Title: Separating Gratitude from Guilt in the Laboratory

Author: David Ong

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Abstract: In contrast to guilt based reciprocity, which hypothesizes that reciprocity is an increasing function of the 2nd order expectation of trustor’s 1st order expectations for reciprocation, we test for reciprocity which is a decreasing function of trustees 2nd order expectations, i.e., that people can reciprocate out of gratitude. To unambiguously decrease 2nd order expectations in our treatment, we broke up a standard trust game into a two stage dictator game where the 1st round dictator was not informed about the possibility of a 2nd round. Furthermore, the 2nd dictator could “silently exit” by not sending anything to the 1st round dictator. We found a significant increase in both the amount of reciprocation and the number of people reciprocating as compared to our standard trust and dictator games controls. Most 2nd dictators became poorer than 1st dictators so inequality aversion can be ruled out. We found support for our hypothesis in the prior data of others who tested for guilt based reciprocity. Our result also seems to reconcile conflicting results in that literature. To our knowledge, this is the first paper which shows that kindness distinct from guilt, shame, efficiency, and inequity aversion could be a motive for reciprocity. Our strong positive reciprocity result also suggests why it has been difficult to find in the past.

Keywords: reciprocity, guilt, gratitude, trust game, double-blind, silent exiting

JEL Classification: C91, C72, D64

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1 Email: LinH@econ.umd.edu, Tel: +1-240-478-0550, Economics Department, University of Maryland at College Park.
2 Corresponding author: dvdong@gmail.com, David Ong, C402, Peking University HSBC Business School, University Town, Shenzhen, P.R.C, 518055.
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Introduction

The importance of reciprocity has long been recognized in economics, particularly in contracting when institutions of legal enforcement are underdeveloped or absent, e.g., in emerging markets, or in social settings where contracts are implicit and informal. Prior experimental literature has largely demonstrated that people are not entirely selfish (in the colloquial sense of the word). However, as Charness and Rabin (2001) pointed out, evidence for positive reciprocity, where recipients of favor respond more generously than dictators with the same amount of endowment, has been elusive (p. 22). They regarded McCabe et al. (2003) as a notable exception in the literature (Charness and Rabin, 2001, p. 22). However, even in that experiment, inequity aversion and psychological motives like guilt and shame, which are not obvious forms of positive reciprocity, are still possible confounds. These psychological confounds could also explain why positive reciprocity has been difficult to find in other experiments where beliefs had generally not been the focus of the experiment and hence, were not well controlled. In standard gift exchange or trust experiments, recipients could believe that the sender gave in order to provoke reciprocation. In that case, the recipients might discount the generosity of the senders’ motives and hence, might feel that positive reciprocity was not warranted. The less the recipient believed that the giver was expecting the reciprocation, the more generous the giving would be. There are a number of works in the psychology literature on ‘gratitude’ or ‘thankfulness’. See for Emmons & McCullough (2004) a survey in psychology. Nowak and Roch (2007) apply the concept to indirect reciprocity. We focused upon an operational definition of gratitude\(^3\) -- reciprocating behavior that is a decreasing function of those 2\(^{nd}\) order expectations. In contrast, the current literature on belief based reciprocity has largely focused on “guilt,” which has been modeled as an increasing function of the beliefs of the receiver/trustee in gift exchange/trust games.

\(^3\) We are grateful to Luz Marina Arias for pointing out that some people are more likely to reciprocate if they knew that a gift giver had not been expecting anything in return.
In this somewhat extended introduction and literature review, we discuss the problems and anomalies in this literature first in order to motivate both our hypothesis of gratitude and our somewhat radical methodology for measuring it. We argue that these largely center around the difficulties of controlling 2\(^{nd}\) order beliefs when players are fully aware of the game structure. We also show that though prior work tested for an alternative hypothesis, we are able to find support for our hypothesis in their data. We then discuss the implications of our study for why positive reciprocity has been difficult to find.

The guilt based reciprocity literature hypothesizes that people reciprocate out of disappointment aversion. Charness and Dufwenberg (2006; hereafter, CD) showed that cheap talk promises prior to trust games were correlated with the average giving of trustors and the average reciprocation by trustees (pp. 1587-1589). Furthermore, they found that average reciprocation of trustees correlated with average trustees’ 2\(^{nd}\) order beliefs in trustors’ 1\(^{st}\) order beliefs in reciprocation (CD, 2006, p. 1589). However, CD did not reveal the actual beliefs of trustors to the trustees with whom they were paired. Thus, they could not establish causation. Ellingsen et al. (2010) in an attempt to establish causation, revealed the actual belief of each trustor to a paired trustee. They found that the trustor’s belief had no significant effect on the paired trustee’s reciprocation, seemingly refuting the guilt based reciprocity hypothesis (Ellingsen et al., 2010, pp. 98-101). Particularly interesting for our gratitude hypothesis was the fact that the majority of trustees who faced the most pessimistic beliefs of trustors reciprocated the most (Ellingsen et al., 2010, p. 101). This indicates a counter-guilt factor consistent with our gratitude based reciprocation hypothesis.

Reuben et al. (2009) tested the guilt based reciprocation hypothesis with a different design. To get trustors to truthfully reveal their expectations to trustees, Ellingsen et al. (2010) did not tell trustors that their beliefs would be revealed to trustees (pp. 98-100). To get trustees to believe that trustors had been truthful, they told the fact of the omission to trustees (Ellingsen
et al., 2010, pp. 98-100). Reuben et al. (2009) did not use asymmetric instructions, which might cause trustees to question whether the experimenter had been forthright with them as well (p. 90). Subjects played twice in each role of sender and receiver (Reuben et al., 2009, p. 90). Reuben et al. (2009) elicited subjects’ beliefs when they were senders during a round when beliefs were unused and then used them in the 2nd round (pp. 89-90). Reuben et al. (2009) also used significantly higher payments for both belief elicitation and for actual play (p. 90). They did find a significant correlation between revealed beliefs of trustors and the reciprocating behavior of the paired trustees (Reuben et al., 2009, p. 90). However, even here when beliefs were well controlled, about 10% of subjects went in the opposite direction predicted by guilt in each of their pessimistic and optimistic belief treatments (Reuben et al., 2009, p. 90).

Stanca et al.’s (JEBO 2009) study of intrinsic motivation suppressed 2nd order beliefs in a non-equilibrium setting. In their setup, the 1st player was not informed of the possibility of reciprocation in the 2nd stage (pp. 4-5). Hence, their 2nd player could not discount the kindness intent of 1st player giving as being merely done in anticipation of reciprocation. They found significantly increased positive reciprocation as compared to their full information control for their strategy elicitation (though not for their decision method elicitation) (Stanca et al., JEBO 2009, pp. 7-11). However, with respect to our hypothesis, this design does not indentify gratitude since guilt is still a possible confound. They controlled for guilt from ex-ante but not ex-post expectations. The 1st dictator was informed of the game structure in the 2nd stage (Stanca et al., JEBO 2009, pp. 12-13). Guilt of the 2nd player was still possible from the disappointment of ex-post expectations. Hence, aversion to possible disappointment could still drive reciprocation. Furthermore, unrelated to their hypothesis, but relevant to our gratitude hypothesis, people could reciprocate from shame. Subjects could not give 0 or low and not be noticed. The design was also not double-blind. Subjects might be
motivated by shame from experimenter or other subjects knowing about non-reciprocation (Hoffman et al. 1994, 1996).

We adopted a design similar to Stanca et al.’s (JEBO 2009), but had more controls for the above mentioned confounds and found strong evidence for positive reciprocity. In contrast to Stanca et al. (JEBO 2009) (p. 7), the proportion of subjects who reciprocated, as well as the proportion of endowment given back by our recipients who received a positive transfer, was significantly higher than both our dictator and standard trust games control.

Furthermore, our results for this apparent positive reciprocity were less likely to have been motivated by guilt as might have been the case in Stanca et al. (2009) or inequity aversion, efficiency, or shame, as might have been the case in McCabe et al. (2003). As in Stanca et al. (JEBO 2009) (pp. 4-5), we controlled anticipation of the guilt from disappointing ex-post expectation by also not informing the 1st round dictator of the possibility of a 2nd round. However, we also further diminished the possibility of guilt from ex-post beliefs by permitting implicit “silent exiting” (Dana et al., 2006, pp. 194-195) with our payment procedure. Inequity aversion as the motive for reciprocation was ruled out in the results.

The fact that trustees gave significantly more when they were less sure that they were expected to reciprocate is evidence that guilt crowds out gratitude. This could help explain the conflicting results in Ellingsen et al. (2010) (p. 101) and Reuben et al. (2009) (p. 90), as well as the scarcity of results on positive reciprocity in prior experiments mentioned by Charness and Rabin (2002) (p. 22). First, the opposition between these belief based incentives of guilt and gratitude could make guilt experiments very sensitive to the beliefs of subjects. Similar experiments are likely to yield conflicting results for even slightly different setups. Secondly, trustees could discount the generosity of trustors giving as motivated by the anticipation of guilt, resent the manipulation, and thus, not reciprocate at all. The conflicting effects of gratitude and guilt can be seen even where positive reciprocity had been found,
e.g., Cox (2004). He isolated the effect of reciprocation by comparing each part of a standard trust game to two independent dictator games (Cox, 2004, pp. 264-266). Since he was not investigating the effects of beliefs, he allowed 2\textsuperscript{nd} order beliefs about trustors’ 1\textsuperscript{st} order beliefs to range freely. 4/13 trustees transferred zero when trustors’ gave less than $10. 5/13 trustees transferred zero when trustors’ gave $10, the maximum amount (Cox, 2004, pp. 271-272). This otherwise counter-intuitive result would make sense if some trustees put more weight on some trustors being manipulative when trustors gave more.

1) Experimental Design

We highlight the novel part of the design first before going into details. We used the decision method of elicitation. To avoid strategic behavior, we gave our subjects instructions step-by-step. We broke the trust game into a two stage dictator game. The 1\textsuperscript{st} stage dictator (1\textsuperscript{st} dictator) was not informed of the 2\textsuperscript{nd} stage to prevent the 1\textsuperscript{st} dictator from giving to incite guilt and also to prevent the 2\textsuperscript{nd} dictator believing that it was the 1\textsuperscript{st} dictator’s motive in giving. The 2\textsuperscript{nd} stage dictator (2\textsuperscript{nd} dictator) was informed of the game structure and the 1\textsuperscript{st} dictator’s lack of knowledge of the 2\textsuperscript{nd} round, and thus also of the possibility of reciprocation by the 2\textsuperscript{nd} dictator. To avoid guilt from ex-post expectations, the 2\textsuperscript{nd} dictator was told that the 1\textsuperscript{st} dictator would not be informed about the source of the money, which we sent by electronic bank transfer with the money which they did not give to the 2\textsuperscript{nd} dictator.

We also had ‘secret giving’ in the sense that we told 2\textsuperscript{nd} players that 1\textsuperscript{st} are not aware of this round and will not be told money is from the 2\textsuperscript{nd} player should he/she send any money.

We adopted secret giving to diminishes the possibility of correlated psychological utility, e.g., player 2 giving to player 1 because he/she gets utility from the thought of player 1 is

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\(^4\) We thank James Cox for providing us with his data. We also thank an anonymous referee and Alexander Smith for suggesting that we make this comparison.
grateful/appreciative, rather than from own gratitude/appreciation. Note that secret giving implies silent exiting but not conversely.

The following is a summary of the procedures. Please refer to the game tree in Figure 1.

**In Stage 1**, subjects were told that they could divide 20 CNY with another player and that the remainder would be sent to their bank account in 3-4 days. Hence, dictator 1’s payoff was 20-x, where x was the amount sent to dictator 2. As mentioned, the dictator 1s’ were not informed of possibility that the 2nd dictators could reciprocate.

**In Stage 2**, the 2nd dictators received 3*transfer from the 1st dictator + show-up fee of 5 CNY. We did this to allow a comparison with standard trust games in the literature, in particular Stanca et al. (2009), whose design we modified. Then the 2nd dictators could decide to give something back. Hence, dictator 2s got 3x+5-y, where y was the amount sent to dictator 1s. Dictator 1’s final payment was 20-x+y.

Dictator 2s were informed that the 1st stage dictators were not informed about multiplication by 3 of dictator 1’s money; we wanted avoid dictator 1 giving out of an efficiency motive to dictator 2. This also allowed us to use 1st stage giving as a control for 2nd stage giving, where the efficiency motive was also absent. Dictator 2 also knew that dictator 1s were not informed about the possibility of reciprocation, nor would they be informed about the source of money, should dictator 2s reciprocate. 2nd dictators were told that this was the last stage and that there would be no further payments. Reciprocated money was sent with the stage 1 payment to dictator 1 through a non-descript electronic deposit which the 1st dictators might not even notice as including an extra payment. They took what money they did not give back upon leaving the experiment.
The experiment involved 90 subjects recruited from graduate students at Shenzhen University Town. For the main treatment, 70 subjects were put into 35 pairs in the two stage dictator game. “Control 1” is the decision of the 1st dictators while the main treatment was the decision of the 2nd dictator. The other 20 subjects were in the standard trust game. “Control 2” is the decision of these trustees. Figure 2 shows the main forms that subjects filled out. We elicited the factual 2nd order beliefs of the trustees in control 2 as to the expectations of the trustor. We also elicited the counterfactual 2nd order beliefs of the 2nd dictators about the first dictators expectations for reciprocation had they known about the 2nd stage in the treatment. See Appendix A for the translation of the instructions.
2) **Main Results**

The summary of our main results is as follows. In our control 1, 22/35 = 63% of our 1st dictators gave a positive amount. This is the standard result. The mean giving (ratio of the average giving over average endowment) of these subjects was 4.4/20 CNY = 22% (same as the average of the ratio). In control 2, 8/10 = 80% of these trustees in this standard trust game reciprocated. Mean giving was 6.3/21.5 CNY = 29%, with the average of the ratio being 31%. However, in the treatment, 22/22 = 100% of the 2nd dictators who received positive transfers from the 1st dictator reciprocated⁵. Mean giving was 10.8/26 CNY = 41%, with the average of the ratio being 39%. The difference in mean giving (and in mean ratio of giving) between the 2nd dictator in the treatment and the trustee in the trust game control was significant at the 5% level using the Mann-Whitney-Wilcoxon test. The difference between the 2nd stage dictator and the 1st stage dictator giving in the treatment was significant at the 1%. Inequity aversion was

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⁵ There was one 1st dictator who transferred 0.1 CNY. Since 0.1 is much closer to 0 than any other positive transfers, we did not include the 2nd dictator who received this 0.1 into the data of 2nd dictators who received amounts greater than 0.
also ruled out by the result. $2/35 = 5.7\%$ of the 1st dictator gave more than half of their endowment. (Only these 1st dictators could have thought they were poorer than their 2nd dictator pair, given that they did not know their transfer would be multiplied by 3 and have 5 CNY added.) However, $17/22 = 77\%$ of the 2nd dictators who received a positive amount from 1st dictators were poorer than their 1st dictator pair, after giving. These 2nd dictators knew the true payoffs of the 1st dictators. This rules out inequity aversion as their motive for giving.\(^6\)

The asymmetric reciprocation between the control trust game and the treatment two stage dictators games can be seen in figure 3 in the reciprocation of 2nd players who received more than zero from the first players. Only $2/10 = 20\%$ of trustees in the trust game control gave more than they were given. In contrast, $14/22 = 64\%$ of 2nd dictators in the treatment gave more than they were given. The difference between the treatment and the control is even clearer when we compare the average giving of the 2nd dictators conditional on 1st dictator giving which is clearly above 1st dictator giving. See Treatment and Control 1 in figure 3 below starting from subject pair number 14-35. Guilt based reciprocation, on the other hand, tends to be mere payback as can be seen in how 2nd dictator giving is generally not more than 1st dictator giving in Control 2 in figure 3.

\(^6\) Cox (2004) also identified reciprocity by ruling out other factors like inequity aversion by having a separate set of control dictator games, rather than comparing the reciprocation of a sequence of dictator games as we have. His experiment was also not about guilt or gratitude, and he did not attempt to identify the impact of beliefs.
Unlike most prior trust games, we allowed our 2nd dictators to still give to 1st dictators even when they had not received any money from the 1st dictator. These 2nd dictators transferred on average 1.8 CNY of their 5CNY show up fee. One possible explanation is that these 2nd dictators may have been trying to induce guilt or shame in the in 1st dictators by making them aware of the game. It could also be a “small money” effect found in Gneezy and Rustichini (2000, pp. 802-806) and Ong and Lin (2011, p. 8). There, when subjects with exogenous and small endowment, they behaved systematically differently.

We also tested the guilt and shame sensitivity of all subjects using a standard test from psychology, the TOSCA-3. See Tangney and Dearing (2002) for details (pp. 207-214). We gave them the test in part as a distracter since they might have found it strange to get “money for nothing”. We found no significant correlation between guilt sensitivity either in 1st dictators’ giving, or in 2nd dictators’ reciprocation, or trustees’ reciprocation in the standard trust game. The lack of significance of the 2nd dictator reciprocation would be predicted if
gratitude was something distinct from guilt. The lack of significance with 1st dictator giving could be due to irrelevance of guilt as a motive. The lack of significance with trustee giving in the standard trust game could be due to small sample size, which in this case was 10 subjects.

3) Discussion

Our results are significantly stronger than Stanca et al.’s (JEBO 2009) results\(^7\). There, identical with us, 1st dictator’s transferred 22% of their endowment in treatment. However, their 2nd dictator reciprocation decreased to 12% for strategy method elicitation and 10% for decision method elicitation. That is considerably less than our 41% decision method elicitation. We chose the decision method because prior studies had shown that the strategy method may not be a reliable tool to measure reciprocity. Fehr et al. (2003) concluded that the strategy method is less emotionally arousing than the decision method (p. 6). Casari and Cason (2009) concluded that measured trustworthiness was lower using strategy method elicitation (p. 158). Our results could also be stronger because people in China are less accustomed to giving by strangers. Hence, the surprise might cause them to reciprocate more.

Lazear et al. (2006) also had a two stage dictator game with an uninformed 1st dictator and a 2nd dictator who could silently exit. They found significant silent exiting by the 2nd dictator (32%) (p. 37), where we found none. However, they restricted the choice set of 1st dictators to either (7,5) or (6,6), which meant that 1st dictators could only give 14% of their endowment (Lazear et al., 2006, p. 15). This would have made an important difference in our experiment where only one of our 22 1st dictators who transferred a positive amount transferred less than 14%. Limiting the amount of giving by 1st dictators could induce a norm/reference point for the 2nd dictator to follow. More importantly, such a ceiling on giving would limit the gratitude that the 2nd dictators could feel. Furthermore, if “type preference” as introduced by Gul and

\(^7\) We thank Luca Stanca for providing us with their data.
Pesendorfer (2010) was a motive for the 2\textsuperscript{nd} dictators’ giving (pp. 1-4), then 1\textsuperscript{st} dictators’ signal of the generosity of their type would be hampered. Under such a restricted choice, a specific 2\textsuperscript{nd} dictator could only know that their paired 1\textsuperscript{st} dictator was not mean. That might warrant only a similar lack of meanness from the 2\textsuperscript{nd} dictator – silent exiting. Furthermore, the lowered ex post belief of the 1\textsuperscript{st} dictator, which could have been anticipated by the 2\textsuperscript{nd} dictator should the 2\textsuperscript{nd} dictator not silently exit, would induce less guilt aversion in the 2\textsuperscript{nd} dictator, decreasing the amount of giving when 2\textsuperscript{nd} dictator does actually not silently exit. Small amounts of money may also have counter intuitive incentive effects as found by Gneezy and Rustichini (2000) (pp. 802-806).

We, similar to a number of other papers, used omission\textsuperscript{8} to get our treatment effect. See Stanca et al. (JEBO 2009) (pp. 4-5), Stanca et al. (AEL 2011) (p.1710) and other experiments where subjects could silently exit from altruistic situation with an unaware opponent: Dana et al. (2006) (pp. 194-195), Lazear et al. (2006), (pp. 14-15), DellaVigna et al. (2012) (pp. 2-3, 14-16), and Andreoni et al. (2011) (pp. 3-6). Ellingsen et al. (2010) also used asymmetric instructions (pp. 98-100). One possible confound from using omission is that subjects who knew about the omission could wonder if they themselves had not been told something. In our case, 2\textsuperscript{nd} dictators could have wondered if there could be another round, of which he 2\textsuperscript{nd} dictator had not been told. 2\textsuperscript{nd} dictators then could have been giving to 1\textsuperscript{st} dictators with the expectation of indirect reciprocity themselves. However, unlike 1\textsuperscript{st} dictators, 2\textsuperscript{nd} dictators were told that this was last round. They would have had to suspect us of outright deception instead of omission.

\textsuperscript{8} Croson’s (2005) survey of the use of deception in economics and psychology experiments notes that “many of the benefits arising from deception can be enjoyed by simple omission (not informing the participants of the subject of the experiment, or doing so only very generally) rather than by commission (explicitly lying to the participants)” (p. 140) citing : Andreoni 1988; Croson 1996a, 1996b; Boles, Croson and Murnighan 2000; Croson, Boles and Murnighan (2003). Hertwig and Ortmann’s (2008) survey affirms this distinction which they also made in 2001 “that withholding information about research hypotheses, the range of experimental manipulations, or the like ought not to count as deception.” and furthermore, “common ground has not (yet) been established with respect to the violation of participants’ default assumptions.” (pp. 61-62)
There are several possible limitations to our study. First, we assumed like Stanca et al. (JEBO 2009) (pp. 4-5) but did not prove that by not informing 1st dictators about the possibility of the 2nd dictator reciprocating, we had diminished the ex ante 2nd order belief of the 2nd dictator that the 1st dictator was expecting reciprocation. We did survey for the counter-factual beliefs of the 2nd dictator of about 1st dictator beliefs had the 1st dictator known about the 2nd round. Unlike Ellingsen et al. (2010) (pp. 99-101), this was highly significantly correlated with actual 2nd dictator reciprocation. This is evidence that ex-post beliefs mattered and therefore, could drive guilt driven reciprocity, as we suggested was a limitation of Stanca et al. (JEBO 2009) with respect to testing our gratitude hypothesis.

This significant correlation, though would not be additional evidence for guilt based reciprocity. 2nd dictator giving and 2nd order counter-factual beliefs could have been driven by the amount and the implied beliefs of 1st dictator giving. In other words, the 2nd dictator gave more when he had more to give, not only because he believed that 1st dictator expected more. We could not separate the independent effects of endowment and implied beliefs on 2nd dictator giving. 2nd order beliefs and the endowment had a correlation of 0.93. The consequent multi-collinearity yielded no significant regression. What the correlation does indicate is that the 2nd order beliefs would have been a confound for the apparent generosity of 1st dictator giving had the counter-factual world been true, supporting ours and Stanca et al. (2009)’s finding that people might reciprocate more when they do not believe that the reciprocation had been expected. The 2nd order beliefs of trustees in the control group confirmed results from CD 2006 (p. 1589). These were correlated with trustee giving at less than 1% level of significance when regressed on both 2nd order beliefs and the endowment from the trustor.

Second, our silent exiting for 2nd dictators was implicit. 2nd dictators were told that 1st dictators were uninformed about the existence of the 2nd round. Furthermore, they were told
that the 1st dictators would not be told that the money was from them, the 2nd players, should they choose to reciprocate. To see that this implies silent exiting, note that the statement which rules out silent exiting, “1st dictator will be told that you did not give back anything” would be inconsistent with the spirit and the letter of this instruction. In any case, the effect of guilt should be weaker than in either standard trust games or in Stanca et al.’s (JEBO 2009) design, if not eliminated. Third, the double-blindness in our experiment was also implicit and unannounced. We simply had no identifiers for 2nd dictators’ identity on the envelopes they submitted for giving. We chose not to make our double-blindness more salient in order to avoid possible demand effects as discussed in Loewenstein (1999) (p. 30). Furthermore, Barmettler et al. (2011) found no difference between implicit double-blindness and single-blindness (p. 14). If the double-blindness was not salient, then subjects might still feel some shame from “appearing” ungrateful before experimenter, in which case, it would be a 2nd order reaction to gratitude. The use of implicit silent exiting and double-blindness should in any case have decreased both shame and guilt and hence should not have led to significantly increased rates of reciprocation in our treatment, if shame and guilt were the cause.

Third, we also had a small number of observations. However, our results should be read as a partial replication of Stanca et al.’s (JEBO 2009) As was discussed in the introduction, our hypothesis was corroborated as the driver of the contra-guilt data in Ellingsen et al. (2010) (p. 101) and in Reuben et al. (2009) (p. 90), and in the apparent conflict between their main results.

Fourth, we leave for future work how to reconcile gratitude and guilt within a general model of reciprocity. As mentioned in the introduction, our strong positive reciprocity result

\textsuperscript{9} The exact statement was “They were not told about the existence of this round. That means, when player 1 made their choice (to give to you), player 1s were not told the amount they transferred will be multiplied by 3 and plus 5. They were also not told that you can pay them back in this round. They will not be told that the money is given by you.”
when guilt was suppressed suggests that gratitude is not merely a substitute for guilt, but can be “crowded out” by guilt. How this crowding out occurs and what determines whether a person feels gratitude or guilt is beyond the scope of this paper. However, one possible reason for our results is that part of gratitude is motivated by “type preference.” 2nd dictators might like to give more to those 1st dictators who gave them more because 2nd dictators prefer to give more to people who have shown themselves to be more altruistic. To test this, Lin and Ong (2011) introduced a 3rd dictator (30 subjects) who observed 1st dictator giving to 2nd dictator and could give to 1st dictator from three exogenous levels of endowment: 6, 20, 32 CNY (p. 9). They also had implicit and explicit double-blindness and silent exiting treatments (Ong and Lin, 2011, p. 10). They found that 3rd player giving correlated with 1st dictator giving but not with endowment when regressed on both in all cases except for the 6 CNY explicit double-blindness and silent exiting treatment (Ong and Lin, 2011, pp. 10-11). In that case the correlation was inverted. They propose that this could be a demand effect interacting with the small endowment (Ong and Lin, 2011, p. 12). They also found no difference between explicit and implicit double-blindness except for the 6 CNY (Ong and Lin, 2011, p. 11). Their result supports our result and implies that type preferences contributed to reciprocation in our experiment. Thus, their result seems to address the limitations we mentioned above in regards to separating the belief from endowment effect and the saliency of implicit double-blindness and silent exiting and sample size.

In summary, our results suggest that guilt and inequity aversion are unnecessary for reciprocation. Gratitude can also drive reciprocation. We found greater reciprocation when guilt was ruled out, indicating that guilt might crowd out gratitude. Our result could help explain opposing results in Ellingsen et al. (2010) (p. 101) and Reuben et al. (2009) (p. 90). The opposing belief dependent incentives would make experiment sensitive to setup. Our strong positive reciprocity result also suggests why it has been difficult to find in the past.
4) References


Appendix A. Instructions of the Experiment

2 Stage Dictator Game

INSTRUCTION FOR STAGE 1

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 make your decisions.

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

Now please open the envelope you were given when you entered the room.

There is a questionnaire and a card in it.

Please put the questionnaire aside and read this card.

In this experiment, you can divide 20 CNY between yourself and another subject.
This experiment is anonymous. You and this player 2 (you play against) are randomly paired.

Now please write down the money you would like to give to this subject on the card. The amount of money you leave to yourself will be sent to your bank account in 3-4 days.

Now please put the card back into the envelope and put the envelope into the box in the back of the classroom. Do not put the questionnaire in it.

Now you can begin to answer the questionnaire. Once you finish it, you can put it on the desk and leave the classroom quietly. Note: there are blanks at the end of the questionnaire to fill in the number of your bank account and name.

If your bank account is not from Pingan Bank, please make sure you write down the information of the deposit bank.

If you have any questions on payoff delivery, please contact the experimenter privately after the experiment.

INSTRUCTION FOR STAGE 2

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 make your decisions.

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

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 others.

Once you have an envelope, 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. The participants of the last stage are called player 1. The participants of this stage, or you, are called player 2. In the last stage, players 1 were asked to divide 20 CNY between himself and player 2.

This experiment is anonymous. You and this player 1 (you play against) are randomly paired.

Now please take the card with ‘for player 2’ written on it from the envelopes.

This card was left by player 1 with the money he would like to give to you from 20 CNY.

Now please count the amount of money in the envelope. This amount is given by the participant of last stage (player 1) multiplied by 3 and plus 5. Please make sure the amount is correct.

Now you can write down the amount you would like to give back to player 1 on the other card and put the stated amount of money back into the envelope. Please take the rest money with you, which is your experimental payment.

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

This is the last stage of the experiment. You will not receive any extra money except for those you take from the envelope.

Now please put the 2 cards as well as the money to player 1 into the envelope and put it into the box in the back of the classroom. Note: please do not put the questionnaire into the envelope. Please take 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.

_Trust Game_

**INSTRUCTION FOR STAGE 1**

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 make your decisions.

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

Now please open the envelope you were given when you entered the room.

There is a questionnaire and a card in it.

Please put the questionnaire aside and read this card.

There are two stages in this experiment. You, player 1, can divide 20 CNY between yourself and another subject, player 2, in this stage. In the next stage, player 2 will receive an amount which equals the amount given by you multiplied by 3 and plus 5 CNY show-up fee. After player 2 receives this amount, they can give any amount from it back to you. Player 2 will keep any remaining amount as their experimental payoff. Your experimental payoff will be the sum of the amount you leave to yourself in this stage and the amount player 2 given to you in the next stage. The next stage will begin in several hours.

This experiment is anonymous and you and this player 2 (you will play against) are randomly paired. All the information of this game is given to both players.
Now please write down the money you would like to give to player 2 on the card. Your experimental payoff will be sent to your bank account in 3-4 days.

Now please put the card back into the envelope and put the envelope into the box in the back of the classroom. Note: please do not put the questionnaire in it.

Now you can begin to answer the questionnaire. Once you finish it, you can put it on the desk and leave the classroom quietly. Note: there are blanks at the end of the questionnaire to fill in the number of your bank account and name.

If your bank account is not from Pingan Bank, please make sure you write down the information of the deposit bank.

If you have any questions on payoff delivery, please contact the experimenter privately after the experiment.

INSTRUCTION FOR STAGE 2

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 make your decisions.

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

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 others.

Once you get an envelope, please 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. The participants of the last stage are called player 1. The participants of this stage, or you, are called player 2. In the last stage, players 1 were asked to divide 20 CNY between himself and player 2.

This experiment is anonymous. You and this player 1 (you play against) are randomly paired.

Now please take the card with ‘for player 2’ written on it from the envelopes.

This card was left by player 1 with the money he would like to give to you from 20 CNY.

Now please count the amount of money in the envelope. This amount is given by the participant of last stage (player 1) multiplied by 3 and plus 5. Please make sure the amount is correct.

Now you can write down the amount you would like to give back to player 1 on the other card and put stated amount of money back into the envelope. Please take the rest money with you, which is your experimental payment.

This is the last stage of the experiment. You will not receive any extra money except for those you take from the envelope.

Now please put the 2 cards as well as the money to player 1 into the envelope and put it into the box in the back of the classroom. Note: please do not put the questionnaire into the envelope. Please take 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.