

Deserving Altruism: Type Preferences in the Laboratory

Very preliminary. Comments welcome!!

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ABSTRACT

Recent and accumulating evidence has established that though people are not completely selfish, they are not as altruistic as might have been suggested by prior experimental results. These papers found decreased giving if the experiment was double-blind, or allowed “silently exiting”. Evidence for positive reciprocity, where subjects give more than dictators with the same endowment, has always been rare. However, Lin and Ong 2011 found significant positive reciprocity in a double-blind trust game in which the 2nd player knew that the 1st player was unaware of the possibility of reciprocation. Furthermore, though 2nd players could silently exit, none did. We test for the possibility that 1st players’ could better signal their “altruism type” in Lin and Ong’s setup. We introduced a 3rd player, again unknown to the 1st player, who could give part of a now exogenously fixed endowment to the 1st player after observing 1st player giving to the 2nd player. We found that 3rd players’ giving to 1st players’ were significantly correlated with 1st players’ giving to the 2nd players, and not significantly correlated with endowment. Furthermore, our exogenous endowment allowed us to show that this result was not consistent with 1st players exerting social influence on 3rd players. Unlike prior studies, we show that the explicitness of double-blindness with silent exiting made a difference only for the lowest level of endowment. Our result supports prior results which showed that player characteristics, like their facial features, can be predictors of behavior. However, to our knowledge, our study is the first to identify the apparent altruism type of the recipient as the stimulus for other’s giving to them.

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Keywords: altruism, type preferences, positive reciprocity, double-blindness, silent exiting, small payments

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I. INTRODUCTION

In standard economics, people are rational and selfish. However, many experiments have demonstrated social preferences consistent with altruism. See Andreoni et al. (2007) for example. But, evidence for positive reciprocity, where people behave more generously than dictators with the same endowment, is still rare. Notable exceptions are Cox (2004, 2005) and McCabe et al. (2000). Moreover, recent experiments have started to investigate the possibility that significant numbers of people are “reluctantly altruistic”: altruistic if “put on the spot” to choose between altruism or selfishness. However, many would choose to be selfish if they could do so under the cover of uncertainty (Dana et al. 2007) about whether their choice was really selfish. Others have chosen to take a lower payoff and “silently exit”, i.e., deprive their intended recipient of altruism if that recipient was not made aware of the opportunity for altruism. See Dana et al. (2006), Lazear et al. (2006), DellaVigna et al. (2009), Grossman (2010), Cain and Dana (2011), Andreoni et al. (2011). This literature supports the finding of an older literature which showed that double-blind experiments similarly decreased giving. In contrast, Lin and Ong’s (2011) (LO 2011) work on “gratitude” found strong positive reciprocity, despite a double-blind set up, and no “silent exiting”, though the option was available. However, silent exiting and double-blindness were implicit in their design. Thus, the salience of their double-blindness can be questioned. The sample size of the treatment (35 subjects) was relatively small. Their main treatment was suppression of the expectation for reciprocation. However, why such suppression might increase reciprocation was not identified.

We hypothesize that “type preferences” introduced by Gul and Pesendorfer (2010) was a contributing motive for gratitude. There has been prior work on altruistic types. Levine 1998 identified altruistic, normal, and spiteful types. However, the data he used in his analysis (which were from experiments of others) was not double-blind, had no silent exiting, and subjects had full awareness of the game structure. Thus, social influence (in the centipede, public goods game), strategic offers (in the ultimatum game), shame, guilt, or conditional reciprocity (in the public goods game) were all possible confounding

causes for reciprocation. Other studies have found that type may be correlated with appearance. Subjects paid for the opponents' photographs in a trust game, and the more 'altruistic' people were willing to pay more (Eckel and Petrie, 2011). Castillo and Petrie (2010) showed that the effect of pictures became insignificant when past behavior was known. Emotional expressivity was also found to predict proposals and rejections in ultimatum games (Schug et al., 2010). Type preferences may also be based on cause of poverty. Fong and Oberholzer-Gee (2011) found that charitable giving to welfare recipients was responsive to information about the cause of poverty, and this responsiveness could be predicted by social preference attitudinal measures. One important limitation of prior work with respect to evidence for type preferences is that the giver could have been strategic and not altruistically motivated. None test for signals of altruism from receivers as the relevant characteristic.

We test for possibility that people are more altruistic to those who seem more "deserving" of altruism. If they were, it would explain the contrast between LO (2011) and the reluctant altruism results. People would be reluctantly altruistic because they are not sure if recipients in dictator game deserved altruism. Similarly, they would be unsure of whether trustors in trust games deserved altruism for their giving to trustees if trustees did not know whether trustors gave in the expectation of reciprocation. LO (2011) did not inform trustors of the possibility of trustee reciprocation. Therefore, trustees could not discount the generosity of trustors' giving. Trustors' giving could signal their altruism type.

Our main contributions are as follows. We show the existence of type-preference by introducing a 3rd dictator with an exogenous endowment who observed the 1st dictator giving to the 2nd dictator in LO (2011) and could then allocate between himself and the 1st dictator, without the 1st dictator. As in LO (2011), the 1st player was unaware of the existence of the 3rd dictator. We showed that the 3rd player's giving to the 1st was correlated with the 1st to the 2nd and not with their exogenous endowment. Shame, guilt, herding, experimenter demand, and inequity aversion are ruled out by our double-blind design with silent exiting.

We make a number of methodological contributions. No prior experiments, to our knowledge, had both double-blindness and silent exiting. Lazear et al. (2011), Dana et al.

(2006) had silent exiting but were not double-blind. No prior experiment tested for the difference between implicit/explicit double-blindness. Barmettler et al. (2011) showed no difference between single-blind and implicit double-blind in ultimatum games, trust games and dictator games. Hoffman et. al. (1994, 1996) showed that explicit double-blindness was different from single-blind in dictator game. Cox et. al. (2005) found no positive reciprocity with their double-blind minitrust game, but found positive reciprocity with single-blindness. We found no difference between explicit double-blind and implicit double-blind with silent exiting, except at low endowment levels. Our results reversed for low endowment and explicit double-blindness with silent exiting corroborating Gneezy and Rustichini (2000) where behavior also reversed for small monetary incentives. Our results indicate that the demand effects of explicit double-blindness mentioned in Lowenstein 1999 may be more important for small endowments.

II. EXPERIMENTAL DESIGN

We introduced a 3rd dictator who observed LO's (2011) 1st dictator giving to their 2nd dictator, and was not told anything else. We did not want the 3rd dictator discount the 1st dictator's motive in giving to the 2nd dictator as a way to induce reciprocation. We also did not want to give the 3rd dictator the impression that 1st dictator was giving to the 2nd dictator with a view of influencing the 3rd dictator. Instructions were given step by step. We used the decision method of elicitation. We introduced a distracter: the TOSCA-3 questionnaire, a standard psychological test for guilt and shame sensitivity (Tangney et al., 2002), which was handed to each subject at the beginning of each treatment to be filled out afterwards. This was to deflect attention away from the dictator game further diminishing possible demand effects. We exogenously endowed the 3rd dictator with 6, 20, and 32 CNY. On top of these three treatments, we had two double-blind (DB) and silent exiting (SE) treatments. In one treatment, DB and SE were implicit. These subjects were surveyed for saliency of the treatment afterwards. Their section began on Sunday Sept. 18, 10:30 am. In explicit treatment, 3rd dictators were told 1st dictators' lack of knowledge of 'this stage'. Their section began on Sunday Sept. 18, 1:00 pm. We had separated the groups because we could not give instructions to one without also giving them to the

other. However, since the possible time-of-day selection effect only strengthens our result that there was no difference between the treatments. To allow for silent exiting, 1st dictator's payment was through electronic deposit without description. Thus, 1st players would not become aware of this stage. If there was no transfer from the 3rd dictator, and may not be become aware even if the 3rd dictator made a transfer. We told the 3rd dictator of this fact in the explicit treatment. Thus, we had 2x3 treatments.

For our subject pool, we re-used the 1st dictators from LO (2011). In that experiment, 90 subjects were recruited from graduate students in Shenzhen University Town. They had 70 subjects in 35 pairs in their 1st stage dictator game. For this experiment, we recruited 30 new subjects as 3rd dictators. 15 subjects were assigned to the implicit group, 15 into the explicit group. Originally, we had 32 subjects. Since we had enough 13 1st dictators who transferred zero, we took out 3 of these in matches to 3rd dictators. However, 2 of the 32 did not show up. One had been matched to a 1st dictator who gave zero. The other had been matched to a 1st dictator who gave 5 CNY.

III. MAIN RESULTS

Figure 1 shows the average transfer of the 2nd and 3rd dictators conditional on every 1st dictator transfer. We ranked by 1st dictator transfers. The horizontal axis is increasing on 1st dictator transfer. We show the 2nd and 3rd dictator giving to the 1st dictator conditional on the 1st dictator giving to the 2nd dictator. The main result is more evident after we plot average giving by the 2nd and 3rd dictator conditional on the 1st dictator giving. There is a clear increase for the 2nd and 3rd dictator giving for every increase in the 1st dictator giving, unlike other trust games, e.g., Cox 2004, where greater giving by the 1st dictator also had counter-reciprocating behavior like more zeros. We attribute this difference to lack of ambiguity of the strategic motives of the 1st dictator giving.

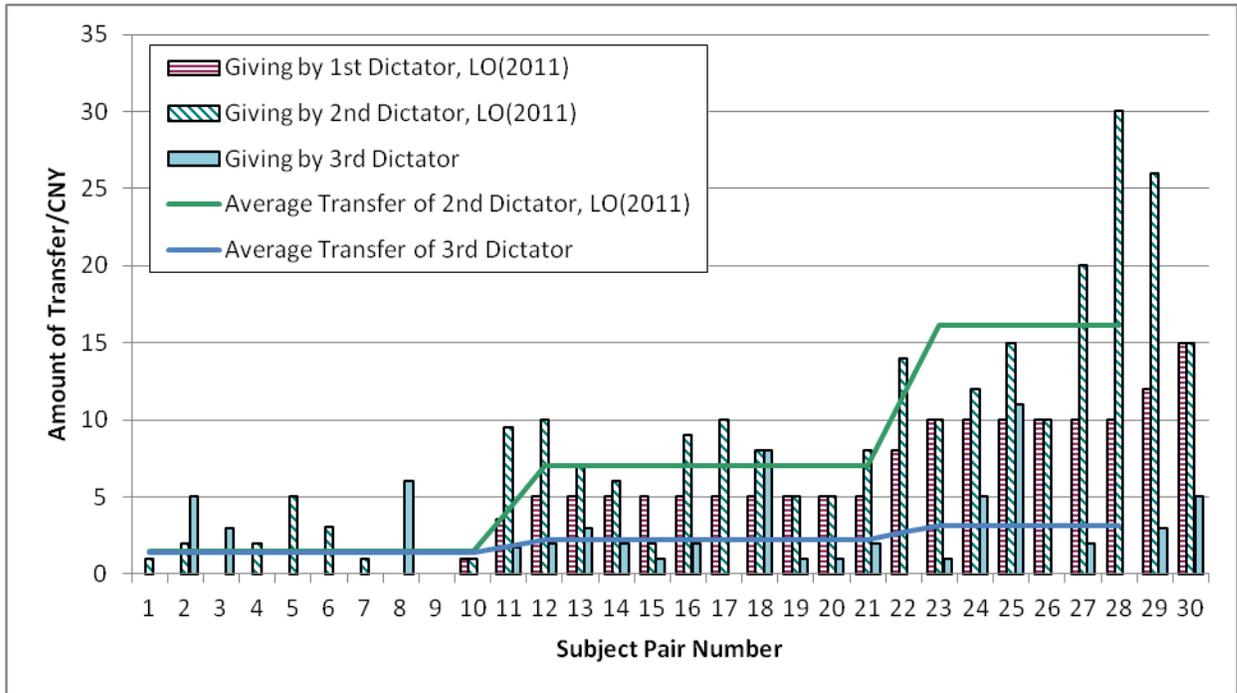


Figure 1: 3rd and 2nd dictator transfers conditional on 1st dictator transfers.

The similarities between the implicit and explicit treatments for the 20 and 32 CNY endowment group is evident in figure 2, where the trends are nearly contiguous.

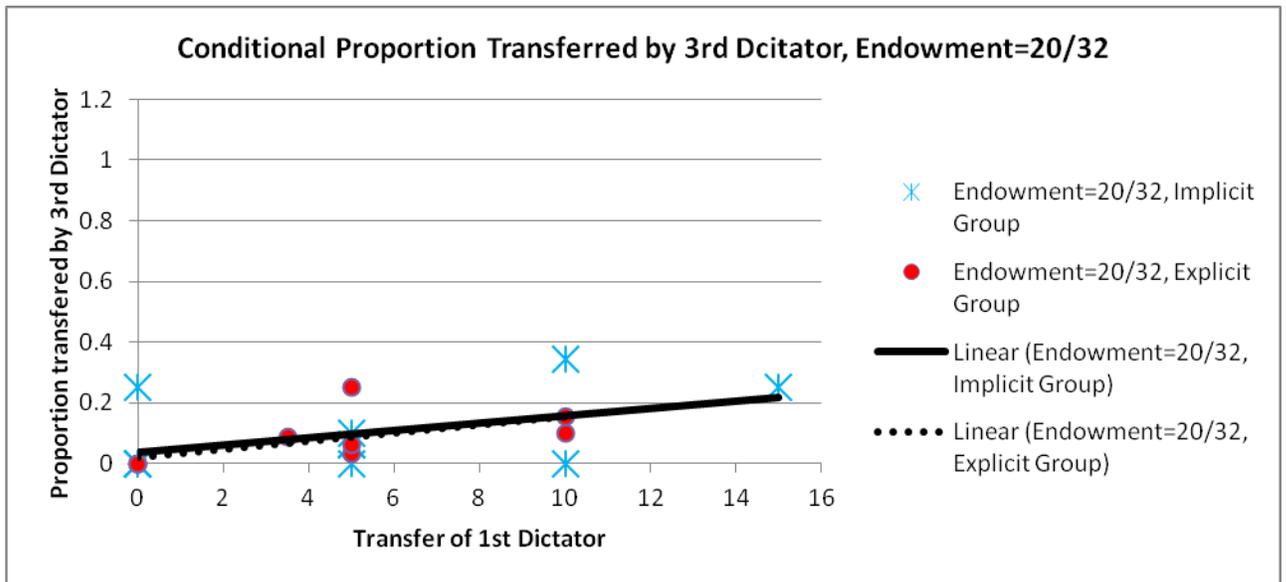


Figure 2: Comparison between implicit and explicit giving for high endowments

The contrast between implicit and explicit treatments for the 6 CNY endowment groups can be seen in figure 3.

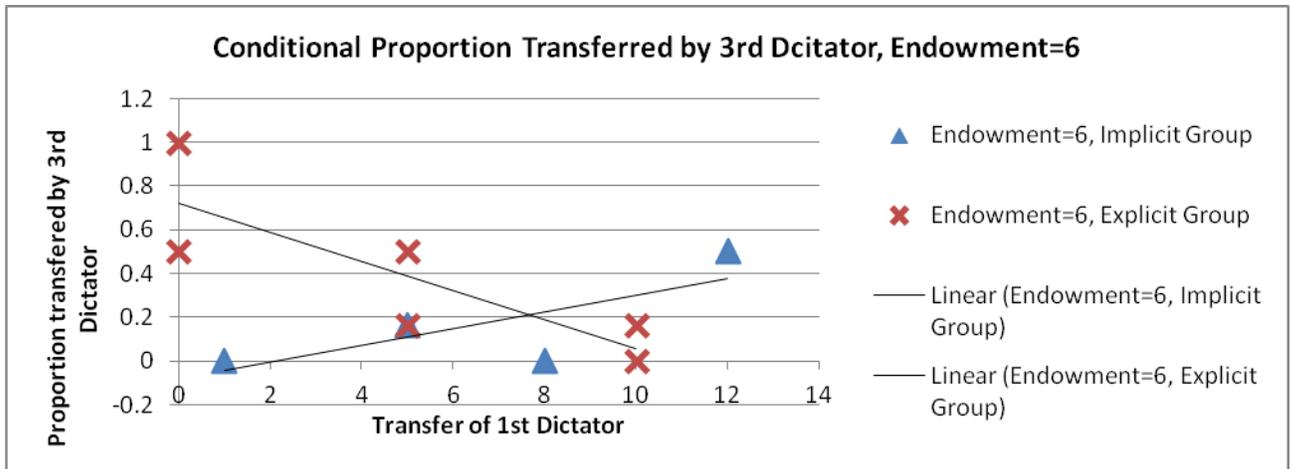


Figure 3: Comparison between implicit and explicit giving for high endowments

The regression of the 3rd dictator giving on the 1st dictators' giving and on the 3rd dictators' endowment follow. The coefficient on the 1st dictator giving was 0.19* and marginally insignificant with a p-value of 0.11, whereas the coefficient on endowment was 0.06 and insignificant with a p-value of 0.22. The same regression excluding 6 CNY endowment showed the coefficient of 1st dictator giving to be highly significant at 0.35** with a p-value of 0.02, while the coefficient on endowment was insignificant at 0.10 with a p-value of 0.18. Thus, the 1st dictator's giving was a better predictor of the 3rd dictator's giving than the 3rd dictator's endowment. In contrast, the regression for only 6 CNY endowment showed a coefficient on the 1st dictator giving of -0.17, with an insignificant p-value of 0.26. If we only include the explicit group then, the coefficient is -0.4** with a highly significant p-value of 0.04.

The Mann-Whitney-Wilcox (MWW) test showed for that the difference between implicit and explicit groups for all data to be insignificant with a p-value of 0.52, increasing to p-value of 1.00 when the 6 CNY endowment was excluded. The p-value decreased to 0.26 when limited to the 6 CNY endowment. See Table 1 in Appendix A for a full comparison.

The results of our TOSCA-3 test for shame and guilt sensitivity of subjects showed no predictive power for the giving of the 3rd dictator for the data as a whole and for all subgroups of treatments, excluding the 6 CNY treatments. This would be expected if the desire to reward altruism was not motivated by either guilt or shame. We did not have enough observations for the 6 endowment.

We found decreased giving in our 3rd dictator to 10% of endowment when compared to other standard dictator games where the usual average is 20%. However, our results are comparable to other double-blind dictator games where subjects gave about 10% percent of endowment (Hoffman et. al., 1994). The giving of our 3rd dictator was likely further diminished by the absence SE, greater social distance from 1st dictators being unaware of the existence of the 3rd dictator. 3rd dictators were also told that 1st dictators would be told neither of the existence of this round, nor why they got money, if they received money from 3rd dictators. The contrast between amounts given here and those given in LO (2011) could also indicate that simultaneous, direct, common knowledge interaction between subjects in trust games can amplify type-preferences.

IV. DISCUSSION

Guilt, shame, or experimenter demand cannot explain the correlation between the 1st dictator and 3rd dictator transfers when the endowment was 20 or 32 CNY. Guilt, if there, was controlled by unawareness and explicit silent exiting. Shame was eliminated by explicit double-blindness. Since experimenter demand (if present) should be different across treatments, and we had no significant differences for treatments above 6 CNY, experimenter demand seems not significant. Social influence (Cason and Mui, 1998) was an unlikely confound. If it were, there should be a greater effect when the endowment was 20 CNY, when 3rd dictator subjects are more similar to 1st dictators, than when the endowment was 32 CNY. However, we found just the opposite. The correlation between the 2nd dictator giving and the 1st dictator giving was 0.36 with a p-value of 0.3 when the 3rd dictator had the identical endowment with the 1st dictator of 20 CNY, but a correlation of 0.77 with a p-value of 0.01, when the endowment was 32 CNY. Inequity aversion was also an unlikely driver of our results. 3rd dictators' giving was not significantly correlated with the assigned endowment of 20 and 32 CNY (2-tail fisher exact test p-value=0.371), the endowment levels for our main results.

We surveyed the beliefs of subjects in our implicit group to check for saliency. Among our 20 and 32 CNY endowment subjects, 11 subjects were in the implicit group. 3 said they understood neither silent exiting nor double-blindness of the design. 4 said

they didn't understand at least one. 4 said understood both. However, in contrast to the rest of the group, regression of their giving on the 1st dictator giving on these alone was not significant. We think that the survey results could be driven by the false consensus effect because those who said they didn't understand were more similar to the explicit group. Why those who said they didn't understand at least one of double-blindness or silent exiting in the implicit group was more similar to the explicit group is still unclear to us.

V. CONCLUSION

We show the existence of type-preferences where the amount of transfer to a first giver by another subject differed according to the signaled 'generosity' of the first giver. Shame, guilt, social influence, and experimenter demand are shown to be unlikely causes of our results. Our use of exogenous endowment allowed us to disentangle the effect of signaled generosity of 1st dictator giving to the 2nd dictator on 3rd dictator giving from the endowment effect usually present with 2 player trust games. We showed that the effect of endowment was insignificant. We also showed no difference between explicit or implicit double-blindness and silent exiting except with the lowest endowment.

VI. REFERENCES

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APPENDIX A. SUMMARY OF TRANSFERS AND ELICITED BELIEFS

Average	Observed 1 st dictator transfer	3 rd dictator transfer	Ex-post 2 nd order belief of 3 rd dictator
Implicit group	5.4(4.8)	2.1(3.0)	5.7(5.6)
(limited to positive 1 st transfer)	7.4(4.0)	2.4(3.3)	5.9(4.7)
Explicit group	4.6(4.0)	2.3(2.4)	5.3(4.7)
(Limited to positive 1 st transfer)	6.9(2.7)	2.5(2.4)	6.0(4.8)

Table 1. Summary of Transfer and Elicited Beliefs

APPENDIX B. INSTRUCTIONS

IMPLICIT TREATMENT

Welcome to our experiment. You are going to make some decisions which will affect your payoff from this experiment. So please make sure that you understand all the rules well before you making your decision.

Please listen to the experimenter's explanation of the rules and complete the experiment carefully. If you have any questions, please directly raise your hand to inform our experimenters. Do not attempt to talk or make any cue to any other participants in the session. Do not attempt to look at other participants' experimental materials. Or we may cancel your participant status and refuse to give you any payment.

Please feel free to ask questions at any point if you feel you need clarification. Please do so by raising your hand. Please DO NOT attempt to communicate with any other participants in the session until the session is concluded.

Now, everyone can draw an envelope from the box in the front of the classroom. Please do not select among the envelopes or draw for other subjects. Once you finish drawing, you can go back to your seat.

Now please open the envelope. There is one questionnaire, 2 cards and some money in the envelope.

This experiment is divided into 2 stages. Player 1 participated in the last stage where he was asked to divide money between himself and another subject (a different subject, we called him/her 'player 2'). After that, he/she was asked to finish the same psychological questionnaire. Player 1's decision in the dividing will be shown to you today. The participants of this round, or you, are called player 3.

Players 1 were not told about the existence of this round. That means, when the player 1 made their choices (to give to you), they were not told the amount they transferred would be multiplied by 3 and plus 5. They were not told that you could pay them back in this round. They were not told that the money was given by you.

This round is the final round of the experiment. Your dividing will not be taken as

material for any other experiment and you will not receive any extra payment.

Now please take the card with 'for player 2' written on it from the envelope. This card was written by player 1 with the money he would like to give to player 2 from his 20 CNY.

Now please count the amount of money in the envelope and see whether it's equal to the amount written on the other card. This is the amount you are able to divide between you and player 1.

Now you can write down the amount you would like to give to the player 1 on the other card and put the money back into the envelope. Please take the rest money off as your experimental payment. Note: you can give any amount you want with the bills and coins in the envelope.

Now please put the money to player 1 and the 2 cards into the envelope and put it into the box in the back of the classroom.

Note: Do not put the questionnaire into it because you will be required to finish it after returning the envelope. Also, remember to take off your experimental payoff.

Please start to answer the questionnaire. After finishing it, please put it in the box in the front of the classroom and leave quietly. If you have any questions, please raise your hand to inform the experimenter.

EXPLICIT TREATMENT

Welcome to our experiment. You are going to make some decisions which will affect your payoff from this experiment. So please make sure that you understand all the rules well before you making your decision.

Please listen to the experimenter's explanation of the rules and complete the experiment carefully. If you have any question, please raise your hand to inform our experimenters. Do not attempt to talk or make any cue to any other participants in the session. Do not attempt to look at other participant's experimental materials. Or we may cancel your participant status and refuse to give you any payment.

Please feel free to ask questions at any point if you feel you need clarification. Please

do so by raising your hand. Please DO NOT attempt to communicate with any other participants in the session until the session is concluded.

Now, everyone can draw an envelope from the box in the front of the classroom. Please do not select among the envelopes or draw for other subjects. Once you finishing drawing, you can go back to your seat.

Now please open the envelope. There is one questionnaire, 2 cards and some money in the envelope.

Note: Once you finished your decision you seal your envelope and place it in the box marked return envelopes in the back of the classroom. Every envelope returned looks the same thus no one else, including our experimenter will know your individual decision.

This experiment is divided into 2 stages. Player 1 participated in the last stage where he was asked to divide money between himself and another subject (a different subject, we called him/her 'player 2'). After that, he/she was asked to finish the same psychological questionnaire. Player 1's decision in the dividing will be shown to you today. The participants of this round, or you, are called player 3.

Players 1 were not told of this experiment. That means they did not know the possibility that they would be paid later by you. And since they have made their decision and left, they will not be recruited or given any information on this experiment in the future.

The money you divide to him/her will be directly sent to player 1's bank account without any description with their payoff in the 1st stage.

This round is the final round of the experiment. Your dividing will not be taken as material for any other experiment and you will not receive any extra payment.

Now please take the card with 'for player 2' written on it from the envelope. This card was written by player 1 with the money he would like to give to player 2 from his 20 CNY.

Now please count the amount of money in the envelope and see whether it's equal to the amount written on the other card. This is the amount you are able to divide between you and player 1.

Now you can write down the amount you would like to give to the player 1 on the other card and put the according money back into the envelope. Please take the rest

money off as your experimental payment. Note: you can give any amount you want with the bills and coins in the envelope.

Now please put the money to player 1 and the 2 cards into the envelope and put it into the box in the back of the classroom.

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Please start to answer the questionnaire. After finishing it, please put it in the box in the front of the classroom and leave quietly. If you have any question, please raise your hand to inform the experimenter.