



Trust, trust games and stated trust: Evidence from rural Bangladesh

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ABSTRACT

Levels of trust are measured by asking standard survey questions on trust and by observing behavior in a trust game using a random sample in rural Bangladesh. Follow-up questions and correlations between stated expectations and the amount sent in the trust game reveal that the amount sent is correlated with a general measure of trust. The trust and need motives combined with expectations explains differences in amounts sent, and this highlights the potential importance of motives that cannot be inferred directly from people's behavior and expectations alone.

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1. Introduction

There is a great deal of recent theoretical and empirical evidence that trust between people fosters co-operation and economic activity and is hence crucial for economic and social development (e.g., Fukuyama, 1995; Knack and Keefer, 1997). Not surprisingly, the interest in measuring and explaining the determinants of trust, especially in developing countries, has therefore increased rapidly over the last decade. Unfortunately, trust turns out to be difficult to measure. The main objective of this paper is to contribute to our understanding about what the main methods used – trust games and trust surveys – actually measure, based on a non-student sample in a developing country.

Trust has been measured either by using attitudinal trust, most commonly framed by the question “Generally speaking, would you say that most people can be trusted, or that you cannot be too careful in dealing with people?” as in the General Social Survey (GSS) question, or by conducting a trust game; see Wilson and Eckel (2011) for a comprehensive state-of-the-art overview of different ways of measuring trust. Trust surveys have been criticized for not implying consequences for the respondents, which is in contrast to trust games (also denoted investment games; Berg et al., 1995) where the decisions have real monetary effects. Briefly, a trust game is a two stage game involving a “sender” and a “receiver.” The sender is given a certain amount of money and has to decide how much of it to send to the anonymous receiver and how much to keep. Any positive amount sent by the sender is normally tripled before it is given to the receiver, who then decides how much to return to the sender. A selfish sender who anticipates that the receiver will be selfish too should send nothing to the receiver, since he/she should realize, by using backward induction, that the receiver has no incentive to send anything back. However, a Pareto improvement is possible if the receiver returns at least one-third of the tripled amount received.

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The amount sent by the sender is typically regarded to be an indicator of trust, and the amount returned by the receiver an indicator of trustworthiness.

Glaeser et al. (2000) combined a trust game with an attitudinal trust survey among undergraduate students at Harvard and found poor correlation between stated trust and the amount sent in the trust game, while the amount returned was significantly explained by stated trust. They concluded “that most work using these survey questions needs to be somewhat reinterpreted” (p. 814). This conclusion should be read in the light of their interpretation that a trust game measures trust and trustworthiness. Recent research on trust games has however found other motivations beyond pure trust and trustworthiness to be important, such as unconditional altruism and risk preferences (e.g., Cox, 2004; Holm and Danielsson, 2005; Karlan, 2005; Schechter, 2007). In the present study, we explicitly asked about the motives for sending and returning money using follow-up questions after a trust game. We also asked about the senders’ expectations with respect to how much they expected to get back from the receivers. In addition, we analyzed the outcome of the trust game *per se* by using conventional descriptive and regression approaches.

We conducted the combined attitudinal trust survey and trust game among household heads in rural Bangladesh. There are at least three advantages of using this sample rather than the more frequently used choice of a student sample in an industrialized Western country:¹ (i) We can afford to use financial stakes that are high in comparison to their normal wages, implying that the participants have strong incentives to take the game seriously and therefore think carefully about how to act. (ii) We obtain more variation in the socio-economic background variables. (iii) Bangladesh’s top ranking in the Transparency International’s corruption perception index five years in a row (2001–2005) (Transparency International, 2003, 2004, 2005)² makes it particularly interesting for a study of trust. The fact that individuals associated with public institutions are perceived to be corrupt may affect overall trust in society, and as argued by, e.g., Alesina and La Ferrara (2002), trust in existing institutions may affect trust in other people. This is different from the bottom-up perspective put forward by Putnam (1993), i.e., that trust develops largely through people’s interactions in local voluntary organizations.

Overall, our results support and extend the recent findings that the motives behind the observed behavior in trust games are more complicated and mixed than often believed. For example, in our case we found that the appearance of trust or trustworthiness may sometimes simply be a reflection of long-term self-interest, derived from the fact that one important stated motive for both sending and returning money was that people believed that they would be punished, either during their lifetime or in an after-life, if they acted too selfishly in the trust game. This type of information, we believe, would have been impossible to obtain based solely on revealed behavior.

The rest of this paper is organized as follows. Section 2 presents our survey and the organization of the experiment, Section 3 presents the main descriptive results from both the attitudinal trust survey and the trust game, Section 4 provides econometric analysis, and Section 5 summarizes and concludes the paper.

2. The survey and the organization of the experiment

Bangladesh consists of six divisions, each made up of several districts. In total, Bangladesh has 64 districts, 16 of which are located in the Dhaka division. The survey and the experiment were conducted in five districts of the Dhaka division: Netrokona, Mymensingh, Manikganj, Gazipur, and Narayanganj. The enumerators were first allocated to different parts of the village and then asked to visit every fourth household. If the household head was not around, the enumerators were instructed to return later and, if still unsuccessful, to use a replacement household.³ The trust game was conducted at the end of a rather extensive household survey. The participants were paid 100 Bangladesh Taka (TK) to complete the whole survey and the trust game. The subjects were informed about the trust game at the end of the survey. Half of the subjects participated in the game as senders, and half as receivers. Both the senders and the receivers were given the following initial information before the game was explained:

You will be confronted with a decision situation that involves real money. How much you earn depends on the choices made by you and another participant, and it may happen that you earn nothing. The other participant is any household head in the nearby village, [. . .], who is Hindu [or Muslim] by religion. That person is also participating in our survey and will get the same participation fee as you once you have completed the questionnaire. You will not know who the other participant is, and that person will not know who you are. Your choices and outcomes will be confidential. Please do not talk to anyone during the experiment.

¹ Needless to say, there are disadvantages too, including lower educational levels among the respondents, which may induce more cognitive errors. Moreover, it is more expensive and logistically difficult to set up a large-scale experiment in the countryside.

² This is a composite index based on the degree of corruption perceived by business people and country analysts. Corruption is defined as the abuse of public office for private gain, e.g., bribe-taking by public officials in public procurement.

³ We have approximately 23 percent replacement households in our final sample, which is almost solely due to lack of availability. Only 2 percent of those who were available did not want to participate in the survey. In the villages, people from the same family chain normally live in clusters of a few households. Thus, a replacement with the next household, or even with the next to the next household, should not bias our results.

Thus, both the senders and receivers obtained information about their co-player's religion. Johansson-Stenman et al. (2009) analyse, in detail, behavioral differences related to religious matching, and find no significant differences between the two religious sub-samples in terms of amount sent or amount returned.⁴

2.1. Visiting the senders

The senders were first explicitly asked if there was a private area of the home, free from interruption by others, that could be used for the experiment. To be able to run the experiment on a sample with illiterate participants, it was not possible to use a double-blind procedure. Instead, our chosen procedure is similar to the one used by, e.g., Barr (2003), Karlan (2005), and Schechter (2007). The enumerator began by reading the instructions to the sender, who was assured response confidentiality. To explain the game, the enumerator then presented the outcomes of different decisions to the sender, in terms of both the amount sent by the sender and the amount returned by the receiver, and it was stressed that the amount sent would be tripled before reaching the receiver. In the original trust game by Berg et al. (1995), the receiver was given the same initial amount of money as the sender. However, for practical reasons in the field, the procedure adopted in the present paper follows, e.g., Glaeser et al. (2000) and Baran et al. (2010), where the receiver was not given any initial money in the trust game. We will return to this issue when interpreting the results.

The senders were then given two envelopes. One of the envelopes contained the original endowment of 200 TK and the other was empty. The senders used the empty envelope for the money that they wanted to send to the receiver, and they were assured that the enumerator would not know their decision. In the experiment, we used thick envelopes in order to prevent enumerators from guessing by eye how much had been sent to the receiver, and thus implicitly the amount kept. The senders were informed that they would be paid within two days.⁵ By turning his/her back to the sender while the money was being put into the envelope, the enumerator ensured that the decision was private. The enumerator waited until the sender was ready, and then asked him/her to close the envelope that was to be sent to the receiver, seal it with a stamp provided by the enumerator, and return it to the enumerator. The senders were instructed to do this even if they had decided to send nothing.

After returning the envelopes, the senders were asked about their expectations regarding how much they believed they would receive back. The senders were given a piece of paper with three boxes numbered one to three, and an empty envelope into which they were to put the paper after privately marking the appropriate box indicating their expectation regarding the return transfer. The alternatives read to them, which were repeated as necessary, were: (i) check the first box if you expect to get less back than the amount you sent, (ii) check the second box if you expect to get roughly the same amount back as the amount you sent, and (iii) check the third box if you expect to get more back than the amount you sent. In the event that they did not send any money, they were asked to check the second box.⁶ Then the sender was asked to close the envelope, seal it with a provided stamp, and return it to the enumerator.

The enumerator then moved on to a discussion about the motives behind the amount chosen to be sent to the receiver. The senders were given a piece of paper with four boxes numbered from one to four and an empty envelope into which they were to put the answer sheet. While it could of course be argued that more options should have been offered when eliciting the motives, the number of alternatives had to be balanced against our desire to maintain complete anonymity among the senders and enumerators. Moreover, since a large fraction of the population were either illiterate or had a very low level of formal education, we had to read all instructions to the subjects (as in, e.g., Henrich et al., 2001). For these reasons, we decided to use only the following four alternatives: (i) it would be unfair not to send anything (fairness motive), (ii) the receiver probably needs the money more than you do (need motive), (iii) you believe that you will get punished either during your lifetime or in an after-life if you are not generous to others (fear-of-punishment motive), and (iv) you believe that you will gain from sending the money (trust motive); the exact wordings are presented in the appendix. The fourth alternative is consistent with the conventional trust interpretation of the game. The subjects were told that they could check more than one box if they wanted to. The fear-of-punishment motive is clearly non-standard. It is motivated by the observation that in most major religions (including Islam and Hinduism, which are studied here, and Christianity) there are strong after-life consequences (and sometimes also present-life consequences) linked to how people live their lives. Moreover, most people on earth, and also clearly in Bangladesh, are religious believers. We therefore conjectured that such fear-of-punishment motives may shape people's behavior in a trust experiment.

Finally, the participants were explicitly asked not to discuss the experiment with anyone, since people who were not given the same opportunity might envy them, which might cause future problems for both them and the organizers. At the

⁴ Still, due to this additional purpose of the dataset, Hindus are overrepresented in our sample compared to a representative sample. The finding that the amount sent in a trust game does not differ is consistent with the findings of Bahry and Wilson (2005), who analyzed different ethnic groups in Russia. It may still seem surprising, however.

⁵ There is always a potential risk of participants distrusting the individuals running the experiment. In order to minimize this risk, university students were used as enumerators. Moreover, it was specifically mentioned that this was a purely academic project run by a local and a Swedish university. Taking into account the large amounts sent, it seems that people on average did largely trust the organizers of the experiment.

⁶ This ensured that the enumerator could not detect those who did not send anything. In the analysis, we separated these responses from the "true" second-box responses.

end of the day, the enumerator handed the envelopes to the principal researcher, who opened them and put the tripled amount into new envelopes.

2.2. Visiting the receivers

The following day, the enumerators were given the new envelopes (see above) to be delivered to the assigned receivers. After ensuring a private environment, the participants were given the instructions for the experiment, including the same examples as those given to the senders. Then they received the envelope containing the tripled amount given by the sender and an empty envelope. The enumerator turned his/her back to the receiver, who had been instructed to use the empty envelope for the amount of money that he/she wanted to return to the sender. After putting the chosen amount in the envelope, the receiver was asked to close it, seal it with a stamp provided by the enumerator, and return it. Then the receiver was asked about his/her motives for returning the chosen amount of money. The receivers were given a piece of paper with three boxes numbered one to three, and an empty envelope into which they were to put the answer sheet after privately marking the appropriate box(es). The private responses to the motive questions were put into an empty envelope, which was immediately sealed. The alternatives presented to the receivers were the same as for the senders with the exception of Alternative 4, which was omitted, as the receiver could not possibly expect to gain financially from returning money to the sender. Thus, they were presented with the following alternatives: (i) it would be unfair not to send anything (fairness motive), (ii) the sender probably needs the money more than you do (need motive), and (iii) you believe that you will get punished either during your lifetime or in an after-life if you are not generous to others (fear-of-punishment motive). At the end of the day, the enumerators returned the envelopes to the principal researcher, who checked and wrote down the amount to be transferred back to the sender. The following day, i.e., on the third day, the envelopes were delivered back to the senders with the amounts the receivers had decided to return.

The questionnaire was developed in English and then translated to Bengali by a translator, after which a second translator conducted a reverse translation. The two translators then met and discussed the differences that had occurred, and agreed on the final wordings.

3. Results

3.1. Trust game results

The average amount sent in the trust game was 92.5 TK, corresponding to 46 percent of the initial endowment of 200 TK. Out of the 256 senders who participated in the experiment, 18 (7 percent) sent nothing and 46 (18 percent) sent everything. Out of the 237 receivers,⁷ 11 (5 percent) did not return anything to the senders,⁸ while 52 (22 percent) returned everything. The average amount returned was 134.6 TK. Thus, it was on average profitable for the senders to send money in this trust game. The results in the literature are mixed on this point. [Cardenas and Carpenter \(2008\)](#) surveyed studies conducted in developing countries and countries undergoing transition and found that senders benefited from trusting in 18 out of the analyzed 28 trust games, whereas [Camerer \(2003\)](#) in a more general overview concluded that the expected net payoff from sending money is typically close to zero (i.e., neither positive nor negative).

Overall, the fractions sent and returned in the present study are quite similar to many recent trust games conducted with non-student samples in developing countries, such as [Ensminger \(2000\)](#) in Kenya, [Barr \(2003\)](#) in Zimbabwe, [Mosley and Verschoor \(2004\)](#) in Uganda, [Barr \(2004\)](#) in Ghana, [Karlán \(2005\)](#) in Peru, [Castillo and Carter \(2007\)](#) in Honduras, [Danielson and Holm \(2007\)](#) in Tanzania, and [Schechter \(2007\)](#) in Paraguay. On the other hand, [Greig and Bohnet \(2008\)](#) found clearly lower fractions among both senders and receivers in Kenya. Moreover, the fractions are quite similar to those found in industrialized countries (e.g., [Berg et al., 1995](#); [Ortmann et al., 2000](#), in USA; [Willinger et al., 2003](#), in France; and [Holm and Danielsson, 2005](#), in Sweden), i.e., countries that are typically found to have a much larger degree of social capital as measured by surveys.

3.2. Expectations and motives

Since most senders probably had some kind of subjective probability distribution regarding the receivers' behavior, rather than a point estimate, and since we were forced to keep the questions simple due to the large number of illiterate subjects, we only asked them to choose among three alternatives.⁹ We asked them whether they expected less, the same, or more than the amount they had sent.¹⁰ As can be seen in [Table 1](#), slightly less than one-third of the senders who sent a positive

⁷ One of the pre-assigned receivers refused to participate.

⁸ In cases where no money was returned, we chose to return a small amount (proportional to the initial amount sent by the sender) to prevent the possibility of our experiment creating bad feelings between villages. In the analysis, of course, we used the actual amounts returned.

⁹ [Garbarino and Slonim \(2009\)](#) dealt with the first of these problems by asking the senders about how much the receivers on average would return if they received \$X (and also \$2X).

¹⁰ One respondent chose all three response alternatives. We therefore exclude this observation when analyzing sender expectations.

Table 1
Average proportion sent in the trust game for different sub-samples of senders.

	Share of respondents	Mean proportion sent
Whole sample ($n = 256$)	100%	46%
Sending a non-zero amount ($n = 238$)	93%	50%
Senders' expectations conditional on sending a non-zero amount to the receiver ($n = 237$) ^a		
Expects to get back less than the amount sent	29%	55%
Expects to get back about the same as amount sent	42%	51%
Expects to get back more than the amount sent	30%	44%
Motives for sending non-zero amounts to the receiver ($n = 237$) [$n = 198$]		
It would be unfair not to send anything (fairness motive)	21% [13%]	49% [51%]
The receiver probably needs this money more than you do (need motive)	26% [20%]	56% [56%]
You will get punished, either during your lifetime or afterwards, if you are not generous to others (fear-of-punishment motive)	51% [46%]	51% [51%]
You believe you will gain from sending the money (trust motive)	23% [21%]	43% [39%]
Stated trust as the level of agreement with the statement "most people can be trusted" ($n = 256$) [$n = 237$]		
Strongly disagree	15% [15%]	43% [46%]
Disagree	22% [21%]	40% [46%]
Partly disagree	31% [32%]	46% [48%]
Partly agree	14% [14%]	53% [57%]
Agree	7% [7%]	57% [57%]
Strongly agree	11% [11%]	49% [53%]

Note: Values in brackets refer to the sub-sample that only stated one motive.

^a Eighteen senders did not send anything. One sender, with a positive transfer, is not analyzed below since he/she chose all three expectations.

amount believed they would gain from sending money, and almost the same proportion believed they would lose, yet still sent money. Hence, already these results provide some indication that the first stage of a trust game may not measure pure trust alone. Furthermore, as the table shows, the pattern regarding the proportion of endowment sent for the three different expectations is the reverse of what one would expect if pure trust were the only influence, since the senders who on average expected less sent more and vice versa. However, most of these differences are not statistically significant at conventional levels, using both the Wilcoxon–Mann–Whitney (WMW) test for pairwise comparisons and the Kruskal–Wallis (KW) test for joint comparisons.¹¹ Yet, these results are in contrast to Bellemare and Kröger (2007), Buchan et al. (2008), Chaudhuri and Gangadharan (2007), Fehr et al. (2002), and Mosley and Verschoor (2004), who, based on samples from the Netherlands, the US, Australia, Germany, and Uganda, respectively, found that the first mover's expectation about the second mover's transfer is positively correlated with the amount sent by the first mover in a trust game.¹²

Table 1 also presents the proportion sent for the different sub-samples classified by motive (given that they sent a positive amount). We asked the senders to select one or several of the four available alternatives explaining the motives behind their behavior. Sixteen percent of the senders and 11 percent of the receivers chose more than one motive to describe their behavior in the trust game.¹³ Perhaps surprisingly, only 23 percent of those sending money expected to actually gain financially from sending money, i.e., the motive typically seen to reflect trust. Yet, it should be acknowledged that our version of the trust game, following, e.g., Glaeser et al. (2000), where the receivers are not given any initial endowment, implies that the role of social preferences is larger than in the original game proposed by Berg et al. (1995). This may have contributed to the large fractions of subjects motivated by *need* and *punishment*. Moreover, and somewhat remarkably, we find that the proportion sent is actually lowest for those who reported that they sent the money because they expected to gain financially

¹¹ At the 5 percent significance level, we cannot, based on WMW tests, reject the null hypothesis that the proportions sent in any of the pair-wise comparisons come from the same distributions. At the 10 percent level, the only case when this can be rejected is when comparing *Expect to get more* with *Expect to get less*. Neither can we reject the null hypothesis that all three samples come from populations with the same distribution at the 10 percent significance level using the KW test. For a more detailed description of these test statistics, see, e.g., Siegel and Castellan (2000).

¹² It is generally difficult to independently measure expectations, see, e.g., Rutström and Wilcox (2009). In many studies, respondents are asked to state the amount of money they expected the receiver would send back *after* deciding how much to send. The amount actually sent may then serve as a cognitive anchor; see Tversky and Kahneman (1974) and Ariely et al. (2003). Similarly, if the expectation question is asked before the sending decision, then the stated expectation may be influenced by how much the sender plans to send, and the actual amount sent may also be influenced by the stated expectation. Another interesting question that we did not ask is how much the receiver in advance expects that the sender will send; see Eckel and Wilson (2006).

¹³ Three senders chose all four alternatives. The other combinations chosen were: fairness, need, and punishment (2); fairness, punishment, and trust (1); trust and need (5); fairness and punishment (11); fairness and need (6); need and punishment (7); and punishment and trust (4).

Table 2

Average proportion sent in the trust game for different sub-samples of senders with respect to motives and expectations. Respondents who chose more than one motive are dropped.

Motivation	Mean proportion sent		
	Expect less back	Expect equal back	Expect more back
Fairness motive	0.53	0.47	0.46
Need motive	0.64	0.61	0.34
Punishment motive	0.56	0.48	0.51
Trust motive	0.19	0.45	0.44

Table 3

Average proportion returned in the trust game for different sub-samples of receivers.

	Share of respondents	Mean proportion returned
Whole sample ($n = 237$)	100%	46%
Returning non-zero amount ($n = 226$)	95%	48%
Motives for returning non-zero amounts to the sender ($n = 222$) [$n = 196$] ^a		
It would be unfair not to send anything back (fairness motive)	30% [24%]	44% [45%]
The sender probably needs this money more than you do (need motive)	32% [27%]	43% [43%]
You will get punished, either during your lifetime or afterwards, if you are not generous to others (fear-of-punishment motive)	54% [48%]	50% [53%]
Stated trust as the level of agreement with the statement "most people can be trusted" ($n = 237$) [$n = 226$]		
Strongly disagree	11% [10%]	31% [33%]
Disagree	30% [31%]	43% [43%]
Partly disagree	30% [31%]	49% [50%]
Partly agree	14% [13%]	47% [51%]
Agree	8% [8%]	52% [58%]
Strongly agree	7% [7%]	60% [64%]

Note: Values in brackets refer to the sub-sample that only stated one motive.

^a Eleven receivers did not return anything. Among those who returned positive amounts, five receivers did not express any motive behind their transfers and are hence not included in the analysis.

from doing so, i.e., the pure trust motive. Several of the differences in amount sent between the motives are statistically significant.¹⁴

In order to gain further understanding of the observed pattern of the amount sent with respect to expectations and motives, consider Table 2 where we report the results for different expectations for each different motive separately. The patterns are then easier to comprehend. It can, for example, be observed that for subjects motivated by trust we observe the expected pattern where those who expect less back also send less, whereas for those motivated by need we see the reverse pattern, which also makes sense; these differences are also statistically significant.¹⁵ Note also that the importance of the need motive may be more prevalent due to our design, where receivers were not given any initial endowment.

Table 3 presents the proportions returned for the different sub-samples in terms of motives. The receivers had three alternatives (since the alternative that they expected to gain financially was not applicable). Eleven percent of the receivers chose more than one motive to describe their behavior in the trust game.¹⁶ A strong driving force for many senders and receivers seems to be fear of punishment, either in this life or after this life; this alternative was chosen by about half of all senders and receivers who sent/returned non-zero amounts.¹⁷ This may reflect that in both major religions in Bangladesh, Islam (88 percent of the population) and Hinduism (11 percent), people believe that they will be judged after death. For a more detailed analysis of the religion angle of the data, see Johansson-Stenman et al. (2009). It should be noted that this motivation is probably not unique to Bangladesh, or even to the Muslim and Hindu worlds. For example, Christians also

¹⁴ Based on the joint KW test, we can at the 5 percent significance level ($P = 0.02$) reject that the proportions sent are independent of the motive reported. Moreover, based on pair-wise WMW tests, we can at the 5 percent significance level reject the null hypotheses that the proportions sent are equal in cases where the motives are (i) need and trust ($P = 0.01$), (ii) fear-of-punishment and trust ($P < 0.01$), and (iii) fairness and trust ($P = 0.04$). Table 2 makes it appear that fear-of-punishment considerations influence the amount returned by the receivers, yet in pair-wise WMW tests we cannot at the 10 percent significance level reject the null hypothesis that the proportion returned arises from the same distribution for any pair-wise combination of the sub-groups of motives. The corresponding result holds for the joint KW test.

¹⁵ Based on pair-wise WMW tests, we can for those motivated by need (trust) reject at the 1 percent (5 percent) significance level the null hypotheses that the proportions sent are equally large for those who expect less back as they are for those who expect equal or more back.

¹⁶ Eleven receivers chose all three motives. The other combinations chosen were fairness and need (1), fairness and punishment (7), and need and punishment (6). It should be noted that five receivers did not indicate any motive, i.e., they returned a blank answer sheet. These responses are dropped from the statistical tests in this section as well as from the econometric analysis in the next section.

¹⁷ Note that this motive is clearly different from that used in Fehr and List (2004) and Fehr and Rockenbach (2003), who considered trust games in which the sender could commit ex-ante to punish the receiver if the receiver would choose to return less than a certain amount.

believe in two different outcomes after death. Indeed, the results in [Hrung \(2004\)](#), which are based on the time pattern of religious and non-religious charitable giving, indicate that many Americans are also influenced by expected after-life consequences. Yet, as noted by a referee, it is not obvious that the importance of the punishment motive is primarily driven by after-life consideration. Indeed, experiments among atheists in Russia (see [Bahry et al., 2005](#); [Bahry and Wilson, 2005](#)) also show that people trust and reciprocate to a large extent. Thus, it is possible that this motive reflects some kind of broader internalized norm.

Taken together, our results support the recent findings by [Ashraf et al. \(2006\)](#), [Bahry and Wilson \(2005\)](#), [Carter and Castillo \(2002\)](#), [Cox \(2004\)](#), and [Holm and Danielsson \(2005\)](#) that other-regarding behavior, beyond reciprocity in the second stage, seems to influence behavior in trust games. Moreover, although there is a great deal of experimental evidence that decisions in trust games can be affected by punishment concerns (see, e.g., [Fehr and Rockenbach, 2003](#); [Charness et al., 2008](#)), our findings provide evidence of another kind of an ultimately selfish motive, namely of people acting to avoid being punished in the long run, i.e., outside the experimental context. This is something that could hardly have been observed from revealed behavior alone.

3.3. Stated trust

Based on the most frequently used standard GSS question, “Generally speaking, would you say that most people can be trusted or that you cannot be too careful in dealing with people?”, only 3 percent felt that most people can be trusted, which indicates a very low level of trust. Since this measure, for obvious reasons, is very crude, we also asked a similar question and allowed for answers along a six-point scale ranging from “strongly disagree that most people can be trusted” to “strongly agree that most people can be trusted” as presented in [Tables 1 and 2](#). Although these responses were a little less extreme, they also indicate very low levels of general trust.¹⁸ Moreover, we also find that there is no strong relation between stated trust and amount sent, although a visual inspection indicates a weak positive relation suggesting that behavior in trust games may be influenced by motives other than trust. There is a clearer pattern between stated trust and amount returned, as revealed by [Table 3](#), which is similar to the results reported in [Glaeser et al. \(2000\)](#).

4. Regression analysis

[Table 4](#) defines the explanatory variables used in the econometric analysis together with their mean values. The high illiteracy of almost 30 percent is particularly striking. As can be seen, we can for no variables reject, based on WMW tests for continuous variables and proportional tests for binary variables, that the senders and receivers are drawn from the same underlying population.

Before moving to the regression analysis, let us briefly look into the correlation coefficients between the different measures of trust and behavioral indications of social capital. As shown in [Table 5](#), stated trust is positively and significantly correlated with both proportion sent and proportion returned, but not with any of the trust variables reflecting actual past behavior. Fractions sent and returned are also poorly correlated with these behavioral variables, although the correlation coefficient between *fraction sent* and *lend money* is significant at the 6 percent level.

This can be compared to [Glaeser et al. \(2000\)](#), who found, for Harvard students, a significantly positive relationship between an index of past trusting (self-reported) behavior and the amount sent in a trust game. Moreover, [Karlan \(2005\)](#) found that the repayment of micro-credit loans in Peru is positively correlated with the fraction returned in their trust game, and [Baran et al. \(2010\)](#) found, for a sample of MBA students in Chicago, that the fraction returned in their trust game (which can be seen as a measure of reciprocity) was strongly correlated with the same subjects’ subsequent donations to the university as alumni.

A possible explanation for the weak correlations in the present study is that the trust game reflects trust between people in different villages, whereas our measures of trusting behavior primarily reflect a more local measure of trust. It is thus possible that we would have obtained much stronger correlations had we instead set up the trust game between different people within each village. We can also observe that the correlation between the behavioral variables are strong and significant, implying that those who tend to leave the door unlocked also tend to more often lend money and other possessions to others.

4.1. The senders’ decisions

In the regression analyses, the dependent variable is the proportion sent, which is bounded at zero and one. Thus, we report the results from Tobit models (marginal effects), where the dependent variable is censored at zero and one. In Model 1, we include only demographic and socio-economic variables. In Model 2, we add variables reflecting motives and expectations. We used several different enumerators to run the trust experiment and there are also four different kinds of religious matching (Muslim sender–Muslim receiver, Muslim sender–Hindu receiver, Hindu sender–Muslim receiver, and

¹⁸ We asked the same question on trust (not reported) in a situation with either low or high stakes. Perhaps not surprisingly, people reported that they trust others more in low-stake situations.

Table 4
Sample statistics and variable definitions.

Variable	Definition	N	Min	Max	Overall mean	Sender mean	Receiver mean	P-Value
Age	Age of respondent in years	512	19	87	44.7	45.0	44.4	0.73
Illiterate	Unable to read and write	512	0	1	0.28	0.29	0.27	0.62
Household Equivalent income	Annual household income in 100,000 Birr adjusted with equivalence and economies of scale. Total income was divided by [(number of adults + 0.5 × number of children) ^{0.75}]	511	0.01	3.63	0.24	0.22	0.25	0.55
Stated trust	Level of agreement with the statement that most people can be trusted: 1 = strongly disagree, 2 = disagree, 3 = partly disagree, 4 = partly agree, 5 = agree, 6 = strongly agree	512	1	6	3.05	3.07	3.04	0.89
Lend money	Frequency of lending money to friends and neighbors: 1 = once a year or less, 2 = about once every other month, 3 = about once a month, 4 = about once a week, 5 = more than once a week	512	1	5	1.81	1.79	1.83	0.73
Lend possessions	Frequency of lending possessions to friends and neighbors: 1 = once a year or less, 2 = about once every other month, 3 = about once a month, 4 = about once a week, 5 = more than once a week	512	1	5	2.75	2.72	2.77	0.75
Unlocked door	Frequency of leaving door unlocked: 1 = once a year or less, 2 = about once every other month, 3 = about once a month, 4 = about once a week, 5 = more than once a week	507	1	5	1.98	2.03	1.92	0.22
Confidence index	Arithmetic sum of confidence on 10 institutions [banks, NGOs, military, police, judiciary, local government, executive government, political parties, rural power elites, educational institutions]: great deal of confidence = 2, only some confidence = 1, and hardly any confidence at all = 0	508	2	20	14.56	14.45	14.69	0.51
Religious participation	Respondent prays at least once per day	511	0	1	0.67	0.66	0.68	0.59
Recent misfortune	Respondent has been a victim of any of the following incidents in the last year: robbery/theft, mugging, personal assault, home attack, land fraud, false criminal accusation, and political harassment	510	0	1	0.19	0.21	0.17	0.23
Member of voluntary association	Has membership in a voluntary group and/or association, e.g., financial, credit, cultural, religious, and political association	509	0	1	0.29	0.29	0.29	0.91

Note: P-Value refers to the question whether the senders and receivers are drawn from the same underlying population. For continuous variables we apply a WMW-test and for binary variables a proportional test.

Table 5Correlation coefficients between different measures of trust and social capital (*P*-values in parentheses).

	Stated trust	Lend money	Lend possessions	Unlocked door	Proportion sent	Proportion returned
Stated trust	1					
Trusting behavior	0.052 (0.24)	1				
Lend possessions	0.039 (0.38)	0.261*** (0.00)	1			
Unlocked door	0.052 (0.25)	0.094** (0.03)	0.107** (0.02)	1		
Proportion sent	0.127** (0.04)	0.116* (0.06)	−0.035 (0.58)	−0.004 (0.95)	1	
Proportion returned	0.187*** (0.00)	0.012 (0.86)	0.077 (0.24)	−0.083 (0.21)	−	1

Note: The full sample is used in all cases except for correlations involving proportions sent and returned. Since no subject was both a sender and a receiver, we can of course not calculate any correlation between proportion sent and returned.

* Statistical significance at 10% level.

** Statistical significance at 5% level.

*** Statistical significance at 1% level.

Hindu sender-Hindu receiver). Based on likelihood ratio tests, we cannot at conventional levels reject that the same fractions are sent for each of the four matching groups, as well as for the different enumerators, regardless of model. Nevertheless, we include dummy variables in order to control for enumerator and religious matching effects in all models.

In contrast to, e.g., Glaeser et al. (2000) and Bahry and Wilson (2005), we find a positive and significant effect at the 5 percent level of stated trust on the amount sent in Model 1, which only contains socio-economic variables. We obtain no significant effects from our social capital variables *member of voluntary association* and *lend income*, which is consistent with Bahry and Wilson's (2005) finding that the parameter associated with their variable "local connectedness" is insignificant.

We provide no separate test of risk preferences, but the obtained positive and significant income effect is consistent with the conclusions of Karlan (2005) in rural Peru and Schechter (2007) in rural Paraguay that the first part of a trust game reflects risk preferences, to a non-negligible extent, rather than trust.¹⁹ Bellemare and Kröger (2007), on the other hand, found no significant income effect. Moreover, Eckel and Wilson (2004), Bahry and Wilson (2005) and Houser et al. (2010) found no relation between risk attitude and the amount sent in a trust game. An alternative explanation for the positive income effect relates to our design where (as mentioned and following, e.g., Glaeser et al., 2000) the receivers were not given any initial endowment. This implies a stronger role for some kind of social preferences in the first step, compared to in the original design by Berg et al. (1995). If the willingness to contribute to others for such reasons is positively related to income, then this effect may well explain our positive income coefficient.

The age profile of the amount sent in Model 1 is inverted U-shaped with a maximum at a very low age, yet the effect is insignificant, in contrast to, e.g., Bahry and Wilson (2005), Bellemare and Kröger (2007), and Glaeser et al. (2002). Sutter and Kocher (2007), on the other hand, report an age pattern where the amount sent increases from childhood to early adulthood, but stays almost constant thereafter, whereas Fehr et al. (2002) found that older people send significantly less. We obtain a negative and significant effect of illiteracy at the 10 percent level in Model 1, which is broadly consistent with Bellemare and Kröger (2007), who found a positive effect of education, whereas Bahry and Wilson (2005) and Fehr et al. (2002) found no significant effect.

Model 2 includes motives as well as expectations divided on trust and non-trust motives. Based on likelihood ratio tests, we can reject the more restrictive Model 1 in favor of Model 2, which includes motives and expectations ($P=0.03$). In line with the pattern in Table 2, those who send based on the trust motive send significantly less if they expect that the receiver will return less, which makes sense and is consistent with some results mentioned earlier. For those who send based on a non-trust motive, on the other hand, we find that those who expect less in return actually send more.

In experiments among subjects with a low level of education, such as the present one, subject comprehension of the experiment is course an important concern. Yet, if subjects would have answered more or less randomly to the motive and expectation questions we would clearly have obtained a very different pattern than the present one.

4.2. The receivers' decisions

The regression analysis of the proportion of money returned by the receivers is also based on a Tobit approach, where the dependent variable is censored at zero and one. In Model 3 in Table 6, we again include only demographic and socio-economic variables, whereas in Model 4 we add variables reflecting motives. As for the sender's decisions, we cannot at conventional levels reject the hypotheses of no enumerator effects and no religious matching effects, based on likelihood

¹⁹ Moreover, Holt and Laury (2002) found that people tend to become more risk averse, in an absolute sense, with higher stakes, and higher incomes imply of course that the stake per income unit decreases, while Johansson-Stenman et al. (2005) found that the amount sent in a trust game on average decreases somewhat with stake size.

Table 6

Tobit regressions on the proportion of money sent and returned in the trust game: marginal effects; standard errors in parentheses.

	Proportion sent		Proportion returned	
	Model 1	Model 2	Model 3	Model 4
Stated trust	0.113** (0.055)	0.082* (0.049)	0.148** (0.068)	0.155** (0.067)
Lend money	0.039 (0.024)	0.020 (0.022)	−0.012 (0.028)	−0.005 (0.027)
Member of voluntary association	0.082 (0.054)	0.054 (0.049)	0.009 (0.064)	0.007 (0.063)
Household equivalent income	0.405*** (0.137)	0.366*** (0.129)	0.021 (0.077)	0.018 (0.076)
Age/10	0.172 (0.118)	0.140 (0.111)	−0.106 (0.144)	−0.090 (0.143)
Age/10 squared	−0.016 (0.012)	−0.147 (0.011)	0.015 (0.015)	0.014 (0.015)
Illiterate	−0.095* (0.056)	0.011 (0.053)	−0.067 (0.067)	−0.049 (0.066)
Fairness motive		−0.035 (0.059)		0.026 (0.068)
Fear-of-punishment motive		−0.005 (0.055)		0.123* (0.064)
Need motive		0.049 (0.059)		
Trust motive and expects a lower amount back than sent		−0.323** (0.129)		
Trust motive and expects about the same amount back as sent		−0.020 (0.089)		
Non-trust motive and expects a lower amount back than sent		0.149** (0.064)		
Non-trust motive and expects about the same amount back as sent		0.027 (0.058)		
Amount sent by sender			−0.062 (0.098)	−0.030 (0.097)
Dummy variables for enumerators	Included	Included	Included	Included
Dummy variables for religious matching	Included	Included	Included	Included
No. of observation	252	234	237	231
Pseudo-R ²	0.12	0.22	0.09	0.11

Note: Non-trust motive means that the respondent has chosen any motive other than the trust motive.

* Statistical significance at 10% level.

** Statistical significance at 5% level.

*** Statistical significance at 1% level.

ratio tests. However, we still include dummy variables reflecting individual enumerators and religious matching in the regressions. Based on likelihood ratio tests, we cannot reject at conventional levels ($P=0.46$) the more restrictive Model 3 in favor of Model 4, which includes motives.

Consistent with Glaeser et al. (2000), we find that stated trust significantly explains the proportion returned, and that stated trust better explains the proportion returned than the proportion sent. That we find no significant effects of age and education is consistent with the findings of Bahry and Wilson (2005), who analyzed different ethnic groups in Russia. That we obtain no significant effects from our social capital variables, except stated trust, is consistent with Bahry and Wilson's finding related to their variable "local connectedness." Moreover, we find that the punishment motive induces people to return a significantly larger share than those motivated by need.

4.3. Stated trust

As shown in Table 7, we explain stated trust by using both an ordered probit model and a standard probit model, where in the latter case we simply divide the 6-point scale in the survey into two parts. Hence, when the response to the statement that most people can be trusted is either *partly agree*, *agree*, or *strongly agree*, the dependent variable is coded as one, whereas for *strongly disagree*, *disagree*, or *partly disagree* it is coded as zero; we report the marginal effects for both models. For both type of models we first include only demographic and socio-economic variables, whereas in the second model (i.e., Models 2 and 4) we add variables reflecting different aspects of social capital and religious participation. For all models, we can reject at the 5 percent level the null hypothesis of no enumerator effects in the stated trust regression; we therefore include dummy variables for enumerator effects. Based on likelihood ratio tests, we can reject at the 1 percent level reject Model 1 in favor of Model 2, but not Model 3 in favor of Model 4 at conventional levels ($P=0.14$).

Contrary to the findings in the trust game, we find that stated trust is significantly and positively affected by confidence in institutions, and also that it is significantly and negatively affected by the occurrence of a recent misfortune. The positive

Table 7
Limited dependent variable regressions on stated trust.

	Ordered probit		Probit	
	Model 1	Model 2	Model 3	Model 4
Household equivalent income	0.048 (0.147)	0.096 (0.151)	0.011 (0.064)	0.027 (0.065)
Age/10	–0.332 (0.230)	–0.310 (0.233)	–0.188* (0.104)	–0.171 (0.105)
Age/10 squared	0.040* (0.024)	0.036 (0.024)	0.020* (0.011)	0.017 (0.011)
Illiterate	0.404*** (0.110)	0.369*** (0.114)	0.125* (0.074)	0.076 (0.078)
Lend money		0.082* (0.046)		0.025 (0.020)
Confidence index		0.046*** (0.015)		0.024*** (0.007)
Religious participation		–0.092 (0.110)		–0.027 (0.050)
Recent misfortune		–0.332*** (0.125)		–0.105** (0.050)
Member of voluntary association		0.005 (0.108)		–0.022 (0.048)
Dummy variables for enumerators		Included		Included
Dummy variables for religious matching		Included		Included
No. of observation	511	501	511	501
Pseudo- R^2	0.05	0.06	0.08	0.12

Note: Coefficients shown for ordered probit and marginal effects for probit model.

* Statistical significance at 10% level.

** Statistical significance at 5% level.

*** Statistical significance at 1% level.

effect of confidence in institutions is broadly consistent with the finding of [Bahry et al. \(2005\)](#) that inter-ethnic stated trust is positively related to trust in politicians. The frequency of lending money weakly predicts stated trust in the survey based on the ordered probit regression, whereas there is no significant effect in the probit regression. A positive relationship between stated trust and *lend money* is consistent with the findings in [Bellemare and Kröger \(2007\)](#) and [Fehr et al. \(2002\)](#). We also find that illiterate people have a significantly higher level of stated trust; we do not have an adequate explanation for this, although these people to a larger extent than others may give the answers they think are expected of them; see [Uslaner \(2002\)](#) for a detailed discussion of links between education and trust. Such an explanation is also consistent with the fact that there is no, or a weaker, effect based on the probit model, suggesting that illiterate respondents may not be more likely to consider other people more trustworthy, but that a slightly higher fraction may here choose the extreme alternative, i.e., that they strongly agree that most people can be trusted.

5. Discussion and conclusion

Although an extremely high level of corruption has been observed in Bangladesh, our results from the trust game do not support the hypothesis that corruption has been transferred to the individual level and developed into a general low level of trust in others. Indeed, the fraction sent, which is assumed to reflect trust, is comparable to what has been found in most other studies (in both developed and developing countries). However, the survey-based stated trust is extremely low compared to in other studies. Moreover, there is a positive relation between stating a high level of trust in public institutions and stated trust in other people. The obvious question, then, is which measure should we rely on? The answer is far from straightforward. As economists, we might have a bias toward relying on observed behavior with monetary incentives. For example, the fact that [Glaeser et al. \(2000\)](#) discovered a poor correlation between stated trust and amount sent in a trust game has been taken to indicate that stated trust does not measure real trust instead of the other way around, i.e., that the amount sent may be a poor measure of trust. As somewhat provocatively expressed by [McCloskey \(1985, p. 181\)](#), “Economists are so impressed by the confusions that might possibly arise from questionnaires that they have turned away from them entirely, and prefer the confusions resulting from external observation,” and by [Sen \(1973, p. 258\)](#), “We have been too prone, on the one hand, to overstate the difficulties of introspection and communication and, on the other, to underestimate the problems of studying preferences revealed by observed behavior.” In our view, it is important to judge the pros and cons of each method without prejudices.

Evidence put forward in favor of stated-trust questions includes the strong measured correlation between the fraction agreeing that most people can be trusted and the number of wallets that were returned in a lost-wallet experiment in the studied cities, as reported in [Knack and Keefer \(1997\)](#). They also found that the standard survey-based measure of trust significantly explains differences in economic growth among countries. Hence, one may argue that whatever stated trust measures, it appears to be something important and something that is overall good for society. On the other hand, the

fraction of wallets coming back is not really a measure of trust, but rather a measure of a particular social norm, which seems, if anything, to be more closely related to trustworthiness than with trust.²⁰ This is consistent with the finding in Glaeser et al. (2000) and in this paper that stated trust is a better predictor of the amount sent back by the receivers than of the amount sent by the senders in a trust game.

Trust as measured by the fraction sent in a trust game has the clear advantage of relying on real monetary incentives, implying that it is costly for participants to deviate from their true preferences and perceptions. This may imply that trust as measured by amount sent in a trust game is less vulnerable to self-serving bias, in terms of self-signaling (e.g., Benabou and Tirole, 2002, 2004) and “purchase of moral satisfaction” (Kahneman and Knetsch, 1992; Kahneman et al., 1999), compared to survey questions. As discussed, there is also empirical evidence, e.g., when comparing the behavior in trust games with the behavior in dictator games, that amount sent, to some extent, does indeed measure trust (e.g., Cox, 2004). What is less clear is the extent to which different motives affect the behavior. Some seem to conclude, implicitly or explicitly, that this extent is sufficiently small, implying that other motives can be ignored. However, the results here, as well as in the other recent studies mentioned, indicate that this conclusion is too strong. In this study, we have shown that factors beyond what can be captured by conventional consequential experiments, in our case the fear of subsequent punishment if behaving too selfishly, seem to be very important for both senders and receivers. Although survey questions do have inherent problems, we cannot see how we could have obtained this kind of result had we been restricted to observing people’s actual game behavior.

At the end of the day, our conclusion is that both measures to some extent reflect trust, as we normally think of the word, but that neither appears to be particularly good. Yet, it is also important to highlight that just as there are different ways of measuring trust, there are also many different types of trust, and it is not at all obvious that people with strong trust in people belonging to their local neighborhood also trust others more generally. For example, that the amount sent in the trust game, as played here between people from different villages, correlates poorly with trusting behavior in terms of lending money and possessions need not imply that amount sent is generally a poor measure of trust. It is possible that it is in fact a reasonably good measure of trust in people from other villages, whereas it is a poor measure of trust within the village.

Moreover, measurement problems are of course not an isolated problem for trust. Indeed, many issues in which we are intrinsically interested, including social welfare, happiness, and freedom, are inherently difficult to quantify. In our view, this fact should not prevent us from doing so, although we should of course be aware of potential problems and possible alternative interpretations. In the case of measuring trust, we believe that at our current stage of knowledge, we should continue with, and try to improve, both survey-based and experiment-based approaches of measuring trust.

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Appendix A. Supplementary data

Supplementary data associated with this article can be found, in the online version, at [doi:10.1016/j.jebo.2011.06.022](https://doi.org/10.1016/j.jebo.2011.06.022).

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²⁰ It may possibly also be a reflection of trust and distrust in the police. If people believe that a handed-in wallet only benefits the local policemen, and not the owner of the wallet, then they may find it meaningless to hand it in.

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