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Trust and Reciprocity:
Implications of Game Triads and Social Contexts*

By James C. Cox

1. Introduction

This paper explores a behavioral phenomenon that has not been included in models of social preferences: the effects of social context on trust, reciprocity, and altruism. An experiment is reported in which the social context varies from a "weak" (or one task) to a "strong" (or two task) social context. Use of the strong social context makes it possible to experiment with trust, reciprocity, and altruism in an environment in which there will be future play but no repeated game with the same person. This introduces into the experiment a feature of everyday life in large cities that contrasts with household and work environments. Much interaction in household and work environments involves repeated games with the same individuals, such as interactions among family members and co-workers, but much social interaction in large cities involves play of one shot games in rich social contexts with future interactions but without repeated games with the same individuals. Examples which illustrate the distinction are provided by informal observations of automobile and bus driver interactions.

The experiment on effects of social context was motivated by the "social history" treatment in the investment game experiment reported by Berg, Dickhaut, and McCabe (1995). They report that informing subjects in the social history treatment about the choices made by others in a previous investment game (the "no history" treatment) causes first movers to send more money (to second movers) and second movers to return more money (to first movers). But the reasons why subjects respond to information provided in the social history treatment are not clear. Does the information suggest to subjects a more familiar, social context that calls forth more generous behavior than the spare, one shot game of the no history treatment? Do data on money returned by many second movers cause some first movers to become more trusting? Or do

the examples of generous behavior by others (first and second movers) cause some first movers to become more altruistic? Do data on money sent by many first movers cause some second movers to become more positively reciprocal? Or do the reports of generous behavior by others cause some second movers to become more altruistic? The experiment reported here investigates effects of social context directly, by varying the context from "weak" to "strong."

The weak social context implementation of the investment game is simply the standard investment game, which is a one shot game, run with a double blind payoff procedure for experimental subjects' anonymity. The strong social context implementation of the investment game uses a design in which there is a second game, implemented with random subject reassignment, that follows the individual subject pair implementation of the investment game. The existence of the second game introduces a stronger social context but it does not introduce a repeated game, in the usual sense, because no one can acquire a reputation given the anonymity of double blind payoffs.

The weak and strong social context treatments are implemented with the triadic experimental design for experiments with the investment game previously reported in Cox (2002, 2004). The triadic design makes it possible to discriminate between transfers resulting from trust or reciprocity towards specific individuals and transfers resulting from (unconditional) altruism. Data from triadic design experiments with the investment game (Cox, 2004) and the moonlighting game (Cox, Sadiraj, and Sadiraj, 2008) figure prominently in empirical support for new theories of conditional reciprocity (Cox, Friedman, and Gjerstad, 2007; Cox, Freidman, and Sadiraj, 2008). Data from the experiment reported here suggest that future models of trusting behavior may need to discriminate between one task and two (or N) task play even when there are no repeated games between the same players.

2. Some Observations on the Effects of Social Context

The possible dependence of other-regarding behavior on the social context of the decision environment can be appreciated from considering informal observations of driving behavior in Arizona and England. Tucson and adjacent communities in Pima County, Arizona comprise a medium-size metropolitan area with population of about 850,000. Because there is only one freeway on the edge of the city, much of the commuter traffic is carried by city streets. The heavy traffic produces frequent interactions between drivers in traffic lanes who own the (both legally and socially defined) right of way and other drivers wanting to enter the traffic lanes from side streets, parking lots, and driveways. Each interaction between a pair of drivers, one with and the other without ownership of the lane right of way, is (for any practical purpose) a "one shot game" in the sense that the probability that *this pair* of drivers will ever encounter each other again is close to 0. Thus it is not possible to develop a personal reputation with any other individual driver. But these right of way games are played in a social context that has some observable implications. My observations of how people play these games are as follows.

Many drivers refuse to let the other driver in during an individual encounter. In a substantial minority of the encounters, the driver owning the right of way delays his or her trip to let the other driver into the traffic lane. In response, most but not all of the drivers receiving the courtesy wave and smile to convey their gratitude. But because of the social context of play, such an interaction between a pair of drivers is not always the end of the actions triggered by the courtesy of the original driver who gave the right of way to another. In some cases, one can see the recipient of the original courtesy extend a similar one to a third party. My personal experience as a participant is as follows. If another driver extends me the courtesy of letting me into the street, the probability is essentially 1 that I will extend the same courtesy to the next driver I encounter *on that trip*.[1] That such "second task" behavior can have implications for the first encounter between two paired drivers can be understood as follows. Suppose that some drivers prefer that

other drivers be courteous. Also suppose that some of the drivers with this preference anticipate that courtesy can be contagious. Then these drivers will have a greater motivation to extend the first courtesy themselves than they would if their interaction with a specific other driver were not embedded in a social context of numerous interactions between pairs of drivers. Thus the richness of the social context of play of the right of way game may be a significant determinant of behavior.

Jim Engle-Warnick made some observations that supplement mine while riding a bus from Oxford to Heathrow Airport. He counted 12 courtesies extended by the bus driver to others, and six courtesies extended by others to the bus driver. There were eight (directly-reciprocal) waves of acknowledgement by recipients of courtesies and three (indirectly reciprocal) courtesies extended to third parties within the short period of time before the original recipients disappeared from view. Upon being asked at the end of the trip why he was courteous to others on the road, the bus driver replied: "Because other people will be more likely to do it." The experiment reported in this paper poses the question of whether subjects in a stylized experiment exhibit behavior consistent with the bus driver's explanation of his/her motivation.

Further interpretations of the informal observations of driving behavior are as follows. A driver may incur a cost to extend a courtesy to another because of other-regarding preferences or because of an intention to promote indirect reciprocity in others. The individual receiving the courtesy may wave and smile to show positive reciprocity. Furthermore, a driver receiving a courtesy may be more likely to subsequently extend a similar courtesy to another, thus exhibiting indirect reciprocity. And, of course, there are examples of discourteous driving behavior with associated patterns of negatively reciprocal responses such as honking, cursing, etc. These interactions are not repeated games, in the usual sense, because the probability that the same pair of drivers will encounter each other more than once is close to 0 in a large urban environment. But the right of way games are played in a social context with the following salient characteristic: while extending a courtesy or responding to one, a driver knows that other interactions lie ahead

but does not know exactly what those interactions (or games) will turn out to be. The design of our rich social context experiment is intended to incorporate this salient characteristic of everyday social interaction into the laboratory in a simple stylized way by informing the subjects that there will be a subsequent task with possible (monetary) payoff but telling them nothing specific about that task.

The experimental protocol explained in section 5 varies the environment from a weak to a strong social context. The experiment involves game triads, described in section 4, that include the investment game introduced by Berg, Dickhaut, and McCabe (1995) and later used by several other authors.

3. The Investment Game

Berg, Dickhaut, and McCabe (hereafter BDMc) implemented the investment game by dividing the subjects into two groups, the room A group and the room B group. Every subject was given an endowment of ten $1 bills. Room B subjects were instructed to keep their $10 endowments. Room A subjects were informed that they could keep all of their $10 endowments or transfer any integer amount to a paired subject in room B. Any amount transferred by a room A subject was multiplied by 3 by the experimenter before being delivered to a room B subject. Subsequently, room B subjects were given the opportunity to return none, part, or all of the amount received to the paired subject in room A. The experiments used a double-blind payoff protocol in which individual subjects' responses were anonymous to both other subjects and the experimenter.

If one assumes there are no other-regarding preferences, then game theory predicts that: (*i*) room B subjects will keep all the money they receive because room B subjects prefer more (of their own) money to less; and (*ii*) knowing this, room A subjects (who care only about their own money) will not transfer any money. This "completely-selfish" subgame perfect equilibrium leaves each pair of subjects with $20, whereas the pair could have ended up with as much as $40. Thus this allocation is Pareto inferior to some alternative feasible allocations.

Results from investment game experiments reported by BDMc were that the average amount transferred by room A subjects was $5.16 and the average amount returned by room B subjects was $4.66. When data from this experiment were provided to subjects in a subsequent experiment (the "social history" treatment), the average amount transferred by room A subjects was $5.36 and the average amount returned was $6.46. There was large variability across subjects in both treatments of the amounts transferred and returned.

Data from investment game experiments support the following interpretation. A room A subject may be willing to transfer money to an unknown room B person if he or she *trusts* that some of the tripled amount transferred will be returned. Further, a room B subject may be willing to return part of the tripled amount transferred if he or she is motivated by *positive reciprocity.* But a room A subject may be willing to make a transfer to a paired subject in room B even if there is no opportunity for the latter to return anything. Data from the investment game do not allow one to distinguish between first mover transfers resulting from trust and transfers resulting from altruistic other-regarding preferences. Similarly, investment game data do not discriminate between second mover transfers resulting from reciprocity and (unconditional) other-regarding preferences because a room B subject may be willing to transfer money to a paired subject in room A even if that paired subject did not give him/her any money. In order to make these distinctions, one needs a more elaborate experimental design.

4.   The Triadic Experimental Design

The triadic experimental design involves three games incorporated into three related treatments.[2] Game A is the investment game. Game B is a dictator game that differs from game A *only* in that the individuals in the "second-mover" group do not have a decision to make; thus they do not have an opportunity to return anything. Game C involves a decision task that differs from game A as follows. The "first movers" do not have a decision to make. Each "second mover" is given a $10 endowment. "First movers" are given endowments in amounts equal to the amounts kept

(i.e. *not* sent) by the first movers in game A. Furthermore, the "second movers" in game C are given "additional dollar amounts" equal to the amounts received by second movers in game A from the tripled amounts sent by the first movers in game A. The subjects are informed with a table of the exact inverse relation between the number of additional dollars received by a "second mover" and the endowment of the anonymously-paired "first mover." Subjects are *not* informed that the amounts of first mover endowments and second mover additional dollar amounts were determined by subjects' decisions in a preceding experiment session with game A.[3]

Treatment B differs from treatment A only in that the "second movers" do not have a decision to make in treatment B. Since an individual "second mover" cannot return anything to the paired first mover" in treatment B, the first mover cannot be motivated by trust that the "second mover" will do so. Treatment C differs from treatment A only in that the "first movers" do not have a decision to make in treatment C. Since an individual "first mover" cannot send anything in treatment C, the paired "second mover" cannot be motivated by positive reciprocity towards the individual "first mover."

5. Experiment Protocol

Two experiments are reported that have different social contexts. The experiment with a strong social context involves a second decision task that follows the first decision task in a treatment involving game A, B, or C. The presence of the second task does not introduce a repeated game because subject anonymity and random matching make it impossible for any subject to acquire a reputation. The experiment with a weak social context does not involve a second task. A summary of the experiment protocol is presented here. The appendix contains a more detailed discussion of the protocol.

5.1 Strong Social Context

The experiment involves three treatments. Treatment A_{SC} implements game A, the investment game, in the strong social context. Treatments B_{SC} and C_{SC} implement games B and C, respectively, in the strong social context.

The instructions for each treatment announce the existence of a second task but do not explain that it is a group decision task involving the investment game. The experiment sessions are run manually (i.e., not with computers). At the end of a session, a coin is flipped in the presence of the subjects to determine whether task one or task two has monetary payoff. Data for task one are reported in this paper.[4] The payoff procedure is double blind: (a) subject responses are identified only by letters that are private information of the subjects; and (b) monetary payoffs are collected in private from sealed envelopes contained in lettered mailboxes.

5.2 Weak Social Context

This experiment includes treatments A_{WC}, B_{WC}, and C_{WC}, the implementation of games A, B and C in a weak social context. The weak social context is one in which there is no second task. These data were previously reported in Cox (2004). Except for elimination of the second task, treatments A_{WC}, B_{WC} and C_{WC} are the same, respectively, as treatments $A_{SC,}$ B_{SC} and C_{SC}.

5.3 Excerpts from Subject Instructions

This section presents some key paragraphs in the subject instructions for the two social contexts. Complete subject instructions for all six treatments are available on the author's home page (http://expecon.gsu.edu/jccox/subjects.html).

The only difference between subject instructions for the strong social context and the weak social context consists of one paragraph. Instructions for all three treatments in the strong

social context included the paragraph while instructions for all three treatments in the weak social

context did *not* include the paragraph. Here is the paragraph.

A Two Part Experiment
Today's experiment has two parts, called Task One and Task Two. One of these
parts will be selected for money payoff, by flipping a coin, at the end of the
experiment. Task One decisions will be made in this room. Task Two decisions
will be made in other rooms, down the hallway. These are the instructions for Task
One.

Instructions for all six treatments contained two paragraphs that explained the double

blind payoff procedures. These procedures are designed to make it impossible for a subject to

acquire a reputation with any other subject or with the experimenter. The purpose of using this

double blind procedure in fairness games is to eliminate concerns the subjects might have about

having their personal decisions known by the experimenters. Double blind protocols provide tests

for the significance of *internalized* norms for reciprocity and fairness.[5] In the present experiment

in the strong social context, the double blind protocol has the additional purpose of controlling for

repeated game effects between the same pair of subjects. All six treatments contained two

paragraphs that explain the double blind payoff protocol. Here are the two paragraphs.

Anonymity
Each person in Group X will be randomly paired with a person in Group Y. No one
will learn the identity of the person she/he is paired with.

Complete Privacy
This experiment is structured so that no one, including the experimenters, the
monitor, and the other subjects will ever know the personal decision of anyone in the
experiment. This is accomplished by a procedure in which you collect your money
payoff, contained in a sealed envelope, from a lettered mailbox that only you have
the key for. Your privacy is guaranteed because neither your name nor your student
ID number will appear on any form that records your decisions in this experiment.
The only identifying mark on the decision forms will be a letter known only to you.
You will be able to collect your money payoffs with privacy by using a key, which
opens a mailbox. The key and mailbox will be labeled with the same letter as your
decision-reporting forms. But you will be the only person who knows your personal
letter.

6. Comparison with the BDMc Data

I first ask whether data from the strong social context and weak social context investment games look like data from the BDMc social history and no history investment games.[6] Next, several significance tests are reported that compare data from the strong social context investment game with data from the social history investment game to ascertain whether these games involve similar behavior.

Table 1 reports means and standard deviations of amounts sent and returned in all four investment games. Figures in the middle column reveal little difference among average amounts sent by first movers in all four implementations of the investment game. In contrast, average amounts returned by second movers appear to fall into two categories: (a) relatively small amounts are returned in the weak social context and no history experiments; and (b) relatively large amounts are returned in the strong social context and social history experiments. This suggests that behavior in the strong social context and social history implementations of the investment game may be similar. The tests reported in Table 2 make a more detailed comparison of these two treatments.

Treatment A_{SC} included 30 pairs of subjects run in two sessions. Treatment SH included 28 pairs of subjects run in three sessions. Both data sets exhibit large variability across subjects. The amount sent varies from 0 to 10 and the amount returned varies from 0 to 20 in the data for both experiments. There are some small differences between the two data sets. As reported in the second and third columns of Table 2, on average the subjects in treatment A_{SC} both sent a little more ($6.00 vs. $5.36) and returned a little more ($7.17 vs. $6.46) than the subjects in treatment SH. On average, the sending (or first-mover) subjects in treatment A_{SC} made a $1.17 profit and those in treatment SH made a $1.10 profit. Also, treatment SH data are noisier than treatment A_{SC} data in that the former have higher standard deviations for amounts sent and returned.

The last row of Table 2 reports two-sample t-tests for differences of means and Smirnov tests comparing the empirical cumulative distributions of treatment A_{SC} and treatment SH data.[7] The difference between mean amounts sent is not significantly different from 0 ($p = 0.430$) according to the two-tailed t-test. The maximum difference between the cumulative distributions of amounts sent in treatment A_{SC} and treatment SH is not significant ($p = 0.491$) according to the two-tailed Smirnov test.

The first and second rows in the right-most five columns of Table 2 report tobit estimates of the relation between amounts returned and amounts sent in treatment A_{SC} and treatment SH. The estimated model is given by

(1) $\qquad R_t = \alpha + \beta S_t + \varepsilon_t,$

where R_t is the amount returned by the second mover in subject pair t and S_t is the amount sent by the first mover in pair t. The bounds for the tobit estimation are the bounds imposed by the feasible sets in the game:

(2) $\qquad R_t \in [0, 3S_t].$

One would expect that the cone created by these bounds might produce heteroskedastic errors. In order to allow for the possibility of heteroskedastic errors, the tobit estimation procedure incorporates estimation of the parameter θ in the following model of multiplicative heteroskedasticity:

(3) $\qquad \sigma_t = \sigma e^{\theta S_t}.$

The 0.055 estimate of the intercept for the treatment A_{SC} data is not significantly different from 0 ($p = 0.966$). The 1.17 slope coefficient for amount sent is significantly different from 0 ($p = 0.000$). Because its standard error is 0.275, the slope coefficient for the treatment A_{SC} data are also significantly different from 2 at a 1% significance level. Thus, the subjects' return behavior is significantly different from the prediction of completely-selfish subgame perfect equilibrium

($\beta = 0$) and the equal-split fairness focal point ($\beta = 2$). Sending subjects did, on average, earn a profit on the amounts they sent, but $\hat{\beta}$ is not significantly different from 1, which is the prediction of the zero-loss (by first movers) fairness focal point. The 0.152 estimate of the parameter θ of the heteroskedasticity model is significant ($p = 0.004$). The right-most column of Table 2 reports the results from a likelihood ratio test for significance of the fitted model. It is highly significant ($p = 0.000$) for the treatment A_{SC} data.

The 0.275 estimate of the intercept for treatment SH data is not significantly different from 0 ($p = 0.858$). The 1.11 slope coefficient for treatment SH data is significantly different from 0 ($p = 0.001$) and significantly from 2 at 1% significance; hence the subjects' return behavior is significantly different from the predictions for the completely-selfish subgame perfect equilibrium and the equal-split fairness focal point. Sending subjects did, on average, make a profit on the amounts they sent, but $\hat{\beta}$ is not significantly different from 1. The 0.096 estimate of the parameter of the heteroskedasticity model is not significant ($p = 0.160$). The result of the likelihood ratio test is significance ($p = 0.000$) for treatment SH data.

The last row of Table 2 reports tobit estimates of the model,

(4) $R_t = \alpha + \beta S_t + \gamma D_t S_t + \varepsilon_t$

where:

(5) $D_t = 1$ for treatment A_{SC} data

$\quad\quad = 0$ for treatment SH data.

This estimation uses the bounds and heteroskedasticticity model given by statements (2) and (3). The estimate of γ is not significantly different from 0 ($p = 0.897$), which provides additional support for the conclusion that the differences between the data from treatment A_{SC} and treatment SH are not significant.

7. Effects of Social Contexts

Table 1 reveals that first mover behavior in investment games in the strong and weak social contexts looks similar: the average amount sent in treatment A_{SC} was \$6.00 while the average amount sent in treatment A_{WC} was \$5.97. In contrast, second movers in the investment game returned more in the strong social context than in the weak social context: the average amount returned in treatment A_{SC} was \$7.17 while the average amount returned in treatment A_{WC} was \$4.77. Data from the full triadic design yield insight into effects of social context on subjects' revealed trust and reciprocity.

7.1 Effects of Social Context on Individual Trust

Data from treatments A_{SC} and B_{SC} can be used to test for the significance of trusting behavior in the strong social context.[8] The first row of Table 3 reports the mean amounts sent by first movers in treatments A_{SC} and B_{SC}. The mean amount sent was slightly larger in treatment A_{SC} than in treatment B_{SC} but the difference is insignificant by a one-tailed means test (p = 0.389). The Smirnov test also reveals no significant difference between amounts sent in treatments A_{SC} and B_{SC} (p = 0.386). Thus there is no support in the data from the strong social context for the hypothesis that the first movers sent part of their endowments to the paired second movers *because of* a trust that the second movers would directly reciprocate to them.

The second row of Table 3 reports that the mean amounts sent in treatments A_{WC} and B_{WC} were, respectively, \$5.97 and \$3.63. The one-tailed *t*-test for difference in means is significant (*p* = 0.010). The one-sided Smirnov test also reveals a significant difference between amounts sent in treatments A_{WC} and B_{WC} (p = 0.045). Thus the weak social context data support the conclusion that the first movers' behavior was partly motivated by trust that the paired second movers would directly reciprocate to them.

7.2 Effects of Social Context on Individual Reciprocity

Data from implementations of games A and C can be used to test for the significance of direct positive reciprocity as a motive for second movers' behavior. The first row of Table 4 reports mean amounts sent and returned in treatments A_{SC} and C_{SC}. On average, the amount returned in treatment A_{SC} exceeded the amount sent by $1.17. The average amount returned in treatment C_{SC} was $1.23 less than the amount "sent." The difference between the treatment A_{SC} and treatment C_{SC} outcomes is in the direction implied by positive reciprocity. The one-tailed *t*-test implies that the mean amount returned in treatment A_{SC} is significantly greater than the mean amount returned in treatment C_{SC} ($p = 0.017$).

The right-most five columns of the first row of Table 4 report tobit estimates of the parameters of the following relation between amounts sent and amounts returned in treatments A_{SC} and C_{SC}:

(6) $R_t = \alpha + \beta S_t + \gamma D_t S_t + \varepsilon_t$,

where

(7) $D_t = 1$ for treatment A_{SC} data

            $= 0$ for treatment C_{SC} data.

This estimation uses the bounds and heteroskedasticity model given by statements (2) and (3). Note that $\hat{\gamma}$ is the estimate of the effect of (direct) reciprocity on amount returned by second movers to first movers. We observe that $\hat{\gamma}$ is positive and significantly greater than 0 ($p = 0.002$). Thus the data provide support for behavior involving positive reciprocity in a strong social context.

The second row of Table 4 reports meant amounts sent and returned in treatments A_{WC} and C_{WC}. On average, the amount returned by second movers in treatment A_{WC} was *less than* the amount sent by $1.03. The average amount returned in treatment C_{WC} was $3.91 less than the

amount "sent." The difference between the treatment A_{WC} and treatment C_{WC} outcomes is in the direction implied by positive reciprocity. A two-sample t-test for difference in means implies that the mean amount returned in treatment A_{WC} is significantly greater than the mean amount returned in treatment C_{WC} ($p = 0.018$).

The right-most five of the second row of Table 4 reports tobit estimates of the parameters of the model given by statements (2), (3), (6), and

(8) $D_t = 1$ for treatment A_{WC} data

= 0 for treatment C_{WC} data.

The estimate $\hat{\gamma}$ is significantly greater than 0 ($p = 0.034$). Hence the data provide support for behavior involving positive reciprocity in a weak social context.

7.3 Effects of Social Context on Behavior in Investment and Dictator Games

Data on average amounts sent and returned in Tables 3 and 4 suggest that the different effects of social context on individual-subject-pair trust and reciprocity come from the absence of an effect of social context on play of first movers in the investment game. Tests reported in Table 5 make this clear. The top row reports t-statistics and p-values for one-sided means tests of the hypothesis that amounts sent and returned are larger in the strong social context. The second row reports D-statistics and p-values for one-sided Smirnov tests.

Both the means test and Smirnov test reveal no significant difference between amounts `sent by first movers in the strong (treatment A_{SC}) and weak (treatment A_{WC}) social contexts. In contrast, the strong social context causes significant increases in all other types of responses. Amounts sent in the first mover dictator control game are significantly larger in the strong social context (treatment B_{SC}) than in the weak social context (treatment B_{WC}). In the investment game, second movers return significantly more in the strong social context (treatment A_{SC}) than in the weak social context (treatment C_{SC}). Finally, amounts "returned" in the second mover dictator

control game are significantly larger in the strong social context (treatment C_{SC}) than in the weak social context (treatment C_{WC}).

8. Concluding Remarks

This paper reports experiments with game triads that include the investment game. Researchers had previously established the replicable result that the majority of first movers send positive amounts and the majority of second movers return positive amounts in investment game experiments. This pattern of results, and results from many other non-market fairness experiments, are inconsistent with the predictions of the *homo economicus* model. Data from triadic design experiments and several other types of experiments provide support for models of conditional reciprocity (Cox, Friedman, and Gjersdtad, 2007; Cox, Friedman, and Sadiraj, 2008).

In this paper the game triads are implemented in two different environments referred to as the strong and weak social contexts. Use of the strong social context makes it possible to conduct an experiment in an idealized environment that resembles the environment of ongoing social interaction that exists outside the laboratory in non-repeated game contexts. In the strong social context, the subjects are informed at the beginning of an experiment session that there will be a second decision task after completion of the investment game or one of its dictator control games. They are further informed that only one of the decision tasks will be randomly selected for money payoff. Because of anonymity and random pairing the existence of the second task does not create a repeated game between individual subjects. The existence of the second task creates a richer social context than exists in the single-task fairness game experiments that typify much of the social preferences literature. The reasons for experimenting with the strong social context are illustrated by the naturally-occurring interaction between drivers on big city roads. The experiment environment without the second task is referred to as the weak social context.

There has been one previous experiment aimed at investigating the implications of varying the social context. In the first investment game experiment, Berg, Dickhaut, and McCabe

(1995) implemented both a no history treatment and a social history treatment in which the subjects were given the data from a preceding no history treatment. The social history treatment is a stronger social context than the no history treatment. Data from my investment game treatment in the strong social context closely resemble data from the Berg, et al. social history treatment.

As reported elsewhere (Cox, 2004) data from the experiment treatment with the weak social context provide evidence of positive reciprocity and trust, as well as unconditional altruism, between anonymously-paired individual subjects. Data from treatments with strong and weak social contexts reveal some notable effects from varying the social context. The mean amounts sent and "returned" are higher in both the first mover dictator control treatment and the second mover dictator control treatment in the strong social context than in the weak social context. The mean amount returned by second movers in the investment game is higher in the strong social context than in the weak context. The mean amount sent by first movers in the investment game is *not* higher in the strong social context than in the weak context. Thus the stronger social context elicits more generous behavior by both "first mover" and "second mover" dictators and by second movers in the investment game but not by first movers in the investment game. Comparisons between treatments in the weak social context support the conclusion that anonymously paired subjects exhibit both trust and reciprocity towards specific individuals. In contrast, comparisons between treatments within the strong social context support the conclusion that anonymously paired subjects exhibit positive reciprocity but do not exhibit trust towards specific paired individuals.

The more generous behavior by dictators and by second movers in the investment game in the strong social context is consistent with the opinion expressed by the Heathrow bus driver, that in a social context of repeated one shot games individuals may behave more generously "Because (…they believe…) other people will be more likely to do it." The different, puzzling result is that introduction of the strong social context did not change behavior of first movers in

the investment game. Indeed, as shown by Table 1, first mover behavior in the investment game is remarkably invariant to changes in design and protocol across four different implementations of the game. But first mover behavior in dictator controls for the investment game changes significantly with the social context. This suggests that future models of trusting behavior may need to incorporate a distinction between one task play and two (or N) task play, even in the absence of repeated games.

Endnotes

1. This is not an example of the type of "indirect reciprocity" modeled in Nowak and Sigmund (1998a, 1998b). In their model of repeated play, agents are rewarded for having a reputation for generous behavior and penalized for having a reputation for ungenerous behavior. In contrast, in the driving example behavior does not appear to be rewarding or punishing individuals because of their personal reputations.

2. The triadic design for the investment game, and the way it decomposes trust, reciprocity, and altruism, are explained in more detail in Cox (2004). Methodological issues concerning the triadic design and across-subjects, double-blind protocols are discussed in Cox, Sadiraj, and Sadiraj (2008).

3. This design feature means that there is no "social history" in the way there is in the Berg, et al. (1995) experiment.

4. The group decision making data from the second task are reported in Cox (2002), as are gender effects.

5. Single blind and double blind payoff protocols have been shown to produce different behavior in fairness games (Hoffman, et al., 1994; Hoffman, McCabe, and Smith, 1995; Cox and Deck, 2005, 2006; Cox, Sadiraj, and Sadiraj, 2008).

6. As noted above, Berg, Dickhaut, and McCabe (1995) used a double blind payoff protocol.

7. Conover (1980) explains the Smirnov test (pgs. 368-373) for comparing two empirical distributions and the Kolmogorov test (pgs.346-356) for comparing an empirical distribution with a theoretical distribution. The Smirnov test is reported here. Both types of tests are sometimes referred to as "Kolmogorov-Smirnov" tests.

8. The tests reported in Table 3 do not use data for four of the subjects in sessions with treatment B_{SC}. Questionnaire responses revealed that three subjects were confused. Data for these three subjects (identified by their payoff key codes) are not used. One other subject was a repeat participant. The payoff key number of this subject was identified after the end of the experiment and the data are not used.

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Table 1. Amounts Sent and Returned in Investment Games

Treatment	Sent Mean [St. Dev.]	Returned Mean [St. Dev.]
A_{WC}	5.97 [3.87]	4.77 [6.63]
A_{SC}	6.00 [2.59]	7.17 [4.82]
NH	5.16 [2.94]	4.66 [5.55]
SH	5.36 [3.53]	6.46 [6.19]

Table 2. Comparison of Treatment A_{SC} and BDMc Treatment SH

Data	Sent Mean	Ret. Mean	Sent Means Test	Sent Smirnov Test	$\hat{\alpha}$	$\hat{\beta}$	$\hat{\gamma}$	$\hat{\theta}$	LR Test
Tr. A_{SC}	6.00 [2.59]	7.17 [4.82]	…..	…..	.055 (.966)	1.17 (.000)	…..	.152 (.004)	25.1 (.000)
Tr. SH	5.36 [3.53]	6.46 [6.19]			.275 (.858)	1.11 (.001)	…..	.096 (.160)	27.1 (.000)
Tr. A_{SC} vs. Tr. SH	…..	…..	.795 (.430)	.219 (.491)	.163 (.861)	1.13 (.000)	.026 (.897)	.124 (.003)	53.6 (.000)

p-values in parentheses.

Standard deviations in brackets.

Table 3. Effects of Social Context on Individual Trust

Social Context	Tr. A Mean Sent	Tr. B Mean Sent	Tr. A vs. Tr. B Means Test	Tr. A vs. Tr. B Smirnov Test
Strong	6.00 {30}	5.81 {38}	.283 (.389)[1]	.168 (.386)[1]
Weak	5.97 {32}	3.63 {30}	2.38 (.010)[1]	.317 (.045)[1]

p-values in parentheses.
[1] denotes a one-tailed test.
Standard deviations in brackets.
Number of subjects in braces.

Table 4. Effects of Social Context on Individual Reciprocity

Social Context	Mean Sent	Tr. A Mean Ret.	Tr. C Mean Ret.	Tr. A vs. Tr. C Means Test	$\hat{\alpha}$	$\hat{\beta}$	$\hat{\gamma}$	$\hat{\theta}$	LR Test
Strong	6.00 {30}	7.17 {30}	4.77 {30}	1.58 (.059)[1]	-.002 (.998)	.748 (.000)	.427 (.002)[1]	.181 (.000)	23.0 (< .005)
Weak	5.97 {32}	4.94 {32}	2.06 {32}	2.14 (.018)[1]	4.20 (.060)	-.759 (.124)	.680 (.034)[1]	.158 (.008)	5.98 (<.025)

p-values in parentheses.
[1] denotes a one-tailed test.
Number of subjects in braces.

Table 5. Effects of Social Context in Investment and Dictator Games

Test	Amounts Sent		Amounts Returned	
	A_{SC} vs. A_{WC}	B_{SC} vs. B_{WC}	A_{SC} vs. A_{WC}	C_{SC} vs. C_{WC}
Means	.037 (.515)[1]	2.73 (.004)[1]	1.51 (.069)[1]	2.64 (.005)[1]
Smirnov	.154 (.479)[1]	.467 (.001)[1]	.369 (.015)[1]	.421 (.004)

p-values in parentheses.
[1] denotes a one-tailed test.

Appendix. Detailed Experiment Procedures

A.1. The Strong Social Context

The subjects first assembled in the sign-in room of the Economic Science Laboratory and recorded their names, student identification numbers, and signatures on a form. Then a monitor was chosen randomly from the subject sample (by drawing a ball from a bingo cage) and given the responsibility of ensuring that the experimenters followed the procedures contained in the subject instructions for calculating money payoffs. The monitor was paid $20 for this job. The other subjects were not informed of the amount of this payment in order to avoid the possible creation of a focal earnings figure. Next the subjects were randomly divided into two equal-size groups, Group X and Group Y and escorted into the large room of the Economic Science Laboratory.   The procedures differed somewhat across the three treatments because of the properties of the experiment design. I will first explain in detail the procedures used in treatment A_{SC} and, subsequently, explain how procedures differed in treatments B_{SC} and C_{SC}.

Treatment A_{SC} involves the investment game. In a treatment A_{SC} session, the Group X subjects were seated at widely-separated computer terminals with privacy side and front partitions. (The computers were not used.) The Group Y subjects were standing at the back of the room at the beginning of the session with treatment A_{SC}. Each Group Y subject was given an envelope labeled "my show-up fee" that contained ten task one $1 certificates. Each subject and the monitor were given copies of the instructions for "task one" (the individual decision task). Then an experimenter read aloud the instructions. After the reading of the instructions was completed, the Group Y subjects were escorted back to the sign-in room by one of the experimenters. (The Group X subjects had no further contact with the Group Y subjects until after all decisions in both decision tasks had been completed.) Then the Group X subjects were given the opportunity to raise their hands if they had questions. If a subject raised his hand, he was approached by an experimenter and given an opportunity to ask questions and receive

answers in a low voice that could not be overheard by other subjects. When there were no more questions, the experimenter left the room and the monitor took over to conduct the first mover individual decision task with the Group X subjects.

The monitor carried a large box that contained smaller boxes equal in number to the number of subjects. Each subject was given the opportunity to point to any remaining small box to indicate she wanted that one. (The boxes all looked the same to the experimenters.) A subject opened her box to find an envelope labeled "my show-up fee" that contained ten task one $1 certificates. The box also contained an empty envelope labeled "certificates sent to a paired person in Group Y" and an envelope containing a lettered task one mailbox key. Finally, the box contained a one-page form that summarized the nature of the first-mover individual decision task. This form and the corresponding forms for other treatments are contained in an appendix available upon request. All envelopes in the box were labeled with the letter on the mailbox key.

Subjects were given 10 minutes to complete this task. When a subject was finished, he put all of the envelopes except the key envelope back in the box and summoned the monitor to collect the box. The monitor then carried the large box full of small boxes into another room for data recording and the preparation of boxes for the Group Y, second-mover subjects. The monitor witnessed all data recording and Group Y box preparations.

While the boxes were being processed, one experimenter escorted the Group X subjects out a side door of the Economic Science Laboratory and down the hall to the breakout rooms of the Decision Behavior Laboratory. Next, another experimenter escorted the Group Y subjects into the Economic Science Laboratory to get ready for their second-mover decisions in the individual decision task.

The Group Y subjects were given boxes by the monitor. Each box contained an envelope with a lettered task one mailbox key. The box contained two empty envelopes, one labeled "my certificates" and the other labeled "certificates returned to the paired person in Group X." The box contained the tripled number of certificates sent by the paired person in Group X and a form

summarizing the decision task. The form is contained in an appendix available upon request. The Group Y subjects had to decide how many of the certificates to put in the envelopes labeled "my certificates" and "certificates returned to the paired person in Group X." The Group Y subjects were given 10 minutes to complete the task. When a subject was finished, she put all envelopes except the key envelope back in the box and summoned the monitor to collect it. The monitor then carried the large box of little boxes to another room and watched the data recording

The second-mover decisions in task one were conducted simultaneously with the first-mover decisions in task two. The first-mover decisions in task two were made by three-person committees that were formed by the experimenter by the order in which the subjects entered the laboratory from the hallway. Thus, the first three subjects were assigned to be in the first committee, the next three in the second committee, and so on. Each committee was seated in its own small breakout room. Each member of each committee was given the written subject instructions for task two. Then an experimenter read aloud the instructions while all breakout room doors remained open. Subjects were then given the opportunity to indicate whether they had any questions. If there was a question, the experimenter entered the appropriate breakout room and closed the door before the question was asked and answered. When there were no more questions, the experimenter left and the monitor took over. The monitor permitted the members of each committee to point to a small box contained in a large box to indicate which remaining box the committee wanted. A committee's box contained an envelope labeled "our show-up fee" that contained 30 task two $1 certificates. The box also contained an envelope labeled "certificates sent to a paired committee in Group Y" and an envelope containing a lettered task two mailbox key. Finally, the box contained a one-page summary of the group decision task. The form is contained in an appendix available upon request. The committees were given 20 minutes to complete their tasks. When a committee was finished, it put all envelopes except the key envelope back in the box and summoned the monitor by opening the door to its breakout room. The monitor carried the large box full of little boxes to the processing room and watched

the data recording and preparation of boxes for the Group Y committees. Next, an experimenter escorted the Group X subjects back to the sign-in room.   After all of the Group X subjects were in the sign-in room and the door was closed, an experimenter escorted the Group Y subjects out a side door of the Economic Science Laboratory and down the hallway to the breakout rooms of the Decision Behavior Laboratory.

The Group Y subjects then made their task two, second-mover decisions. Each Group Y committee was given an envelope labeled "our show-up fee" that contained 30 task two $1 certificates. The procedures for reading instructions, answering questions, and the role of the monitor were like those for the first-mover, Group X subjects. Each Group Y committee's box contained the tripled number of certificates sent to it by the paired committee in Group X. The box also contained an envelope with a task two key, a summary instruction form, and two empty envelopes. The empty envelopes were labeled "our certificates" and "certificates returned to the paired committee in Group X."

After the Group Y committees finished their task two decisions, they were escorted back down the hall to rejoin the Group X subjects in the Economic Science Laboratory. Next, an experimenter flipped a coin in the presence of all of the subjects and the monitor. The monitor announced whether the coin came up heads or tails. If heads (tails) then each task one (Two) $1 certificate was exchanged for one United States dollar. While the subjects' money payoffs were calculated, they filled out the questionnaires. In addition to the salient money payoff, each subject was paid $5 upon depositing her completed questionnaire in a box. After the questionnaires were completed the Group X subjects went together to obtain sealed envelopes containing their money payoffs from lettered mailboxes. They had been asked to exit the building after obtaining their envelopes and not to open their envelopes until out of the building. After the Group X subjects had left, the Group Y subjects obtained their payoff envelopes from the lettered mailboxes.

The procedures for treatment B_{SC} differed as follows from the treatment A_{SC} procedures explained above. The Group Y subjects did not make a decision in task one. The procedures for

treatment C_{SC} differed as follows from those for treatment A_{SC}. At the beginning of task one, the Group Y subjects were seated in the Economic Science Laboratory and the Group X subjects were standing at the back. The Group X subjects did not make a decision in task one.

A.2. The Weak Social Context

Treatments B_{WC} and C_{WC} were conducted in the same way, respectively, as treatments B_{SC} and C_{SC} except there was no task two.