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Channels of Peer Effects and Guilt Aversion in Crime: Experimental and Empirical Evidence from Bangladesh

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Abstract

This study empirically disentangles the channels of peer effects in crime through an experiment conducted in rural Bangladesh. The first part of this study assumes that individuals exhibit guilt aversion, which predicts the peer effects via guilt sensitivity and belief. By incorporating peer effects in a take-away game, the criminal player is informed about the victim player's belief only in the treatment group, so that the peer effects in the treatment group are driven only through guilt sensitivity. The experimental results suggest that peer effects affect and bring about changes in belief. The second part elicits guilt sensitivity to test guilt aversion. I find robust supporting evidence for my results, and reject the alternatives such as pure altruism and trustworthiness. Finally, external validity is also confirmed: the criminal behaviour of subjects in the experiment is correlated with their attitude towards illegal activities in the real world, and individuals are less likely to suffer from property crime in villages with a higher guilt sensitivity neighbourhood.

JEL Codes: C91; C93; D63; K42

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1. Introduction

Although economic studies have theoretically and empirically examined the determinants of crime since Becker (1968), the astoundingly high variance of crime rates across time and space continues to pose a puzzle even today (Glaeser et al., 1996).[1] What is the role of social norms in controlling crimes? How does peer behaviour affect the crime-reduction effects of norms? By addressing these questions, I attempt to uncover the mechanism of crime disparity.

While previous studies have argued that peer effects causing multiple equilibria can explain crime disparity, two issues remain.[2] First, there is no consensus on whether peer effects indeed explain crime disparity. While some empirical studies have found evidence to support this claim (Glaeser et al., 1996; Zenou, 2003; Patacchini and Zenou, 2008; Bayer et al., 2009; Patacchini and Zenou, 2012), others have not (Ludwig and Kling, 2007; Dahl and DellaVigna, 2009). This mixed empirical evidence is partly derived from the difficulties in identifying peer effects using administrative data (Manski 1993, 2000), which indicates the importance of additional studies using experimental approaches, such as those employed by Falk and Fischbacher (2002) and Keizer et al. (2008).[3]

The second unresolved issue is that while previous studies examine the magnitude of peer effects, they do not identify the channels of peer effects. There exist at least three potential channels for their occurrence. The first channel is *strategic complementarity*,

[1] Glaeser et al. (1996) show examples of crime disparity in developed countries. Crime rates in even rural areas of developing countries vary across regions. In Jhalakati district, southern Bangladesh, it is 2.3 times as high as that in the adjacent district, Pirojpur. Additionally, the rate in Bogra district is 4.2 times as high as that in Naogaon district (Faruk and Khatun, 2008).

[2] Peer effects are defined as the degree by which initial peer behaviour predicts subsequent changes in individual behaviour.

[3] However, it is noteworthy that these empirical studies use novel identification strategies to address the issue, although I do not discuss the details here.

namely that the material payoffs of individuals increase when they conform.[4] Second, peer effects occur when the utility loss caused by *extrinsic incentives*—including disapproval by peers and a negative social image—decreases with an increase in crime incidents among peers (Rasmusen, 1996; Funk, 2005). Third, *intrinsic motivations*, such as guilt and pride, may also dissuade people from committing crimes. The motivations may, however, decline as crime incidence among peers increases (Funk, 2005). By nature, extrinsic incentives arise only when the individual's identity and actions are observable to his/her reference group, while intrinsic motivations do not require that anyone else knows how the person acts (Zafar, 2011).

The present study bridges these gaps in the literature by disentangling the channels of peer effects in crime, particularly by examining intrinsic motivations. In order to achieve this goal, this study conducts two closely related analyses. In the first part of this paper, I assume that individuals exhibit guilt aversion and attempt to identify the channels. The second part aims at experimentally and empirically testing the validity of guilt aversion. This study also shows the external validity of my experimental design that approximates property crimes in the real world.

An intriguing aspect of guilt aversion is that it is belief-dependent; it presumes that decision makers experience disutility of guilt if they believe their behaviour falls short of the expectation of others (Charness and Dufwenberg, 2006). This preference predicts two potential channels of peer effects: changes in guilt sensitivity and belief. The former indicates that criminals become less sensitive towards letting their victims down in a crime-prone community. The latter means that when crime is common, criminals anticipate that citizens have higher expectations about the risk of crime victimisation, which in turn decreases the

[4] For example, the probability of detection can decrease as the crime incidence committed by peers increases (Sah, 1991). High crime incidences among peers also raise the marginal return from illegal activities and decline that from legal activities (Murphy et al., 1993).

guilt felt by the criminals when committing crimes against such people.[5]

The two channels isolated by this study are important for two reasons. First, recent studies have argued whether heterogeneity in behaviour of individuals is caused by heterogeneity in beliefs or preferences (Ellingsen et al., 2012; Fehr and Hoff, 2011; Fischbacher and Gächter, 2010; Vanberg, 2008). This study contributes to this aspect of the literature. Second, policy implications would differ for the two channels (see Section 7).

The second part of this study develops a unique approach to elicit the guilt sensitivity parameter at the individual level in order to test the validity of guilt aversion. Previous studies experimentally examined the correlation between the subjects' beliefs and choices and found mixed evidence of guilt aversion (Charness and Dufwenberg, 2006; Reuben et al., 2009; Ellingsen et al., 2010; Kawagoe and Narita, 2011). This study looks at the correlation between choices and guilt sensitivity, as well as the correlation between choices and beliefs, and shows robust supporting evidence. This study also addresses concerns regarding alternative hypotheses explaining behavioural patterns such as pure altruism and trustworthiness.

To achieve these goals, three types of experiments were conducted. First, two sessions of take-away games were employed to approximate crime in the experimental framework (Eichenberger and Oberholzer–Gee, 1998; Falk and Fischbacher, 2002; Schildberg– Hörisch and Strassmair, 2010). Second, a trust game with hidden actions was used to elicit guilt sensitivity and trustworthiness. This game is frequently used in the literature pertaining to guilt aversion (Charness and Dufwenberg, 2006; Vanberg, 2008; Charness and Dufwenberg, 2010; Ellingsen, et al., 2010; Charness and Dufwenberg, 2011; Kawagoe and Narita, 2011).

[5] This is also consistent with the broken windows theory of criminology (Kelling and Wilson, 1982; Wilson and Kelling, 2003). This theory claims that if a neighbourhood ignores the incidence of petty crime, such as broken windows, potential criminals would anticipate that the neighbourhood does not care about crimes, which in turn would lead to crimes that are more serious.

Finally, I also conducted a dictator game to elicit pure altruism.

This study addresses three major issues regarding experimental studies. First, there is little experimental evidence from developing countries. Second, in many studies, the subjects are self-selected to participate in the experiment, causing estimation results to be biased. The third issue concerns external validity (Loewenstein, 1999; Guala and Mittone, 2005); the behaviour in the experiment might not represent that in the real world. To address these issues, this experiment was conducted in rural Bangladesh. Of the 285 randomly selected households, 279 participated in the experiment. Furthermore, this study used survey data collected from the participant households to examine the external validity.

It is particularly insightful to study the impact of intrinsic motivations on crime incidence in developing countries, such as Bangladesh, since developing countries have long grappled with problems arising from ineffective law enforcement. Therefore, intrinsic motivation and extrinsic incentives are expected to play significant roles in controlling crime in such areas. Note that the experimental design is applicable to other countries as well.

The results show that the behavioural patterns in the experiment are consistent with guilt aversion, whereas participants' altruism and trustworthiness are not consistent; participants with higher guilt sensitivity and/or with lower second-order belief are less likely to commit crime. In addition, peer effects occur through changes in second-order belief. Finally, I show the external validity of the approximated criminal behaviour and the elicited guilt sensitivity; the criminal behaviour in the experiment is correlated with the individuals' attitude to crime in the real world, and individuals are less likely to suffer from property crime in villages with higher guilt sensitivity neighbourhood.

The remainder of the paper is structured as follows. The next section describes the design and treatment of a take-away game. The external validity of the experiment in approximating crime is also discussed. Section 3 summarises the theoretical framework of

guilt aversion. Section 4 formalizes the testable hypotheses to disentangle the channels of peer effects and presents the results. Section 5 elicits the guilt sensitivity of subjects and tests guilt aversion. Section 6 shows the external validity of guilt sensitivity. Finally, Section 7 concludes.

2. Survey and Experimental Design

2.1 *Sampling of Subjects*

In December 2010, a total of 288 households were randomly sampled in 16 rural villages (18 households per village) in the Satkhira district of Bangladesh. Of the total households sampled, 285 participated in the household survey.[6] After completion of the survey, the survey enumerators requested the households to participate in the experiment to be conducted eight months later and informed them about the reward payment for their cooperation in the survey.

In August 2011, 279 out of the 285 surveyed households participated in the experiment.[7] A total of 36 subjects from 2 villages were invited per day and were allocated to 2 rooms. The experiments were conducted over eight days. I randomly chose half the subjects (nine from each village) and allocated them to room C. The remaining subjects were allocated to room T. The subjects in room C were further divided into subjects for even- and odd-numbered experiment days. Thus, the subjects were randomly divided into three groups: the treatment group (137 subjects) in room T, the control group (71 subjects) in room C, and the remaining (71 subjects) in room C. I used the data from the third group in Section 6 only, because they participated in different games.

[6] Table A1 presents the summary statistics of the survey data. The details about the sampling process are described in Appendix A.

[7] In order to assure the sample size, I did not randomly select the experiment participants within households.

Each subject participated in various games, such as the take-away game, dictator game, trust game with hidden action, risk preference game, and trust game with complete information. Each subject received his/her payoff from only one randomly selected decision after finishing all the games. Therefore, they did not know the decision from which they received the payoff and were aware that each subject had earned money from a different decision. This is important for two reasons. First, it alleviates the correlation of choices within subjects across games due to the wealth effect. Second, if subjects were to earn money from all games and discuss the payoffs after the experiment, they might have been able to infer the choices of the other subjects. This would have violated subject anonymity, potentially affecting behaviour. A detailed description about the experiment implementation is presented in Online Appendix.

2.2 *Take-away Game*

This study uses a take-away game (gangster game) to replicate criminal activities in an experimental setting. This game is commonly employed to investigate crime and anti-social behaviour experimentally (Eichenberger and Oberholzer–Gee, 1998; Falk and Fischbacher, 2002; Schildberg–Hörisch and Strassmair, 2010).

This game is played anonymously by a pair of subjects matched randomly: Player A and Player B. At the beginning of the game, experimenters give BDT 400 to Player A and Player B receives nothing. The amount given to Player A is equivalent to about four days' worth of income in the study area. While Player B receives nothing initially, he/she can take away BDT 0, BDT 50, BDT 100, BDT 150, BDT 200, BDT 250, BDT 300, BDT 350, or BDT 400, as much as he/she wishes, from Player A. Player A cannot control the endowment from Player B. Therefore, if Player B decides to take away x amount, the material payoff of Player A is $400–x$ and that of Player B is x.

Amount x is considered to be the approximation of crime or anti-social behaviour. However, one may have concerns about this approach, since taking money in the experiment is unsocial but not illegal. The decision rule to take money in this game may be different from the one to commit illegal activities in the real world. Therefore, I test the external validity of this game by the following approach. The subjects were asked about the extent to which they could justify seven illegal activities and two legal, but unsocial activities, with the answers being a number between 1 (never justifiable) and 10 (always justifiable).[8] I compute the difference between the average score of the legal but unsocial activities and the average score of illegal activities. This quantifies the extent to which the subjects consider the illegal activities unjustifiable in the real world compared to the unsocial but legal ones. Intriguingly, this indicator is negatively and significantly correlated with the amount that the subject takes away in the experiment (ρ=-0.117, p-value = 0.091), supporting the external validity.

Furthermore, the subjects might still not equate this experiment to real crime if the amount of endowment is too small for them to play the game seriously. I addressed this issue by adjusting the amount to about four times their daily income, thus, increasing the incentive to play the game seriously.

Finally, the subjects may think of the endowment as an unexpected gift from the experimenters to Player A, and therefore, consider equal allocation, called the 50–50 norm, as suitable (Andreoni and Bernheim, 2009). This is possible since the households of both Player A and B participated in the household survey prior to the experiment. Therefore, they may believe they deserve to keep some of the payoff as a reward for the survey. Thus, I use two

[8] The questionnaire design is based on the World Values Surveys. Seven illegal activities include (1) avoiding a fare on public transport, (2) someone accepting a bribe in the course of their duties, (3) for a man to beat his wife, (4) stealing someone's property, (5) stealing someone's property when it is common in the area (6) stealing someone's property when nobody is around, and (7) stealing his/her property from the rich. Two unsocial activities include (1) not helping strangers who are facing hardships and (2) throwing away trash/garbage in a public space.

measures to approximate crime: the level of x (like previous studies) and a binary variable, which takes unity if the subject violates the 50–50 norm ($x > 200$).

2.2.1 *Session 1*

I conducted two sessions of the take-away game. The first session applied the strategy method regarding the role of the subjects, so that all subjects made decisions as both Player A and B.[9] They were asked the following questions:[10]

[T1-1] *Suppose you are Player **B**. How much money will you take away from Player A?*

[T1-2] *Suppose you are Player **A**. How much money do you think Player B will take away from you?*

The latter question represents Player A's belief about the choice of Player B who is paired with him/her. Table A2 presents the summary statistics of the experimental data.

2.2.2 *Session 2 with the treatment*

The pairs were randomly matched in the second session also. Each subject was asked about the choice as Player B only, and the experimental design was changed slightly. For the control group, the experimenters informed each subject about the amount the subjects in the previous days took away from their paired Player A (peer information) as follows:

[T2-1] You *are chosen as Player B. Some participants in the previous days took away about BDT _____, and Player A paired with you also knows this. How much money will you then take away?*

Regarding the peer information, since most subjects chose to take away BDT 100, BDT 200,

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

[10] The experimenters asked other questions as well, but this study does not use the associated answers.

or BDT 300, one value was randomly chosen for each subject.[11] Player B decided how much money to take away conditional on this information. The peer effects in this study are defined by the correlation between the peer information and *x*.

On the other hand, for the treatment group, the experimenters informed each subject about (the paired) Player A's belief about *x* and the peer information as follows:

[T2-2] *You are chosen as Player B. Some participants in the previous days took away about BDT _____, and Player A paired with you also knows this. Additionally, Player A anticipates that you will take away BDT _____. How much money will you then take away?*

Regarding the information about Player A's belief, I used the result of the paired subject from [T1-2]. After showing the theoretical framework in the next section, I will discuss in Section 4 how this experimental design identifies the channels of peer effects.

3. Impact of Peer Information on Guilt Averse Individuals

The guilt aversion preference of Charness and Dufwenberg (2006) considers a particular type of guilt in that an individual experiences a utility loss if he/she believes his/her behaviour falls short of someone's expectation and lets the latter down. This concept was theoretically formalized by Battigalli and Dufwenberg (2007), and extended to experimental studies by Charness and Dufwenberg (2006), Miettinen and Suetens (2008), Vanberg (2008), Reuben et al. (2009), Ellingsen et al. (2010), Dufwenberg et al. (2011), Bellemare et al. (2011), Kawagoe and Narita (2011), Ellingsen et al. (2012), Battigalli et al. (2013), and Beck et al. (2013).

In the context of the take-away game, Player B feels guilty if he/she believes that he/she takes away more than what Player A expects. This guilt feeling is interpreted as guilt

[11] Therefore, each individual within the experiment room received different information.

aversion. Suppose τ^A represents Player A's expectation about x. Then, the excess amount Player B takes away is indicated by $\max\{x-\tau^A, 0\}$. Since τ^A is unobservable for Player B, he/she does not know exactly by how much his/her choice lets Player A down. Hence, he/she makes decisions based on his/her expectation about τ^A, which is denoted by τ^B. In other words, τ^A and τ^B are the first- and second-order belief about x, respectively. Therefore, $\max\{x-\tau^B, 0\}$ indicates how much Player B believes he/she lets Player A down by taking away as much as x. The utility function of Player B with the guilt averse preference can be described as follows:

$$u^B = x - g \max\{x-\tau^B, 0\}, \qquad (1)$$

where g represents the guilt sensitivity parameter. In this formula, the optimal level of x is 400 if g is less than 1 and τ^B otherwise. This leads to the following predictions: (1) Subjects with $g \geq 1$ are less likely to take away more than τ^B and (2) x increases with an increase in τ^B and a decrease in g.

In this utility function, there would be two potential channels through which peer information affects x. First, guilt sensitivity declines by knowing that the peers take away a substantial amount ($\partial g/\partial x^P < 0$, where x^P is the amount the peers take away) (Funk, 2005; Sliwka, 2007).[12] In other words, Player B does not experience guilt disutility from taking away more than Player A's expectation, even though he/she believes his/her choice lets Player A down. Second, the second-order belief increases with the level of peer information ($\partial \tau^B/\partial x^P > 0$). Given that the peers take away a substantial amount, Player A's belief about x increases. Expecting this, Player B would also form a high second-order belief. Therefore, he/she believes that taking away a large amount should not let Player A down in such a

[12] Related studies include Lindbeck (1997) and Lindbeck et al. (1999) who consider that the moral costs of going against norms decreases with an increase in the share of norm violators among the population. Sliwka (2007) assumed that individuals switch their preference, whether selfish or fair, depending on the proportion of the preferences among the population. Funk (2005) also developed a model to incorporate the change in preference based on the distribution of preferences among the population.

situation. This, in turn, increases the amount of take away.

4. Testing the Existence of Peer Effects and the Channels

4.1 *Testable Hypothesis*

This section describes the strategy to disentangle the channels of peer effects. My experimental design allows peer effects—defined as the correlation between peer information x^P and the amount of take-away money x—to occur only through changes in intrinsic motivation; each subject is anonymously paired with a randomly selected opponent to rule out extrinsic incentives.[13] In addition, peer information does not affect the material payoff of any subject, ruling out strategic complementarity. This strategy is similar to Zafar (2011), who investigated how conformity to pro-social behaviour occurs among individuals.

Furthermore, the comparison between the control and treatment groups isolates the impact of change in the second-order belief from total peer effects. Since the experimenters in the control group provided the subjects with peer information only, the correlation between x and peer information captures the total impact of peer information. On the other hand, in the treatment group, the experimenters informed the subjects about Player A's first-order belief and also provided them with peer information. By letting Player B know about Player A's belief, the second- and first-order beliefs coincide. This makes the second-order belief a random variable observable to researchers.[14] Since Player B's second-order belief is randomly determined regardless of peer information, if a correlation is found in the treatment group, it should be attributed to a different channel, that is, the one different from the change

[13] One might be concerned that the design is not perfectly anonymous, given the fact that the researchers and experimenters can observe the choices of the subjects. However, this effect may be ignored, according to Barmettler et al. (2012).

[14] The strategy to inform the first-order belief was first suggested by Ellingsen et al. (2010). This procedure reduces the scope for (false) consensus effects.

in the second-order belief. Thus, the following testable hypothesis is established:

TESTABLE HYPOTHESIS 1: *(a) If peer effects do not occur through any channels related with intrinsic motivation, x should not be correlated with peer information in either group.*
(b) If peer effects occur mainly through changes in the second-order belief, then x should be positively correlated with peer information only in the control group; once the second-order belief is controlled for, peer effects should disappear.
(c) However, if peer effects occur mainly through other channels, such as change in guilt sensitivity, then x should be positively correlated with peer information in both groups with the same magnitude.
(d) If peer effects occur through both channels, then a positive correlation should be found in both groups, and the magnitude of correlation should be larger in the control group.

In order to test the hypothesis, the following equation is estimated:

$$x_i = \alpha_0 + \alpha_1 x_i^{PC} + \alpha_2 x_i^{PT} + \alpha_3 T_i + \epsilon_i, \quad (2)$$

where x_i^{PC} (x_i^{PT}) denotes peer information reported to the subjects in the control (treatment) group. It takes zero for the subjects in the treatment (control) group. T_i takes unity if individual *i* belongs to the treatment group and zero otherwise. In this specification, $\alpha_1 = \alpha_2 = 0$ supports part (a) of the testable hypothesis, $\alpha_1 > 0$ and $\alpha_2 = 0$ is consistent with (b), $\alpha_1 = \alpha_2 > 0$ supports (c), and finally $\alpha_1 > \alpha_2 > 0$ implies (d).

4.2 *Results*

Figure 1 depicts the mean amount taken away by Player B relative to the groups and the level of peer information. One can observe that the average amount increases with the level of peer

information only in the control group. Table 1 presents the regression results of Equation 2. I employ two econometric models. First, I use x as the dependent variable and estimate the ordered probit model, given that it can take only nine numbers ranging from BDT 0 to 400. The second model is the probit model with the dependent variable taking unity if x exceeds BDT 200. This represents the violation of the 50–50 norm. In order to control for individual and village heterogeneity, I also include village fixed effects in Columns (2) and (5), and the choice made in the first session in Columns (3) and (6). Since the subject's choice may be correlated within the same experiment room each day, I use cluster-adjusted standard errors.

Table 1 shows that the coefficients of peer information in the control group are statistically significant, while those of the treatment group show smaller magnitude and are statistically insignificant. These findings are consistent with peer effects through the change in the second-order belief about x.

However, there is the possibility of an alternative interpretation. Intrinsic motivations may include other preferences, such as pure altruism, envy, and trustworthiness. Social comparison and the anchoring effect can also cause conformity in this setting (Tversky and Kahneman, 1974; Cason and Mui, 1998; Bohnet and Zeckhauser, 2004; Frey and Meier, 2004). Therefore, α_1 and α_2 in Equation 2 may capture these mixed effects. Yet, these alternatives cannot explain the difference in peer effects between the groups.

In order to address these issues more systematically, Section 5 tests guilt aversion relative to the other types of preferences. Furthermore, two types of robustness checks are discussed in Appendix B: the credibility of informed belief and the potential concern that informed belief and peer information may affect behaviour through different channels.

5. Testing Guilt Aversion

5.1 *Methodology to Elicit Guilt Sensitivity*

This section tests the following hypotheses, which are derived from Equation 1.

TESTABLE HYPOTHESIS 2: *A subject with higher guilt sensitivity* $(g \geq 1)$ *is less likely to take away in excess of his/her second-order belief.*

TESTABLE HYPOTHESIS 3: *On average, the take-away amount increases with an increase in the subject's second-order belief and a decrease in his/her guilt sensitivity.*

Eliciting the guilt sensitivity of individuals poses a challenge in testing these hypotheses. In order to elicit the preference parameter, I conduct a trust game with a hidden action. This game is commonly used in the guilt aversion literature (Charness and Dufwenberg, 2006; Vanberg, 2008; Charness and Dufwenberg, 2010; Ellingsen et al., 2010; Charness and Dufwenberg, 2011; Kawagoe and Narita, 2011). Therefore, while the elicited guilt sensitivity would be sensitive to experimental design, I still consider this as the most suitable game.

The structure of the game is summarised in Figure 2. Each subject is paired with a randomly chosen opponent in the other experiment room. They are assigned as Player A and Player B. The pairs in this game are not the same as those in the take-away game. This game has three stages. In the first stage, Player A chooses *In* or *Out*. If he/she chooses *Out*, the game is over, and both subjects receive BDT 100 each. If *In* is chosen, the game proceeds to the second stage, where Player B either chooses *Roll the die* or *Don't roll.* If Player B does not roll the die, he/she earns BDT 280, while the paired Player A receives BDT 0. If Player B decides to roll the die, it goes to the third stage, where Player B's payoff is BDT 200, regardless of the face of the die. However, Player A's payoff depends on the face of the die. Player A receives nothing if the face shows 1 and BDT 240 otherwise. A selfish Player B is expected to choose *Don't roll*, and therefore, the selfish Player A, who expects this choice,

chooses *Out*. A trustworthy Player B, on the other hand, chooses *Roll the die* and Player A, trusting him, chooses *In*.

Player B, who is guilt averse, experiences disutility if he does not roll the die when Player A expects him to. The level of disutility depends on the extent to which he anticipates that Player A trusts him. ρ^A represents Player A's belief about *Roll the die*, conditional on Player A choosing *In*. ρ^B is Player B's belief about ρ^A. Since the expected material payoff for Player A is BDT 200 when Player B rolls the die, Player B believes Player A expects BDT $200\rho^B$ when choosing *In*. However, if Player B chooses *Don't roll*, Player A yields nothing. Thus, Player B believes that if he/she does not roll the die, his/her choice will let Player A down by BDT $200\rho^B$. This causes Player B to achieve a utility as much as BDT $280–200\rho^B g$ by choosing not to roll the die. If it exceeds the utility obtained from rolling the die (BDT 200), Player B will behave in a selfish manner. This implies that Player B rolls the die if and only if $\rho^B g>0.4$. Therefore, subjects with a certain level of guilt sensitivity should switch their choice from *Don't roll* to *Roll the die* as ρ^B increases. The switching point varies depending on their guilt sensitivity.

I use this property. The experimenters explain to the participants that there are seven potential Player As with different levels of ρ^A.[15] The subjects are asked their decision about rolling the die for each potential opponent as follows:[16]

[H-4] *Player A expects that none of the 18 participants in this room will roll the die. Then, which option will you choose?*

[H-5] *Player A expects that 3 of the 18 participants in this room will roll the die. Then, which*

[15] In order to help uneducated subjects understand the experiment setting, I use the proportion of individuals who are anticipated to choose trustworthy behaviour rather than the probability of behaving in a trustworthy manner.

[16] In other words, I use the strategy method regarding the level of the first order belief. It is shown by Amdur and Schmick (2012) that the feeling of guilt does not differ between the cases of direct-response and strategy methods.

option will you choose?

[H-6] *Player A expects that 6 of the 18 participants in this room will roll the die. Then, which option will you choose?*

[H-7] *Player A expects that 9 of the 18 participants in this room will roll the die. Then, which option will you choose?*

[H-8] *Player A expects that 12 of the 18 participants in this room will roll the die. Then, which option will you choose?*

[H-9] *Player A expects that 15 of the 18 participants in this room will roll the die. Then, which option will you choose?*

[H-10] *Player A expects that everybody will roll the die. Then, which option will you choose?*

Then, I compute the guilt sensitivity of each individual from the switching point, as summarised in Table 2. This study applies the strategy method. All subjects make decisions as both players, in order to elicit the guilt sensitivity of all subjects. Among the 208 subjects, only one switched to the opposite choice.

The last column in Table 2 presents the distribution of guilt sensitivity among the subjects. It is significant to note that 25.5% of subjects behaved in a trustworthy manner even when Player A's belief was zero. This cannot be explained simply by guilt aversion, because the net gain from choosing *Don't roll* is positive regardless of guilt sensitivity. Rather, this is consistent with trustworthiness and pure altruism, implying that the indicator of guilt sensitivity partially captures these social preferences. The econometric analysis in Section 5.3 addresses this concern.

5.2 *Results: Testable Hypothesis 2*

Figure 3 depicts the correlation among the informed first-order belief, guilt sensitivity, and the take-away amount. Since this analysis requires data pertaining to informed first-order

belief, I use the sample of the treatment group only. Among the 137 subjects, 28% took away more than their beliefs, 25% took away the same amount, and 47% took away less. The straight line in the figure indicates the 45° line. The subjects in the region above this line are expected to experience disutility. As summarised in the Testable Hypothesis 2, guilt aversion predicts that subjects with higher guilt sensitivity should be found with less frequency in this region than in other regions (i.e. on the line and in the region below the line).

Figure 3 shows that above the line, 59% of subjects have guilt sensitivity equal to or greater than 1, while the corresponding statistic in the other regions is 42%. The independency between them is rejected (p-value = 0.083). The mean guilt sensitivity of the subjects above the line and in the other regions is 1.03 and 1.26, respectively. This is also consistent with the prediction, while the difference is statistically insignificant (p-value = 0.106). Importantly, the finding that an individual's belief significantly affects his/her choices cannot be explained by the other preferences, such as pure altruism, which predict that only resource allocation matters.

5.3 *Results: Testable Hypothesis 3*

While the results in Section 5.2 support guilt aversion, the elicited guilt sensitivity may be correlated with other preferences, such as trustworthiness and pure altruism. Furthermore, unlike the belief variable, guilt sensitivity could be correlated with other subject characteristics that affect behaviour, such as wealth and demographics. Therefore, this section tests guilt aversion using an econometric approach and controls for potential bias. Specifically, I elicit the approximation for altruism and trustworthiness from the other games, and estimate the following model to isolate the effects of these preferences and other subject characteristics from guilt sensitivity:

$$x_i = \beta_0 + \beta_1 \tau_i^A + \beta_2 x_i^{PT} + \beta_3 g_i + H_i\gamma + Pref_i\delta + \epsilon_i, \quad (3)$$

where τ_i^A is the paired Player A's first-order belief about x_i, which is reported to subject i. g_i is the guilt sensitivity. Guilt aversion predicts $\beta_1 > 0$ and $\beta_2 < 0$. $Pref_i$ includes the approximation for pure altruism and trustworthiness. Pure altruism is measured by the sending proportion in the dictator game. In order to elicit trustworthiness, the subjects were asked whether to roll the die or not prior to informing the beliefs of opponents in the trust game. The measurement of trustworthiness takes unity if the subject rolls the die, and zero otherwise. More details are in Online Appendix. Table 3 shows that these variables are indeed positively and significantly correlated. H_i includes subject characteristics as well as the village fixed effects. While peer information is also included in the equation, this is irrelevant in testing the preference.

Table 4 presents the result of Equation 3. It shows robust supporting evidence of guilt aversion even after controlling for the other characteristics. The explanatory variables in columns (1) and (5) include only two exogenous variables, and those in columns (2) and (6) also include guilt sensitivity. An increase in belief increases the amount of take away, and an increase in guilt sensitivity decreases the amount. I control for the subject characteristics and other preferences in columns (3), (4), (7), and (8). The results are qualitatively the same. Intriguingly, altruism and trustworthiness do not explain the pattern of experimental criminal behaviour well, while the signs of the coefficients are consistent with the prediction.

Finally, three types of robustness checks are discussed in Appendix C: change in the form of the utility function, the credibility of informed beliefs in the trust game, and the order effects. The results do not change qualitatively.

6. External Validity: Guilt Sensitivity to Predict the Victimisation Experience

Using the survey data, this section examines the validity of guilt sensitivity in predicting

crime incidence in the real world. Guilt averse individuals are expected to be less likely to commit crimes. Yet, in practice, it is difficult to collect data on crimes committed by the respondents, as they might not report their true crime experience(s).

Therefore, I examine the determinants of victimisation by following Barslund et al. (2007) and Gaviria and Pagés (2002). I test the following hypothesis: individuals residing in more guilt averse neighbourhoods are less likely to be victims of crime. Equation 4 is estimated using the cross-section probit model:

$$V_i = 1\left[\pi_0 + \pi_1 \bar{g} + H_i\pi_2 + \overline{Pref}\pi_3 + \pi_4 g + Pref\pi_5 + \epsilon_i > 0\right], \qquad (4)$$

where V_i takes unity if the household of subject *i* experienced victimisation of property crimes between May 2009 and December 2010 (when the survey was conducted) and zero otherwise.[17] $\bar{g}$ and $\overline{Pref}$ indicate the mean level of g_i and $Pref_i$ in the village, respectively. Guilt aversion predicts that π_1 should be negative.

A potential concern while exploring the correlation between crime victimisation and preference at the village level is the possibility of bias due to reverse causality and omitted variables. Since the experiment was conducted after the household survey, any previous experiences of crime victimisation may have affected the preference of individuals. This, in turn, would affect the mean preference at the village level. Therefore, I control for the preference variables *at the individual level* (g_i and $Pref_i$) as well. These variables capture the effect of past crime victimisation on individual preference, addressing the possibility of reverse causality. In order to address the omitted variables, I compare the results between the specifications with and without controlling for the observable characteristics. As shown below, the results are robust to the inclusion of more independent variables.[18]

[17] The study areas were attacked by Cyclone Aila in May 2009. Crime incidence increased since then in some areas.

[18] Since mean guilt sensitivity is a village-level variable, the independent variables include the fixed

Table 5 reports the estimation results.[19] The fifth and sixth columns focus on the households where the schooling years of the head of the household was higher than the sample median, and on the households where the proportion of males over 15 years of age was lower than the sample median, respectively. These households are more likely to be the victims of property crimes than other households (Cook, 1986; Gaviria and Pagés, 2002; Barslund et al., 2007). Therefore, such households might benefit from high guilt sensitivity at the village level more than others. Table 5 shows that the coefficients of mean guilt sensitivity are negative for all specifications and statistically significant for five specifications. Furthermore, as expected, the impact of mean guilt sensitivity is larger in the subsample estimations.

Regarding the other variables, altruism and trustworthiness do not explain crime patterns in the real world, which is consistent with the findings from the experiment. Higher schooling years of the head of the household, an approximation of wealth, leads to a higher propensity of crime victimisation (Barslund et al., 2007; Gaviria and Pages, 2002). The damages from cyclone Aila, which hit the study area in May 2009, also increase the probability of crime victimisation, possibly because it is easier for criminals to steal from evacuated houses. It is also shown that those living close to markets are more likely to be a victim, consistent with Cook (1986), who claims that commercial places tend to suffer from a relatively high victimisation rate.

7. Conclusion

This study uncovered the channels through which social interaction influences the crime reduction effects of guilt. By conducting an artefactual field experiment in Bangladesh, I

effects at the sub-district level to control for community-level heterogeneity.

[19] Given the victimisation data was collected around the time of Aila (May 2009–December 2010), there is a possibility that the analysis results may be based on the inflated disaster-induced crime rates.

found evidence of peer effect through changes in the second-order belief. I also found robust supporting evidence for guilt aversion by using experimental and survey data.

A possible policy implication may be derived from this study. The existence of peer effects implies that communities benefit when law enforcement authorities monitor minor disorders. Furthermore, peer effects caused by changes in beliefs indicate that those in crime-prone communities are tempted to commit crime only when they believe that others anticipate high risk of crime victimisation. Therefore, those who grow up in crime-prone areas would not necessarily commit crime in other cities where residents anticipate lower risks. Consistently, there is empirical evidence that teenagers who move from poor and crime-prone neighbourhoods to more affluent neighbourhoods are less likely to commit violent crimes than those who stay in poor neighbourhoods (Ludwig et al., 2001).

These findings must be interpreted with caution, however, since they hinge on the validity of my identification strategy and the small and limited sample dataset considered for this study. Future studies in this area may use different approaches to draw conclusions, and may experiment with diverse and broader datasets, pertaining to varying contexts.

Appendices

Appendix A: Sampling and Survey Data

This study uses household survey and experiment data collected in the Satkhira district located in southwest Bangladesh. This area has two distinctive properties. First, it was severely affected by cyclone Aila in May 2009, and people experienced decline in income and loss of assets. Second, it is prone to crime.

The household survey was conducted in December 2010. The questionnaire covers data from January 2009 until December 2010 on the magnitude of cyclone damage, evacuation behaviour, geographical characteristics, crime incidence, self-reported social

capital, demographic characteristics, labour and non-labour incomes, asset holdings, savings, relief from the government and non-governmental organisations, membership of microfinance institutions, food and non-food consumption, and relationship with the other sample households in the cluster. Table A1 presents the summary statistics on the used variables.

I employed the multistage stratified random sampling methodology. In the first stage, I selected the three sub-districts (*Upazila*) of Kaliganj, Ashashoni, and Samnagar, based on their economic status, the intensity of cyclone damage, and crime incidences. In the second stage, I randomly sampled two unions from each sub-district.[20] In the next stage, four villages from each union and one cluster from each of the villages were randomly selected. Finally, 18 households from each cluster were chosen. Since 5 households were unavailable for the survey, I obtained a total of 427 out of 432 sample households from 24 villages.

Appendix B: Robustness Checks for Section 4

B.1. *The Informed Belief in the Take-away Game May Not Be Credible*

The subjects might simply not trust the informed belief in the take-away game, if it is unreasonably low or high. If this is the case, the response to the informed belief could be non-linear. To address this issue, I replace the belief variable with a binary variable, which takes unity if the belief is higher than 200 and zero otherwise. The result does not change qualitatively. While the regression result is not reported here, it is available upon request to the author.

B.2. *Belief and Peer Information May Affect the Choice of Player B through Different Channels*

[20] 'Union' is an administrative unit in Bangladesh. Each union includes multiple villages.

The subjects who are informed of higher belief and/or higher peer information may infer that the other subjects are more self-interested. This reduces the anticipated payoff of the subjects from participation in experiments, which in turn increases the take-away amount for two reasons. First, they might attempt to assure a certain level of earning from the experiment. Second, they might retaliate against such selfish subjects. These effects cause overestimation of the impact of peer information and belief.

However, first, these potential concerns cannot explain the difference in the coefficient of peer information between the control and treatment groups, which is the main finding of Table 1. Furthermore, if this is the case, those who are informed of higher belief and/or provided peer information should behave in a less trustworthy manner in the following trust game with hidden action. However, such evidence is not observed. Therefore, I consider that these concerns are ignorable.

Appendix C: Robustness Checks for Section 5

C.1. *The Utility Function (Equation 1) May Be Non-linear*

One might be concerned regarding the linearity in the utility function. Therefore, I also consider alternative guilt sensitivity computed under the following utility function: $u^B = x - g \max\{x - \tau^B, 0\}^2$. The estimation results are presented in Table C1. They do not change qualitatively.

C.2. The Informed Belief in the Trust Game May Not Be Credible

In the trust game with hidden action, I used the strategy method across seven levels of first-order beliefs to infer guilt sensitivity. However, the anticipated payoff for Player A when choosing *In* is lower than that when choosing *Out,* if his/her belief about *Roll the die* is less than nine people ($\rho^A < 0.5$). Such a low belief might not be credible for Player B,

causing the elicited guilt sensitivity to be inaccurate. Therefore, I use the questions with belief equal to or greater than nine people ([H-7] to [H-10]) and compute an alternative sensitivity variable, which takes unity if the subject always rolls the die when the informed belief is nine people or higher and zero otherwise. The estimation result is reported in Table C2. Again, the result is robust.

C.3. Belief and Peer Information in the Take-away Game May Affect Guilt Sensitivity

The inferred guilt sensitivity might be attributed to the information participants received in the previous experiments. Specifically, it might be influenced by peer information and informed beliefs in the take-away game. To address this concern, I regress guilt sensitivity on these variables. They are not significantly correlated to each other. The result is not reported in the paper but is available upon request to the author.

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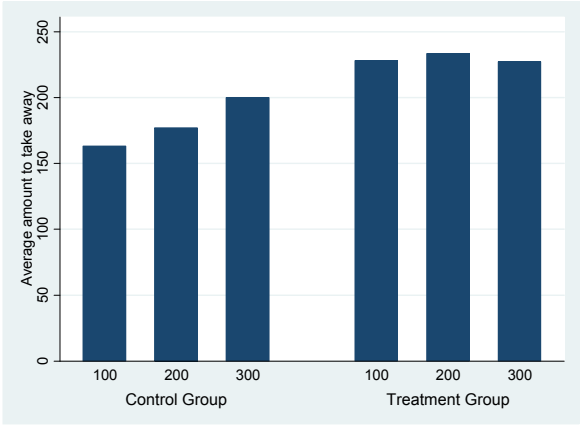

Fig. 1. *Correlation between* x^P *and* x *(Testable Hypothesis 1)*

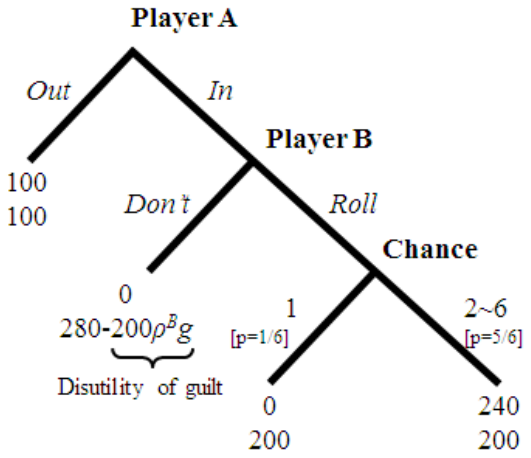

Fig. 2. *Structure of the Trust Game with Hidden Action*

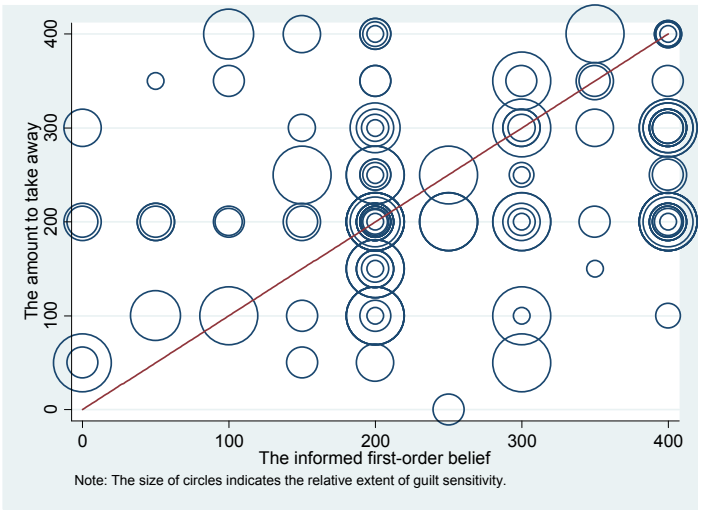

Fig. 3. *Correlation among First-order Belief, Guilt Sensitivity, and the Take-Away Amount*

(Testable Hypothesis 2)

Table 1

Existence of Peer Effect and Channels (Testable Hypothesis 1 and Equation 2)

Dependent Variable	x			1 if $x > 200$		
Methodology	Ordered Probit			Probit		
	(1)	(2)	(3)	(4)	(5)	(6)
	Coefficient	Coefficient	Coefficient	MEM	MEM	MEM
α_1: Informed amount peers take away (x^{PC})	0.0020*	0.0022**	0.0023*	0.0010***	0.0012***	0.0012***
	(0.001)	(0.001)	(0.001)	(0.000)	(0.000)	(0.000)
α_2: Informed amount peers take away (x^{PT})	−0.0001	−0.0001	−0.0008	−0.0005	−0.0005	−0.0008
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
α_3 : Treatment group dummy	0.8904**	1.0122***	0.9797**	0.3639**	0.4517***	0.4110***
	(0.418)	(0.379)	(0.408)	(0.152)	(0.147)	(0.117)
The amount to take away in the first session			0.0103***			0.0045***
			(0.002)			(0.001)
1 if taking away more than BDT 200 in the first session			−1.5019***			−0.6422***
			(0.386)			(0.143)
Observations	208	208	208	208	208	208
Village fixed effects	No	Yes	Yes	No	Yes	Yes

Notes. 'MEM' stands for the marginal effect at the mean. The clustered robust standard errors are in parentheses.

***, **, and * indicate significance at 1%, 5%, and 10%, respectively.

Table 2

Switching Point and Inferred Guilt Sensitivity (N = 279)

Range of belief to choose		Range of guilt sensitivity	Guilt sensitivity in this study (g)	Proportion (%)
Roll the die	*Don't roll*			
None	0~18 (0~100%)	$0 < g < 0.4$	0.2	8.2
18 (100%)	0~15 (0~83%)	$0.4 < g < 0.48$	0.44	2.5
15~18 (83~100%)	0~12 (0~67%)	$0.48 < g < 0.6$	0.54	5.0
12~18 (67~100%)	0~9 (0~50%)	$0.6 < g < 0.8$	0.7	21.9
9~18 (50~100%)	0~6 (0~33%)	$0.8 < g < 1.2$	1.0	22.6
6~18 (33~100%)	0~3 (0~17%)	$1.2 < g < 2.4$	1.8	3.6
3~18 (17~100%)	0 (0%)	$2.4 < g$	2.4	5.7
0~18 (0~100%)	None	Guilt aversion with altruism and/or trustworthiness	2.4	26.5
Others[#]		Inconsistent with guilt aversion, altruism, and trustworthiness	Missing	3.9

Notes. # indicates that these subjects switched their answers to the opposite or switched multiple times. These observations are not used in the analysis.

Table 3

Correlations among Preference Variables (N = 208)

	Guilt sensitivity	Altruism
Altruism	0.202	
	(0.004)	
Trustworthiness	0.510	0.121
	(0.000)	(0.082)

Notes. The correlation coefficients are reported. The p-values are in parentheses.

Table 4

Testing Guilt Aversion (Testable Hypothesis 3 and Equation 3)

Dependent Variable	x				1 if $x > 200$			
Methodology	Ordered Probit				Probit			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Coefficient	Coefficient	Coefficient	Coefficient	MEM	MEM	MEM	MEM
Informed first-order belief (τ^A, τ^B)	0.0026***	0.0027***	0.0025***	0.0025***	0.0010***	0.0011***	0.0014***	0.0014***
	(0.0007)	(0.0007)	(0.0005)	(0.0005)	(0.0004)	(0.0004)	(0.0004)	(0.0004)
Informed amount peers take away (x^{PT})	−0.0002	0.0002	-0.0005	-0.0006	−0.0005	−0.0004	-0.0007	-0.0007
	(0.0009)	(0.0009)	(0.0015)	(0.0014)	(0.0005)	(0.0006)	(0.0009)	(0.0009)
Guilt sensitivity (g)		−0.2906***	-0.2964***	-0.2469**		−0.1123**	-0.0885*	-0.0867*
		(0.1095)	(0.0896)	(0.1062)		(0.0567)	(0.0510)	(0.0445)
Altruism (*Pref*)				-0.0036				-0.0025
				(0.0048)				(0.0022)
1 if trustworthy (*Pref*)				-0.1300				0.0005
				(0.3743)				(0.1949)
Large assets			0.4463***	0.4193***			0.5300***	0.4850***
			(0.1129)	(0.1082)			(0.1943)	(0.1610)
Small assets			-2.3477*	-1.9560			-3.1049***	-2.9700***
			(1.2187)	(1.5199)			(1.0537)	(1.0626)
Age of head			-7.4209**	-7.0995*			-5.9116	-5.8237
			(3.7115)	(3.7496)			(4.1469)	(4.1882)
Squared age of head			7.1255*	6.7245*			6.0407	6.0534
			(3.7871)	(3.9023)			(4.5927)	(4.6653)
Schooling years of head			-0.0380	-0.0460			0.0001	-0.0049
			(0.0523)	(0.0532)			(0.0260)	(0.0269)
1 if head is married			-0.5701**	-0.5618**			-0.1683	-0.1551
			(0.2403)	(0.2348)			(0.1958)	(0.1930)
Household size			-0.0422	-0.0344			-0.0139	-0.0117

			(0.0933)	(0.0909)			(0.0417)	(0.0364)
Proportion of males over 15			0.1682	0.1255			0.0365	-0.0044
in the household			(0.5580)	(0.5455)			(0.2109)	(0.2267)
1 if Muslim			-0.1641	-0.1510			0.0615	0.0682
			(0.3189)	(0.3201)			(0.1903)	(0.1931)
Duration of inundation			0.1692	0.1527			0.2151**	0.2102**
at working place			(0.1987)	(0.1972)			(0.1043)	(0.1039)
Height of inundation			-0.1066	-0.1010			-0.0991**	-0.0999**
at working place			(0.0940)	(0.0958)			(0.0417)	(0.0417)
Distance to market			0.0062	-0.0039			-0.0137	-0.0265
			(0.1405)	(0.1496)			(0.1070)	(0.1094)
1 if subject is household			-0.3463	-0.3379			-0.1866	-0.1685
head			(0.2843)	(0.2936)			(0.1294)	(0.1322)
1 if subject is male			-0.2643	-0.2614			-0.2007	-0.2064
			(0.2528)	(0.2741)			(0.1337)	(0.1378)
Age of subject			0.0224	0.0228			0.0129**	0.0126**
			(0.0182)	(0.0167)			(0.0059)	(0.0050)
Schooling years of subject			0.0313	0.0360			0.0222*	0.0242*
			(0.0231)	(0.0235)			(0.0123)	(0.0133)
Observations	137	137	137	137	137	137	137	137
Village fixed effects	No	No	Yes	Yes	No	No	Yes	Yes

Notes. The subsample of the treatment group is used. The coefficients are reported. 'MEM' stands for the marginal effect at the mean. The clustered robust standard errors are in parentheses. ***, **, and * indicate significance at 1%, 5%, and 10%, respectively.

Table 5

Guilt Sensitivity and Victimisation of Property Crime in the Real World

	(1)	(2)	(3)	(4)	(5)	(6)
Mean guilt sensitivity ($\bar{g}$)	-0.292	-0.292*	-0.343*	-0.401**	-0.638**	-0.829***
	(0.182)	(0.175)	(0.183)	(0.178)	(0.293)	(0.252)
Mean altruism ($\overline{Pref}$)	0.004	0.003	0.004	0.004	0.004	0.009
	(0.005)	(0.004)	(0.005)	(0.004)	(0.006)	(0.006)
Mean trustworthiness ($\overline{Pref}$)	0.027	0.005	0.191	0.207	0.606	0.571
	(0.340)	(0.356)	(0.292)	(0.298)	(0.381)	(0.455)
Guilt sensitivity (g)	0.050	0.053	0.041	0.040	0.072	0.077
	(0.042)	(0.040)	(0.046)	(0.045)	(0.073)	(0.070)
Altruism (*Pref*)	0.001	0.001	0.001	0.002	0.003***	0.000
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)
1 if trustworthy (*Pref*)	0.037	0.041	0.065	0.074	0.057	-0.024
	(0.067)	(0.065)	(0.068)	(0.064)	(0.115)	(0.127)
Large assets			-0.085	-0.087	-0.044	-0.152
			(0.111)	(0.114)	(0.116)	(0.258)
Small assets			0.016	-0.001	-0.524	0.781
			(0.716)	(0.691)	(0.886)	(2.665)
Age of head			-0.003	-0.111	-2.688	0.990
			(1.089)	(1.140)	(2.758)	(3.763)
Squared age of head			0.053	0.087	1.768	-0.803
			(0.975)	(1.103)	(2.615)	(4.101)
Schooling years of head			0.022**	0.030**	0.021	0.046*
			(0.011)	(0.013)	(0.023)	(0.025)
1 if head is married			-0.003	-0.011	0.412*	-0.311
			(0.121)	(0.119)	(0.219)	(0.217)
Household size			-0.036	-0.037	-0.033	-0.138***
			(0.027)	(0.026)	(0.038)	(0.053)
Proportion of males over 15 in the household			-0.084	-0.149	0.057	0.754
			(0.234)	(0.248)	(0.356)	(1.208)
1 if Muslim			-0.013	-0.001	-0.036	0.288**
			(0.058)	(0.060)	(0.092)	(0.140)
Duration of inundation at working place			-0.025	-0.026	0.027	-0.108
			(0.037)	(0.038)	(0.049)	(0.111)
Height of inundation at working place			0.062**	0.070***	0.049	0.145*
			(0.025)	(0.025)	(0.039)	(0.075)
Distance to market			-0.042*	-0.053*	-0.097***	-0.044
			(0.024)	(0.027)	(0.034)	(0.059)
1 if subject is household head		-0.077		-0.191**	0.029	-0.072
		(0.067)		(0.082)	(0.094)	(0.104)
1 if subject is male		-0.039		0.053	-0.302***	-0.494***
		(0.085)		(0.078)	(0.112)	(0.188)
Age of subject		0.002		0.003	0.118	0.344
		(0.003)		(0.003)	(0.132)	(0.227)
Schooling years of subject		0.006		-0.012	0.005	0.012
		(0.009)		(0.012)	(0.005)	(0.010)
Observations	268	268	268	268	139	107
Fixed Effects	Upazila	Upazila	Upazila	Upazila	Upazila	Upazila

Notes. The marginal effects at the means are reported. The clustered robust standard errors are in parentheses. ***, **, and * indicate significance at 1%, 5%, and 10%, respectively.

Table A1

Summary Statistics of Survey Data

Variable	Mean	S.D.
1 if victimised by property crime after cyclone	0.46	0.50
1 if victimised by violent crime after cyclone	0.25	0.43
Large assets (BDT 10^6)	0.17	0.40
Small assets (BDT 10^6)	0.03	0.06
Age of head (10^2 years)	0.45	0.13
Schooling years of head	4.77	3.97
1 if head is married	0.89	0.31
Household size	4.20	1.64
Proportion of males over 15 in the household	0.38	0.18
1 if Muslim	0.46	0.50
Duration of inundation at working place (months)	0.97	1.31
Height of inundation at working place (feet)	2.03	1.96
Distance to market (km)	1.61	1.27
Observations	208	

Note. 'S.D.' stands for Standard Deviation.

Table A2

Summary Statistics of Experiment Data and Subject Characteristics

Variable	Observations	Mean	S.D.
Experiment data used in this study			
The amount to take away in session 1	208	255.53	131.07
The amount to take away in session 2 (x)			
Control group	71	180.28	133.22
Treatment group	137	229.56	90.66
Informed amount peers take away (x^P)			
Control group (x^{PC})	71	201.41	81.93
Treatment group (x^{PT})	137	197.08	82.20
Informed first-order belief (τ^A)	137	253.65	112.89
Guilt sensitivity (g)	207	1.29	0.80
Altruism (*Pref*)	208	0.42	0.29
Trustworthiness (*Pref*)	208	0.61	0.49
Subject characteristics			
1 if subject is household head	208	0.51	0.50
1 if subject is male	208	0.68	0.47
Age of subject	208	35.44	13.89
Schooling years of subject	208	6.04	4.01

Table C1

Testing Guilt Aversion (Testable Hypothesis 3 and Equation 3)-Quadratic Moral Cost

Dependent Variable	x			1 if $x > 200$		
Methodology	Ordered Probit			Probit		
	(1)	(2)	(3)	(4)	(5)	(6)
	Coefficient	Coefficient	Coefficient	MEM	MEM	MEM
Informed first-order belief (τ^A, τ^B)	0.0027***	0.0025***	0.0025***	0.0011***	0.0014***	0.0014***
	(0.0007)	(0.0005)	(0.0005)	(0.0004)	(0.0004)	(0.0004)
Informed amount peers take away (x^{PT})	0.0002	-0.0005	-0.0006	−0.0004	-0.0007	-0.0007
	(0.0009)	(0.0015)	(0.0014)	(0.0006)	(0.0009)	(0.0009)
Guilt sensitivity under quadratic moral cost (g)	−8.1363***	-8.5322***	-7.1170***	−3.1114**	-2.4432**	-2.2141*
	(2.8371)	(1.9953)	(2.5114)	(1.4689)	(1.2190)	(1.3405)
Altruism (*Pref*)			-0.0035			-0.0026
			(0.0048)			(0.0022)
1 if trustworthy (*Pref*)			-0.1498			-0.0169
			(0.3605)			(0.1929)
Large assets		0.4547***	0.4262***		0.5410***	0.4889***
		(0.1138)	(0.1081)		(0.2007)	(0.1688)
Small assets		-2.4003**	-1.9920		-3.1015***	-2.9528***
		(1.1774)	(1.5010)		(1.0452)	(1.0771)
Age of head		-7.3178*	-7.0357*		-5.8521	-5.8062
		(3.7347)	(3.7649)		(4.1684)	(4.1797)
Squared age of head		7.0697*	6.6793*		6.0023	6.0429
		(3.8207)	(3.9455)		(4.6143)	(4.6643)
Schooling years of head		-0.0396	-0.0477		-0.0005	-0.0059
		(0.0519)	(0.0534)		(0.0257)	(0.0271)
1 if head is married		-0.5489**	-0.5455**		-0.1616	-0.1472
		(0.2423)	(0.2381)		(0.1971)	(0.1950)
Household size		-0.0477	-0.0377		-0.0155	-0.0122
		(0.0940)	(0.0904)		(0.0431)	(0.0363)
Proportion of males over 15 in the household		0.1488	0.1181		0.0290	-0.0042
		(0.5525)	(0.5325)		(0.2183)	(0.2257)
1 if Muslim		-0.1670	-0.1569		0.0641	0.0693
		(0.3230)	(0.3230)		(0.1911)	(0.1920)
Duration of inundation at working place		0.1714	0.1538		0.2151**	0.2096**
		(0.1956)	(0.1956)		(0.1032)	(0.1038)
Height of inundation at working place		-0.1092	-0.1029		-0.0992**	-0.1000**
		(0.0925)	(0.0941)		(0.0423)	(0.0423)
1 if subject is household head		-0.0025	-0.0090		-0.0179	-0.0293
		(0.1400)	(0.1501)		(0.1067)	(0.1097)
1 if subject is male		-0.3379	-0.3307		-0.1811	-0.1639
		(0.2895)	(0.2974)		(0.1309)	(0.1323)
Age of subject		-0.2627	-0.2632		-0.1998	-0.2067
		(0.2460)	(0.2685)		(0.1335)	(0.1361)
Schooling years of subject		0.0223	0.0227		0.0127**	0.0125**
		(0.0185)	(0.0170)		(0.0059)	(0.0050)
Distance to market		0.0319	0.0370		0.0222*	0.0246*
		(0.0237)	(0.0241)		(0.0122)	(0.0134)
Observations	137	137	137	137	137	137
Village fixed effects	No	Yes	Yes	No	Yes	Yes

Notes. The subsample of the treatment group is used. The coefficients are reported. The clustered robust standard errors are in parentheses. ***, **, and * indicate significance at 1%, 5%, and 10%, respectively.

Table C2

Testing Guilt Aversion (Testable Hypothesis 3 and Equation 3)-Binary Guilt Sensitivity

Dependent Variable	x			1 if $x > 200$		
Methodology	Ordered Probit			Probit		
	(1)	(2)	(3)	(4)	(5)	(6)
	Coefficient	Coefficient	Coefficient	MEM	MEM	MEM
Informed first order belief (τ^A, τ^B)	0.0027***	0.0025***	0.0025***	0.0011***	0.0014***	0.0014***
	(0.0007)	(0.0005)	(0.0005)	(0.0004)	(0.0004)	(0.0004)
Informed amount peers take away (x^{PT})	0.0002	-0.0006	-0.0006	−0.0004	-0.0007	-0.0007
	(0.0009)	(0.0015)	(0.0014)	(0.0006)	(0.0009)	(0.0009)
Binary guilt sensitivity (g)	−0.4296**	-0.5136***	-0.4190***	−0.1547*	-0.1498*	-0.1363
	(0.1781)	(0.1251)	(0.1427)	(0.0918)	(0.0766)	(0.0848)
Altruism (*Pref*)			-0.0037			-0.0027
			(0.0049)			(0.0023)
1 if trustworthy (*Pref*)			-0.1809			-0.0245
			(0.3455)			(0.1861)
Large assets		0.4872***	0.4508***		0.5413***	0.4886***
		(0.1152)	(0.1136)		(0.1937)	(0.1561)
Small assets		-2.3357**	-1.9045		-3.0841***	-2.9329***
		(1.1677)	(1.5076)		(1.0351)	(1.0783)
Age of head		-7.1268**	-6.8888*		-5.8378	-5.8066
		(3.6315)	(3.6540)		(4.1731)	(4.1817)
Squared age of head		6.9036*	6.5231*		6.0092	6.0573
		(3.7527)	(3.8713)		(4.6317)	(4.6851)
Schooling years of head		-0.0396	-0.0484		-0.0006	-0.0063
		(0.0516)	(0.0534)		(0.0255)	(0.0274)
1 if head is married		-0.5410**	-0.5400**		-0.1562	-0.1416
		(0.2494)	(0.2436)		(0.1983)	(0.1965)
Household size		-0.0543	-0.0411		-0.0172	-0.0129
		(0.0963)	(0.0914)		(0.0446)	(0.0366)
Proportion of males over 15 in the household		0.0904	0.0777		0.0131	-0.0139
		(0.5398)	(0.5067)		(0.2229)	(0.2236)
1 if Muslim		-0.1475	-0.1434		0.0707	0.0749
		(0.3325)	(0.3308)		(0.1903)	(0.1918)
Duration of inundation at working place		0.1854	0.1633		0.2193**	0.2132**
		(0.1940)	(0.1945)		(0.1035)	(0.1038)
Height of inundation at working place		-0.1111	-0.1036		-0.0996**	-0.1006**
		(0.0919)	(0.0936)		(0.0422)	(0.0422)
1 if subject is household head		-0.0113	-0.0143		-0.0217	-0.0323
		(0.1350)	(0.1472)		(0.1072)	(0.1097)
1 if subject is male		-0.3403	-0.3324		-0.1811	-0.1635
		(0.2968)	(0.3037)		(0.1333)	(0.1351)
Age of subject		-0.2757	-0.2771		-0.2045	-0.2122
		(0.2454)	(0.2655)		(0.1301)	(0.1306)
Schooling years of subject		0.0232	0.0237		0.0130**	0.0128**
		(0.0187)	(0.0173)		(0.0060)	(0.0053)
Distance to market		0.0340	0.0394		0.0226*	0.0253*
		(0.0243)	(0.0245)		(0.0118)	(0.0135)
Observations	137	137	137	137	137	137
Village fixed effects	No	Yes	Yes	No	Yes	Yes

Notes. The subsample of the treatment group is used. The coefficients are reported. The clustered robust standard errors are in parentheses. ***, **, and * indicate significance at 1%, 5%, and 10%, respectively.