Incentive for risk sharing and trust formation: Experimental and survey evidence from Bangladesh

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9 January 2018

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Abstract

Although interpersonal trust is essential for socioeconomic development, the formation mechanism of trust is not well understood. Using dyadic data from an experiment and a household survey in rural Bangladesh, this study evaluates whether the incentive for risk sharing increases trust between villagers. Incentive for risk sharing in the dyad is characterized by their negative income correlation, large difference in their risk preferences, and large difference in their income volatilities. The empirical results demonstrate supporting evidence for this hypothesis: incentive for risk sharing, particularly negative income correlation, facilitates trust. These findings suggest that the introduction of safety net programs such as health insurance, which reduce the incentive for risk sharing, may break down trust. This implication could make an important contribution to our understanding of the effect of industrialization on social capital—an effect that has long been discussed.

JEL Codes: C91, D12

Keywords: Trust formation; social capital; risk sharing; experiment; Bangladesh

* The author would like to express gratitude to the editor and two anonymous referees of the journal, Avner Ben-Ner, Toshiji Kawagoe, Takeshi Murooka, Yoshimichi Sato, Yasuyuki Sawada, Tomomi Tanaka, and Koji Yamazaki for their valuable comments. Special thanks are due to the participants at IAREP-SABE Joint Conference 2015, the Institute of Developing Economies, Japanese Economic Association, Japan Society for International Development, Meeting on Applied Economics and Data Analysis, and Theoretical Economics and

Agriculture Annual Meeting. The author is also grateful to Naonori Kusakabe and Data Analysis and Technical Assistance Ltd. for their valuable cooperation in the household survey and experiment. The usual disclaimer applies.

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Funding

This work was supported by a Grant-in-Aid for Young Scientists (B) from Japan Society for the Promotion of Science (25780172).

1. Introduction

Interpersonal trust is an essential element of socioeconomic development, particularly in developing countries. In rural communities, members often engage in cooperative behaviour such as irrigation maintenance and credit transactions without a contract enforcement institution (Bardhan, 2000; Bouma et al., 2008; Hayami, 2009; Karlan, et al. 2009; Sawada et al. 2013). Therefore, trust between community members—namely *particularized trust*—plays an important role. Trust of strangers (i.e., *generalized trust*) also encourages cooperation in one-shot situations (Fukuyama, 1995; Yamagishi and Yamagishi, 1994). Therefore, it is important for policymakers and researchers to better understand the formation mechanisms of particularized and generalized trust.

Previous studies have suggested that the two types of trust accumulate through different processes: particularized trust is formed through repeated interactions between community members (Banfield, 1958; Shapiro et al., 1992), while generalized trust is determined by one's personal predisposition (Platteau, 1994a, 1994b; Uslaner, 2002; Yamagishi and Yamagishi, 1994). While various determinants of generalized trust have been discussed in the literature,[1] the formation mechanism of particularized trust is less well understood. Some studies have found that particularized trust increases with the proximity between individuals, such as that characterized by ethnicity (Bouckaert and Dhaene, 2004), neighbourhood (Etang, et al. 2011), and friendship (Binzel and Fehr, 2013), and when low communication costs are involved (Fisman and Khanna 1999). Although these findings are insightful, they do not explain the mechanism of particularized trust formation. This study attempts to address this issue.

One likely explanation for the formation of particularized trust is grounded in the

[1] Generalized trust has been found to be related to the income level of the community and its inequality, political characteristics, and degree of industrialization (Berggren and Jordhal, 2006; Bjornskov, 2006; Miguel et al., 2006).

experience of cooperation. Experiencing cooperation with an individual can facilitate particularized trust by increasing the expectation of his/her trustworthiness, the accuracy of the expectation, and the willingness to risk the trusting behaviour (Banfield, 1958; Shapiro et al., 1992). Indeed, previous studies have consistently shown that individuals who experience collective actions with their community members exhibit higher trust (Durante, 2009; Feigenberg, et al. 2013; Gneezy, et al. 2016; Schechter, 2007a; Shoji et al., 2012).

In a rural society, risk sharing arrangements provide valuable opportunities for cooperation. Given the income volatility and poor access to a formal insurance market, villagers have an incentive to help each other by lending money or providing food (Fafchamps, 2010). However, the extent of cooperation between individuals has been found to vary along with their incentive to share risk (Coate and Ravallion, 1993; Fafchamps, 1999; Kimball, 1988; Kocherlakota, 1996; Ligon et al., 2002). This suggests that the incentive for risk sharing could foster particularized trust by facilitating opportunities to cooperate. However, there is also a counterargument to this assumption: people do not need to trust others if their cooperation is already guaranteed by the incentive (Gambetta, 1998; Sitkin and Roth, 1993; Yamagishi and Yamagishi, 1994). As a result, the impact of incentive on particularized trust is theoretically ambiguous.

The goal of this study is to test whether two villagers linked by a strong incentive to share risk actually form higher particularized trust. I used experimental and survey data of 1,920 dyads collected from 279 randomly selected households in 16 Bangladeshi villages. The incentive for risk sharing between individuals is characterized by their negative income correlation over eight agricultural seasons (Coate and Ravallion, 1993; Rosenzweig and Stark, 1989), as well as large differences in their risk preferences and in their income volatilities (Chiappori & Reny, 2016; Legros & Newman, 2007; Schulhofer-Wohl, 2006). There was a

devastating cyclone during the survey period, and the level of cyclone damage varied across villages and even between households within the same village (Shoji & Murata 2018), which provided opportunities to share risks. In order to elicit particularized trust, the current study modified the experimental design of Berg et al. (1995). However, a distinguishing feature of the study's design is that the research was conducted under both anonymous and non-anonymous conditions in order to elicit generalized and particularized trust, respectively. The maximum amount of payoff from this experiment is equivalent to nine days' income in the study area.

Particularized trust was elicited only once after the household survey. Therefore, the study exploited the cross-sectional variation and regressed the current particularized trust on the incentive for risk sharing, as well as on the trustor fixed effects and the predetermined dyadic characteristics that could be associated with the baseline particularized trust. In order to address the potential endogeneity issue of income correlation and volatility, the study employed negative weather shocks such as drought and flood as instruments. An underlying assumption for the use of shock variables is that the timing of experiencing shocks is exogenous after controlling for household fixed effects.

The estimation results are as follows. First, households share more risks in the village with a higher incentive to do so; the cyclone-affected households can borrow more from informal sources if their income is negatively correlated with the other villagers, and if their risk preference and income volatility differ from those of the others. Second, the empirical results obtained through the dyadic regression approach suggest a positive effect of the incentive for risk sharing on particularized trust; negative income correlation in the dyad is significantly associated with an increase in their particularized trust. This is robust to the possibilities of various specification errors, such as weak instruments, reverse causality,

measurement errors, omitted variable bias, and computation method of standard errors. It is also shown that particularized trustworthiness increases with the gap in their risk preferences. Further, these results cannot be explained by unobserved heterogeneity in the in-group favouritism. More important, this bias would work against my central findings. Therefore, it should be interpreted that the current result is a lower bound of actual causal effect.

The findings of this current study relate closely to those of Gneezy et al. (2016), Durante (2009), and Shoji et al. (2012). Gneezy et al. (2016) conducted an anonymous trust game in two communities: sea fishermen who work in groups and lake fishermen who work individually. The authors found a significantly higher level of trust among the former than among the latter, implying that the experience of group activities facilitated trust between community members. Durante (2009) found that weather shocks experienced between the 16th and 18th centuries predicted variations in generalized trust across European countries, presumably because the repeated experience of risk sharing over the centuries influenced community members' personal predisposition. Shoji et al. (2012) evaluated the causal impact of the incentive for Sri Lankan farmers to contribute to community work—such as through participation in community meetings and payment for religious festivals—on trust among villagers. The current study differs from these previous ones in that it elicits the particularized trust of each community member and examines the dyadic relationship, while the previous studies utilized measures of trust on the entire community. This use of trust measures at the individual level allows us to uncover the underlying mechanism of trust formation in more detail by exploiting the variation in particularized trust on the part of one trustor across trustees. Furthermore, Durante (2009) and Shoji et al. (2012) elicited trust by asking subjective questions; however, other scholars have expressed serious doubts about the validity of such self-reported information (Glaeser et al., 2000). In contrast, this study used an experimental approach to quantify particularized trust.

This study attempts to make three further contributions to the literature. First, it uncovers the relationship between risk and trust in the context of developing countries. Since such countries suffer from poor access to formal insurance and contract enforcement institutions, both risk sharing and trust are essential to their economic development. However, the study's findings regarding the positive association between trust and the incentive for risk sharing predict that the introduction of formal health insurance—which reduces the incentive for risk sharing—could break trust down. Second, combining my findings with those of previous studies suggests the potential of a poverty trap. It has been shown in the literature that an increase in trust helps individuals achieve more efficient risk sharing (Carter and Maluccio, 2003; Ligon and Schechter, 2012). On the other hand, the findings of this study indicate that a stronger incentive for risk sharing also causes higher trust. Thus, community members with low initial trust might experience a slower growth of trust than those with high initial trust. Finally, the study suggests the potentially negative effect of social proximity on trust. While some previous studies have found social proximity to have a positive effect on trust, others have presented insignificant effects (Delavande and Zafar, 2015; Johansson-Stenman et al., 2009). The current study's findings might partially explain those results: proximity leads to positive income correlation and similarity in the risk preference and income volatility. These features reduce the incentive to share risks. These implications could make an important contribution to our understanding of the effect of industrialization on social capital—an effect that has long been discussed (Miguel et al., 2006; Polanyi, 1944/1957).

The rest of this paper is organized as follows. Section 2 presents the conceptual framework and establishes the testable hypotheses of this study. Section 3 describes the dataset. Section 4 documents identification strategy and Section 5 shows estimation results. Finally, Section 6 concludes.

2. Conceptual Framework

2.1. Risk-Sharing Arrangements in Rural Economies

In developing countries, villagers' incomes fluctuate over time due to various risks, such as natural disasters, sickness, and unemployment. Given their poor access to formal insurance, they share risks to smooth consumption, by lending money or providing food to those who suffer from negative income shocks (Collins et al., 2009; Fafchamps, 2010; Fafchamps and Lund, 2003; Jack and Suri, 2014; Udry, 1994). Such a risk-sharing arrangement, therefore, plays the role of informal insurance, and it is a major source of cooperation in the village economy.

Existing studies show evidence of risk sharing arrangements in rural economies (Ogaki and Zhang, 2001; Park, 2006; Townsend, 1994). However, it has also been found that limited commitment problems crucially restrict the efficiency of risk sharing (Albarran and Attanasio, 2003; Charness and Genicot, 2009; Coate and Ravallion, 1993; Dubois et al., 2008; Fafchamps, 1999; Foster and Rosenzweig, 2001; Kimball, 1988; Kocherlakota, 1996; Kruger and Perri, 2006; Laczó, 2014; Ligon et al., 2002). Since participation in risk-sharing arrangements is voluntary for the villagers, they can deviate from the arrangement at any time without incurring monetary cost; however, such villagers are excluded from future arrangements and end up in an autarky economy. Therefore, efficient risk sharing can be observed only when the extrinsic incentive to maintain the arrangement is sufficiently high for all villagers.

This study exploits three dyadic characteristics to quantify the incentive for risk sharing. First, the incentive is high between the individuals with negatively correlated incomes, as it enables them to pool more risks (Coate & Ravallion, 1993; Rosenzweig & Stark, 1989). Second, it also increases between those with a large gap in risk preferences

(Chiappori & Reny, 2016; Legros & Newman, 2007; Schulhofer-Wohl, 2006); a risk-neutral household is willing to offer a better deal for insurance than is a risk-averse household, so those who demand the most insurance share risk with the least risk averse. Consequently, the risk sharing network in equilibrium can be characterized by negative assortative matching with respect to the risk preference. Finally, a large gap in the income volatilities between individuals may also be associated with the incentive.[2] Someone whose income is stable over time can give more to a neighbour in need, and the latter can then pay him/her back more during good times.

However, it should be noted that the explanatory power of negative assortative matching with respect to the risk preference and income volatility depends on the sharing rule of risk sharing arrangement; if it follows the equal sharing rule, the incentive for risk sharing rather becomes *smaller* with the gaps (Attanasio et al., 2012; Jaramillo et al., 2015). Further, this may be a plausible assumption at least between some villagers, such as close kinship members; it is common in rural Bangladesh that relatives reside in the same village and form a small community, *bari*. The *bari* plays the role of a risk sharing network (Park, 2006).

2.2. Definition of Trust

This study defines trust in line with Coleman (1990) and Fehr (2009): *an individual trusts if she voluntarily places resources at the disposal of another party without any legal commitment from the latter*. Trust therefore is a behaviour motivated by a trusting belief and trusting preference (Coleman, 1990). A trusting belief indicates a subjective expectation about a trustee's behaviour under the situation where the trustee does not have an extrinsic incentive to behave in a prosocial manner (Ashraf et al., 2006; Barr, 2003; Sapienza et al., 2013). Trusting preference, on the other hand, is the willingness to take the risk of trusting

[2] I am grateful to an anonymous referee for suggesting the second and third hypotheses.

behaviour. This social preference is referred to as betrayal aversion (Bohnet and Zeckhauser, 2004; Bohnet et al., 2008). Trusting behaviour is facilitated by three factors: (1) the expected level of trustee's trustworthiness, (2) the accuracy of the expected trustworthiness, and (3) the willingness to take the risk of trusting behaviour. For those who exhibit betrayal aversion, an improvement in the accuracy of expected trustworthiness (the reduction of information asymmetries) has a positive effect on the trusting behaviour.

Although one could take prosocial behaviour by knowing that his or her opponent does not have an incentive to betray him or her, such behaviour is not considered trust in the definition used here. Therefore, the definition used here differs from Hardin's (2002) *calculative trust*, which considers trust a rational expectation.

2.3. Trust Formation Mechanism and Testable Hypotheses

Existing studies suggest that the incentive for risk sharing potentially increases the three factors mentioned above (Banfield, 1958; Shapiro et al., 1992; Sheppard and Sherman, 1998). In particular, Shapiro et al. (1992) claim that particularized trust is formed in three stages, and that the incentive of cooperation is a necessary condition for the first stage. In the first stage, the strong incentive to share risks between individuals enforces their cooperation, which could, in turn, cause them to collude.[3] Therefore, one believes that the probability for his/her opponent to betray him/her is low, given the incentive to share risks. In other words, calculative trust grows during this stage.

In the second stage, the frequent cooperation grounded in the incentive enhances regular communication between individuals. This helps them accumulate knowledge about each other's trustworthiness and personality. This knowledge, therefore, improves the accuracy of the trusting belief.

[3] This is consistent with the argument of Murgai et al. (2002) and Genicott and Ray (2003).

Finally, the repeated interactions help both the trustor and trustee form social preference. On the one hand, the trustee behaves in a more trustworthy manner, increasing the trustor's belief about the trustee's trustworthiness. On the other hand, the trustor becomes more willing to take the risk of trusting behaviour as these interactions are experienced. Therefore, these studies suggest that even cooperation as a result of a self-interested motive could facilitate social preference toward the opponent.

Given this argument, this study tests the following hypothesis:

Hypothesis:

Individual *i* should exhibit higher particularized trust in *j* when:

(a) their incomes are negatively correlated,

(b) the gap in their risk preferences is larger, and

(c) the gap in their income volatilities is larger.

It should, however, be noted that this study considers Hypothesis (a) to be particularly important among the three, given the potential issue pertaining to the explanatory power of negative assortative matching with respect to the risk preference and income volatility (see Section 2.1).

It is also important that these hypotheses are theoretically ambiguous. Gambetta (1998), Sitkin and Roth (1993), and Yamagishi and Yamagishi (1994) claim that people do not need to trust others if their cooperation is guaranteed by the extrinsic incentive. If this is the case, we should rather observe the opposite or an insignificant relationship.

3. Data Description and Experimental Design

3.1. Household Survey

The study site is Satkhira district, located in southwest Bangladesh. This area is suitable for examining the incentive for risk sharing and trust formation: this district was severely affected by Cyclone Aila in May 2009. It caused significant economic loss, destroying around 250,000 acres of cropland and killing 150,000 livestock. The level of cyclone damage varied across villages and even between households within the same village; farmers cultivating rice in the lowlands and shrimp farmers were the most severely and persistently affected, while vegetable farmers in the highlands and even lowland farmers cultivating jute could relatively mitigate the cyclone damage. Construction workers rather benefited from increases in demand for house repairing. These situations provided opportunities among villagers to share risks.

The household survey was conducted in the district in December 2010, 19 months after the cyclone. I employed a multistage stratified random sampling methodology. In the first stage, I selected the three sub-districts (*upazila*) of Samnagar, Kaliganj, and Ashashoni, based on their economic status and the intensity of the cyclone damage. In the second stage, I randomly sampled two *unions* from each sub-district.[4] In the next stage, four villages from each *union* and one cluster (*para*) from each of the villages were randomly selected. Finally, I selected 18 households from each *para*.[5] Since five households were unavailable for the survey, I obtained a total of 427 of 432 sample households from 24 *para*.

Key variables in the survey data are the income correlation and the gap in the income volatilities between trustor and trustee. In order to elicit them, retrospective information was collected on household income and experience of weather shocks for eight periods over the period January 2007–December 2010—namely, the dry and rainy seasons of each year. The household income includes income from self-employed (farm and non-farm), employed (farm and non-farm), and rent income, and income from each component is elicited by asking

[4] The *union* is an administrative unit in Bangladesh; each *union* contains multiple villages.
[5] In the survey area, the average *para* size is 72.5 households.

the net value. De Mel et al. (2009) have shown that computing income from this direct method provides a more accurate measure than detailed questions on revenues and expenses. To check the validity of this method, I compare the income computed from the detailed information such as the harvest volume of crops, wholesale prices, and input costs. Although this information is available only for the recent four periods, they do not significantly differ (*p-value*=0.411, *N*=1116). The weather shock variables are self-reported binary variables that take unity if the household experienced shocks—such as drought or inundation—and zero otherwise. From this retrospective information, I compiled a pseudo-panel dataset. Therefore, eight-period panel data were available to compute variations in and correlations of household income. Since these data are retrospective and self-reported, they may suffer from measurement error; this point is taken up in Section 4.2.

In this survey, the interviewers asked about current relationships with each of the other 17 survey households in the same *para*. The collected information included the distances between their residences and whether they attend the same mosque, temple, or church. In cases where the respondents did not know the opponents, they could not answer these questions, and so the dataset includes some missing values.

Summary statistics are presented in Appendix A1 of Online Appendix. It appears that 32% of the sample households experienced inundation of their residence during Cyclone Aila, and 19% of the survey households borrowed from neighbours, friends, or relatives after the cyclone, implying that risk sharing among villagers plays the role of insurance.[6] In addition to the cyclone attack, negative income shocks such as inundation and drought occur with a probability of around 10–20% during the survey period.

[6] Admittedly, the sources of informal loans may include relatives in other villages. However, it is common in Bangladesh that relatives reside in the same village and form a small community, *bari*. The *bari* plays a role of a risk sharing network (Park, 2006). Thus, this study presumes that the informal loans were transacted mainly within the village.

3.2. Experiment

Eight months after the household survey, the heads of the survey households in Kaliganj and Ashashoni were invited to partake in an economic experiment that used real money.[7] In cases where the household head was not available, the next senior person representing the household (usually the spouse) was recruited to maintain the sample size. Ultimately, 279 of the 285 households participated in the experiment. The experiment was implemented at the local government office, and each day I conducted the experiment with 36 participants invited from two villages. All 36 participants were first gathered in a room, and they were then randomly allocated to two rooms. Each participant played the take-away game, dictator game, trust game with hidden action, risk preference game, and trust game; however, this study uses the results only from the dictator, risk preference, and trust games. The experimenters were hired in Bangladesh, and since participants had an average of fewer than six years of schooling (Table A1), the experimenters explained the experimental design slowly and carefully. More details about the experimental procedure and instruction and the result of these games are reported in the Online Appendix.

After finishing all the games, each participant rolled two coloured dice. Each colour represented a decision made during the games, and he/she received his/her payoff from only one randomly selected decision.[8] Therefore, the participants did not know from which decision they received the payoff and were aware that each participant had earned money from a different decision. This payment process was explained to the participants prior to starting the first game. This process is important for two reasons. First, it alleviates the correlation of choices within participants across games due to the wealth effect. Second, if participants had been able to earn money from all games and discuss the payoffs after the

[7] The experiment in Samnagar was cancelled due to flooding.

[8] Besides the payoff from the games, I also provided BDT100 as a participation fee. This is the same amount as the median daily wage, as per the survey data.

experiment, they might have been able to infer the choices of the other participants. This would have violated participant anonymity and potentially affected their behaviour.

3.2.1. Trust Game

This study used the trust game of Berg et al. (1995) to elicit particularized trust. This game is played by a randomly matched pair of participants. These participants are randomly assigned roles—namely, a first mover (trustor) and a second mover (trustee). When the game starts, the experimenter provides the first mover with an endowment of 300 Bangladeshi taka (BDT) and the second mover with nothing. This amount is equivalent to about three days' worth of income in the study area. In the first stage of this game, the first mover decides how much of the BDT300 to send to the second mover and how much to keep. He or she can send BDT300, 250, 200, 150, 100, 50, or none. The amount sent is tripled by the experimenter before it reaches the second mover. In the second stage, the second mover can return some of the received amount to the first mover. Thus, the material payoff for the first and second movers is $300 - t + r$ and $3t - r$, respectively, where t and r denote the amount to be sent to the second mover and the amount to be returned to the first mover, respectively. The maximum amount of payoff from this game is BDT900 (to the second mover, when $t = 300$ and $r = 0$). In the study area, this is equivalent to nine days' worth of income.

A feature differentiating my experimental design from that of Berg et al. (1995) is that it is conducted under both anonymous and non-anonymous conditions, in order to elicit generalized and particularized trust, respectively.[9] The first mover first decides how much to send, without knowing to whom he or she is sending the money.[10] After this, the participants

[9] Although the strategy method has some potential concerns, Brandts and Charness (2011) show that, based on the results of a large number of previous studies, the results of the strategy and direct-response methods are comparable.

[10] Strictly speaking, this approach may not capture generalized trust precisely, if the participants were aware that half of the participants were from the same village.

make decisions under a non-anonymous condition: I randomly divided the 18 participants from the same *para* into two groups (nine in each group), and the experimenter shows each participant the name list of the other eight participants in the same group to ask how much to send, if this game were played with each of them. Thus, eight dyadic observations were collected from each participant.

After all participants have made decisions in the role of first mover, they make decisions as the second mover. Again, they are asked how much to return under both anonymous and non-anonymous conditions; the experimenter initially informs each second mover how much he or she actually received from the paired first mover, but does not inform from whom he or she received it. After deciding how much to return under the anonymous condition, they are again shown the name list of the eight participants, and the participants decide the amount to return to each of them.[11]

In this game, the second mover has no extrinsic incentive to return money at the second stage. Expecting this, it is rational for the first mover to keep all the money. In other words, calculative trust does not exist for first movers. However, those who still expect trustworthy behaviour from the second mover and/or those who are willing to trust him or her will send money. Therefore, the fraction the first mover sends to the second mover, $t/3$, captures trust, and the fraction that the second mover returns to the first mover, $100r/3t$, indicates trustworthiness.

3.2.2. Risk Preference Game

Risk preference is elicited based on the methodology of Schechter (2007b). This game is similar to that involving the first mover of the trust game, but it is played alone. When the

[11] The pair was determined prior to the game, and therefore, the endowment was transferred between the real pair. However, the participants were not informed about the name of the real pair, but only the names of eight potential opponents.

game starts, the experimenter provides an endowment of BDT300 and a die. Participants decide how much to bet on the die. They can bet BDT300, 250, 200, 150, 100, 50, or none. After the participant decides how much to bet, he or she rolls the die. The payoff in this game depends on how much he or she bets on the die and the result of the bet, as follows: $300 + z(Odds - 1)$. Here, z denotes the amount participant i bets on the die, and $Odds$ takes 0 if the die lands on 1, 0.5 if the die lands on 2, 1 if the die lands on 3, 1.5 if the die lands on 4, 2 if the die lands on 5, and 2.5 if the die lands on 6. The level of risk aversion is measured by the proportion of endowment to be bet on the die, given that more risk-averse individuals are expected to bet lower amounts.

3.2.3. Dictator Game

This study uses the dictator game to elicit the pure altruism of participants. The experimental design follows that of Forsythe et al. (1994). This game is played anonymously by a randomly matched pair of participants, referred to as the dictator and recipient. When the game starts, the experimenters provide an endowment of BDT400 to the dictator and nothing to the recipient. The dictator can then allocate BDT400, 350, 300, 250, 200, 150, 100, 50, or none to the recipient. The extent of pure altruism is measured by the proportion of endowment allocated to his or her recipient.[12] This study elicits this preference parameter from all participants by using the strategy method across the roles in the game.

4. Identification Strategy

4.1.Estimation Model

This study elicits the level of individual *i*'s particularized trust by employing the proportion of endowment transferred from i to j in the non-anonymous trust game. Although previous

[12] Since this game was conducted after a take-away game, the rule was explained in this context. See Slide "Take Away (1)-4" in the Online Appendix for details.

studies employ *i*'s expectation about the amount *j* will return (Barr, 2003), this cannot capture the trusting preference or the accuracy of trusting belief, which are also important components of trust. A potential drawback of my measure, however, is that the amount of transfer can be attributed to other motives than particularized trust, such as generalized and particularized altruism, reputation building, risk preference, and generalized trust. However, as I will argue in Section 5, these factors cannot fully explain the observed patterns of transfer.

The hypotheses are tested by regressing individual *i*'s particularized trust in *j* on their income correlation, absolute value differences in their risk preferences and their income volatilities, as well as summations of their risk preferences and their income volatilities. Specifically, this section estimates the following dyadic regression model:

$$Trust_{vij} = \beta_0 + \beta_1 Corr_{vij} + \beta_2 \left|Bet_{vi} - Bet_{vj}\right| + \beta_3 \left(Bet_{vi} + Bet_{vj}\right) + \beta_4 \left|SD_{vi} - SD_{vj}\right| \quad (1)$$
$$+ \beta_5 \left(SD_{vi} + SD_{vj}\right) + Prox_{vij}\beta_6 + X_{vj}\beta_7 + D_{vi} + \varepsilon_{vij}$$

where $Trust_{vij}$ represents the proportion of endowment transferred from participant *i* (trustor) to *j* (trustee) in *Para v*; $Corr_{vij}$ denotes the correlation coefficient of incomes between households *i* and *j*; SD_{vi} denotes the standard deviation of household *i*'s income;[13] and Bet_{vi} represents *i*'s willingness to take risks, as quantified by the proportion of endowment bet in the risk preference game. The hypothesis predicts that $\beta_1 < 0$, $\beta_2 > 0$, and $\beta_4 > 0$. $Prox_{vij}$ includes social proximity variables such as geographic distance between their houses; two dummy variables representing the relationship between *i* and *j*, such as relative and stranger (the reference group is friend/acquaintance); and three dummy

[13] One may suggest the use of coefficient of variation rather than the standard deviation, given that the latter is positively correlated with the income level. However, it is inappropriate to use it, since the household income can take a negative value, particularly during disasters. Therefore, this study isolates the effect of average income by controlling for the holdings of productive assets.

variables indicating whether the gender of the participants, the place of worship and occupations of the households are the same, respectively.[14] X_{vj} is *j*'s characteristics, such as the proportion of endowment allocated in the dictator game (pure altruism), proportion returned in the trust game under the anonymous condition (generalized trustworthiness), socio-economic status, and demographics.[15] Finally, D_{vi} denotes the trustor (household) fixed effects, capturing the trustor's income volatility, generalized trust, risk preference, and socio-economic characteristics.

Given the dyadic structure of the model, the residuals could be correlated over *i* and over *j*. An approach to adjust the correlation of residuals is the use of dyadic standard errors proposed by Fafchamps and Gubert (2007). However, since the residuals may be correlated across households within *para* as well, I use the clustered standard errors at *para* level.

4.2. Endogeneity of Income Dynamics

An issue in the estimation of Equation (1) is endogeneity of income correlation ($Corr_{vij}$) and volatility (SD_{vi}). Social capital plays a significant role in the income-earning behaviour in developing countries (Fafchamps, 2004), and therefore, trust might also affect income correlation and volatility. In order to address this issue, I employ the method of Albarran and Attanasio (2003), which isolates the unanticipated component from the total income by estimating the following equation:

$$Income_{vit} = \sum_{k=1}^{5} \gamma_k Shock_{vitk} + D_{vi} + \epsilon_{vit} \quad t = 1,2, \dots 8, \tag{2}$$

[14] As explained in Section 3, the dyadic data—such as the distance between residences and whether they go to the same mosque/temple/church—are unobserved when individual *i* considers *j* as a stranger. In these cases, the missing data are replaced with 0, and a dummy variable for stranger is added to the independent variables (Greene, 2011). Thus, the dummy for stranger captures both the social proximity effect and the effect of missing values in the dyadic characteristics.

[15] In line with Schechter (2007a), participant *j*'s generalized trustworthiness is assumed to be exogenous for *i*.

where $Income_{vit}$ denotes household i's seasonal income per month at period t; $Shock_{vitk}$ takes unity if household i experiences exogenous income shock k at period t, and 0 otherwise; and D_{vi} represents household fixed effects that control for the anticipated component of income. The shock variables include inundation due to cyclones and floods; soil salinization caused by tidal waves; drought; pests, weeds, and animals; and the loss of productive assets. These weather shocks affect agricultural and non-agricultural labour income significantly, and the probability of being affected by the shocks is uncontrollable and unpredictable for households, after controlling for the household fixed effects that capture the geographic characteristics and time-invariant socio-economic characteristics. Therefore, I define the income explained by these shock variables, $\sum_{k=1}^{5} \hat{\gamma}_k Shock_{vitk}$, as unanticipated income. Finally, I use the unanticipated income to compute the exogenous standard deviation and correlation coefficient. The estimation result of Equation (2) is reported in Table A2. As expected, the inundation of fields significantly affected household income.

The use of weather shocks for the instrument, however, has the following issues that need to be addressed. First, the characteristics of households vulnerable to the weather shocks may be systematically different from those that are not. For example, they work on the riverbank. They are also less concerned about risk management, and therefore do not invest in pump tube-wells or pesticides. However, the occupation and location of the working place are time-invariant, and the ownership of tube-well and input cost for farming do not change across seasons in the dataset.[16] Thus, the heterogeneity across households should be captured by the household fixed effects. After controlling for the fixed effects, the timing of shock should depend on exogenous factors, such as the precipitation in each season and the path of tropical cyclones.

[16] The statistics are not reported in the paper but are available from the author upon request.

Second, a positive correlation in the experience of shocks between two households may capture their geographic and socio-economic proximity and, therefore, in-group favouritism. This has a direct positive effect on particularized trust (Etang et al., 2011). Although this study controls for various dyadic characteristics in Equation (1), the potential of unobserved characteristics leads to the violation of exclusion restriction. However, as long as the estimated impact of income correlation on particularized trust is negative, this issue should not affect the interpretation of results. Further, I test the robustness to unobserved dyadic characteristics, by using the method of Bellows and Miguel (2009). This quantifies how influential the unobserved characteristics should be to fully explain the estimated effect. As shown in the Online Appendix A3, the results are robust to the issue.

The third issue pertains to reverse causality; an increase in one's trust in a particular neighbour might influence his/her income-earning behaviour and therefore the probability of being affected by the weather shocks. However, given that the average household can call for help to as many as 11.9 neighbours (Table A1), it is unlikely that the development of trust in a neighbour can be that influential. Nevertheless, for those with a small risk sharing network, this might be possible. Therefore, as a robustness check, in the Online Appendix A3 this study estimates Equation (1) excluding the households that can call for help to fewer than five neighbours.

Fourth, given that the shock variables are self-reported, some households may over-report their hardships more than others, even though their actual damages are comparable, violating the exclusion restriction of the instrument. However, if one's tendency to over-report is time-invariant, the household fixed effects in Equation (2) should be able to mitigate this problem. This assumption is likely to hold, since the data on the shock variables were surveyed only once in December 2010. Therefore, the criteria for reporting the shocks could be the same across the eight periods. Nevertheless, one might still be concerned that the

recall bias causes the measurement error in the shocks of the earlier period to be more serious, particularly if the household head is older. Therefore, in the Online Appendix A3, this study estimates the model that excludes households where the head is aged over 60.

Fifth, the correlation of unanticipated income may affect particularized trust through channels other than the incentive for risk sharing. For example, the dyads that were affected by negative shocks at the same time may feel emotional sympathy, and this facilitates particularized trust even without sharing their risks. However, again, it cannot explain the negative impact of income correlation on trust.

Sixth, Table A2 in the Online Appendix shows that the instruments are weak: F-value for the joint significance of the instruments is 4.45. This is presumably because the instruments are self-reported and binary variables, and hence cannot measure the severity of shocks. This measurement error causes attenuation bias in the first-stage result. Therefore, the coefficients of $Corr_{vij}$, $\left|SD_{vi} - SD_{vj}\right|$, and $SD_{vi} + SD_{vj}$ in the second stage should be biased to the direction of OLS estimates, and the standard errors become larger. This issue is discussed in Section 5 by comparing the results of OLS and IV.

Finally, this approach makes the correlation coefficient unidentified for 39% of the dyads; 64 households reported no shocks during the survey periods, causing the variation in unanticipated income to be zero. Therefore, in line with Greene (2011), I replace the correlation coefficient with zero for such dyads and run the regressions with an additional independent variable that takes unity if the correlation is unidentifiable and 0 otherwise. Furthermore, since these 64 households may not have answered the household survey seriously, as a robustness check this study also estimates the model without these households in the Online Appendix A3.

4.3. Incentive and Risk-Sharing Behaviour

Thus far, this study assumes that the incentive for risk sharing facilitates risk sharing behaviour, such as informal loan transactions among villagers. Before showing the main result of particularized trust formation, this section tests the validity of this assumption. It is straightforward to investigate the impact of incentive to share risks between a dyad on their transactions. However, such information is unfortunately unavailable in this dataset, and therefore this study provides suggestive evidence by examining the consumption and borrowing behaviour at the household level. Specifically, it tests whether the cyclone-affected households in the community with higher incentive to share risk can borrow more from informal sources than those in the community with lower incentive.

The following borrowing equation is estimated:

$$Borrow_{vi} = \Delta Income_{vi}\left(\beta_1 + \overline{Corr_{vl}}\beta_2 + \overline{\left|Bet_{vl} - Bet_{vJ}\right|}\beta_3 + \overline{\left(Bet_{vl} + Bet_{vJ}\right)}\beta_4 + \overline{\left|SD_{vl} - SD_{vJ}\right|}\beta_5 + \overline{\left(SD_{vl} + SD_{vJ}\right)}\beta_6\right) + \overline{Prox_{vl}}\beta_7 + X_{vi}\beta_8 + V_v + \varepsilon_{vi} \quad (3)$$

where $Borrow_{vi}$ is the amount of informal loans which household i in *Para* v borrowed from his or her relatives, neighbours, and friends within 12 months after the cyclone in 2009. $\Delta Income_{vi}$ is the first difference of monthly income between the agricultural season before the cyclone and the season after.[17] This variable is instrumented by the specification employed in Equation (2). $\overline{Corr_{vl}}$ is the mean of $Corr_{vij}$ over j. The other variables are defined in the same manner. Finally, V_v indicates the *Para* fixed effects. The argument in Section 2.1 suggests that disaster-affected household i should be able to borrow more from the community members if i's income is negatively correlated with their incomes, and i's risk preference and income volatility are different from theirs. Therefore, it is expected that

[17] The dependent variable does not take the first difference, given that the informal loans were rarely observed in the period before the cyclone.

$\beta_2 > 0,\ \beta_3 < 0$, and $\beta_5 < 0$.

The estimation result from a Tobit model is reported in Table 1. Column 1 presents that the households in the average community borrow from informal sources in the face of negative income shocks. Particularly, the association between them is larger when *i*'s household income is negatively correlated with the neighbours' income (Columns 2 & 5). It is also shown that *i* can borrow more if his/her risk preference and income volatility differ from those of the neighbours (Columns 3, 4, & 5). These results support the validity of underlying assumption.

[Table 1]

5. Results

Table 2 presents the estimation results of Equation (1). Columns 1 to 7 present the determinants of *i*'s particularized trust in *j*. The table shows strong support for the main hypothesis, Hypothesis (a); particularized trust is high if incomes are negatively correlated between the trustor and trustee. This is robust across columns and robust to the issue of weak instrument; first, the signs of these coefficients are the same in the OLS and IV estimations, and the absolute values of IV coefficients are larger. Second, the standard errors in the IV estimates are larger, but the coefficients are still statistically significant. Further, this finding is also robust to the possibilities of various specification errors, such as reverse causality, measurement errors, omitted variable bias, and computation method of standard errors (Online Appendix A3).

Regarding Hypotheses (b) and (c), they do not fit the data as much as Hypothesis (a), as expected. The coefficients of the difference in risk preferences demonstrate expected signs for all the columns, but they are statistically insignificant. The difference in income volatilities presents insignificant and negative association with particularized trust, counter to

the hypothesis.

Two points should be emphasized, however, regarding the poor explanatory power of risk preference and income volatility. First, negative income correlation as well as large gaps in the risk preferences and income volatilities in the dyad may capture mixed effects on trust. On the one hand, they increase the incentive for risk sharing and facilitate trust, but on the other hand, they weaken the in-group favouritism between individuals. Hence, fully controlling for the latter effect should strengthen the estimation results. In fact, I examine the potential roles of unobserved dyadic characteristics in the Online Appendix A3, and observe a consistent result. Therefore, the current result should be considered a lower bound of actual causal effect. Second, the explanatory power of negative assortative matching in the risk preference and income volatility relies on the sharing rules as argued in Section 2.1. Hence, it may not be surprising even if Hypotheses (b) and (c) present weaker explanatory power than (a).

It is also important that the dependent variable might be affected by motives other than particularized trust. First, the proportion of transfer is attributed to the trustor's generalized trust, risk preference, generalized pure/impure altruism, and fairness (Ashraf et al., 2006; Cox, 2004; Ligon and Schechter, 2012; Schechter, 2007b). In addition, given that the participants made decisions in front of the experimenter, concerns about their social image in the eyes of the experimenters could have potentially affected their behaviour. However, presuming that the impact of these factors does not vary across the eight trustees, the trustor fixed effects, D_i, can capture these effects.

Second, the incentive for risk sharing might increase the transfer through the development of particularized altruism to *j* rather than trust to *j*. Admittedly, it is not easy to rule out this possibility, given the unavailability of data on particularized altruism. Therefore, it might be appropriate to interpret the empirical result as evidence on the formation of social

capital/preference rather than trust formation. However, the table suggests that particularized altruism cannot fully explain the patterns of transfer. If particularized altruism drives the larger amount of transfer, the trustors should transfer more endowment to the poor and cyclone-affected individuals, who can yield a larger utility gain from the transfer. Therefore, we should observe the negative coefficients of the trustee's asset holdings and education, and the positive coefficient of cyclone damage. However, such results are not observed.

Finally, if the participants do not perceive the behaviour in the experiment to be fully anonymous, they may transfer more to the opponents who they will ask for help in the future (i.e., those with high incentive to share risks), so that they can build reputation. However, if this is the case, they should transfer more to the opponents who they got to know recently too, because the marginal effect of transfer on building reputation is expected to be larger. The table, however, shows the opposite effect.

Regarding the other independent variables, most social proximity variables—such as kinship, gender, occupation, and religion—are insignificant. The coefficient of dummy for stranger is positive because it captures the influence of unmeasured dyadic characteristics as mentioned in Footnote 14. In addition, the trustor does not necessarily trust a trustworthy and/or altruistic trustee; this finding is consistent with those of Fershtman and Gneezy (2001) and Binzel and Fehr (2013), while it is inconsistent with that of Falk and Zehnder (2013). The result does not change qualitatively when controlling for particularized rather than generalized trustworthiness.[18] This may be because the main motive for the transfer to *j* is high willingness to adopt trusting behaviour rather than high expected trustworthiness.

In addition to the main estimation model of particularized trust, Columns 8 to 14 of the table examine *j*'s particularized trustworthiness to *i* for robustness, which is measured by the proportion of received amount returned to *i*. In this model, I control for the amount *i* sent

[18] The estimation result is not reported in this paper, but is available from the author upon request.

to *j*, instead of *j*'s generalized trustworthiness. Intriguingly, it shows that *j*'s particularized trustworthiness is significantly higher if the gap in their risk preferences is larger. This is in line with Hypothesis (b). Income correlation in the dyad also demonstrates negative coefficients in three of four columns, and two are statistically significant. Finally, the difference in income volatilities is positively associated with particularized trustworthiness, as expected, while the coefficients are statistically insignificant.

6. Conclusions

This study shows that the incentive to share risk facilitates particularized trust formation among villagers in rural Bangladesh. It also provides suggestive evidence that the higher trust is caused by the increased opportunities of cooperation.

Some possible policy implications may be derived, given that the provision of incentive for cooperation is found to facilitate social capital. Improvements to formal institutions so as to help villagers smooth consumption for idiosyncratic shocks can diminish trust among villagers, since it makes them less dependent on informal risk sharing. This in turn offsets returns to programs by lowering the efficiency of risk sharing. Such trust-reducing policies include the introduction of health insurance, an increase in the wages of unskilled labour, and reductions in the transaction costs of livestock sales. On the other hand, participation in microfinance might enhance trust, as it strengthens potential punishments against deviation. In line with this, there is empirical evidence showing the positive effect of microfinance participation on social capital (Feigenberg et al., 2013). Furthermore, job training programs that introduce a new industry in the community may also increase particularized trust, since they cause the income dynamics of villagers to become less correlated, and increase the net gains derived from risk sharing.

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Table 1: Incentive for Risk Sharing and Informal Loans

	(1)	(2)	(3)	(4)	(5)
$\Delta Income$	-8.958*	-25.242***	21.777	6.203	46.286*
	(5.177)	(7.709)	(13.864)	(15.072)	(26.127)
$\Delta Income$ x $\overline{Corr}$		34.032***			28.593*
		(11.358)			(14.700)
1 if $\overline{Corr}$ is unidentified		-0.970			-3.246
		(7.101)			(5.934)
$\Delta Income$ x $\overline{\lvert \mathrm{Bet}_{vi} - \mathrm{Bet}_{vj} \rvert}$			-0.409		-0.541**
			(0.257)		(0.265)
$\Delta Income$ x $\overline{(\mathrm{Bet}_{vi} + \mathrm{Bet}_{vj})}$			-0.191***		-0.239***
			(0.064)		(0.075)
$\Delta Income$ x $\overline{\lvert \mathrm{SD}_{vi} - \mathrm{SD}_{vj} \rvert}$				-65.620***	-40.536*
				(19.327)	(23.909)
$\Delta Income$ x $\overline{(\mathrm{SD}_{vi} + \mathrm{SD}_{vj})}$				3.761	-19.607
				(15.023)	(15.663)
Household Characteristics					
Generalized trustworthiness	0.069	0.050	0.080	0.047	0.062
	(0.089)	(0.083)	(0.094)	(0.085)	(0.089)
% allocated in the dictator game	-0.016	-0.000	-0.038	0.007	-0.012
	(0.054)	(0.052)	(0.050)	(0.050)	(0.048)
Age of participant	-0.095	-0.071	-0.093	-0.089	-0.047
	(0.169)	(0.172)	(0.159)	(0.180)	(0.160)
Schooling years of participant	0.073	0.105	-0.011	0.032	0.059
	(0.485)	(0.484)	(0.438)	(0.510)	(0.461)
Land holdings	-11.873	-11.470	-14.149	-11.942	-13.920
	(12.833)	(13.149)	(14.164)	(13.021)	(14.368)
Liquid assets	57.637**	45.504*	66.081**	61.103**	66.394**
	(24.671)	(25.011)	(26.334)	(25.198)	(29.634)
1 if engaging in farming	-2.184	-4.107	-1.841	-2.477	-3.451
	(3.259)	(3.469)	(3.658)	(3.293)	(3.943)
1 if engaging in processing	-6.237	-6.862	-5.832	-7.353	-7.272
	(6.912)	(7.406)	(7.146)	(6.847)	(6.862)
Inundation of residence (Months)	-5.905	-5.800	-8.062**	-5.775	-8.137**
	(4.392)	(3.663)	(3.804)	(4.219)	(3.557)
Mean dyadic characteristics at the Para level					
Years since knowing	-0.087	-0.093	-0.081	-0.092	-0.100
	(0.254)	(0.240)	(0.251)	(0.242)	(0.224)
1 if relative	-17.302	-13.009	-17.479	-10.124	-5.427
	(10.936)	(10.462)	(11.495)	(12.956)	(13.985)
1 if stranger	-155.068***	-140.793**	-147.133***	-161.200**	-164.539**
	(57.902)	(54.470)	(56.516)	(66.738)	(74.192)
1 if the participants' gender are the same	-1.589	-3.649	5.099	-3.573	2.664
	(7.779)	(8.694)	(7.105)	(8.340)	(7.957)
1 if the same occupation category	-1.657	-0.708	-5.754	-1.011	-5.175
	(8.786)	(8.879)	(8.971)	(8.805)	(9.326)
1 if going to the same mosque	-16.422**	-16.407**	-12.087	-17.261**	-14.902**
	(8.155)	(6.906)	(7.537)	(8.092)	(6.848)
Distance between houses (km)	-41.188	-42.864*	-41.932	-41.638*	-46.612*
	(26.371)	(23.931)	(30.173)	(24.021)	(25.735)
Observations	251	251	251	251	251
Village FE	Yes	Yes	Yes	Yes	Yes

Clustered standard errors at the Para level are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 2: The Impact of Incentive on Particularized Trust

	i's trust in *j*							*j*'s trustworthiness in *i*						
	IV	OLS	OLS	IV	OLS	IV	OLS	IV	OLS	OLS	IV	OLS	IV	OLS
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
Dyadic Characteristics														
Income correlation	-2.318*	-2.110**				-3.200*	-2.372**	-0.467	-3.901*				0.090	-4.271**
	(1.207)	(0.932)				(1.559)	(0.987)	(4.596)	(1.939)				(5.173)	(1.459)
1 if correlation is unidentified	0.833					3.314		6.670					0.705	
	(1.574)					(2.211)		(5.784)					(6.990)	
$\lvert \mathrm{Bet}_{vi} - \mathrm{Bet}_{vj} \rvert$			0.009			0.009	0.010			0.086**			0.077**	0.087**
			(0.015)			(0.012)	(0.014)			(0.032)			(0.033)	(0.032)
$\mathrm{Bet}_{vi} + \mathrm{Bet}_{vj}$			-0.000			0.001	-0.001			0.063			0.055	0.062
			(0.014)			(0.013)	(0.014)			(0.067)			(0.065)	(0.067)
$\lvert \mathrm{SD}_{vi} - \mathrm{SD}_{vj} \rvert$				-2.610	-0.169	-6.808	-0.143				0.849	0.192	0.796	0.448
				(3.767)	(0.872)	(4.924)	(0.855)				(6.609)	(1.182)	(7.012)	(1.070)
$\mathrm{SD}_{vi} + \mathrm{SD}_{vj}$				1.740	0.002	6.347*	-0.073				-22.188**	-0.413	-19.757	-0.726
				(3.096)	(0.759)	(3.441)	(0.754)				(8.848)	(1.694)	(11.894)	(1.609)
Years since knowing	0.335***	0.332***	0.330***	0.326***	0.332***	0.333***	0.336***	-0.130	-0.151	-0.156	-0.133	-0.151	-0.134	-0.146
	(0.073)	(0.074)	(0.072)	(0.072)	(0.073)	(0.070)	(0.074)	(0.109)	(0.104)	(0.107)	(0.114)	(0.097)	(0.116)	(0.096)
1 if relative	0.080	-0.120	-0.044	-0.050	-0.109	0.297	-0.147	0.644	0.231	0.455	0.345	0.301	0.476	0.306
	(2.780)	(2.799)	(2.772)	(2.781)	(2.768)	(2.772)	(2.762)	(3.641)	(3.513)	(3.593)	(3.650)	(3.544)	(3.757)	(3.560)
1 if stranger	5.381*	5.287*	5.391*	5.462*	5.413*	5.493*	5.223*	-12.509*	-13.239**	-13.949**	-12.253*	-13.048*	-13.146**	-14.391**
	(2.563)	(2.563)	(2.599)	(2.602)	(2.541)	(2.603)	(2.491)	(6.132)	(6.063)	(5.942)	(6.119)	(6.235)	(5.911)	(5.978)
1 if the participants' gender are the same	0.935	0.807	0.932	0.928	0.962	0.916	0.824	2.923*	2.871	3.353*	2.493	3.161*	2.760	3.182*
	(1.251)	(1.268)	(1.292)	(1.255)	(1.244)	(1.291)	(1.297)	(1.647)	(1.650)	(1.723)	(1.530)	(1.616)	(1.630)	(1.749)
1 if the same occupation category	0.006	0.249	-0.007	-0.057	-0.016	0.088	0.273	-4.260	-4.547	-4.570	-4.323	-5.036	-3.915	-4.062
	(1.523)	(1.503)	(1.577)	(1.577)	(1.545)	(1.532)	(1.525)	(3.568)	(3.270)	(3.165)	(3.145)	(3.233)	(3.323)	(3.204)
1 if going to the same mosque	-1.195	-1.288	-1.163	-1.100	-1.172	-1.026	-1.300	-0.991	-1.177	-0.897	-1.135	-0.964	-1.091	-1.225
	(2.779)	(2.694)	(2.707)	(2.732)	(2.704)	(2.801)	(2.644)	(4.946)	(4.799)	(4.669)	(4.941)	(4.835)	(4.806)	(4.683)
Distance between houses (km)	1.946	2.244	2.348	2.528	2.398	1.901	2.144	-7.631	-7.489	-7.847	-6.924	-7.276	-7.533	-8.242
	(5.344)	(5.300)	(5.297)	(5.413)	(5.372)	(5.449)	(5.315)	(7.220)	(7.134)	(6.816)	(7.099)	(7.328)	(6.635)	(6.835)
Trustee Characteristics														
Generalized	-0.016	-0.016	-0.015	-0.013	-0.015	-0.014	-0.017							

trustworthiness	(0.014)	(0.014)	(0.013)	(0.013)	(0.014)	(0.012)	(0.013)							
Amount sent by *i*								-0.034	-0.031	-0.036	-0.037	-0.032	-0.039	-0.033
								(0.024)	(0.026)	(0.027)	(0.023)	(0.026)	(0.024)	(0.027)
% allocated in the	-0.012	-0.014	-0.014	-0.016	-0.013	-0.014	-0.011	0.280***	0.274***	0.277***	0.280***	0.276***	0.282***	0.279***
dictator game	(0.015)	(0.015)	(0.016)	(0.016)	(0.015)	(0.016)	(0.015)	(0.061)	(0.062)	(0.062)	(0.060)	(0.065)	(0.059)	(0.063)
Age of participant	-0.040	-0.049	-0.041	-0.041	-0.043	-0.043	-0.053	-0.050	-0.064	-0.035	-0.040	-0.053	-0.029	-0.056
	(0.034)	(0.037)	(0.037)	(0.035)	(0.033)	(0.034)	(0.037)	(0.102)	(0.109)	(0.101)	(0.101)	(0.096)	(0.101)	(0.100)
Schooling years of	-0.074	-0.098	-0.084	-0.087	-0.094	-0.068	-0.107	0.874	0.837	0.895*	0.888*	0.861	0.917*	0.878*
participant	(0.095)	(0.108)	(0.099)	(0.099)	(0.098)	(0.093)	(0.106)	(0.503)	(0.487)	(0.470)	(0.488)	(0.493)	(0.477)	(0.474)
Land holdings	0.015	-0.187	-0.212	-0.237	-0.028	0.132	0.090	1.195	0.791	-0.511	0.826	1.117	-0.244	0.086
	(1.282)	(1.263)	(1.367)	(1.255)	(1.394)	(1.308)	(1.516)	(4.848)	(4.914)	(4.944)	(4.842)	(5.442)	(4.839)	(5.712)
Liquid assets	-0.066	0.528	-0.121	-0.658	-0.177	-0.928	0.735	-4.984	-7.237	-3.074	3.103	-8.099	6.849	-1.402
	(10.687)	(10.558)	(11.081)	(10.223)	(10.019)	(10.794)	(10.433)	(32.877)	(35.272)	(36.416)	(30.138)	(33.914)	(31.611)	(34.551)
1 if engaging in	1.365	1.085	1.345	1.260	1.418	0.978	1.115	4.816	4.260	4.635	6.472	4.892	6.188	4.307
farming	(1.624)	(1.648)	(1.625)	(1.618)	(1.654)	(1.545)	(1.627)	(3.476)	(3.346)	(3.465)	(3.743)	(3.514)	(3.797)	(3.351)
1 if engaging in	3.401**	2.819*	3.163**	3.148**	3.436**	3.484**	3.088**	4.483	3.193	3.057	4.058	4.361	3.354	2.941
processing	(1.379)	(1.469)	(1.352)	(1.413)	(1.310)	(1.309)	(1.324)	(4.577)	(4.343)	(4.196)	(4.489)	(4.797)	(4.334)	(4.420)
Inundation of	-0.185	-0.299	-0.305	-0.407	-0.293	-0.344	-0.294	3.401	2.892	3.072	3.942	2.944	4.035	3.158
residence (Months)	(0.795)	(0.840)	(0.817)	(0.826)	(0.835)	(0.801)	(0.838)	(3.801)	(3.675)	(3.522)	(3.652)	(3.710)	(3.603)	(3.627)
Observations	1,920	1,920	1,920	1,920	1,920	1,920	1,920	1,920	1,920	1,920	1,920	1,920	1,920	1,920
Number of trustors	279	279	279	279	279	279	279	279	279	279	279	279	279	279
Trustor FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Clustered standard errors at the Para level are in parentheses. *** p<0.01, ** p<0.05, * p<0.1