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Omar Al-Ubaydli, Daniel Houser, John Nye, Maria Pia Paganelli, and
Xiaofei (Sophia) Pan

September 2011

Discussion Paper

Interdisciplinary Center for Economic Science
4400 University Drive, MSN 1B2, Fairfax, VA 22030
Tel: +1-703-993-4719 Fax: +1-703-993-4851
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The causal effect of market participation on trust: An experimental investigation using randomized control

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Abstract

In randomized control laboratory experiments, we find that those primed to think about markets exhibit more trusting behavior. We randomly and unconsciously prime experimental participants to think about markets and trade. We then ask them to play a trust game involving an anonymous stranger. We compare the behavior of these individuals with that of a group who are not primed to think about anything in particular. Priming for market participation affects positively the beliefs about the trustworthiness of anonymous strangers, increasing trust.

JEL codes: D02, D23, D64, D84, O12, O43, P10

Keywords: trust; markets; institutions; belief; priming

¹ Al-Ubaydli (corresponding author), Houser, Nye and Pan: Department of Economics, George Mason University; Paganelli: Department of Economics, Trinity University.

1. Introduction

We report data from a laboratory experiment that suggests participation in markets causes trusting behavior to increase. We say that trust is present when one party (sender) places resources at the disposal of another party (responder) under the expectation that this will increase the sender's payoff, and in the absence of any legal commitment by the responder. An essential feature of trust is that the sender must feel vulnerable in a way that is not captured purely by probabilistic risk (Fehr 2009).

The positive effects of trust on economic growth are well documented (Knack and Keefer 1997, Zak and Knack 2001, Aglan and Cahuc 2010, and North 1990), while the effect of formal institutions – including markets – on trust has generated less consistent findings. In his investigation of the effect of markets and competitiveness on morality, Chen (2010) highlights the diverse views in the literature: on the one hand, commerce leads to more gentle manners (Montesquieu 1989), it cordializes mankind (Paine 1984), and enhances man's virtues (Smith 1984, Hume 1985; see also Bowles 2011). On the other hand, the competitive instinct degrades judgment (Veblen 1994) and it undermines society's moral foundations (Marx 2000). Additionally, markets hurt altruism and cooperation (Bowles 1998) and formal institutions, such as markets, have adverse effects on informal institutions and social norms (Frey and Oberholzer-Gee 1997, Deci et al. 1999, Falk and Kosfeld 2006).

Existing empirical research has not resolved these contradictions. Henrich et al.'s (2001, 2004, 2010) study of small-scale societies suggests that exposure to markets increases the strength of other-regarding preferences yet has little to no effect on cooperation. In other laboratory studies, Herrmann et al. (2008) find that cooperation is enhanced by exposure to markets, yet Reeson and Tisdell (2010) find the opposite. Using macro data, Zak and Knack (2001) find a strong relationship between the incidence of markets/formal institutions and generalized trust. In most of the empirical studies, exposure to markets or other formal institutions is naturally-occurring rather than randomly assigned by the investigator, a point that was strongly made by Zak and Knack (2001). This may help to explain the conflicting results.

To circumvent problems of endogeneity, we employ randomized control.² Our design consists of randomly priming participants, without their awareness, to think about markets and then having them play a simplified version of the trust game (Berg et al. 1995). We find a positive effect of market-priming on the amount that senders send to their anonymous partners. Using a Cox (2004) decomposition, we offer evidence that this increase is a consequence of increased trust rather than the increased strength of other-regarding preferences.

We explain our results by synthesizing and extending the extant theory linking markets to trust. In the absence of markets, interactions with anonymous strangers are characterized by danger, exploitation and mistrust (Henrich et al. 2010), and are therefore avoided. Markets open the door to fruitful

² While we are not the first to employ randomized control, we point out in section 2 that our design offers several key advantages over preceding efforts.

exchanges with strangers (North 1990), increasing the likelihood that people believe a stranger behaves positively. The result is enhanced trust even in the settings where there is no ability to punish defectors.

North (1990) argues that the proliferation of formal institutions helped move society from narrowly personal to more broadly anonymous exchange and thereby helped launch the remarkable economic growth witnessed since the 18th century. As institutions improved generalized trust in commerce could arise as a byproduct of clear rules, greater competition, and enhanced enforcement. But most existing work on institutional change has focused on the direct, incentive-based effect of these improved rules. Our results suggest a different and indirect mechanism that reinforces this direct institutional effect.

2. Background and motivating theory

Consider the simplified trust game (henceforth STG) shown in Figure 1, which is played with an anonymous partner. The sender starts with \$8 and can choose to send \$0, \$2, \$4 or \$6 to the responder. Any amount sent is tripled before the responder receives it. Upon seeing the sender's choice, the responder chooses how much of the tripled amount to return to the sender, keeping the rest for herself.

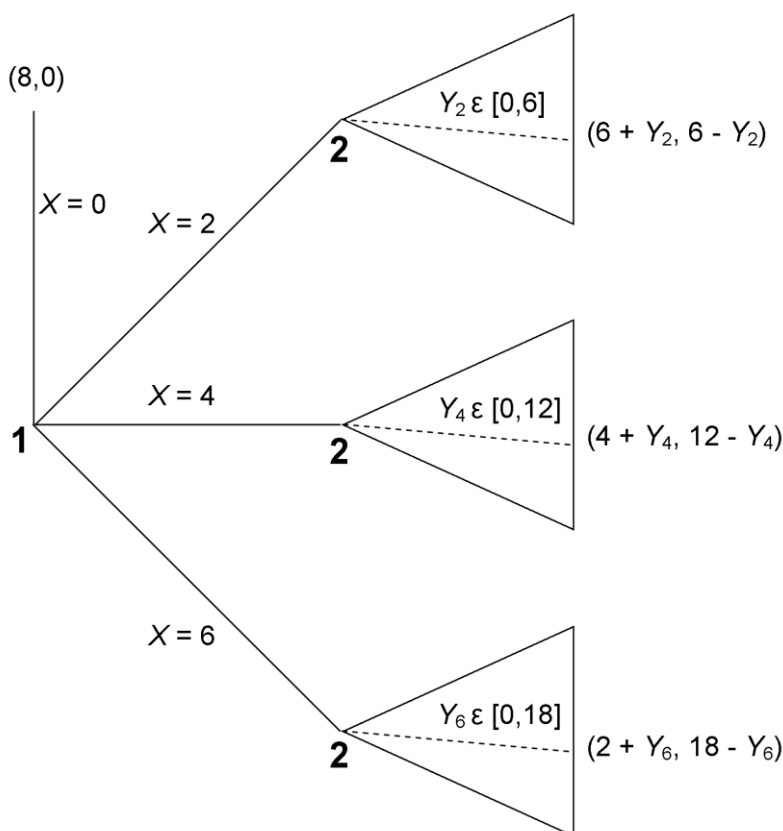


Figure 1: Simplified trust game

Player 1 = Sender, Player 2 = Responder

In the unique subgame perfect Nash equilibrium the sender will send nothing. This is robust to any intervention that does not alter the game's payoffs and information structure. This model predicts zero trust and no causal effect of market participation on trust is possible. Yet the observed data is regularly inconsistent with this model (e.g., Berg et al. 1995). More refined alternatives take into account, for example, other-regarding preferences (Fehr and Schmidt 1999, Dufwenberg and Kirchsteiger 2004) and context dependence (Burnham et al. 2000), opening the door for a model that predicts a causal effect of markets on trust.

The STG can be interpreted as an allegory of a positive-sum interaction with an anonymous stranger. During most of our evolutionary history, transactions with people beyond the local group have been full of danger, mistrust and exploitation (Henrich et al. 2010). Humans are swift to form beliefs (especially concerning outgroups), and they are remarkably adept at basing the beliefs on observed correlations (Hilton and Hippel 1996). Thus in any society characterized by limited interactions with anonymous strangers, individuals believe the outcome of interacting with a stranger will be negative, inducing the decision-maker to be cautious and refrain from exposing herself to adverse outcomes.

On the other hand, extending market transactions permits value-adding interactions between anonymous strangers (North 1990 and Henrich et al. 2005, 2010). Thus with the proliferation of markets, individuals believe the outcome of interacting with a stranger will be positive, eliciting the decision-maker to believe the anonymous stranger can be trusted. Decision-makers therefore will trust anonymous strangers more. This is not to say that individuals start treating anonymous strangers as intimate friends; rather that they treat them with less distrust than they would in the absence of market transactions.

Our main hypothesis is that markets increase trust by increasing how much people believe anonymous strangers are worthy of trust.

The trust game yields information not just on trust but also on trustworthiness: whether a responder deserves the trust of a sender. Thus the trust game yields information also on whether the belief about the anonymous stranger is correct or not. In an environment characterized by infrequent interactions with anonymous strangers, the danger of being taken advantage, if trusting, is real. The anonymous stranger is not worthy of trust. Believing the anonymous stranger takes advantage of an unsuspecting trustful other is correct. On the other hand, markets open the door to frequent interactions with anonymous strangers that do not involve skullduggery. The anonymous stranger may now be worthy of the trust of the sender. The belief about the increased trustworthiness of the anonymous stranger is updated accordingly, and correctly so. Believing that the anonymous stranger is trustworthy more likely correct.

Hence we add a corollary to our main hypothesis: markets increase trust by *correctly* eliciting people to expect trustworthiness in anonymous strangers.

Towards an experimental design

The empirical relationship between exposure to markets and trust (and trustworthiness) is widely studied. But the principal difficulty is that the overwhelming proportion of these studies, be they experimental or otherwise, rely on naturally-occurring variation in the main treatment variable – exposure to markets. Micro level studies, such as Henrich et al. (2004) and Herrmann et al. (2008), use aggregate indices of market interactions or the enforcement of rule of law, as do macro level studies such as Zak and Knack (2001). There is full awareness of the potential endogeneity of the treatment variable, as Zak and Knack (2001) summarize:

While these findings on associations between trust and formal institutions and social distance are consistent with our model, they are presented here as preliminary tests that do not fully resolve causality issues. For example, cohesive and trusting societies may more easily agree on an efficient, stable set of property rights, or on policies to reduce inequality and discrimination. Treating formal institutions and social distance proxies as endogenous would require identifying instruments for them that are otherwise unrelated to trust, which is quite difficult. (p. 314)

We employ randomized control to circumvent the problem. Note that the causal mechanism that we are investigating does not operate via any effect on incentives – only via context. In controlled experiments, when manipulating context, experimenter demand effects and contrast effects are concerns (Bargh and Chartrand 2000, Mussweiler and Strack 1999): human targets, who are aware of a manipulation and its goal, may artifactually modify their behavior. Experimental economists are typically unconcerned by such demand effects since they are investigating the causal effect of changes in incentives or other substantial institutional features, changes that are likely to swamp the effect of contextual factors (Smith 1982). Our concerns stem from restricting our focus to payoff-irrelevant features of the environment.

Our manipulation derives from the priming literature (Bargh 2006; see Benjamin et al. 2010 for an example from the economic literature). We lead participants in some treatments to think about markets and trade. Immediately following this, we ask them to play a STG involving an anonymous stranger. We compare the behavior of treated individuals with that of a control group who did not experience such priming. This allows us to capture the effect of markets on the beliefs about anonymous strangers.³ If the market-primed group trusts more, then this is evidence in favor of our main hypothesis that markets have a positive effect on trust.

³ We chose not to measure beliefs directly for several reasons. First, and most importantly, priming effects can evaporate quickly (Bargh 2006), and inserting a belief-elicitation task between the priming and the main task risked attenuating the treatment effect. Second, we were concerned by the possibility of participants hedging against their decisions rather than stating their true beliefs (under incentivized beliefs).

3. Experiment I: Simple trust game

Procedure

Participants were students from George Mason University. Each session had either eight or ten participants performing two tasks. The first was a priming task (For a full discussion of priming techniques, see Bargh 2006). We made a control and treatment group perform a task that differed only in the mental constructs it activated: ‘markets’ in the treatment vs. ‘no coherent theme’ in the control. To avoid experimenter demand effects and contrast effects, participants were unaware that the goal of the priming task was indeed to prime their thinking (Bargh and Chartrand 2000).⁴

The priming was the standard method of a word rearrangement task. Each participant faced 15 lists of five words. In each list, the words were randomly arranged, and the participant needed to form a grammatically correct sentence using four of the five words. For example, in the list <flew, eagle, the, plane, around>, an acceptable solution was <the eagle flew around>. (There were usually multiple possible solutions for each five word list.) The task was uncompensated and participants were given a six minute time limit.

We created a list of words associated with markets and trade using a thesaurus and we validated our choices by asking a separate group of participants to list words that make them think about markets and/or trade (both of which are standard methods in the psychology literature; see the appendix for the full lists). The difference between treatment and control was that in the treatment, 12 of the 15 lists had a word that was relevant to markets and/or trade. For example, the fourth list in the treatment was <open, shop, was, the, noon>, while the corresponding list in the control was <open, park, was, the, noon> (underlining added here only to aid exposition). Participants in each session were randomly assigned either treatment or control. To confirm that participants were unaware of the priming, we performed a funneled debriefing at the end of the experiment (see below).

The second task was the STG shown in Figure 1.⁵ Participants were randomized into the role of either the sender or the responder and were anonymously matched with a unique partner. Senders and responders were in the same room and all instructions were read out aloud (see appendix). The responders used the strategy method, where strategies were three-dimensional. Behavior in the game yielded a measure of trust (senders) and trustworthiness (responders; see Experiment II below for a Cox 2004 decomposition).

⁴ Reeson and Tisdell (2010) use competitive vs. regular versions of the public goods game to examine the effect of competitiveness on subsequent contributions in the regular public goods game, and they find a negative effect of competitiveness. Our design differs from their design in that we look at markets in general (rather than competitiveness, which is one aspect of markets); we also use unconscious priming methods to minimize the risk of experimenter demand effects and contrast effects (Bargh and Chartrand 2000).

⁵ We used the STG rather than the conventional trust game because we wanted to use the strategy method for the responders, and so we had to limit the number of their decision nodes. We wanted to use the strategy method to maximize the data acquired from each participant.

In each of the first twelve sessions, following the second task, we randomly selected a couple of participants who were in the treatment and asked them to perform a “funneled debriefing” (from Bargh and Chartrand 2000, see the appendix for instructions). This is a short, verbally-administered survey that investigates the extent to which the participant was aware of the priming and the goals of the experiment. In every case, participants were unaware they were being primed.

Research hypotheses

Let $X \in \{0,2,4,6\}$ denote the sender’s strategy, let $(Y_2, Y_4, Y_6) \in [0,6] \times [0,12] \times [0,18]$ denote the responder’s three-dimensional strategy, and let $D = 0$ under control and $D = 1$ under treatment.

Hypothesis 1a: Senders send more when primed to think about markets: $E(X|D = 1) > E(X|D = 0)$.

Hypothesis 1b: Responders return more at each node when primed to think about markets: $\forall x, E(Y_x|D = 1) > E(Y_x|D = 0)$.

Note that our hypotheses depend only upon the subjects having had substantial market experience; see Henrich et al. (2010) for examples of treatment effects varying across cultures.

Results

We ran 14 sessions of the STG, yielding a total of 130 observations. The main descriptive statistics are in Table 1.

	Sent	Returned if 2 sent	Returned if 4 sent	Returned if 6 sent
Control (n = 31)	2.4 (2.0)	0.65 (1.4)	3.1 (1.9)	5.2 (3.2)
Treatment (n = 34)	3.4 (1.9)	0.85 (1.5)	3.2 (1.8)	5.5 (3.1)

Table 1: Sample means (and standard deviations) for simple trust game

Result 1a: Senders send substantially more when primed to think about markets.

Senders sent an average of 1.0 more under treatment (market prime) than under control. (Recall that the amount sent is tripled.) This difference is large (over half a standard deviation) and it is significant at the $p < 0.05$ level using a t-test or Mann-Whitney test. Using a regression to control for session effects leads to an increase in the magnitude of the treatment effect and a decrease in the p-value ($p < 0.01$).

Result 1b: Responders return negligibly more when primed to think about markets.

At each of their three decision nodes, responders return more under treatment than under control. The difference is small (15%, 5% and 10% of a standard deviation, respectively), and statistically insignificant (all p-values exceed 0.40 regardless of the statistical test employed).

4. Experiment II: Distinguishing trust from other-regarding preferences

In Experiment I we do not equate senders choosing to send more with senders *trusting* more. Cox (2004) shows that in the trust game, sender choices are driven by trust and other-regarding preferences.⁶ Thus while we detect a treatment effect for senders, we need an additional experiment to conclude that it is the result of increased trust rather than an increase in the strength of other-regarding preferences.

Procedure

Experiment II used different participants to Experiment I. Each session had two tasks, the first being identical to the priming task in Experiment I. The second task (Figure 2) was a sender-dictator version of the STG: a task equivalent to the STG except that it is common knowledge that responders have to return exactly 0 at each of their decision nodes.

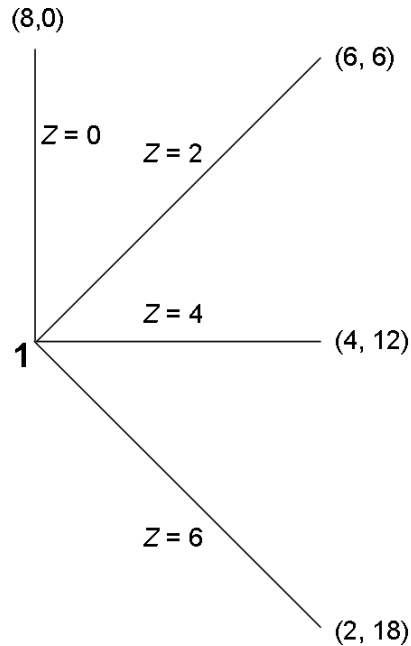


Figure 2: Sender-dictator version of the simplified trust game

Player 1 = Sender, Player 2 = Non-playing responder

Now senders can only be motivated by other-regarding preferences; they face no uncertainty and their payoffs only ever decrease as the amount sent increases. The instructions (see the appendix) mimic Cox (2004) in that they try to keep the language as close as possible to the language in the STG instructions.

Research hypotheses

Let $Z \in \{0,2,4,6\}$ denote the sender's strategy and let $D = 0$ under control and $D = 1$ under treatment.

⁶ Other-regarding preferences include pure altruism, warm glow, inequity aversion etc.

Hypothesis 2: The amount sent by senders is not affected by being primed to think about markets: $E(Z|D = 1) = E(Z|D = 0)$.⁷

If Hypothesis 2 is not falsified, then we can conclude that Result 1a reflects an increase in trust rather than in the strength of other-regarding preferences or some combination.

Results

We ran 9 sessions of the sender-dictator STG, yielding a total of 45 observations. Key descriptive statistics are in Table 2.

	Sent
Control (n = 22)	2.3 (1.7)
Treatment (n = 23)	2.3 (1.7)

Table 2: Sample means (and standard deviations) for sender-dictator simple trust game

Result 2: When senders are motivated exclusively by altruistic concerns, sender behavior is unaffected by being primed to think about markets.

The difference in means is less than 1% of a standard deviation, and any statistical test of the difference yields $p > 0.9$. Thus, when senders send more under market prime in the STG (Result 1a), we can conclude that this is the result of increased trust.^{8,9}

5. Discussion

Using randomized control, we show that exposure to markets improves individuals' beliefs about the trustworthiness of anonymous strangers and therefore increases trust in them.

⁷ Studies such as Henrich et al. (2004) and Herrmann et al. (2008) suggest that market priming may actually increase the strength of other-regarding preferences, and so one could plausibly adjust hypothesis 2 accordingly.

⁸ An alternative way of examining this is to pool the data from the STG and the sender-dictator STG and to run a regression with three main explanatory variables: a sender-dictator dummy, a market prime dummy and an interaction (as well as session dummies). The size and significance of the interaction reflects the importance of changes in trust vis-à-vis other-regarding preferences. Unsurprisingly, executing this regression lends further support to Result 2.

⁹ Despite the statistical insignificance in Result 1b, we also conducted a responder-dictator version of the STG in case changes in other-regarding preference were being offset by changes in trustworthiness. We found that there was no substantial change in either.

We explain this result by arguing that in the absence of markets, interactions with strangers are negative. Market proliferation allows people to get used to the idea of good things happening when they interact with strangers, thus encouraging more trusting behavior. Participation in markets, rather than making people suspicious, actually makes people more likely to trust anonymous strangers. Our results seem therefore to empirically corroborate the idea of *doux commerce* (Hirshman 1977).

In addition to finding a positive effect on trust, we found that market priming generates a negligible positive effect on trustworthiness. This may be due to the strategy method, which in some cases has been shown to attenuate treatment effects (see Brandts and Charness 2010). Alternatively it could be caused by the belief about anonymous strangers affecting trust and trustworthiness differently. Context may affect trust via the belief of the trustworthiness of an anonymous stranger and may affect the trustworthiness of the anonymous stranger via the image that the anonymous stranger has of themselves. It could also be that markets affect trust and trustworthiness through different channels: through markets one learns that anonymous strangers are trustworthy and therefore one becomes more trustful, and through markets one learns that positive reciprocity generates positive payoffs and therefore one becomes more trustworthy. This last hypothesis seems in line with the results of Bicchieri et al. (2011) that trustworthiness may be linked to a social norm of reciprocity, while trust is not. We hope that future research can shed additional light on the difference between trust and trustworthiness (also see Glaeser et al. 2000).

In the aggregate data, economic development is strongly positively associated with the prevalence of markets, and with generalized trust. Incentives and information are generally used to explain why market institutions may have a positive causal effect on development (Smith 1984, Hayek 1945), but do not shed light on the relationship between market institutions and generalized trust. It can be difficult, therefore, to provide reliable policy recommendations. Randomized control, such as we used in this study, increases the reliability of formalizing policy recommendations (Banerjee and Duflo 2009). In particular, our data clarify the causal relationship between markets and trust and may contribute to sound policy recommendation for economic development.

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Appendix: Experimental instructions

There should be an even number of participants. Sign them in and have them fill out the consent forms. Show them to their desks.

Welcome. Today you will be participating in two experiments. We ask that you refrain from any communication with other participants. If you have any questions, please raise your hand.

The first experiment is a study of how people use the English language. Each of you has in front of you a list of 15 sets of words. For each set of words, please make a grammatical four word phrase or sentence and write it down in the space provided. The sentences **MUST HAVE FOUR WORDS**. For example, in the list “flew, eagle, the, plane, around,” one could make the sentence “the eagle flew around.”

Does anybody have any questions? You have 6 minutes. Please begin.

Set	Control	Treatment
1	him loves analyze she to	him loves trade she to
2	went Chicago he always to	went market he always to
3	car drove well he his	car drove well he his
4	open park was the noon	open shop was the noon
5	eat coat to wants he	exchange coat to wants he
6	necklace was beautiful retired the	necklace buy beautiful retired the
7	best music classical is art	best music commercial is art
8	longed croissants for they because	paid croissants for they because
9	is arrangement convenient no dog	is arrangement profitable no dog
10	ball the hoop toss normally	ball the hoop toss normally
11	successful was project hopefully the	successful was deal hopefully the
12	works furniture designing lives she	works furniture selling lives she
13	save does study usually he	save does study usually he
14	sounds must computers should record	transactions must businesses should record
15	lots big stadiums tomorrow have	lots big malls tomorrow have

After 6 minutes, collect the sheets and hand out the instructions for the next experiment.

The second experiment is a study of economic decision making. You have been randomly separated into two groups: blues and greens. The top of the page of instructions confirms whether you are a blue or green. Each blue has been anonymously matched with a unique green partner. You will never know the identity of your green partner. Any earnings in this experiment are in addition to your show-up fee of \$5.

Specific instructions for **simple trust game**.

Each “Blue” starts with \$8 and each green starts with \$0. Each “Blue” chooses how much money to send to their “Green” partner: \$0, \$2, \$4 or \$6, keeping the rest for themselves. Whatever gets sent to the “Green” partner gets tripled:

- \$0 stays \$0
- \$2 becomes \$6
- \$4 becomes \$12
- \$6 becomes \$18

Finally, each “Green” chooses whether to return some, all or none of the tripled amount to their “Blue” partner, keeping the rest for themselves.

“Greens” will make three decisions: how much to return if their “Blue” partner sends \$2, how much to return if their “Blue” partner sends \$4, and how much to return if their “Blue” partner sends \$6 (if the blue sends over \$0, then the “Green” has no decision to make). “Greens” will make their decisions before finding out how much their “Blue” partners sent.

Once everyone has made their decisions, the choice made by each “Green” that we will use to calculate earnings will be the one that corresponds to what choice their “Blue” partner actually made. The other choices made by each “Green” will not affect anybody’s earnings.

Specific instructions for **sender-dictator simple trust game**.

Each “Blue” starts with \$8 and each “Green” starts with \$0. Each “Blue” chooses how much money to send to their “Green” partner: \$0, \$2, \$4 or \$6, keeping the rest for themselves. Whatever gets sent to the “Green” partner gets tripled:

- \$0 stays \$0
- \$2 becomes \$6
- \$4 becomes \$12
- \$6 becomes \$18

“Greens” do not make any decisions.

Specific instructions for **responder-dictator trust game**.

Each “Blue” starts with one of four amounts: \$8, \$6, \$4 or \$2. Each “Green” starts with an amount that is determined by the amount that their “Blue” partner starts with:

- If their “Blue” partner starts with \$8 the “Green” starts with \$0.
- If their “Blue” partner starts with \$6 the “Green” starts with \$6.
- If their “Blue” partner starts with \$4 the “Green” starts with \$12.
- If their “Blue” partner starts with \$2 the “Green” starts with \$18.

“Green”s will make three decisions: how much to send to their “Blue” partner if they start with \$6, how much to send to their “Blue” partner if they start with \$12, and how much to send to their “Blue” partner if they start with \$18. They will make these decisions before finding out how much they started with.

“Blues” do not make any decisions.

Once the “Greens” have made their decisions, the choice made by each “Green” that we will use to calculate earnings will be the one that corresponds to how much they actually started with. The other choices made by each “Green” will not affect anybody’s earnings.

Collect all materials.

That concludes the experiments. We will now calculate your earnings and pay you in private.

Do a funneled debriefing for a subset of the subjects.

At the end of the experiment, select 1-to-2 participants at random and offer them the opportunity to earn \$3 by participating in a very brief oral survey. Administer this survey face-to-face and individually.

Thank you for agreeing to take part in this survey. You will receive \$3 for participating regardless of your answers. Please answer the following questions honestly. Note that there are no “correct” answers.

1. What do you think the purpose of this experiment was?
2. What do you think this experiment was trying to study?
3. Did you think that any of the tasks you did were related in any way? (If “yes”) In what way were they related?
4. Did anything you did on one task affect what you did on any other task? (If “yes”) How exactly did it affect you?
5. When you were completing the scrambled sentence test, did you notice anything unusual about the words?