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Exposure to intimate partner violence and repayment of microcredit: Evidence from field experiments in Bangladesh

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ABSTRACT

This paper investigates the impact of intimate partner violence on poor women's repayment behavior in microcredit. In a laboratory-based field experiment, we extended collateral-free small loans to 485 currently married women in rural Bangladesh and observed their repayment decisions over multiple loan cycles. In a post-experiment survey, we asked subjects about their experience of spousal violence. We find that women who experienced physical or sexual violence in the last 12 months are more likely to strategically default on their loans relative to those who did not experience such violence. We conducted several robustness tests, and the results suggest that the negative correlation between victimization and loan repayment rates is unlikely to be explained by selection into victimization, non-random underreporting of violence, or the subjects' attitude toward risk.

1. Introduction

Gender-based violence is a major public health problem that affects one-third of women globally (World Health Organization, 2013). A growing literature reports significant health effects and economic costs associated with intimate partner violence – the most common form of gendered violence that describes physical or sexual assault of a spouse or a sexual intimate (Adler and Johnson, 2015; Garcia-Moreno et al., 2005; United Nations, 2015). In this study, we investigate the impact of such violence on survivors' loan repayment behavior in microcredit.[1] Prior research has shown that participation in microcredit programs has mixed effects on the risk of victimization (Chin, 2012; Kim et al., 2007; Schuler et al., 1998; Pronyk et al., 2006). What remains unknown is whether borrowers' exposure to violence may affect the credit risk of microfinance institutions (MFIs).

Women in violent relationships endure physical pain, continuing mental torture, and humiliation. As a result, they experience aversive emotions, such as grief, anxiety, fear, anger, embarrassment, avoidance, and distraction (Campbell, 2002; Coker et al., 2002; Kumar et al., 2005; Mullen et al., 1988). When the intensity of an aversive emotion escalates, people pay increasing attention to activities and outcomes that can immediately alleviate its intensity (Pessoa, 2013). Such attention-narrowing may have two effects on human behavior (Loewenstein, 1996, 2000). First, overweighing present desires may result in short-sighted trade-offs between immediate and delayed rewards. Second, negative emotions may turn one's focus inward, resulting in selfishness. We argue that the interrelations among victimization, negative affective conditions, and individual preferences have important implications in the context of microloan repayment.

MFIs use behavior-inducing mechanisms to enhance repayment discipline among borrowers (Banerjee, 2013). They offer repeat loans upon satisfactory repayment and cutoff access to future borrowing when loans are not repaid. Such *dynamic incentives* improve repayment rates by ameliorating moral hazard (Giné et al., 2010; Karlan et al., 2009). Due to the attention-narrowing effect, however, battered women may prefer the immediately available benefit of non-repayment to the delayed reward of a repeat loan. Another widely used mechanism in microlending is *joint liability*. In the classic group-lending model of the Grameen Bank, women borrowers form small groups and remain collectively responsible for repayment. If a borrower defaults on such a loan, the other group members repay on behalf of their defaulting peer. In this setting – in which borrowers have incentives to free ride – other-regarding

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[1] Microcredit refers to the provision of collateral-free small loans for the purpose of creation and expansion of small business (see, for a discussion, Honohan, 2008; Islam et al., 2015; Mersland and Strøm, 2009; Servin et al., 2012; Strøm et al., 2014). As of December 2012, 203 million borrowers have received microloans from 3700 microfinance institutions (Reed et al., 2014). More than three-quarters of these borrowers are women.

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(or social) preferences influence loan repayment decisions (Abbink et al., 2006; Anthony and Horne, 2003). If victimization causes selfishness, battered women may show a lower willingness to contribute to group repayment and rely on their borrowing partners for the overall success of the group. Thus, borrowers' exposure to intimate partner violence may worsen repayment rates by reducing the effectiveness of dynamic incentives and joint liability.

We conducted a laboratory experiment in the field in rural Bangladesh, where both the severity of domestic violence and the coverage of microfinance are high (Bajracharya and Amin, 2013; Bangladesh Demographic Health Survey, 2011).[2] In a modified microloan repayment game (Abbink et al., 2006), we extended collateral-free small loans to 485 currently married women. Each subject invested her loan in a risky project. When positive project returns were realized, she could either repay the loan with interest or strategically default. A subject participated in one of four experimental treatments. There were two treatments of individual loans and two treatments of joint liability-based loans. Within each liability structure, one treatment incorporated dynamic incentives and the other did not. In a post-experiment survey, we asked subjects about their experience of intimate partner violence.

Using ordinary least squares (OLS), we regress loan repayment rates on exposure to intimate partner violence. We use a selection on observables strategy (Cassar et al., 2013) and test whether our results are sensitive to the inclusion of a large number of control variables, such as the determinants of victimization, factors that cannot be affected by victimization, and factors that can affect both victimization and repayment decisions. We find that dynamic incentives are significantly less effective for women who experienced spousal violence in the last 12 months. In treatments with dynamic incentives, battered women are more likely to strategically default on both individual and joint liability loans relative to those who had not experienced violence in the recent past. Dynamic incentives are almost universally employed by MFIs irrespective of the liability structure of a loan (Banerjee, 2013; Tedeschi, 2006). Our findings, therefore, imply that the widespread prevalence of domestic violence among poor women – who constitute the majority of microloan recipients – is a significant threat for the sustainability of MFIs.

There are, however, three potential caveats regarding the validity of our results. First, pre-existing differences between battered and non-battered women may explain differences in their repayment rates. Second, our results may suffer from reporting bias because self-reported measures of domestic violence are prone to non-random underreporting (Aizer, 2010; Ellsberg et al., 2008). Third, subjects' attitude toward risk may explain their decisions in a loan repayment game (Chakravarty et al., 2015). In a series of robustness tests, we confirm that the negative effect of victimization on loan repayment rates is unlikely to be explained by selection into victimization, underreporting of violence, or the subjects' attitude toward risk.

The paper is organized as follows. We discuss the experimental design in Section 2. The empirical strategy is discussed in Section 3. We discuss our main findings in Section 4, and the results of the robustness tests appear in Section 5. Section 6 summarizes and concludes.

2. Experimental design and survey

2.1. Subject recruitment

A multi-stage sampling method was used to recruit subjects. Using simple lotteries, we chose seven districts from the seven administrative divisions of Bangladesh and then selected three villages from each of these seven districts. For each village, we prepared a list of households that owned at most half an acre of arable land.[3] Using a simple lottery, we then selected 25 households from each village. We visited those households and invited a currently married woman of age 18–44 from each household to participate in our experiments.[4] We explicitly excluded current microcredit borrowers because their decision within the experiments might have been influenced by their actual experience with such loans, introducing confounding effects (Chakravarty and Shahriar, 2015). A random selection key (a 12-face die) was used so that every woman fulfilling these selection criteria within a household had an equal probability of being chosen. A total of 485 women accepted the invitation and appeared at the experimental site at a pre-specified time. Descriptive statistics of the subjects are presented in Table 1. Each subject received TK 100 for participation and earned additional monies depending on the choices she made during the course of the experiments.[5]

2.2. Experimental procedure

Upon arrival at the experimental site, subjects were directed to a large room, where we provided experimental instructions in Bangla, which is spoken and understood by all subjects (the English version of the instructions is provided in Appendix A). The loan repayment game consisted of multiple rounds. At the beginning of each round, a subject received a loan of TK 50. We informed the subjects that the loan had to be repaid – with 20% interest – in one installment. After the disbursement of loan, we took each subject individually to a private room, where she invested her loan in a fictitious risky project and, depending on the status of her project (success or failure), made loan repayment decisions. Thus, a subject's experimental decisions were not influenced by that of the other subjects in the same session.

Each subject played a simple ball-drawing game to determine the status of her project. She drew a ball from a non-transparent jar containing one red and five green balls. A green ball implied project success, whereas the red ball implied project failure (details of the ball-drawing game are provided in Appendix A). If the project failed, the subject received zero (i.e., she lost her investment of TK 50), and she could not repay her loan. On the other hand, if the project succeeded, she received a project return of

[2] A truly randomized experiment is unthinkable when investigating the impact of violence. The use of survey data is also problematic because it is difficult to identify the direction of causality. Anecdotal evidence suggests that in credit-constrained rural Bangladesh, men are contented to see their wives receiving microloans and investing in family business (Goetz and Gupta, 1996; Kabeer, 2001; Schuler at al., 1998). However, when women borrowers fail to repay these loans, they rarely receive any financial support from their husband. Rather, they sell off valuable assets, draw on savings, or cut back consumption to repay loans in arrears. All these factors lead to an intensification of tensions within the household, often spilling over into violence. Thus, default and delinquencies with actual microloans may increase the likelihood of victimization.

[3] This eligibility criterion of land-holding is maintained by many major Bangladeshi MFIs, such as the Grameen Bank, Association for Social Advancement (ASA), and Bangladesh Rural Advancement Committee (BRAC) (Hossain, 1988; Sharma and Zeller, 1997). Members of the local Union Council (the lowest form of local government in Bangladesh) helped us prepare this list.

[4] Many standardized surveys on domestic violence have been conducted with women of reproductive age. Examples include the WHO Multi-Country Study on Women's Health and Violence against Women and the Bangladesh Demographic Health Survey (2011). Women of reproductive age refer to all women aged 15–44 or 15–49 years (World Health Organization, 2006). We recruited adult women of reproductive age.

[5] Taka (TK) is the official currency of Bangladesh. When the experiments were conducted in 2014, one US dollar was equivalent to TK 78, and the daily per capita income in Bangladesh was TK 232 (http://data.worldbank.org/indicator/NY.GDP.PCAP.CD).

Table 1
Summary statistics.

Variables	Description	Mean (std. dev.)
Subject's age	Age of a subject (Min: 18, Max: 42)	28.87 (7.14)
Husband's age	Age of a subject's husband (Min: 22, Max: 50)	34.71 (7.44)
Subject's education	Years of school attended by a subject (Min: 0, Max: 13)	3.42 (3.44)
Husband's education	Years of school attended by a subject's husband (Min: 0, Max: 16)	9.87 (3.85)
Household assets	Natural logarithm of the value of a subject's household assets such as arable land, dwelling house, cattle, and other valuables.	10.76 (5.24)
Household size	The number of members in a subject's household (Min: 3, Max: 8)	4.49 (1.02)
Earning member	1 if subject is involved in income-generating activities; 0 otherwise.	0.28 (0.45)
No male child	1 if subject has no living male child; 0 otherwise.	0.35 (0.48)
Muslim	1 if subject is Muslim; 0 otherwise	0.87 (0.34)
Unpaid dowry	1 if a dowry was demanded on subject's marriage but it was either not paid at all or paid only partially; and zero if a subject's marriage did not involve any dowry, or her husbands' dowry demand was met in full.	0.27 (0.44)
Intimate partner violence (IPV)	1 if a subject reported physical or sexual violence by her husband in the last 12 months (i.e., if she answered "Yes" to any of the nine violence related questions); 0 otherwise.	0.28 (0.45)
Ever violence indicator	1 if a subject had ever experienced physical or sexual violence by her husband; 0 otherwise.	0.43 (0.49)
Self confidence	1 if a subject answered "Very confident" to the following question; 0 otherwise. "If you were at a community meeting, how confident are you that you could raise your opinion in public?" (a) Very confident; (b) somewhat confident; (c) not confident at all.	0.25 (0.43)
Financial confidence	1 if a subject answered "Very confident" to the following question; 0 otherwise. "In the event of a crisis (e.g., house burn), how confident are you that you alone could raise enough money to feed your family for 4 weeks?" (a) Very confident; (b) somewhat confident; (c) not confident at all.	0.08 (0.27)
Autonomy	1 if a subject answered "No" to at least three of these cases; 0 otherwise. "Do you need your husband's permission" (a) To make small household purchases? (b) To visit a doctor? (c) To take children to the doctor? (d) To visit family or relatives? (e) To cook food for the family?	0.25 (0.43)
Challengers	1 if a subject disagrees with all these three statements; 0 otherwise. (a) "A good wife always obeys her husband even if she disagrees". (b) "A woman should do most of the household chores even if the husband is not working". (c) "It is a wife's obligation to have sex with her husband even if she does not feel like it".	0.13 (0.34)
Trust	1 if a subject answered either (a) or (b) in response to the following question; 0 otherwise: "Generally speaking, would you say that most people can be trusted or that you can't be too careful in dealing with people?" (a) People can almost always be trusted (b) People can usually be trusted (c) You usually cannot be too careful in dealing with people (d) You almost always cannot be too careful in dealing with people	0.84 (0.37)
Fairness	1 if a subject answered either (c) or (d) in response to the following question; 0 otherwise: "Do you think most people would try to take advantage of you if they got a chance, or would they try to be fair?" (a) Try to take advantage almost all of the time (b) Try to take advantage most of the time (c) Try to be fair most of the time (d) Try to be fair almost all of the time	0.74 (0.44)
Cooperation	1 if a subject answered (a) in response to the following question; 0 otherwise: "Would you say that most of the time people try to be helpful, or that they are mostly just looking out for themselves?" (a) Try to be helpful (b) Just look out for themselves	0.83 (0.37)
Non-violent shocks	1 if a subject answered "yes" to any of the two questions; 0 otherwise: (a) In the last 12 months, did you evacuate your home due to flood or river erosion? (b) In the last 12 months, did an earning member of your household die?	0.21 (0.41)
Gender difference in school attendance (Source: Bangladesh Bureau of Statistics, 2015. Figures reported in percent.)	School attendance rate is measured as the number of individuals of age 5–24 years who ever attended school divided by the number of individuals in this age group. In each survey district, school attendance rates are separately measured for males and females. The latter is then subtracted from the former.	3.64 (2.14)

TK 120.[6] A subject with a successful project had two choices: she could either repay or she could strategically default. The excess monies – left after repaying (or not repaying) the loan – were converted to real currency at the end of the experiments. We did not allow subjects to repay loans using project returns from previous rounds (if any). The liability structure of the loan and the outcome of non-repayment varied across the four treatments discussed below. A subject participated at most in one treatment. We refer the reader to the experimental instructions in Appendix A to gain a

[6] Our choice of the likelihood of project success and project returns is consistent with that of Cassar et al. (2007), Cassar and Wydick (2010), and Chakravarty et al. (2015). These researchers conducted similar microloan repayment games in the field in Armenia, Bangladesh, Guatemala, India, Kenya, South Africa, and the Philippines.

deeper understanding of how we operationalized the experimental treatments.

2.2.1. The benchmark treatment: individual loans without dynamic incentives

A total of 123 subjects participated in this treatment. The number of participants per session varied between four and eight. A subject was individually responsible for repaying her loan. If her project failed, she could not repay. In contrast, a subject with a successful project either repaid TK 60 (TK 50 of principal plus TK 10 of interest payment) or strategically defaulted. After a subject had revealed her repayment decision, the game proceeded for her to the next round. A subject always received a new loan of TK 50 at the beginning of the new round, even if she was unable or unwilling to repay in the previous round. In this task, therefore, strategic default was compatible with selfish own-income maximization, and any deviation from this can be considered as a subject's *willingness* to repay microloans in the absence of any behavior-inducing mechanism.

In each new round, the same procedure was repeated. After the sixth round of play, however, we continued the game only with a probability of 1/6 (we used a simple lottery to determine whether to continue). This was necessary because if subjects know the end point of the game, their repayment performance may deteriorate toward the end of the experiments due to the well-known unraveling problem (Cassar et al., 2007). To avoid such concerns, we conducted this treatment – and all the other treatments discussed below – without any pre-determined end point.[7]

2.2.2. Individual loans with dynamic incentives

Except for the outcome of default, the procedure of this treatment was the same as that of the benchmark treatment discussed above in every respect. If a subject defaulted on her loan in this treatment – either strategically or due to project failure – we did not provide her with a repeat loan. Thus, the game ended for her upon default. By contrast, if a subject repaid her loan, the game proceeded to the next round, she received a new loan of TK 50, and the whole process was repeated. A total of 112 subjects participated in this treatment with four to eight subjects per session.

2.2.3. Joint liability-based loans with dynamic incentives

A total of 135 subjects participated in 27 sessions. In every session, there were five subjects, who were considered members of a joint liability-based borrowing group.[8] We informed subjects that loan repayment would be a joint responsibility. The group as a whole was responsible for repaying TK 300 – the total amount disbursed to the group plus interest payment at a rate of 20%. A subject whose project failed could not contribute to group repayment because she did not have enough money. A subject with a successful project could either strategically default or contribute to group repayment. However, the exact amount of individual contribution and the subsequent payout from this treatment depended on the number of contributing members (i.e., those who were both able and willing to repay). For instance, with all five members contributing to the repayment pot, each member contributed TK 60 to pay off the loan and kept TK 60. If only four members repaid, each repaying member contributed TK 75 while keeping TK 45. Thus, if the number of contributors was small, the contribution of the repaying members was large. Because TK 300 was required to pay off the group loan, at least three members had to contribute. If at least three members contributed, the group-repayment obligation was fulfilled, and the game proceeded for the entire group to the next round. At the beginning of the new round, each member of the group received a new loan of TK 50 and the same procedure was repeated. If fewer than three members contributed, the repayment obligation was not fulfilled; no one from the group received any new loan and the game ended.

Table 2
Payoff allocation.

Outcome of the project	Repayment decision of the subject	Subject's net payout from a given round of play	Proceed to the next round?
Individual loans without dynamic incentives			
Failure	Default	0	Yes
Success	Default	120	Yes
Success	Repay	60	Yes
Individual loans with dynamic incentives			
Failure	Default	0	No
Success	Default	120	No
Success	Repay	60	Yes
Joint liability-based loans with dynamic incentives			
Failure	Default	0	
If at least three partners repay			Yes
If fewer than three partners repay			No
Success	Default	120	
If at least three partners repay			Yes
If fewer than three partners repay			No
Success	Repay		
If four partners repay		60	Yes
If three partners repay		45	Yes
If two partners repay		20	Yes
If fewer than two partners repay		0	No
Joint liability-based loans without dynamic incentives			
Failure	Default	0	Yes
Success	Default	120	Yes
Success	Repay		
If four partners repay		60	Yes
If three partners repay		45	Yes
If two partners repay		20	Yes
If fewer than two partners repay		0	Yes

2.2.4. Joint liability-based loans without dynamic incentives

The experimental procedure of this treatment was the same as that of the third treatment, discussed above, except for the fact that access to repeat loans was not dependent on the repayment performance of the group. Thus, *every* member of a group received a repeat loan at the end of a loan cycle irrespective of her or her group members' repayment decisions. A total of 115 subjects participated in this treatment in 23 sessions. Five subjects participated in each session as group members. The structure of a subject's payouts in a given round of each treatment is summarized in Table 2.

2.3. Subjects' exposure to intimate partner violence: the post-experiment survey

In a post-experiment survey, we asked each subject about her experience of spousal violence in the recent past. We understood that a subject's willingness to disclose violence could be influenced by the characteristics of the interviewers, such as their sex, age, marital status, and interpersonal skills (Ellsberg and Heise, 2002; Ellsberg et al., 2001). Accordingly, we hired female interviewers to conduct this survey. All the interviewers were married, in their mid-forties, had completed secondary education, were unemployed at the time of the interview, and had survey experience.

[7] To minimize contamination from subjects taking into account an impending end-game, we utilized data from the first six rounds of every treatment in our analyses. Our main results remain unchanged after including the small amount of data from later rounds.

[8] This group size is consistent with that of the Grameen Bank's classic group loan scheme. Subjects had no role in determining who their group members would be. We avoided playing the loan repayment game with self-selected groups because of the perplexing effects of self-selection on repayment rates. Close social ties in self-selected groups may enhance repayment rates if subjects do not want to upset their partners by not repaying loans. Strong ties may also worsen repayment rates if the members show more forgiveness toward defaulters (see, for a discussion, Chakravarty et al., 2015).

The interviewers administered the survey according to the World Health Organization's ethical safety guidelines for research on violence against women.[9] For instance, efforts were made to ensure privacy during the interview. Each subject was individually interviewed in a private room after obtaining her informed consent, and we ensured that no interruptions were made by other subjects or experimenters during the interview. The survey questions were adopted from the Domestic Violence Module of the Demographic Health Survey (DHS).[10] This module has also been developed following the ethical guidelines of the World Health Organization (Kishor and Johnson, 2006). The questions are phrased in a supportive and non-judgmental manner and they avoid loaded terms such as abuse, rape, and violence. We closely followed the guidelines provided by Jansen et al. (2004) and trained the interviewers on how to address reactions to questions on domestic violence.

The primary violence outcome is measured based on the following nine questions. In the past 12 months, did your husband (a) Push you, shake you, or throw something at you? (b) Slap you or twist your arm? (c) Punch you with his fist or with something that could hurt you? (d) Kick you or drag you? (e) Try to strangle you or burn you? (f) Threaten you with a knife, gun, or other type of weapon? (g) Attack you with a knife, gun, or other type of weapon? (h) Physically force you to have sexual intercourse with him when you did not want to? (i) Force you to perform other sexual acts you did not want to? Bajracharya and Amin (2013), Chin (2012), Kim et al. (2007), and Pronyk et al. (2006) used similar survey questions to explore the incidence and severity of domestic violence in Bangladesh and South Africa.

We interpret a positive answer to any of these questions as an exposure to intimate partner violence. A total of 136 subjects (i.e., 28% of the sample) reported experiencing either physical, or sexual, violence by her husband within the last 12 months.[11] As a secondary measure of violence, we asked each subject if any of the nine types of violence had *ever* been committed by her husband during the marriage. We asked additional questions to gain insights into the factors that might be related to victimization or repayment behavior of the poor women. These factors are discussed in detail in Section 4.1 (see also Table 1).

3. Empirical strategy and identification

We estimate *loan repayment rate* as the number of times a subject repaid her loan divided by the number of times her project was successful. For example, suppose a subject played the game for six rounds. In four out of six rounds, her project was successful but she repaid her loans only twice. Her loan repayment rate is estimated to be 0.5.[12] This measure of repayment rate is consistent with that of Cassar et al. (2007) and Cassar and Wydick (2010), who used similar loan repayment games in their studies.

We regress loan repayment rate on exposure to intimate partner violence using the following equation.

$$\begin{aligned} Y_{ij} = {} & \propto_0 + \propto_1 IPV_{ij} + \propto_2 IL\_DRI + \propto_3 JL + \propto_4 JL\_DRI \\ & + \propto_5 IPV_{ij}{}^{*} IL\_DRI + \propto_6 IPV_{ij}{}^{*} JL + \propto_7 IPV_{ij}{}^{*} JL\_DRI \\ & + \beta_k \sum_k X_{ijk} + \theta C_j + \varepsilon_{ij} \end{aligned} \quad (1)$$

We use an OLS technique. Standard errors are corrected for heteroscedasticity. The outcome variable Y_{ij} measures the repayment rate of subject i in village j. On the right-hand side of the equation, we introduce a dummy variable for the exposure to intimate partner violence, IPV_{ij}, which equals one if subject i in village j experienced violence in the last 12 months and zero otherwise. There are three dummy variables for the four treatments; each treatment represents a specific type of microloan product as discussed in Section 2. The benchmark treatment of individual loans without dynamic incentives is the base category. *IL_DRI* is a dummy variable for the treatment of individual loans with dynamic repayment incentives; *JL* is a dummy variable for the treatment of joint liability loans without dynamic incentives; and *JL_DRI* is a dummy variable for the treatment of joint liability loans with dynamic repayment incentives. We use three treatment-*IPV* interaction terms, which allows us to compare the repayment rates of battered and non-battered women in each of the four treatments. The coefficient on IPV_{ij} (α_1) captures the difference in repayment rates of the victims and non-victims in the benchmark treatment. The differences in loan repayment rates of battered and non-battered women in the treatments of (a) individual loans with dynamic incentives, (b) joint liability loans without dynamic incentives, and (c) joint liability loans with dynamic incentives are measured, respectively, by the sum of the coefficients on (a) IPV_{ij} and $IPV_{ij}{}^{*}IL\_DRI$ ($\alpha_1+\alpha_5$), (b) IPV_{ij} and $IPV_{ij}{}^{*}JL$ ($\alpha_1+\alpha_6$), and (c) IPV_{ij} and $IPV_{ij}{}^{*}JL\_DRI$ ($\alpha_1+\alpha_7$). X_{ijk} is a set of individual, household, and community controls; C_j is a set of village fixed effects; and ε_{ij} is a zero-mean error term.

We run four different specifications of Eq. (1). Specification 1 (S1) is the parsimonious specification, in which we regress loan repayment rate on the *IPV* dummy, the three treatment dummies, and their interaction terms. Specification 2 (S2) augments S1 by controlling for factors that cannot be affected by spousal violence, such as the age of a subject, the age of her husband, the religious affiliation of a subject, and the gender of her children. Specification 3 (S3) augments S2 by controlling for the factors that may affect the risk of victimization (in Section 4.1, we run a separate regression to identify the determinants of victimization). Finally, in Specification 4 (S4), we control for all the observed factors that may be associated with either the risk of victimization or the repayment behavior of our subjects. All specifications, except for the parsimonious one (S1), include village fixed effects, which enables us to isolate the variation in violence experienced across neighbors within the same village.

4. Results

To identify the factors that may affect poor women's risk of victimization, we regressed *IPV* on individual, household, and community-specific variables using a simple logit model. In Section 4.1, we discuss the results of this logit regression. Our main experimental results – based on OLS estimation of Eq. (1) – are discussed in Section 4.2.

4.1. Determinants of the risk of victimization

Prior studies reveal higher incidence of violence among younger, poorer, and less educated couples in Bangladesh (Schuler et al., 2008). Consequently, we introduce four variables in the logistic regression: *Subject's age, Husband's age, Subject's education*, and *Husband's education*. Education is measured in terms of years of school attended. The socio-economic status of a subject and that of her household are captured by three variables. The dummy variable, *Earning member*, equals one if the subject is involved in income-generating activities outside the home and zero otherwise.

[9] http://www.who.int/gender-equity-rights/knowledge/who_fch_gwh_01.1/en/.

[10] http://dhsprogram.com/pubs/pdf/DHSQMP/domestic_violence_module.pdf.pdf.

[11] Different estimates suggest that 16–37% of the married women in Bangladesh experienced spousal violence within the past 12 months (Bates et al., 2004; Chin, 2012; Koenig et al., 2003; Naved and Persson, 2005; Schuler et al., 1996).

[12] A total of 14 subjects did not have any success with their project. We considered their loan repayment rates to be zero. Upon removal of these observations, however, our main results hold.

The value of a subject's *Household assets* is measured as the market value of arable land, dwelling house, cattle, and other valuables such as ornaments, furniture, personal vehicle, and other household goods. The variable *Household size* captures the number of members in a subject's household. We also include the religious affiliation of a subject. Because 87% of our subjects are Muslim, we use a *Muslim* dummy variable in the logit regression.

In the patriarchal societies of the Indian subcontinent, parents greatly value male children as a source of old-age security. As a result, having a male child increases the husband's satisfaction and reduces the risk of domestic violence (Bloch and Rao, 2002; Rao, 1997). We therefore introduce the lack of a living male child as a predictor of victimization (*No male child* equals one if the subject has no living male child, zero otherwise). Husband's unmet demand for a dowry is another possible predictor (Bates et al., 2004; Bloch and Rao, 2002; Naved and Persson, 2005, 2010). In our post-experiment survey, we asked each subject whether any dowry was demanded at the time of her marriage, and if demanded, we asked whether it was paid in full, not paid at all, or paid only partially by her parents. We create a dummy variable, *Unpaid dowry*, that equals one if a dowry was demanded on the subject's marriage but it was either not paid at all or paid only partially; and zero if a subject's marriage did not involve any dowry or her husbands' dowry demand was met in full.

Following Kim et al. (2007), we introduce measures of women empowerment as possible determinants of victimization. There are two dimensions of women empowerment: (a) internal qualities, such as self-confidence or critical thinking skills, which contribute to individual agency, and (b) the ability to make independent decisions and to challenge gender norms of male dominance. We designed a set of questions to capture these dimensions based on the survey instruments of the Bangladesh Demographic Health Survey (2011) and the IMAGE study (Intervention with Microfinance for AIDS and Gender Equity) conducted in South Africa. The exact wording of these questions can be found in Table 1. A subject's *Self-confidence* is measured as a dummy variable that equals one if she answered "very confident" to the following question: "If you were at a community meeting, how confident are you that you could raise your opinion in public?" The variable *Financial confidence* equals one if the subject answered "very confident" to the following question: "In the event of a crisis (e.g., house burn), how confident are you that you alone could raise enough money to feed your family for 4 weeks?" We asked each subject whether she needs her husband's permission to make small household purchases, to visit a doctor, to take children to the doctor, to visit family or relatives, or to cook food for the family. The dummy variable, *Autonomy,* equals one if the subject answered "no" to at least three of these cases. To measure a subject's ability to challenge gender norms, we asked whether she agrees with the following statements: a good wife always obeys her husband even if she disagrees; a woman should do most of the household chores even if the husband is not working; and it is a wife's obligation to have sex with her husband even if she does not feel like it. The variable *Challengers* equals one if the subject disagrees with all three of these statements and zero otherwise.

We asked subjects three questions from the General Social Survey (GSS), which measure an individual's perception on trust, fairness and cooperation. *Trust, Fairness,* and *Cooperation* each equal one if the subject answered positively to the trusting, fairness, and cooperation questions, respectively, from the GSS.[13] Finally, we collected information from the latest population census report (Bangladesh Bureau of Statistics, 2015) on gender differences in school attendance rate in each of the seven districts where we conducted our experiments. School attendance rate is measured as the number of individuals of age 5–24 years who ever attended school divided by the number of individuals in this age group. For each district, school attendance rates are separately measured for males and females, and the latter is then subtracted from the former. We introduce this variable as a proxy of the relative position of women in society.

Table 3
Determinants of the risk of intimate partner violence.
Dependent variable: 1 if subject reported spousal violence in the last 12 months; 0 otherwise.

Variables	Marginal effects from logistic regression (robust standard errors in parentheses)	
	(1)	(2)
Subject's age	0.005	0.007
	(0.008)	(0.009)
Husband's age	−0.009	−0.011
	(0.008)	(0.008)
Subject's education	−0.005	−0.005
	(0.006)	(0.006)
Husband's education	0.008	0.008
	(0.006)	(0.006)
Household assets	0.001	0.001
	(0.004)	(0.004)
Household size	0.016	0.014
	(0.019)	(0.020)
Earning member	0.277***	0.281***
	(0.057)	(0.051)
No male child	0.170***	0.163***
	(0.050)	(0.051)
Unpaid dowry	0.175***	0.196***
	(0.053)	(0.052)
Muslim	0.136***	0.131***
	(0.049)	(0.051)
Self confidence	−0.061	−0.058
	(0.045)	(0.046)
Financial confidence	−0.074	−0.067
	(0.069)	(0.069)
Autonomy	−0.152***	−0.148***
	(0.041)	(0.042)
Challengers	0.001	0.004
	(0.063)	(0.065)
Trust	0.018	0.009
	(0.053)	(0.057)
Gender difference in school attendance	0.003	0.021
	(0.009)	(0.041)
Village fixed effects	No	Yes
Pseudo *R*-squared	0.278	0.289
Wald chi-squared	114.23	118.13
Prob > chi2	0.000	0.000
N	485	485

Note: results are based on the (post-experiment) survey data. The definitions of the variables are provided in Table 1. *** Significant at the 1% level; ** Significant at the 5% level; * Significant at the 10% level.

The marginal effects of the logistic regression are reported in the first column of Table 3. In the second column, we report the results of the same regression after controlling for village-fixed effects. The results suggest that women who are involved in income-generating activities outside the home are at a greater risk of victimization. This finding is consistent with the male backlash theory (Aizer, 2010), which predicts that in low-income patriarchal societies – such as the Bangladeshi villages – greater financial independence of women challenges male dominance. This, in turn, makes men more violent toward their wives. Women with no male children are at a significantly greater risk of being abused, and those who enjoy more autonomy in household decision making are less likely to be abused. We find significant evidence of

[13] The General Social Survey is a sociological survey – conducted by the National Opinion Research Center and the University of Chicago – used to collect data on demographic characteristics and attitudes of residents of the United States. The exact wording of the relevant GSS questions is presented in Table 1.

dowry-related violence in rural Bangladesh. A husband's unmet demand for a dowry increases the likelihood of domestic violence. Finally, our results indicate that Muslim women are at significantly greater risks of victimization relative to other – predominantly Hindu – women. This particular finding is consistent with that of Koenig et al. (2003).

4.2. Experimental results: impact of intimate partner violence on loan repayment rate

The hypothesis we test in this paper is that exposure to intimate partner violence reduces the efficiency of joint liability and dynamic repayment incentives and, thereby, worsens loan repayment rates in microcredit. As mentioned in Section 3, we run four different specifications – S1 through S4 – of Eq. (1) to test this hypothesis. Results are reported in Table 4. We find no significant impact of intimate partner violence on loan repayment rates in the benchmark treatment. When dynamic incentives are added to individual loans, the average repayment rate of non-battered women improves by 31–35 percentage points ($p < 0.01$), but that of the battered women remains statistically the same. As a result, in the treatment of individual loans with dynamic incentives, the average repayment rate of the survivors of domestic violence is 26–33 percentage points lower than that of the non-victims ($p < 0.01$). When joint liability is added to the benchmark treatment without dynamic incentives, the repayment performance of the victims and non-victims remains the same. Finally, when both joint liability and dynamic incentives are added to the benchmark treatment, the average repayment rate of non-battered women improves by 29–32 percentage points ($p < 0.01$), whereas that of the battered women remains statistically unchanged. In the treatment of joint liability loans with dynamic incentives, the average repayment rate of the victims is 20–30 percentage points lower than that of the non-victims ($p < 0.01$ or 0.05).

These findings suggest that irrespective of the liability structure of a loan, dynamic incentives are significantly less effective for women who experienced physical or sexual assault in the last 12 months. Joint liability, after controlling for the effects of dynamic incentives, does not significantly improve loan repayment rates,[14] and there is no evidence that victimization reduces its effectiveness. In sum, women in violent spousal relationships are more likely to strategically default on their microloans due to a weakening dynamic repayment incentive, which is employed by most of the MFIs across the world regardless of the type of a loan. We also find that trusting behavior and household wealth are positively associated with loan repayment rates. These findings are consistent with, respectively, Karlan (2005) and Chakravarty et al. (2015). Furthermore, women from Muslim families have better repayment rates.

5. Robustness tests

Although reverse causality to victimization is unlikely in an experimental setup, three caveats remain that may affect the validity of our results. First, to the extent that there are unobserved characteristics of the poor women that may explain both victimization and repayment behaviors, the coefficients estimated from Eq. (1) could be biased. Individual trustworthiness, for example, is a possible omitted factor. Extant research suggests that women who are perceived as trustworthy by their husband are less likely to be exposed to spousal violence (Garcia-Moreno et al., 2005), and that individual trustworthiness is positively associated with microloan repayment decisions (Karlan, 2005). Lower repayment rates among battered women, therefore, might not be driven by their exposure to domestic violence; these rates might simply be the result of the victimization variable proxying for unobservable lower trustworthiness. Second, the estimated coefficients may also suffer from reporting bias. It has been observed in rural Bangladesh that women from culturally conservative families are less likely to share their experience of spousal violence with surveyors (Chin, 2012; Koenig et al., 2003). Underreporting of spousal violence – rather than having a non-violent spouse – might explain greater willingness to repay in our experiments if subjects with high repayment rates also happen to be more conservative. Third, subjects' attitude toward risk may affect their loan repayment decisions in a microloan repayment game (in Section 5.4, we discuss this possibility in greater detail). To address these concerns, we conducted a series of robustness tests, the results of which are discussed below.

5.1. Intimate partner violence and loan repayment decision in a matched subsample

When it is not possible to randomly assign a treatment – such as the exposure to violence – one can estimate the treatment effect in a reliable manner by finding an observational analogue of the counterfactual (Becchetti et al., 2014; Khandker et al., 2010; Stuart, 2010). One way of doing so is to identify a sample of matched subjects in which everyone has a similar likelihood of receiving the treatment, but only one group of subjects had received the treatment and the other had not. The mean outcome difference between these two groups is less likely to suffer from selection bias. In this section, we restrict our analysis to a subsample of women based on this principle.

In our post-experiment survey, we asked subjects about their opinion on whether a husband is justified in beating his wife under a series of circumstances, for example, if she burns the food, if she argues with him, if she goes out without telling him, if she neglects their children, or if she refuses to have sexual intercourse with him. These questions are adopted from the Bangladesh Demographic Health Survey (2011). We take a subsample of women who believe that a husband is justified in beating his wife under any of these conditions. Prior research suggests that the acceptance of violence is a good proxy for violence propensity (Chin, 2012; Garcia-Moreno et al., 2005; Jewkes et al., 2002). On the one hand, women learn to accept violence in circumstances in which they themselves are victims. On the other hand, women who view violence as "normal" are more likely to enter and remain in violent relationships. The repayment behavior of women who perceive violence as normal but were not exposed to actual violence, therefore, serves as a proxy of the counterfactual – the repayment behavior of the victims had they not been victimized.

A total of 214 subjects (44%) reported that a husband is justified in beating his wife.[15] Ninety one of them did not experience violence, whereas the others (123) were exposed to violence in the last 12 months. We rerun Eq. (1) for this subsample separately and report the results in Table 5. We first regress loan repayment rates on the *IPV* dummy, the three treatment dummies, and their interaction terms (column 1). Next, we add control variables that cannot be affected by spousal violence, such as *Subject's age, Husband's age, Muslim,* and *No male child* (column 2). Column 3 augments column 2 by adding variables that may cause victimization, such as *Earning member, Autonomy,* and *Unpaid dowry*. Fi-

[14] This particular finding is consistent with that of Attanasio et al. (2015) and Giné and Karlan (2014). These authors conducted randomized controlled experiments in Mongolia and the Philippines, respectively, and found no difference in repayment rates between individual and joint liability-based loans.

[15] More than half of the participants of the WHO Multi-Country Study in rural Bangladesh mentioned that wife-beating is normal under certain circumstances (Garcia-Moreno et al., 2005).

Table 4
Exposure to intimate partner violence and repayment of microcredit.
Dependent variable: loan repayment rate.

Variables	S1	S2	S3	S4
IPV	−0.022	0.013	0.026	0.028
	(0.079)	(0.078)	(0.084)	(0.086)
IL_DRI	0.315***	0.354***	0.341***	0.333***
	(0.055)	(0.055)	(0.055)	(0.055)
JL	0.001	0.018	0.019	0.018
	(0.059)	(0.057)	(0.058)	(0.059)
JL_DRI	0.289***	0.314***	0.306***	0.316***
	(0.057)	(0.061)	(0.061)	(0.060)
*IPV*IL_DRI*	−0.235**	−0.342***	−0.346***	−0.331***
	(0.116)	(0.117)	(0.115)	(0.113)
*IPV*JL*	−0.001	−0.030	−0.035	0.001
	(0.116)	(0.110)	(0.112)	(0.125)
*IPV*JL_DRI*	−0.182*	−0.256**	−0.268**	−0.330***
	(0.109)	(0.118)	(0.119)	(0.119)
Subject's age		0.009	0.008	0.007
		(0.008)	(0.008)	(0.008)
Husband's age		−0.006	−0.005	−0.006
		(0.008)	(0.008)	(0.007)
Subject's education				−0.005
				(0.005)
Husband's education				0.006
				(0.004)
Household assets				0.013***
				(0.004)
Household size				−0.019
				(0.017)
No male child		0.011	0.001	0.012
		(0.036)	(0.037)	(0.037)
Unpaid dowry			−0.011	−0.011
			(0.048)	(0.047)
Muslim		0.141***	0.142***	0.116**
		(0.045)	(0.045)	(0.045)
Earning member			−0.032	−0.023
			(0.039)	(0.039)
Self confidence				−0.054
				(0.037)
Financial confidence				−0.029
				(0.063)
Autonomy			0.001	−0.012
			(0.039)	(0.041)
Challengers				0.025
				(0.051)
Trust				0.095**
				(0.043)
Fairness				0.019
				(0.039)
Cooperation				0.023
				(0.046)
Gender difference in school attendance				0.067
				(0.042)
Constant	0.331	0.111	0.129	−0.077
	(0.041)	(0.114)	(0.114)	(0.171)
Village fixed effects	No	Yes	Yes	Yes
R-squared	0.128	0.173	0.184	0.235
N	485	485	485	485

Note: results are from OLS regression. Repayment rates are measured based on data from the first six rounds of the repayment game. Standard errors, corrected for heteroscedasticity, are in parentheses. *IPV* is a dummy variable that equals one if a subject experienced spousal violence in the last 12 months, and zero otherwise. There are three dummy variables for the four experimental treatments. The benchmark treatment of individual loans without dynamic incentives is the base category. *IL_DRI* is a dummy variable for the treatment of individual loans with dynamic repayment incentives; *JL* is a dummy variable for the treatment of joint liability loans without dynamic incentives; and *JL_DRI* is a dummy variable for the treatment of joint liability loans with dynamic repayment incentives. There are three interaction terms between the *IPV* dummy and each of the three treatment dummies. The definitions of the control variables are provided in Table 1. *** Significant at the 1% level; ** Significant at the 5% level; * Significant at the 10% level.

nally, in column 4, we control for all the observed factors reported in Table 1. We find that battered and non-battered women do not show different willingness to repay in the two treatments without dynamic incentives. In the treatment of individual (joint liability) loans with dynamic incentives, the average repayment rate of battered women is 36–38 (34–40) percentage points lower than that of non-battered women. In both cases, the difference is statistically significant at the conventional level. These results are qualitatively similar to those reported in Table 4. Thus, it can be reasonably augured that the negative correlation between victimization and loan repayment rates is unlikely to be driven by non-random selection into victimization.

Table 5
Intimate partner violence and repayment of microcredit in a matched subsample.
Dependent variable: loan repayment rate.

	Subsample of women who believe that a husband is justified in beating his wife under certain circumstances			
	(1)	(2)	(3)	(4)
IPV	−0.026	−0.032	−0.003	0.009
	(0.130)	(0.139)	(0.138)	(0.156)
IL_DRI	0.394***	0.407***	0.421***	0.372**
	(0.139)	(0.144)	(0.142)	(0.143)
JL	0.056	0.069	0.067	0.058
	(0.142)	(0.150)	(0.148)	(0.163)
JL_DRI	0.412***	0.397***	0.398***	0.375**
	(0.130)	(0.137)	(0.134)	(0.146)
*IPV*IL_DRI*	−0.332**	−0.354**	−0.372**	−0.388**
	(0.166)	(0.174)	(0.184)	(0.184)
*IPV*JL*	−0.148	−0.154	−0.156	−0.198
	(0.176)	(0.181)	(0.179)	(0.201)
*IPV*JL_DRI*	−0.337**	−0.319**	−0.338**	−0.412**
	(0.161)	(0.156)	(0.165)	(0.204)
Constant	0.333***	0.185	0.157	0.254
	(0.109)	(0.196)	(0.195)	(0.303)
R-squared	0.186	0.202	0.208	0.322
N	214	214	214	214
Control variables	Not included	Controlled for factors unaffected by victimization	Controlled for factors that may affect victimization	Controlled for all observable factors listed in Table 1

Note: results are from OLS regression. Repayment rates are measured based on data from the first six rounds of the repayment game. Standard errors, corrected for heteroscedasticity, are in parentheses. *IPV* is a dummy variable that equals one if a subject experienced spousal violence in the last 12 months, and zero otherwise. There are three dummy variables for the four experimental treatments. The benchmark treatment of individual loans without dynamic incentives is the base category. *IL_DRI* is a dummy variable for the treatment of individual loans with dynamic repayment incentives; *JL* is a dummy variable for the treatment of joint liability loans without dynamic incentives; and *JL_DRI* is a dummy variable for the treatment of joint liability loans with dynamic repayment incentives. There are three interaction terms between the *IPV* dummy and each of the three treatment dummies. The definitions of the control variables are provided in Table 1. *** Significant at the 1% level; ** Significant at the 5% level; * Significant at the 10% level.

5.2. Decline in violence and loan repayment decision

In this section, we restrict our analysis to a subsample of subjects who had *ever* experienced physical or sexual violence by husband. In our post-experiment survey, we asked each subject if any of the nine types of violence had ever been committed by her husband during marriage. A total of 210 (43%) subjects reported experiencing ever-violence.[16] Among these subjects, 74 reported experiencing no violence in the last 12 months; that is, these women experienced a decline in violence as of the time of data collection. The other 136 subjects reported that they experienced violence in the last 12 months; that is, they had experienced ongoing violence. For the subsample of women reporting ever violence, we run an OLS regression using the following equation (standard errors are corrected for heteroscedasticity).

$$\begin{aligned} Y_{ij} = {} & \gamma_0 + \gamma_1 Decline_{ij} + \gamma_2 IL\_DRI + \gamma_3\ JL + \gamma_4 JL\_DRI \\ & + \gamma_5 Decline_{ij}{}^{*} IL\_DRI + \ \gamma_6 Decline_{ij}{}^{*} JL \\ & + \gamma_7\ Decline_{ij}{}^{*} JL\_DRI + \delta_k \sum_k X_{ijk} + \sigma C_j + \ \mu_{ij} \end{aligned} \quad (2)$$

The outcome variable Y_{ij} is the measure of repayment rate of subject *i* in village *j*. $Decline_{ij}$ is a dichotomous variable that equals one if subject *i* in village *j* did not experience violence in the last 12 months and zero otherwise. There are three dummy variables for the four experimental treatments and three *Decline*-treatment interaction terms. X_{ijk} is a set of controls; C_j is a set of village fixed effects; and μ_{ij} is a zero-mean error term.

Under the assumptions that individual, household, and community level unobservables affect both ever- and current-occurrences of violence in the same way (i.e., the unobservables are time invariant) and that the omitted variable bias has additive effects, Eq. (2) effectively differences out the sources of bias that may arise from non-experimental comparison groups (Chin, 2012).[17] Another advantage of this analysis is that the magnitude of underreporting problem is less severe in this subsample: women who report ever-violence are less likely to underreport their current violence experience relative to those who report never experiencing such violence.

Table 6 reports the effect of a decline in violence on loan repayment rates. First, we regress loan repayment rates on the dummy variable, *Decline,* the three treatment dummies, and their interaction terms (column 1). Next, we add control variables that cannot be affected by spousal violence, such as *Subject's age, Husband's age, Muslim,* and *No male child* (column 2). We run a separate logit regression (results not reported for the purpose of brevity) and identify the factors that are associated with a decline in violence. We find that the same variables that cause victimization – *Earning member, Autonomy, Unpaid dowry, Muslim,* and *No male child* – are also associated with a decline in violence. Column 3 augments column 2 by adding *Earning member, Autonomy,* and *Unpaid dowry* as controls. Finally, in column 4, we control for all the observed factors listed in Table 1.

There is no significant impact of decline in spousal violence on loan repayment rates in the two treatments without dynamic incentives. In the treatment of individual (joint liability) loans with

[16] Different estimates suggest that 32–74% of married women in rural Bangladesh had ever experienced spousal violence (Chin, 2012).

[17] See Wooldridge (2002, pp. 61–62) for a useful discussion on how potential sources of bias can be differenced out in a model that assumes an additive effect of the omitted variable.

Table 6
Decline in intimate partner violence and repayment of microcredit.
Dependent variable: loan repayment rate.

	Subsample of women who had ever experienced spousal violence during marriage			
	(1)	(2)	(3)	(4)
Decline	0.032	0.062	0.075	0.077
	(0.041)	(0.059)	(0.057)	(0.056)
IL_DRI	0.064	0.038	0.029	0.009
	(0.104)	(0.107)	(0.109)	(0.115)
JL	0.003	0.015	0.018	0.012
	(0.103)	(0.095)	(0.096)	(0.105)
JL_DRI	0.091	0.004	0.002	0.053
	(0.093)	(0.109)	(0.110)	(0.112)
*Decline*IL_DRI*	0.254***	0.301***	0.311***	0.278**
	(0.094)	(0.107)	(0.107)	(0.118)
*Decline*JL*	0.040	0.065	0.076	0.042
	(0.121)	(0.110)	(0.109)	(0.110)
*Decline*JL_DRI*	0.278**	0.277**	0.256**	0.294***
	(0.138)	(0.143)	(0.120)	(0.102)
Constant	0.325***	0.204	0.153	-0.009
	(0.068)	(0.197)	(0.201)	(0.312)
R-squared	0.293	0.347	0.355	0.393
N	210	210	210	210
Control variables	Not included	Controlled for factors unaffected by victimization	Controlled for factors that explain a decline in victimization	Controlled for all observable factors listed in Table 1

Note: results are from OLS regression. Repayment rates are measured based on data from the first six rounds of the repayment game. Standard errors, corrected for heteroscedasticity, are in parentheses. *Decline* is a dummy variable that equals one if a subject did not experience spousal violence in the last 12 months; and zero otherwise. There are three dummy variables for the four experimental treatments. The benchmark treatment of individual loans without dynamic incentives is the base category. *IL_DRI* is a dummy variable for the treatment of individual loans with dynamic repayment incentives; *JL* is a dummy variable for the treatment of joint liability loans without dynamic incentives; and *JL_DRI* is a dummy variable for the treatment of joint liability loans with dynamic repayment incentives. There are three interaction terms between *Decline* and each of the three treatment dummies. The definitions of the control variables are provided in Table 1. *** Significant at the 1% level; ** Significant at the 5% level; * Significant at the 10% level.

dynamic incentives, the average repayment rate of women who experienced a decline in violence is 28–38 (30–37) percentage points higher than that of the women who experienced ongoing violence as of the time of data collection. In both cases, the differences are statistically significant at either the 5% or 1% level. These findings indicate that irrespective of the liability structure of a loan, dynamic incentives are significantly less effective for women who experienced ongoing violence relative to those who experienced a decline in violence. These findings further strengthen our argument that exposure to domestic violence worsens repayment rates in microcredit.

5.3. Exposure to non-violent shocks and repayment of microloans

In this section, we assess whether non-violent shocks, such as natural disasters (flood or river erosion), or the death of an earning member of the family, affect repayment behavior similarly to domestic violence.[18] Both natural disasters and the death of a family member are traumatic events (Cameron and Shah, 2015; Lerner and Keltner, 2001; Teno et al., 2001). They destroy physical properties and/or reduce income-earning opportunities. Thus, it is possible that these non-violent shocks would produce traumatic responses that are similar to the responses generated by domestic violence – which could be an indicator of potential omitted variable effects on our main results reported in Table 4. To test this possibility, we create a dummy variable, *Non-violent shocks*, that equals one if – in the last 12 months – a subject either evacuated her home because of flood or river erosion or experienced the death of an earning member of the family and zero otherwise. We insert *Non-violent shocks* as an explanatory variable in Eq. (1) while omitting the domestic violence measure. The results are reported in Table 7. We first regress loan repayment rates on the *Non-violent shocks* dummy, the three treatment dummies, and their interaction terms (column 1). Next, we add control variables that cannot be affected by non-violent shocks, such as *Subject's age, Husband's age, Muslim*, and *No male child* (column 2). Finally, in column 3, we control for all the observed factors listed in Table 1. It is evident that exposure to non-violent shocks does not affect loan repayment decision of the poor women in any of the treatments of our experiments. That is, they do not produce the same responses as intimate partner violence, which attenuates – to some extent – the risk that the results reported in Table 4 are due to omitted variable-effects.

5.4. Subjects' attitude toward risk and loan repayment decision

An important feature of our microloan repayment game is that by repaying dynamic incentives-based loans, a subject is exposed to a risky investment decision in which she may lose her initial endowment (characterized as a loan) if the project fails. In addition, by repaying joint liability-based loans, a subject may lose her entire project return (TK 120) if the game does not proceed to the next round because the contribution of her group members is insufficient. Thus, subjects' relative level of risk aversion might explain the results reported in Table 4 if battered women with low repayment rates also happen to be more risk averse. To formally examine this issue, we conducted a separate risk aversion experiment in all of the 21 villages, in which we conducted our loan repayment game. Four to seven subjects from each village participated in an experimental session. A total of 124 currently married women participated – who were eligible for receiving microcredit but were not members of a microcredit program at the time

[18] This robustness test is motivated by Voors et al. (2012). These authors examined whether exposure to violent conflict in a civil war affects survivors' time, social, and risk preferences. As a robustness test, they examined whether non-violent shocks, such as natural disasters, produce similar responses as violent conflict.

Table 7
Exposure to non-violent shocks and repayment of microcredit.
Dependent variable: loan repayment rate.

	(1)	(2)	(3)
Non-violent shocks	0.036	0.049	0.055
	(0.091)	(0.087)	(0.085)
IL_DRI	0.255***	0.269***	0.3260***
	(0.055)	(0.055)	(0.055)
JL	0.005	0.022	0.037
	(0.056)	(0.054)	(0.057)
JL_DRI	0.258***	0.260***	0.243***
	(0.055)	(0.056)	(0.057)
*Non-violent shocks*IL_DRI*	0.042	0.012	−0.013
	(0.120)	(0.118)	(0.116)
*Non-violent shocks*JL*	−0.016	−0.061	−0.088
	(0.129)	(0.123)	(0.123)
*Non-violent shocks*JL_DRI*	−0.069	−0.081	−0.068
	(0.124)	(0.124)	(0.123)
Constant	0.316***	0.129	−0.028
	(0.039)	(0.107)	(0.177)
R-squared	0.099	0.134	0.182
N	485	485	485
Control variables	Not included	Controlled for factors unaffected by non-violent shocks	Controlled for all observable factors listed in Table 1
Village fixed effects	No	Yes	Yes

Note: results are from OLS regression. Repayment rates are measured based on data from the first six rounds of the repayment game. Standard errors, corrected for heteroscedasticity, are in parentheses. *Non-violent shocks* is a dummy variable that equals one if – in the last 12 months – a subject evacuated her home due to flood or river erosion, or experienced the death of an earning member of the family; and zero otherwise. There are three dummy variables for the four experimental treatments. The benchmark treatment of individual loans without dynamic incentives is the base category. *IL_DRI* is a dummy variable for the treatment of individual loans with dynamic repayment incentives; *JL* is a dummy variable for the treatment of joint liability loans without dynamic incentives; and *JL_DRI* is a dummy variable for the treatment of joint liability loans with dynamic repayment incentives. There are three interaction terms between *Non-violent shocks* and each of the three treatment dummies. *** Significant at the 1% level; ** Significant at the 5% level; * Significant at the 10% level.

of data collection. To avoid any contamination effects, we ensured that our new subject pool had no overlap with the subject pool that played our loan repayment game.

We used of a standard risk-taking game similar to that conducted by Gneezy et al. (2009). At the beginning of a session, a subject received TK 120. Her task was to decide what portion of this endowment [0, 120] she desired to bet in a lottery that returned three times the bet with one-half probability and nothing with one-half probability. Once the subjects were comfortable with the rules and the expected payout of the game, we took each of them individually to a private room where they revealed their decision. Subjects were informed that the monies earned from the game would be paid in private at the end of the experiment. Appendix B provides the experimental instructions. At the end of the experiment, we interviewed each subject individually in the private room based on the same survey instrument we used in our loan repayment game.

The purpose of this game was to detect whether battered and non-battered women display different attitudes toward risk over *similar* monetary stakes involved in our main loan repayment game.[19] Forty out of the 124 subjects (32%) reported domestic violence in the last 12 months. On average, non-battered women bet TK 78.6 (standard deviation 0.218) whereas battered women bet TK 80.8 (standard deviation 0.234) of their endowment of TK 120. A two-sample *t*-test (assuming unequal variances and unequal sample sizes) rejects the hypothesis that victims have different risk aversion than non-victims over similar monetary stakes involved in our original loan repayment game. Thus, subjects' attitude toward risk can be excluded as an alternative explanation of our findings.

[19] In our loan repayment game, a subject risks a maximum of TK 120 by repaying her loan in a given round. In contrast, if the game continues up to the sixth round and a subject repays her loan in every round (alternatively, if all the subjects of a group repay their joint liability loans in every round up to the sixth round), a subject earns TK 360.

6. Conclusions

This paper investigates the relationship between poor women's exposure to intimate partner violence and their repayment behavior in microcredit. In an experimental laboratory, we extended collateral-free small loans to 485 currently married women in rural Bangladesh and observed their repayment decisions over multiple loan cycles. In a post-experiment survey, we asked subjects about their experience of physical or sexual violence by their husband. We found that exposure to spousal violence reduces the effectiveness of dynamic repayment incentives – a widely used borrower-disciplining device in microcredit. When loans are offered with dynamic incentives, battered women are more likely to strategically default on both individual and joint liability loans relative to those who did not experience spousal violence in the recent past. We conducted a series of robustness tests, and the results suggest that the negative correlation between victimization and loan repayment rates is unlikely to be explained by selection into victimization, non-random underreporting of violence, or the subjects' attitude toward risk.

Governments and development agencies devote substantial resources to minimize the incidence and severity of violence against women (United Nations, 2015). Much of these resources have been channeled through microfinance institutions based on the belief that opening up economic opportunities – through access to credit – would improve women's status within households and reduce

their likelihood of being abused by partners (International Labour Organization, 2008). However, practitioners, policy makers, and academics unanimously agreed at the 2014-Microcredit Summit Campaign in Mexico that for most microcredit providers, domestic violence is merely an afterthought in client outcome assessment (Gray, 2014). Furthermore, many socially motivated MFIs have recently been converted to fully commercial profit-seeking organization. Concerns have emerged that these organizations will deviate from their original mission of women empowerment (Cull et al., 2009; Mersland and Strom, 2010; Shahriar et al., 2016). Against this backdrop, our findings suggest that the widespread prevalence of domestic violence among poor women is a significant threat for the financial sustainability of microcredit providers. As such, irrespective of their profit status, MFIs have incentives to make strong strides to reduce the incidence of gender-based violence among their clients. After all, empowering women is smart banking at the bottom of the income pyramid.

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Appendix A. Experimental instructions for microloan repayment game

(Read loudly by the experimenter at the beginning of the experiments)

Good afternoon everyone. We are a group of students carrying out research about how women in rural Bangladesh repay their microloans. As a part of our study, we would like you to participate in a simple loan repayment activity and a post-activity survey. You will receive TK 100 for your participation. Based on the decisions you will make in this activity, you can earn more money in addition to the show-up fee. You are not allowed to talk to each other. If you have any questions, please raise your hand and you will be personally attended to. This activity consists of multiple rounds.

At the beginning of the first round, you will receive a loan of TK 50. The loan has to be repaid – with 20% interest – in one installment. After the disbursement of loan, you will be *individually* taken to another room, where you will invest your loan in a risky business. To determine whether your business is successful or not, you have to pick a ball from a non-transparent jar. There are one red and five green balls in the jar.

- If you draw the red ball, your business fails and you will earn zero. That means you lose your investment (the TK 50 loan).
- If you draw a green ball, your business is successful and you will earn TK 120. You may use these monies to repay your loan, but only in this particular round of the activity. The excess monies after repaying, or not repaying, the loan will be converted to real currency at the end of this activity.

(Read loudly by the experimenter before specific treatments)

Individual loans without dynamic incentives

If your project fails, you will not be able to repay the loan because you have no money. If your project succeeds, you have two choices. You may repay TK 60 (TK 50 of principal plus TK 10 of interest). Or, you may decide not to repay. Once you make your repayment decision, you will receive a new loan of TK 50, and the game will proceed to the next round. The same process will be repeated in the new round.

Individual loans with dynamic incentives

If your project fails, you will not be able to repay the loan because you have no money. If your project succeeds, you have two choices. You may repay TK 60 (TK 50 of principal plus TK 10 of interest). Or, you may decide not to repay. If you do not repay your loan in this round – either because your project had failed or because you do not want to repay – the activity will end for you. In contrast, if you repay, the game will proceed to the next round, you will receive a new loan of TK 50, and the same process will be repeated.

Joint liability loans with dynamic incentives

Please note that you belong to a group of five individuals in this activity, and loan repayment is a joint responsibility of your group. That means your group as a whole is expected to repay TK 300 – the total amount disbursed to the group plus interest payment.

If your project fails, you will not be able to contribute to group repayment because you have no money. If your project succeeds, you have two choices. You may contribute to group repayment, or you may decide not to contribute. If you choose to contribute, we will tell you shortly how much you have to contribute to the repayment pot. After revealing your repayment decision, you will return to the large room and wait quietly. Each of your group members will go through the same decision-making process. Once all the members of your group make their repayment decisions, we will again bring you individually to the private room. Then, we will tell you how many members of your group contributed to the repayment pot in this round. But we will not reveal the identity of the non-contributing members, or their reasons for non-contribution.

- If your project fails, you have to rely on your partners for repayment. The repayment obligation of TK 300 will be equally distributed among your group members, who are both able and willing to contribute to the repayment pot.
  - If at least three members of your group contribute, the group repayment obligation will be fulfilled. So, you will receive another loan of TK 50, and the whole process will be repeated.
  - If fewer than three members contribute, the group repayment obligation will not be fulfilled. So, you will not receive any new loan, and the activity will end.
- Suppose your project succeeds but you decide not to contribute to group repayment. As before, the repayment obligation of TK 300 will be equally distributed among your group members, who are both able and willing to contribute to the repayment pot.
  - If at least three members of your group contribute, the group repayment obligation will be fulfilled. So, you will receive another loan of TK 50, and the whole process will be repeated.
  - If fewer than three members contribute, the group repayment obligation will not be fulfilled. So, you will not receive any new loan, and the activity will end.
- Suppose your project succeeds and you decide to contribute to group repayment. Your required contribution to the repayment pot will be determined as follows.
  - If all of your four partners contribute, we will collect TK 60 from you, and from each of your group members. The group

repayment obligation will be fulfilled. You will receive another loan of TK 50, and the whole process will be repeated.
- If any three of your partners contribute, we will collect TK 75 from you, and from each of the other contributing members. The group repayment obligation will be fulfilled. You will receive another loan of TK 50, and the whole process will be repeated.
- If any two of your partners contribute, we will collect TK 100 from you, and from each of the other contributing members. The group repayment obligation will be fulfilled. You will receive another loan of TK 50, and the whole process will be repeated.
- If fewer than two partners contribute, we will collect TK 120 from you, and from the other contributing member of your group (if any). But you will not receive any new loan because the repayment obligation has not been fulfilled. The activity will end at this point.

Joint liability loans without dynamic incentives

Please note that you belong to a group of five individuals in this activity, and loan repayment is a joint responsibility of your group. That means your group as a whole is expected to repay TK 300 – the total amount disbursed to the group plus interest payment.

If your project fails, you will not be able to contribute to group repayment because you have no money. If your project succeeds, you have two choices. You may contribute to group repayment, or you may decide not to contribute. If you choose to contribute, we will tell you shortly how much you have to contribute to the repayment pot. After revealing your repayment decision, you will return to the large room and wait quietly. Each of your group members will go through the same decision-making process. Once all the members of your group make their repayment decisions, we will again bring you individually to the private room. Then, we will tell you how many members of your group contributed to the repayment pot in this round. But we will not reveal the identity of the non-contributing members, or their reasons for non-contribution.

- If your project fails, you have to rely on your partners for repayment. The repayment obligation of TK 300 will be equally distributed among your group members, who are both able and willing to contribute to the repayment pot.
- Suppose your project succeeds but you decide not to contribute to group repayment. As before, the repayment obligation of TK 300 will be equally distributed among your group members, who are both able and willing to contribute to the repayment pot.
- Suppose your project succeeds and you decide to contribute to group repayment. Your required contribution to the repayment pot will be determined as follows.
  - If all of your four partners contribute, we will collect TK 60 from you, and from each of your group members. The group repayment obligation will be fulfilled.
  - If any three of your partners contribute, we will collect TK 75 from you, and from each of the other contributing members. The group repayment obligation will be fulfilled.
  - If any two of your partners contribute, we will collect TK 100 from you, and from each of the other contributing members. The group repayment obligation will be fulfilled.
  - If fewer than two partners contribute, we will collect TK 120 from you, and from the other contributing member (if any). The group repayment obligation will not be fulfilled.

Please note that irrespective of your and your partners' repayment decisions, every member of this group will receive another loan of TK 50, and the whole process will be repeated.

(Read loudly by the experimenter at the end of every treatment)

There will be an interview following this activity. You will receive payment for your participation and any earnings from today's activity after the interview.

Appendix B. Experimental instructions for risk-taking game

Thank you for joining us today to participate in this activity. You will receive TK 100 for participation. Based on the decisions you make, you may earn more money in addition to the show-up fee.

At the beginning of this activity, you will receive TK 120. You will be asked to choose any portion of this amount (between 0 and 120) that you wish to invest in a risky option. The rest of the money will be accumulated to your total balance.

The risky investment: there is an equal chance that the investment will fail or succeed. If the investment fails, you lose the amount you invested. If the investment succeeds, you receive three times the amount invested.

How do we determine if you win? After you have chosen how much you wish to invest, you will toss a coin to determine whether you win or lose. If heads appears, you win three times the amount you chose to invest. If tails appears, you lose the amount invested.

Examples

1. If you choose to invest nothing, you will get TK 120 for sure. That is, the coin flip would not affect your earnings.
2. If you choose to invest all of the TK 120, then if heads appears in the coin-toss, you win TK 360 . If tails appears, you win nothing.
3. Suppose you choose to invest TK 20. You will receive TK 100 for sure, and the rest (TK 20) will be invested in the risky option. Then if heads appears in the coin-toss, you win TK 60 from the investment, and if tails appears, you win nothing from the investment.

If you have any questions, please raise your hand and you will be personally attended to.

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