment); (ii) subjects are paid for each round or for a randomly chosen round (the latter payment scheme reduces trust); (iii) the amount sent is doubled or tripled (receivers reciprocate less when amount is tripled) and (iv) whether subjects play one role or both roles (there is less reciprocity when participants play both roles). One manipulation that may be considered as a framing effect is whether players play against another human or a computer. The former leads to greater trust (though the effect is only marginally significant).

There is also a large literature in public goods games, where behavior differs depending on whether a particular action is defined as "giving" to the public good or "taking" from a common pool, even though the underlying strategic context is identical. See, for instance, Andreoni (1995), Brewer and Kramer (1986), Cookson (2000), Fleishman (1988), Park (2000), Sonnemans, Schram, and Offerman (1998), Willinger and Ziegelmeyer (1999) among others. Cookson (2000), Cox and Stoddard (2015) and Zelmer (2003) provide reviews of this line of work. Levin, Schneider, and Gaeth (1998) provide a typology of different types of framing effects and a comprehensive review of such effects across a wide variety of games. The set of essays included in Kahnemann and Tversky (2000) also provide a useful guide for understanding such framing effects.

To examine the impact of framing on trust and reciprocity, we present the results of two different studies. In Study 1, we use a design where, starting with a treatment in which subjects get written instructions using abstract language only, we make incremental changes till we get to a treatment that provides, what we refer to below as "goal framing" (Levin et al., 1998), where we clearly identify the conflict between the self-interested outcome and the social optimum. When we do so, participants not only exhibit higher levels of trust and reciprocity, trusting also pays off, in the sense that the returns to trust are significantly greater than zero.

In Study 2, we look at the issue of correlation between trusting decisions in this game and trusting responses elicited by survey responses. Prior studies typically fail to find positive correlation between the two. We use the Social Values Orientation (SVO) Scale, used by Yamagishi (1986), Yamagishi and Sato (1986) and Yukawa (1985) to measure each subject's level of trust and see whether and to what extent that trust correlates with trusting decisions, in terms of sending money, as the sender in the trust game. We show that trusting decisions elicited using the SVO scale show significantly positive correlations with the decision to send money using the goal frame.

We are certainly not arguing that the data from this one study, with a single parametrization of the game, can definitively resolve the issue of whether trust pays off or not. We are simply making a methodological point that the absence of extra returns to trust in previous experiments, may not necessarily reveal participants' inherent preferences but reflect different perceptions of the game. Different presentations of the same experiment may be interpreted as being different situations. Eckel and Wilson (2004), for instance, find that participants in their trust game, within and across the roles of senders and receivers, differ in their perception of the game. In real life, we are often confronted with either familiar or unfamiliar situations, where we may or may not know the appropriate social norms or their weight. The principal question then is the connection between instructions and (i) their impact on logical understanding and (ii) their inducement of the appropriate social norms.[5] We proceed as follows. In Section 2, we provide details of our experimental design and procedures for both studies. We present our results in Section 3. We make some concluding remarks in Section 4.

2. Experimental design and procedure

The procedures for implementing the two studies are very similar and therefore we describe them for both studies in this section. All sessions were conducted in the DECIDE lab at the University of Auckland. A total of 382 people (191 sender receiver pairs) took part in Study 1 while a total of 56 subjects (28 sender and receiver pairs) took part in Study 2. The participants are typically first year undergraduate students in business and economics with no prior experience of this game. Participants

[5] We thank an anonymous referee for valuable feedback in formulating this argument.

are seated in cubicles with a computer. There are dividers separating the cubicles so that no participant can see any other participant's computer screen.

We use the Veconlab website to conduct the experiments.[6] See Holt (2009) for more details. Once participants log on to the Veconlab website, they are randomly assigned to the role of either a sender or a receiver and these roles remain unchanged for the entire session. Participants are told that they will play the trust game for ten rounds during the course of the experimental session. Senders and receivers are randomly re-matched at the end of each round ("stranger" matching protocol) and participants are made aware of this via the instructions. We now present the details of the two studies.

2.1. Study 1

In Study 1 we implement five different treatments which vary only in the nature of the instructions given to the subjects. We refer to these five treatments as *Private knowledge, Common knowledge, Context-Neutral, Context-Loaded A* and *Context-Loaded B*. We explain the details in Section 2.3 below. This is a between subjects design with subjects taking part in only one of those five treatments.

After receiving their instructions (shown in Appendix A), participants log into their computers and start playing the game. At the beginning of each round, each participant is endowed with ten experimental dollars which are equivalent to NZ $1.[7] Senders move first by transferring any or the entire amount to receivers by entering the appropriate number from zero to ten in the relevant box on the senders' screen. They are free to enter decimal amounts but very few did so, choosing to enter whole numbers instead. Any amount transferred is tripled before it reaches the receivers. Once the receiver receives this tripled amount, she has to decide how much, if any, of this amount to send back to the sender. Any amount returned by the receiver is not tripled. At the end of each round, both senders and receivers get to see the decisions made by their pair members and their own earnings. The round ends at this point and the next round starts with different pair members. The game proceeds in the exact same way for all ten rounds. At the end of the ten rounds the subjects are asked to fill out a demographic questionnaire. This is shown in Appendix B.

The majority of previous studies have used one-shot games, where participants play the game once only; except in our study participants play the game for ten rounds with random re-matching between rounds. This "stranger" matching protocol preserves the one-shot nature of the interaction, while allowing for learning and gathering experience. It also lets us to study the dynamics of decision making and contributes towards mitigating possible cognitive demand effects. See Andreoni and Croson (2008, chap. 82) for arguments along similar lines. Chaudhuri and Sbai (2011) use a similar design with random re-matching over ten rounds in their study of gender differences using the Berg et al. (1995) trust game. The session ends at that point and participants are paid their earnings privately. Each session lasts about 50 min. On average, subjects earn NZ $16.47 excluding the NZ $5 show-up fee.

2.2. Study 2

In Study 2, subjects take part only in one of the five treatments from Study 1, namely the *Context-Neutral* treatment. The treatment is run in the exact same way as in Study 1, with subjects playing the stage game for ten rounds with stranger matching from one round to the next. At the end of those ten rounds, subjects fill out the exact same demographic questionnaire as in Study 1 but in addition they are also asked to fill out the SVO questionnaire. This is a short questionnaire which asks subjects to respond to five questions. The questionnaire handed out to the subjects is shown in Appendix C.

These questions are: *(1). Most people tell a lie when they can benefit by doing so. (2) Those devoted to unselfish causes are often exploited by others. (3) Some people do not cooperate because they pursue only their own short-term self-interest. Thus, things that can be done well if people cooperate often fail because of these people. (4) Most people are basically honest. (5) One should not trust others until one knows them well.* Each question is answered by choosing one out of five responses: strongly disagree; disagree, neutral, agree and strongly agree. A response of strongly disagree is given a score of one point, with strongly agree getting five points, except for Question 4 which is reverse scored. The minimum score is 5 while the maximum is 25.

The session ends at that point and participants are paid their earnings privately. In Study 2, on average, subjects earned NZ $14.22 excluding the NZ $5 show-up fee. Given that Study 2 merely collects additional data for the Context-Neutral treatment we could have merged this data with that of Study 1, but given that the data was collected at two different points in time, we have decided to analyze the data for the two studies separately.

2.3. Details of the different treatments in Study 1

There are five different treatments in Study 1. The first one is the *Private knowledge* treatment. Here participants are only provided with written instructions of the game using neutral, i.e., context-free, language. Words such as trust and reciprocity

[6] http://vecoblab.econ.virginia.edu/admin.htm

[7] At the time of the experiments NZ $1 was around US $0.78.

are not used. Instructions for all the treatments are provided in Appendix A. This treatment will serve as our control treatment.

Participants in the *Private knowledge* treatment may suffer from, at least, two different sources of uncertainty. First, there may be uncertainty regarding whether everyone in the session has paid attention to the instructions and whether the participants share a common comprehension of the rules of the game. Van Huyck, Battalio, and Beil (1990) refer to this as "strategic uncertainty". Second, there may be uncertainty regarding the goals of the game, as to what the game is designed to measure. Treatment 2, the *Common knowledge* treatment, is designed to address the first of those two issues. Here, participants play the game with the exact same written instructions as in the *Private knowledge* treatment, except, prior to starting the session the experimenter also reads the instructions out loud to the participants. The idea is that by reading the instructions out loud we make sure that everyone has heard the instructions and everyone also knows that everyone else has heard the instructions. See Chaudhuri, Graziano, and Maitra (2006), Chaudhuri, Schotter, and Sopher (2009) and Chwe (2001) for arguments about how and why reading instructions aloud create a common comprehension of the message.

Clearly we are using the phrase "*common knowledge*" in a loose sense and we could equally refer to this as a public knowledge treatment. However, there are prior studies which suggest that such reading aloud of messages does enhance a common perception of said message. Therefore, we have decided to stick with the "common knowledge" phrase as a more evocative description of what we are trying to achieve. Chaudhuri, Graziano, and Maitra (2006) show how such reading aloud of messages leads to greater (and at times, full) cooperation in a public goods game while Chaudhuri, Schotter, and Sopher (2009) show how such common comprehension of the message can lead to coordination at the payoff-dominant equilibrium in a weak-link game with multiple payoff ranked equilibria.

Research in psychology suggests that auditory stimuli, as in messages one hears, are processed and understood differently than visual stimuli, messages that one reads on one's own or stimuli that is both heard and seen. See, for instance, Haan, Appels, and Aleman (2000), Penney (1989) and Tindall-Ford, Chandler, and Sweller (1997). In his book, Chwe (2001) discusses at length how society goes about creating social institutions and processes that promote a common understanding and perception of particular messages, events, signals and rituals. So, if the absence of extra returns to trust are caused not by a failure to understand the game's incentives, but from a sense of strategic uncertainty as to whether all the other participants have accurately comprehended the game, then we expect to see a sharp increase in trust and reciprocity in going from *Private* to *Common knowledge*.

Our third treatment is *Context-Neutral*.[8] Here, participants are provided with the same written instructions from the other two treatments. In addition the instructions given to participants in this treatment contain the following two extra paragraphs at the end. All of this is read out loud.

> *One way to think about this situation is as follows: the receiver has no incentive to send any money back to the sender because the round ends immediately after that. Anticipating that, the sender should hang on to his $10.00 and send nothing to the receiver. This means they will both end the round with $10.00 each.*
> *But suppose the sender decides to transfer $10.00 to the receiver. Then the receiver will get $30.00. If the receiver sends back an amount more than $10.00 then it is easy to see that both the sender and the receiver can make more money than if they simply hung on to their $10.00 in each and every round.*

Even if every participant is convinced that everyone else has heard the instructions read out loud in the *Common knowledge* treatment, nevertheless not every one may be convinced that every other participant has interpreted the game in the same way, or drawn the same conclusions regarding the experimenter's purpose, as the participant has. Adding the extra information shown above should contribute towards removing this source of uncertainty. This treatment undertakes what Levin et al. (1998) refer to as "goal framing", in which the goal of an action or behavior is framed.

The Fourth treatment is called *Context-Loaded A*. The instructions are read out loud and the language is similar to that in *Context Neutral*, except here we explicitly introduce the words "trust" and "trustworthiness" in the second paragraph. The additional instructions here state:

> *One way to think about this situation is as follows: the receiver has no incentive to send any money back to the sender because the round ends immediately after that. Anticipating that, the sender should hang on to his $10.00 and send nothing to the receiver. This means they will both end the round with $10.00 each.*
> *But suppose the sender decides to trust the receiver by sending $10.00. Then the receiver will get $30.00. If the receiver behaves in a trustworthy manner and sends back an amount more than $10.00 then it is easy to see that both the sender and the receiver can make more money than if they simply hung on to their $10.00 in each and every round.*

The fifth and final treatment is called *Context-Loaded B*. The instructions, also read out loud, are identical to the *Context-Loaded A* treatment except we interchange the two above-mentioned paragraphs. In the *Context-Loaded A* treatment the explanation of the sub-game perfect equilibrium comes first while the trust and reciprocity based argument comes second.

[8] We use the word "context" rather than "frame" because each of our five treatments presents a different frame. We feel that "context" is a more emotive and apt short-hand for those treatments, where the underlying conflict between self-interest and the social optimum is made clear to the subjects.

This order is reversed in the *Context-Loaded B* treatment to make sure that the order of presentation does not make a difference in participants' behavior.[9]

If we find that the major differences in behavior arise in going from the *Private knowledge* to the *Common knowledge* treatment then we would conclude that uncertainty about a common comprehension of the message, whether everyone has really paid attention to the instructions or not, is preventing trust from paying off. However, if the *Context* treatments impact behavior more, then the implication is that uncertainty about the ultimate purpose of the game plays a larger role. Finally, comparing the neutral and loaded context treatments allows us to see if participants are successful in interpreting the actions the same way with or without the explicit use of words like "trust" and "trustworthiness". As we show below, the context framing clearly matters, but the differences between the three context treatments – in terms of amounts sent and returned – are minor. This suggests that subjects are able to infer the returns from trust and trustworthiness even in the *Context-neutral* treatment, which eschews any of those loaded terms.

This is a between subjects design, with each participant taking part in only one of those five treatments. As noted above, we followed up on the results obtained in the first study, by having another 56 subjects who took part in the *Context-Neutral* treatment only. Table 1 provides details about number of sessions and participants in each study. Also as noted above, in Study 1 upon concluding the ten rounds of the trust game, subjects filled out only the demographic questionnaire shown in Appendix B, while subjects in Study 2 filled out both the demographic questionnaire and the SVO questionnaire.[10] We do not use data from the demographic questionnaire in this paper. Here we focus only on treatment differences. In a companion paper, Chaudhuri and Li (2016), we explore gender differences in behavior under the different treatments.

3. Results of Study 1

We start by looking at the issue of trust across the five treatments, where trust is measured by the amount sent by the sender. After that we focus on reciprocity, measured by the proportion of the amount received returned by the receivers across treatments.

3.1. Sender behavior

3.1.1. Result 1: Subjects transfer the most in the Context treatments and the least in the Private knowledge treatment

Fig. 1 shows the average amount sent in each treatment aggregated over ten rounds. This is smallest ($2.56 out of $10 or 26%) in *Private knowledge* and largest ($67.5%) in *Context-loaded B*. Fig. 2 shows the average amount sent in each of ten rounds across five treatments. It is clear that compared to the *Private knowledge* treatment, average amount passed is higher in the *Common Knowledge* treatment in each of the ten rounds. Furthermore, except for one round, the average amount sent in each of the three *Context* treatments is always higher than that in the *Common knowledge* treatment for all ten rounds.

We start by carrying out non-parametric tests before presenting results from parametric regression models. Given the random re-matching of participants, within-session observations are not independent and a session itself constitutes one independent observation. Therefore, in Table 2, we provide Wilcoxon ranksum tests comparing the average amounts sent across the five treatments in round 1 only, which constitute independent observations and provides a clean test to see if the treatments are making a difference at the very outset. This gives us 41 independent observations for the amount sent in the *Private knowledge* treatment, 39 in *Common knowledge*, 34 in *Context-Neutral,* 42 in *Context-loaded A* and 35 in *Context-loaded B*. It is clear from this table that in the first round, compared to *Private knowledge*, the amount transferred is significantly higher in the three *Context* treatments while the difference is not significant for the comparison between *Common knowledge* and *Private knowledge*. Compared to *Common knowledge*, the amount transferred is also higher in two out of three *Context* treatments and in round 1, at least, amount sent in *Context-loaded B* is higher than that in *Context-Neutral*.

In Table 3 we use regression analysis to compare the amounts sent in the five treatments. First, we use random-effects regression with robust standard errors clustered on individuals. We use random effects rather than fixed effects because our regressors include both time varying and time invariant variables such as round and treatment dummies respectively. Sec-

[9] One issue worth addressing vis-à-vis the *Context* treatments is whether this may lead to experimenter induced demand effects. We do not believe this is a concern, for, at least, two reasons. First, our instructions include both a negative as well as a positive message by telling the subjects that strategically the sender should send no money, yet both pair members are better off if they trust and reciprocate. To make sure that the order in which the arguments appear does not make a difference we counter-balance the two arguments. Second, our *Context* treatments are designed to remove what Zizzo (2010) calls "cognitive" demand effects; those that arise from incorrect task construal. However, in doing so we are careful to make sure that we do not introduce "social" demand effects, by implementing both the *Context Neutral* and *Context Loaded* treatments, given that in *Context Neutral* we do not use any loaded words such as trust or reciprocity.

[10] We tried to make sure that we had at least 20 subjects in each session so that the chance of a particular sender meeting a given receiver more than once across 10 rounds was minimized. Due to the vagaries of subject recruitment, we were not always successful as is clear from Table 1. However, in the software used subjects never get to see the subject ID of who they are paired with. This reduces the scope for signalling and reputation building. However, it is quite possible that the dynamics of a session which contains 16 subjects (8 pairs) may well be different from that which contains 24 subjects (12 pairs). We looked carefully at the patterns of sending and receiving across sessions within the same treatment and we did not find significant differences in these and therefore we feel pooling the data from the sessions within each treatment is a reasonable approach.

Table 1
Details of experimental design for both Studies 1 and 2.

Treatments	Private knowledge	Common knowledge	Context neutral	Context loaded A	Context loaded B
Details of study 1					
Number of sessions	4	4	4	4	4
[Participants per session]	[20, 20, 20, 22]	[16, 24, 18, 20]	[18, 16, 18, 16]	[18, 24, 20, 22]	[16, 16, 20, 18]
Total Participants	82	78	68	84	70
Number of sender-receiver pairs	41	39	34	42	35
Details of study 2					
Number of sessions	–	–	3	–	–
[Participants per session]			[22, 18, 16]		
Total Participants	–	–	56	–	–
Number of sender-receiver pairs	–	–	28	–	–

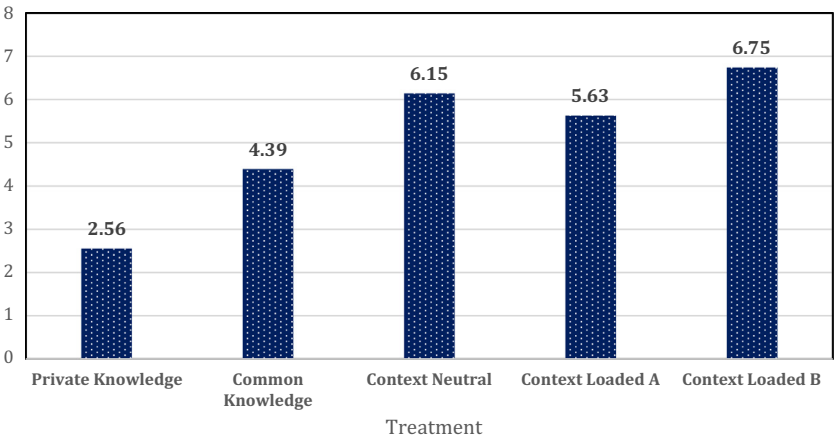

Fig. 1. Average amount transferred aggregated over all rounds across treatments.

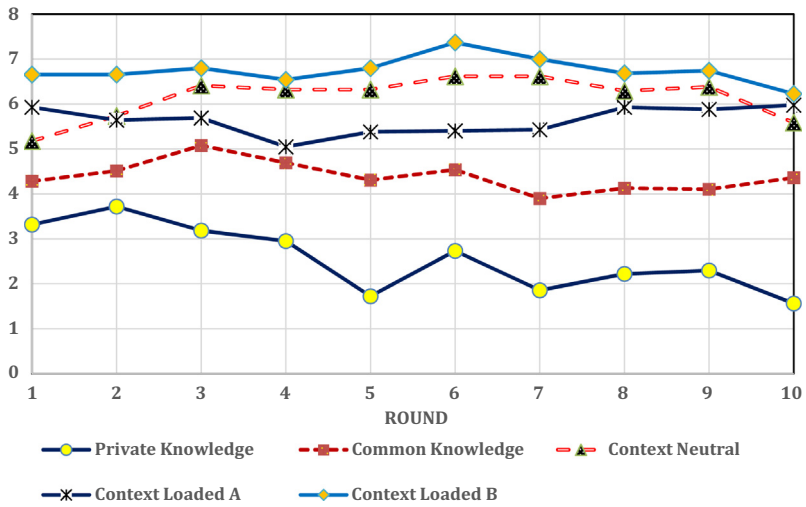

Fig. 2. Average amount sent over 10 rounds by treatment.

Table 2
Pair-wise Wilcoxon ranksum tests for amount sent between treatments in Round 1 (Study 1).

	Common knowledge (n = 39)	Context neutral (n = 34)	Context-loaded A (n = 42)	Context-loaded B (n = 35)
Private knowledge (n = 41)	z = 1.57	z = 2.86	z = 3.22	z = 4.56
	p = 0.17	p = 0.00	p = 0.00	p = 0.00
Common knowledge (n = 39)	–	z = 1.45	z = 2.04	z = 3.4
		p = 0.15	p = 0.04	p = 0.00
Context Neutral (n = 34)			z = 1.05	z = 2.26
	–	–	p = 0.29	p = 0.02
Context-loaded A (n = 42)				z = 0.89
	–	–	–	p = 0.37

Table 3
Regression results for amount sent (Study 1).

Amount sent	Random effects		Tobit	
Round	−0.0406**	−0.199***	−0.124**	−0.529***
	(0.019)	(0.040)	(0.056)	(0.126)
Common knowledge	1.834***	1.041	3.878***	2.143**
	(0.644)	(0.717)	(0.507)	(1.063)
Context neutral	3.591***	2.201***	6.842***	3.413***
	(0.667)	(0.744)	(0.532)	(1.104)
Context loaded A	3.075***	1.831***	5.992***	2.852***
	(0.632)	(0.704)	(0.509)	(1.060)
Context loaded B	4.193***	3.160***	7.581***	4.838***
	(0.662)	(0.738)	(0.528)	(1.100)
Round ∗ common knowledge		0.144**		0.324*
	–	(0.058)	–	(0.175)
Round ∗ context neutral		0.253***		0.635***
	–	(0.060)	–	(0.181)
Round ∗ context loaded A		0.226***		0.583***
	–	(0.056)	–	(0.175)
Round ∗ context loaded B		0.188***		0.509***
	–	(0.059)	–	(0.180)
Constant	2.779***	3.652***	1.086**	3.262***
	(0.461)	(0.501)	(0.472)	(0.756)
R^2	0.148	0.152	–	–
Log Likelihood	–	–	−4020.499	−4012.077
Wald χ^2	56.42	80.10	–	–
Prob > χ^2	0.000	0.000	–	–
Observations	1910	1910	1910	1910
Left-censored observations	–	–	442	442
Uncensored observations	–	–	954	954
Right-censored observations	–	–	514	514

Standard errors are presented in parentheses.
* Denote significance at 10% level.
** Denote significance at 5% level.
*** Denote significance at 1% level.

ond, we present results from a Tobit model, which may be more appropriate given that the dependent variable is bounded by ten from above and zero from below and there is a significant proportion of choices at the two extremes.

For each of the two regressions we present two specifications. The first one is the simplest specification which includes round and four treatment dummies with the *Private knowledge* treatment as the reference category; in the second specification we also include interaction terms between round and the treatment dummies to identify any underlying dynamics in the amount sent over time. We choose to cluster errors on individuals rather than sessions or both because in each treatment we have only four sessions. Given the relatively small number of sessions (20 sessions over five treatments) clustering on sessions would lead to less precise estimates. Moreover, given the random re-matching of senders and receivers from one round to the next we expect within session correlation between individuals to be limited. Therefore we argue that clustering on individuals is the appropriate approach in this case.

The regression results show that the amount sent in the three *Context* treatments are significantly higher than that sent in the *Private knowledge* treatment. The *Common knowledge* dummy is positive and significant in three out of four cases; the only exception is in the case of the random effects regression where we include the interaction terms involving round and the treatment dummies. If we focus on the simpler specification in each case (i.e., the one with only the treatment dummies) then a Wald-test rejects the null hypothesis of equality between the *Common knowledge* and three *Context* dummies

both for the random effects regression ($\chi^2 = 13.58$, $p < 0.01$) as well as the Tobit regression ($\chi^2 = 19.62$, $p < 0.01$) suggesting that amount sent in each of the three *Context* treatments exceeds that sent in *Common knowledge*.[11]

The coefficient for round is negative and significant suggesting that the amount sent decreases over time. However, if we look at the specifications involving the interactions between round and the treatment dummies, then the results suggest that compared to *Private knowledge*, the rate of decay is slower in the *Common knowledge* and *Context* treatments. The preponderance of the evidence here then implies that subjects in the three *Context* treatments sent the most followed by *Common knowledge* and then the *Private knowledge* treatment with negligible differences between the *Context* treatments. These findings are consistent with the patterns depicted in Fig. 2.

3.1.2. Result 2: Average earnings are significantly higher in the Context treatments

As mentioned above, the bulk of prior studies report that on average trust does not pay. So the next question is: are there extra returns to trust or not? To answer this we now focus on how much senders earned in each of the five treatments. The average earnings in the different treatments are respectively \$9.24 in *Private knowledge*, \$10.10 in *Common knowledge*, \$11.27 in *Context neutral*, \$10.78 in *Context-loaded A* and \$10.89 in *Context-loaded B*. So, on average, trust did not pay except in the *Context* treatments, where, on average, returns ranged from 8% to 13%. This result is similar to that in social history treatment of the Berg et al. study where participants earned a return of 10% on average. However, as we show below our results vis-à-vis earnings are more striking than those in Berg et al. (1995).

In Table 4, we use Wilcoxon ranksum tests to compare cumulative earnings at the end of ten rounds of play for each subject as our unit of observation. This gives us 41, 39, 34, 42, and 35 for the *Private knowledge*, *Common knowledge*, *Context-Neutral, Context-loaded A* and *Context-loaded B* treatments respectively. The results suggest that compared to *Private knowledge*, earnings are higher in all the other treatments. Furthermore, earnings in *Context-Neutral* are higher than those in *Common knowledge*.

The non-parametric tests presented above do not control for covariates and so in Table 5 we use random effects regression with robust standard errors clustered on individuals. The dependent variable here is earnings per round in each treatment. Independent variables include: (1) *round*, (2) four treatment dummies, with the *Private knowledge* treatment as the reference category, and (3) *amount transferred* in a particular round. The coefficients of the three *Context* treatment dummies are highly significant while that of the *Common knowledge* treatment is only marginally significant (at 10%). Furthermore joint Wald tests suggest that the earnings are higher in the *Context* treatments compared to *Common knowledge* treatment. This corroborates the evidence provided by the simple averages above that the earnings are higher in the three *Context* treatments compared to *Common knowledge* as well as *Private knowledge*.

An alternative way of looking at potential earnings is simply to ask what the modal transfer was in each treatment. This happens to be \$0 for *Private Knowledge* with nearly 50% of all transfers in this treatment over the ten rounds corresponding to this amount. On the other hand the modal transfer is \$10 for the three *Context* treatments with approximately one-third of transfers being this amount in each of the three treatments. For *Common Knowledge* there are two modes – one at \$0 and another at \$10 with 22% people choosing one of those two amounts.

Clearly, sending nothing means that the sender earns \$10 by keeping the entire endowment. However, aggregated over the five treatments, the average earning when sending all \$10 is \$11.62, a return of 16.2%. However there are wide variations in these earnings. For *Private knowledge*, sending all \$10, results in a large loss with an average return of \$6.54. There is an approximately 9% return in *Common knowledge* with average returns of \$10.87. But in the three *Context* treatments the returns are much larger with average returns of \$12.84 (28%) in *Context-Neutral*, \$12.52 (25%) in *Context-Loaded A* and \$11.41 (14%) in *Context-Loaded B*. Table 6 summarizes this information.

These returns to trust when the underlying contingencies implicit in the transaction are made clear to subjects in the three *Context* treatments are statistically and economically larger and more dramatic than those in the Berg et al. "social history" treatment. Furthermore, it is noteworthy that providing context leads to a pronounced shift in the amount transferred towards \$10 and this in turn generates returns to the tune of 14–28%.

3.2. Receiver behavior

3.2.1. Result 3: The average proportion returned increases with context

In this section we investigate the effect of information that subjects receive on reciprocity. To do this we look at the proportion returned by the receivers out of the amount sent to them. Fig. 3 shows the average proportion returned across the different treatments. At 38%, the proportion returned is highest in *Context Neutral* treatment, followed by 37% in *Context-loaded B* treatment, 36% *in Context-loaded A* treatment, 32% in *Common knowledge* and 24% in *Private knowledge*.

Readers would immediately appreciate what these differential returns tell us about the prospect of sender earnings. Clearly receivers need to return at least a third of the amount sent to them by the senders in order to make the sender

[11] When we carry out pair-wise Wald tests comparing the three *Context* dummies we do not get significant differences except for one instance. The *Context-loaded B* dummy is significantly different from the *Context-loaded A* dummy for the simple Tobit model (without treatment-round interactions) suggesting that amount sent in *Context-loaded B* is greater than that in *Context-loaded A* in the simple Tobit model. None of the other tests return significant differences regardless of whether we include the interaction terms or not. This suggests that by and large amounts sent in the three *Context* treatments are not different from one another. We have omitted the details.

Table 4
Pair-wise Wilcoxon ranksum tests for cumulative earnings between treatments (Study 1).

	Common knowledge (n = 39)	Context neutral (n = 34)	Context-loaded A (n = 42)	Context-loaded B (n = 35)
Private knowledge (n = 41)	z = 2.49 p = 0.01	z = 4.76 p = 0.00	z = 2.96 p = 0.00	z = 3.52 p = 0.00
Common knowledge (n = 39)	–	z = 2.61 p = 0.01	z = −0.68 p = 0.51	z = 1.59 p = 0.11
Context Neutral (n = 34)	–	–	z = −1.64 p = 0.10	z = −0.73 p = 0.46
Context-loaded A (n = 42)	–	–	–	z = 0.76 p = 0.45

Table 5
Random effects regression on senders' earnings per round (Study 1).

Earnings	Random effects
Round	−0.0805**
	(0.037)
Common Knowledge	0.764*
	(0.423)
Context Neutral	1.851***
	(0.450)
Context Loaded A	1.387***
	(0.424)
Context Loaded B	1.440***
	(0.453)
Amount transferred	0.0493
	(0.034)
Constant	9.559***
	(0.369)
Observations	1910
R^2	0.0283

Standard errors are presented in parentheses.
* Denote significance at 10% level.
** Denote significance at 5% level.
*** Denote significance at 1% level.

Table 6
Modal transfers, earnings and returns across different treatments (Study 1).

Treatments	Private knowledge	Common knowledge	Context neutral	Context loaded A	Context loaded B
Modal transfer	$0	($0) [$10]	$10	$10	$10
Percentage of total transfers	50%	(22%) [22%]	33%	39%	32%
Average earning from modal transfer	$10	($10) [$10.87]	$12.84	$12.52	$11.41
Returns to trust	0%	(0%) [9%]	28%	25%	14%

no worse off than he would be if he simply hung on to his initial endowment. This is true only in the three *Context* treatments. Fig. 4 shows the average percentage returned over the ten rounds of each treatment. We see that, on average, the percentage returned is higher in each of ten rounds for the three *Context* treatments than the *Private knowledge* treatment.

In Table 7, we present results of pairwise Wilcoxon ranksum tests for the equality in distributions. Once again we look at the proportions returned in round 1 only since these are the only independent observations. The results suggest that the proportioned returned are significantly higher in both *Common knowledge* and all three *Context* treatment compared to *Private knowledge*.

Next we corroborate these differences in the proportion returned using random effects regression with standard errors clustered on individuals. Table 8 presents these regression results. In the first model, independent variables include (1) round, (2) amount received by the receiver in that round prior to making a decision about how much to send back; (3) four treatment dummies, with the *Private knowledge* treatment being the reference category. In a second model we add interaction terms involving round interacted with the four treatment dummies. Results suggested that compared to *Private knowledge* proportion returned is not higher in *Common knowledge* but it is indeed higher in the *Context* treatments. The difference is particularly pronounced for *Context Neutral* where the dummy is significant at 5% or better while the coefficients for the other two *Context* treatments are significant at least at 10% if not better. Once again the conclusion is that the *Context* treatments lead to higher levels of reciprocity, which, coupled with their higher levels of trust, result in higher earnings in these treatments.

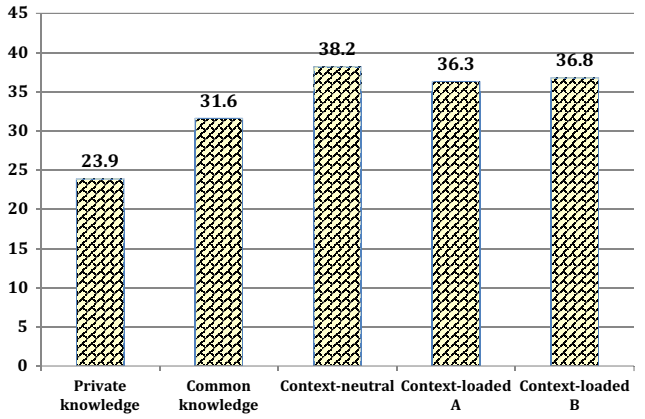

Fig. 3. Average percentage returned aggregated over 10 rounds across treatments.

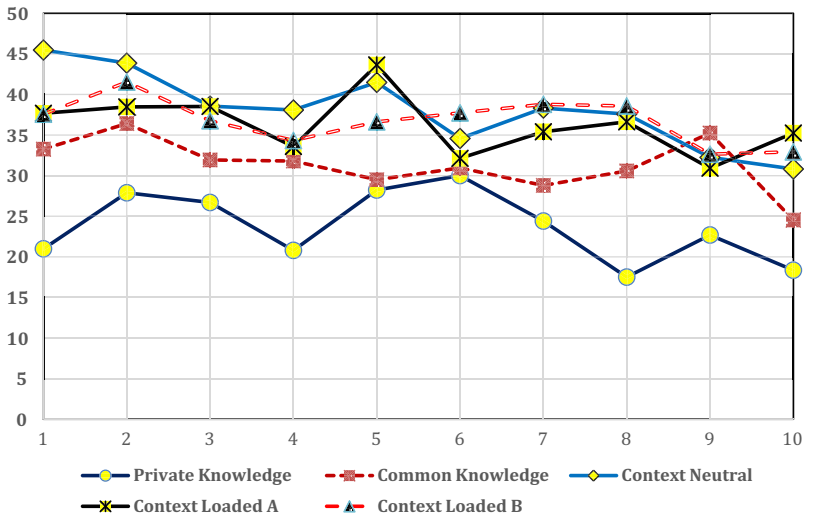

Fig. 4. Average percentage returned over 10 rounds across treatment.

Table 7
Pair-wise Wilcoxon ranksum tests for proportion returned in round 1 (Study 1).

	Common knowledge (n = 35)	Context neutral (n = 32)	Context-loaded A (n = 37)	Context-loaded B (n = 34)
Private knowledge (n = 32)	z = 2.25 p = 0.02	z = 4.09 p = 0.00	z = 2.57 p = 0.01	z = 2.89 p = 0.00
Common knowledge (n = 35)	–	z = 2.26 p = 0.02	z = 0.73 p = 0.47	z = 0.93 p = 0.35
Context Neutral (n = 32)	–	–	z = −1.47 p = 0.14	z = −1.24 p = 0.22
Context-loaded A (n = 37)	–	–	–	z = 0.26 p = 0.8

3.3. Results from Study 2

In Study 1 we found that the levels of trust and reciprocity were significantly higher when the conflict between the self-interested outcome and the social optimum was made clear to subjects. This, in turn, leads to considerable extra returns to trust, particularly in the three context treatments. Given this finding, in Study 2, we wanted to see if trusting decisions, measured by amount sent in the *Context* treatments, correlate better with trusting decisions made via responses in psychological questionnaires. As noted in the introduction, prior studies usually find little correlation between the decisions made by the sender in the trust game and their responses to questions regarding trust in survey questionnaires. Below, we show that once provided context, the senders who appear as high trustors using the SVO questionnaire, also send larger amounts in the trust

Table 8
Random effects regression on proportion returned (Study 1).

Proportion returned	Random effects regression	
Round	−0.00994***	−0.00831**
	(0.002)	(0.004)
Amount received	0.0108***	0.0109***
	(0.002)	(0.002)
Common knowledge	0.0549	0.0749
	(0.048)	(0.054)
Context neutral	0.126**	0.148***
	(0.050)	(0.056)
Context loaded A	0.0905*	0.0942*
	(0.048)	(0.054)
Context loaded B	0.1031**	0.0954*
	(0.050)	(0.055)
Round ∗ common Knowledge	–	−0.00406
		(0.005)
Round ∗ context Neutral	–	−0.00431
		(0.005)
Round ∗ context Loaded A	–	−0.000895
		(0.005)
Round ∗ context loaded B	–	0.00122
		(0.005)
Constant	0.237***	0.229***
	(0.035)	(0.040)
Wald χ^2	93.48	95.55
Prob > χ^2	0.0000	0.0000
Observations	1468	1468

Standard errors are presented in parentheses.
* Denote significance at 10% level.
** Denote significance at 5% level.
*** Denote significance at 1% level.

game. This implies a strong positive correlation between their dispositions to trust using either the amount sent in the modified trust game or their responses on the SVO questionnaire.

In Study 1, we found that while providing context for the game led to greater trust on the part of the senders, differences in behavior in the three context treatments were negligible. Therefore, as explained in the experimental design above, in Study 2 we run 3 more sessions with 56 subjects (28 sender-receiver pairs), except here the subjects take part in the Context-Neutral treatment only.[12] Upon completing the ten rounds of the trust game, subjects are asked to fill out the demographic questionnaire and the SVO questionnaire.

3.3.1. Result 4: There is significant correlation between amount sent (which is a proxy for trust) in the Context-Neutral trust game treatment and the level of trust elicited via SVO questionnaire

In Study 2, we have ten decisions for amount sent for each of the 28 subjects; so a total of 280 observations; while we have 28 observations for the SVO score, one for each of 28 subjects. As noted above, it is not appropriate to treat the amount sent in each of ten rounds as independent observations. Therefore, in what follows, we use two approaches: (1) look at round 1 decisions only, since these are truly independent observations. (2) Look at the average amount passed by each of 28 subjects over the ten decision rounds. This latter may also be treated as an independent observation. In both cases we have 28 independent observations.

We start by computing the correlation coefficient between amount passed and the SVO score. In the case of round 1 decisions only, this is positive but narrowly misses conventional levels of significance. (Spearman's rho = 0.298; n = 28; p = 0.12) However, if we look at the correlation coefficient between average amount sent by each subject over ten rounds and the SVO score then this is significant at 7%. (Spearman's rho = 0.346; n = 28; p = 0.07). An alternative way to approach the same question is to undertake a median split for the SVO scale and look at the behavior of those who score above or below the median. The median SVO score for the senders in our study is 11.5 (out of 25) with 14 subjects above and 14 below. If we look at round 1 only, then we find that on average those below the median transferred $4.57 while those above the median transferred $6.86. This is significant at 8% using a *t*-test (t = 2.285; p = 0.08) but narrowly missed conventional significance levels using a non-parametric Wilcoxon ranksum test. (z = 1.546; p = 0.12). However, if we look at the average amount sent, aggregated over ten rounds, then we find that on average those below the median sent $2.49 while those above the median sent $5.49, a difference that is significant at the 5% level using either a *t*-test (t = 2.55, p = 0.047) as well as a non-parametric Wilcoxon rankrum test. (z = 2.001, p = 0.045).

[12] The argument is that any correlation picked up by this treatment, which does not use words like "trust", will also be picked up by the other two context treatments which use "more loaded language".

Table 9
Regression results for amount sent (Study 2).

Amount sent	Random effects		Random effects tobit	
Round	−0.227***	−0.227***	−0.780***	−0.799***
	(0.057)	(0.057)	(0.178)	(0.177)
SVO score	*0.396**	*0.470***	*1.152**	*1.431***
	(0.212)	*(0.220)*	*(0.637)*	*(0.658)*
Gender	–	−1.525	–	−5.549
		(1.322)		(3.934)
Constant	0.958	0.714	−5.334	−6.288
	(2.512)	(2.506)	(7.475)	(7.346)
Observations	280	280	280	280
Wald χ^2	19.62	21.00	22.61	24.15
p-value	0.00	0.00	0.00	0.00
Log likelihood	–	–	−417.568	−416.565
Left censored observations	–	–	111	111
Uncensored observations	–	–	89	89
Right censored observations	–	–	80	80

Standard errors are presented in parentheses.
* Denote significance at 10% level.
** Denote significance at 5% level.
*** Denote significance at 1% level.

In Table 9, we use regression analysis to demonstrate the correlation between amount sent and the SVO score. The dependent variable is the amount sent. As in Table 3 of Study 1, we present results for random effects regression and random effects Tobit - with two specifications in case. The first specification includes round and the SVO score while the second also controls for the gender for the sender. It is clear that the SVO score is positive and significant (either at 5% or at 10%) in all specifications suggesting that those who score high on the SVO scale also send more money as the sender in the trust game with context.

Turning to the decision to reciprocate trust, the SVO score did not correlate with the decision to reciprocate, whether we use non-parametric tests or regression analysis. This implies that a higher SVO score did not translate into a higher proportion returned (even after controlling for amount received). Aksoy, Eckel, Harwell, and Kovaliukaite (2015) report a similar finding using the GSS trust question; they do not find a correlation between responses to this question and a participant's degree of trustworthiness. However, the decision to reciprocate is, at heart, a pie-splitting task that is more closely related to notions of altruism and does not have strategic implications. Chaudhuri, Sopher, and Strand (2002) argue that trust and trustworthiness are fundamentally different constructs. Therefore, it may not be surprising that the SVO score, which is designed to measure trust, does not do a good job of predicting reciprocity.

Furthermore, Chaudhuri, Khan, Lakshmiratan, Py, and Shah (2003) document that the relationship between trust and trustworthiness is nuanced. In their study using a binary trust game, the authors find that those who score above the median in the SVO scale are both trusting and trustworthy; while the behavior of those, who score below the median, is less consistent. However, in Chaudhuri et al. (2003), each subject played both as the sender and the receiver, which allowed for highlighting this result. The design of the current study, where subjects play only one role, either sender or receiver, renders studying this infeasible.

4. Conclusion

In this study, we explore five different ways of presenting the Berg et al. (1995) trust game. It can be argued that each of the five is a valid way of conducting the game. Yet, we find dramatic and interpretable framing effects on behavior. Using "goal framing", which clearly articulates the conflict between individual self-interest and the social optimum, leads to significantly greater levels of trust and reciprocity. With such framing, the modal amount transferred is the entire endowment of $10 and doing so generates returns to the tune of 14–28%. It appears that explicit use of loaded terms, such as "trust" and "trustworthiness", is not essential; provided the right framing, subjects seem quite capable of inferring the strategic imperatives, even without resorting to emotive words. Finally, amounts transferred in the game with context are significantly positively correlated with trust measured using the SVO scale. Social scientists from diverse backgrounds are increasingly using experimental games to study questions of social capital and related issues. The evidence presented here suggests that researchers studying issues related to social preferences should pay attention not only to structural features of the game, but also to its presentational aspects. As Cookson (2000) argues, there may potentially be interaction between presentational and structural variables, with the latter having a smaller, or possibly even opposite effect (as in Cronk's (2007) "*osotua*" frame) under one frame than another.

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Appendix A

Participant ID:
Password:

Instructions

General Instructions

Welcome. The University of _____ has provided funding in order to conduct this research. The instructions are simple. If you follow them closely and make appropriate decisions, you may make an appreciable amount of money. For this experiment all earnings are denoted in experimental dollars. At the end of the session you will be paid your earnings in cash at the rate of 10 experimental dollars = NZ $1. This money is in addition to the $5 show-up fee that you get.

The experiment will be conducted using computers. In a minute we will give you the instructions for logging in to the experiment.

Please do not talk at any point during the experiment. If you have any questions then please raise your hand and one of us will come to you to answer it.

Specific instructions

This experiment consists of ten rounds. In each round, each of you will be matched with another participant in the room. The person that you are matched with will change from one round to the next, i.e., you will not be matched with the same person for all rounds. You will not learn the identity of the person you are matched with at any time.

One of you will be designated as the **sender** and the other one as the **receiver**. The decisions that you and the other person make will determine the amounts earned by each of you.

At the beginning of each round both the sender and the receiver will have $10.00.

The sender will decide how much money (if any) out of his $10.00 to pass on to the receiver and how much (if any) to keep. Any amount passed gets tripled (multiplied by 3) before it is received by the receiver, who then decides how much (if any) to keep and how much (if any) to pass back to the sender.

The sender earns the amount kept initially (out of his $10.00) plus any amount that the receiver passes back.

The receiver keeps any amount not returned. (This is in addition to the $10.00 given to the receiver at the beginning of the round). Any amount returned by the receiver is not tripled. The game ends after the receiver's decision.

You will be able to read the specific instructions once more after you log in to the computer.

In the Context-neutral treatment, the following two paragraphs are added to the instructions at this point:

One way to think about this situation is as follows: the receiver has no incentive to send any money back to the sender because the round ends immediately after that. Anticipating that, the sender should hang on to his $10.00 and send nothing to the receiver. This means they will both end the round with $10.00 each.

But suppose the sender decides to transfer $10.00 to the receiver. Then the receiver will get $30.00. If the receiver sends back an amount more than $10.00 then it is easy to see that both the sender and the receiver can make more money than if they simply hung on to their $10.00 in each and every round.

In the Context-loaded A treatment the following two paragraph are added to the instructions at this point:

One way to think about this situation is as follows: the receiver has no incentive to send any money back to the sender because the round ends immediately after that. Anticipating that, the sender should hang on to his $10.00 and send nothing to the receiver. This means they will both end the round with $10.00 each.

But suppose the sender decides to trust the receiver by sending $10.00. Then the receiver will get $30.00. If the receiver behaves in a trustworthy manner and sends back an amount more than $10.00 then it is easy to see that both the sender and the receiver can make more money than if they simply hung on to their $10.00 in each and every round.

In the Context-loaded B treatment the following paragraph is added to the instructions:

One way to think about this situation is as follows: suppose the sender decides to trust the receiver by sending $10.00. Then the receiver will get $30.00. If the receiver behaves in a trustworthy manner and sends back an amount more than $10.00 then it is easy to see that both the sender and the receiver can make more money than if they simply hung on to their $10.00 in each and every round.

But the receiver has no incentive to send any money back to the sender because the round ends immediately after that. Anticipating that, the sender should hang on to his $10.00 and send nothing to the receiver. This means they will both end the round with $10.00 each.

Are there any questions?

Please log on to the computer using the login instructions given to you.

Login instructions

- Login to the computer (using your user name and password).
- Check that you are logged in to your Net Account.
- Open Internet Explorer.
- Enter the following web address and press enter:
  http://veconlab.econ.virginia.edu/login.htm.
- The "Veconlab Participant Login Screen" screen should be displayed.
- Click on 'Login'.
- The 'Veconlab: Enter Session Name' screen should be displayed.
  Enter the Session Name: aicXX. Click on 'Submit'.
- The 'Veconlab Participant Login' screen should be displayed.
  Fill in the boxes. Click on 'Continue'.
- The computer will assign you a Participant ID Number. Please write down your ID number and Password at the top of the page of your instructions in the space provided. It is important that you remember the password! This password will help us to go back and retrieve your data should you happen to close the browser window by mistake during the session.
- Please follow the instructions displayed on screen.

Appendix B

Demographic questionnaire (handed out at the conclusion of the experiment)

Please answer ALL of the questions on this survey as accurately as you can.

1. What is your Age? _____

2. What is your Gender? _____

3. What is your field of study at the University? _____

4. Are you an Undergraduate Student (which year) or a Postgraduate Student?

UG (Year _____), PG

8. What is your after-tax weekly income from ALL SOURCES (including salary, allowances & scholarships)?

- Less than $250
- Between $250 and $750
- Between $750 and $1,250
- More than $1,250

7. Were you born in New Zealand? YES NO

8. Which ethnic group do you belong to?

- New Zealand European
- Maori
- Samoan
- Cook Island Maori
- Niuean
- Chinese
- Indian Subcontinent (including Pakistan and Bangladesh)
- Other (Please specify) _____

Appendix C. SVO questionnaire

Participant ID:

Please answer ALL of the questions below by selecting one of the five options listed below each question. Please put a circle around your preferred answer.

Question 1: Most people tell a lie when they can benefit by doing so.

Strongly Disagree	Disagree	Neutral	Agree	Strongly Agree

Question 2: Those devoted to unselfish causes are often exploited by others.

Strongly Disagree	Disagree	Neutral	Agree	Strongly Agree

Question 3: Some people do not cooperate because they pursue only their own short-term self-interest. Thus, things that can be done well if people cooperate often fail because of these people.

Strongly Disagree	Disagree	Neutral	Agree	Strongly Agree

Question 4: Most people are basically honest.

Strongly Disagree	Disagree	Neutral	Agree	Strongly Agree

Question 5: One should not trust others until one knows them well.

Strongly Disagree	Disagree	Neutral	Agree	Strongly Agree

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