Does exposure to violence affect reciprocity? Experimental evidence from the West Bank

Elisa Cavatorta * Daniel John Zizzo † Yousef Daoud ‡

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Abstract

This paper studies how reciprocity is affected by exposure to violence in early age. We combine a research design that isolates the exogenous exposure to violence with a lab-in-the-field experiment to study how reciprocity in the forms of conditional cooperation and vindictive behavior in adolescents varies as a result of exposure to violence. We focus on young Palestinians in the West Bank region of the Palestinian territories. We find that exposure to violence affects reciprocity of Palestinian adolescents: youth more exposed to violence engage in more reciprocal behavior in both the domain of cooperation and that of aggression. Part of the effect is explained by changes in the beliefs about other people's behavior.

Keywords: reciprocity; cooperation; conflict; violence; Palestine.

JEL classification codes: C72, C91, D91, I25.

*Corresponding author: Department of Political Economy, King's College London, United Kingdom. Email: elisa.cavatorta@kcl.ac.uk

†University of Queensland. Email: d.zizzo@uq.edu.au

‡Doha Institute for Graduate Studies and Birzeit University. Email: ydaoud@birzeit.edu

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1 Introduction

We present a lab-in-the-field experiment that aims to identify the effect of exposure to violence on the development of reciprocal behavior, both in the domain of cooperation and in the domain of aggression. The experiment was conducted with 1,172 Palestinian adolescents in secondary education schools in the West Bank region of the Palestinian territories, and we will explain below our strategy to identify exogenous variation in exposure to violence in their lives.

Reciprocity is a powerful feature of human behavior. Experimental evidence has shown it at work to support cooperation in Prisoner's Dilemmas and other public good game (e.g. Chaudhuri, 2011; Fischbacher, Gächter and Fehr, 2001), trust games (e.g. Berg, Dickhaut and McCabe, 1995; Pelligra, Reggiani and Zizzo, 2016) or other settings - including market settings - with hidden action (e.g. Fehr and Gächter, 1998, for a review). If one forms groups of reciprocal agents playing with each other, one finds an especially high cooperation level in a repeated public good game (Burlando and Guala, 2005). Reciprocity can support the creation of social ties and the development of social capital in an economy (e.g. Fukuyama, 1995; Pacheco et al., 2008; Putnam, 2000). The flip side of reciprocity in the domain of cooperation is however reciprocity in the domain of aggression: Bolle, Tan and Zizzo (2104) and Wang (2017) find that, in 'vendetta games' where no mutual gain is possible, initial aggressive behavior leads to cycles of retaliation and counter-retaliation, which makes both parties worse off than they were. The Gospel of Matthew's (5: 38-39) recommendation to turn the other cheek if someone slaps you is framed as an explicit rebuke to reciprocity in the form of 'an eye for an eye, and a tooth for a tooth'. Depending on the setting, perverse social capital can result, leading to worse societal outcomes (Pelling and High, 2005; Rubio, 1998).

While twin heritability studies show that some heterogeneity in reciprocity across people is genetic, they also corroborate the likely role of environmental factors in the development of heterogeneous reciprocal preferences (Cesarini et al., 2008; Wallace et al., 2007; Zizzo, 2003)). van den Bos et al. (2011) present neurobiological evidence matched with trust games behavior to support the claim that adolescence is an important time for the development of reciprocal preferences. As further discussed in section 2, exposure to violence may strengthen reciprocal preferences by lowering the threshold for perceiving the behavior of other agents as a threat (e.g. Guerra, Rowell Huesmann and Spindler, 2003; Novaco and Chemtob, 1998; Schwartz and Proctor, 2000).

Given the importance of reciprocal behavior in shaping social capital, cooperation and conflict, understanding the effect of exposure to violence on reciprocity is an important

question. From a practical viewpoint, in areas where there is significant exposure to violence, it matters to schools to know whether educational strategies need to be adapted to best benefit children and adolescents with such a significant exposure. Bauer et al. (2016) presents a meta-analysis on the effect of war violence on altruism, finding more altruism, though the change in preferences tend to favour the in-group (Choi and Bowles, 2007). Ethnographic studies suggest that the injustice of being subject to violence instils a 'pleasure in agency' (Wood and Goodwin, 2001) and a 'desire for revenge' (Petersen, 2001). As far as we know, however, ours is the first study that, using a controlled experimental methodology, has tried to identify what difference exposure to violence makes on reciprocal behavior.

Our sample is drawn from a region - the West Bank region of the Palestinian territories - where there is significant variability in exposure to violence, hence enabling us to test the effect of exposure to violence on reciprocity. In the experiment, we employ public good games to measure reciprocity in the domain of cooperation, using the methodology adopted by Fischbacher, Gächter and Fehr (2001). We use vendetta games (as described in section 3.2) as a natural and simple way to measure reciprocity in the domain of aggression.

Section 2 describes the experimental hypotheses. Section 3 presents the experimental design and procedures. Sections 4 describes the results. These are discussed in section 5, which also provides concluding remarks to the paper.

2 Hypotheses

The main focus of this paper is to study whether adolescents more exposed to violence (such as violent interactions, provocations, and the like) are likely to behave differently in reciprocal interactions with their peers. Our two main hypotheses are as follows:

Hypothesis 1: Exposure to violence generates a tendency to react more aggressively against unfriendly actions: that is, to engage in greater reciprocity in the domain of aggression.

Hypothesis 2: In the domain of cooperation, exposure to violence generates a tendency to disengage more often when others are uncooperative: that is, to engage in greater reciprocity in the domain of cooperation.

Hypotheses 1 and 2 are informed by established theories of learned cognition developed in psychology. The fundamental idea of the social learning literature in psychology (Bandura, 1973, 1986) is that social behavior is determined by cognitive schemas or narrative

scripts that we develop by observing others while growing up (e.g. role models or people around us). By observing others' actions and experiencing the consequences of actions, the child acquires a set of cognitive schemas that guide social behavior. Growing up in a violent context may therefore have an effect on children's and adolescents' reciprocity due to the development of particular cognitive schemas supporting forms of retaliation.[1].

One important cognition affected by violence exposure, and key in reciprocal exchanges, is the perception of threat. It has been shown that the repeated observation of aversive interactions in the form of hostile exchanges, provocation, direct violence or frustration impacts the reactivity to perception of 'threats'. In the context of traumatized victims of violence and veterans, Chemtob et al. (1988, 1997) and Novaco and Chemtob (1998) argue that traumatized individuals have a substantially lowered threshold to perceiving situations as threatening (hostile appraisal). This makes them more susceptible to perceive external events, cues or actions as a threat to their personal safety.[2] This effect may have neurocognitive bases. Neuroscience studies based on functional MRI observations show that exposure to physical abuse and violence leads to heightened activity in brain structures related to 'threat' stimulation, such as the amygdala and anterior insula. A heightened hostile appraisal may represent a form of adaptation to violent environments: this may serve as a defence during childhood but may transform into a vulnerability later in life as it becomes an obstacle to cope with additional stressors.

According to Chemtob et al. (1997) and Novaco and Chemtob (1998), hostile appraisal schemas activate a biologically predisposed "survival mode" which involves fight reactions and possible revenge intentions to ward off potential threats. Once activated, the survival mode triggers the use of mental 'rules' governing actions for personal protection. Survival mode is peremptory and carries a coping response urgency that suppresses the

[1]Several studies on the topic have highlighted a number of aggression-promoting cognitive schemas that are likely to be affected by violence. Examples are: the endorsement of normative beliefs that aggression is more acceptable Guerra, Rowell Huesmann and Spindler (2003) or the appropriate response to ambiguous peer provocation Schwartz and Proctor (2000); the development of aggressive fantasies and aggressive scripts Guerra, Rowell Huesmann and Spindler (2003); erosion of 'self-efficacy' and the believe that little can be done to avoid violent confrontations (see Kirk and Hardy (2014), and the related concept of 'street efficacy' in Sharkey (2006)); misfunctioning in anger and other emotional regulation mechanisms (Novaco and Chemtob, 1998). All of these cognitions would predict an increase in aggressive behavior. Several studies have documented empirically that exposure to violence predicts externalizing behavioral problems in children and adolescents, such as aggressive and anti-social behavior. For example, Singer et al (1999) document a correlation between community violence and violent behavior in American youth between grade 3 to 8; Schwab-Stone et al. (1999) documents it for children in grade 6 to 10. Other studies include Flannery, Singer and Wester (2001). Correlations between exposure to ethnic-political violence and aggressive behavior are found in Barber (2008) and Dubow et al. (2009)

[2]This is also discussed in Crick and Dodge (1996); Dodge et al. (2015) under the name of hostile-attributional bias.

typical inhibitory controls of aggressive behavior. In other words, the activation of hostile appraisal schemas potentiates aggressive reactions.[3] The conjecture that repeated exposure to violent interactions might lead to the adoption of revenge goals to avert perceived threats has been noted in the literature (Guerra, Rowell Huesmann and Spindler, 2003, p.1574) but, to our knowledge, it has not yet been tested.

To further study the impact of exposure to violence on retaliatory behavior, we focus of one potentially important manifestation of hostile appraisal: the individual beliefs about how other people would react. Believing that other people will be likely to react aggressively will increase the likelihood that the subject herself or himself will adopt the same behavior. Violence exposure may change the perception of what others might do and in turn this perception guides subjects' behavior.[4]

3 Experimental design and procedures

In this section we describe the research design tailored to test the hypothesis that exposure to violence sensitizes people acting in reciprocal interactions. We first describe our sampling frame and the research design we use to isolate the exogenous variation in violence exposure. We then describe the experimental tasks we use to elicit reciprocity in the domain of cooperation and aggression. Additional implementation details are described in the online appendix.

[3] A possible alternative individual reaction to hostile appraisal is a fear and flight reaction. Individual differences make some people more susceptible to fear and flight reactions (i.e. internalizing problems) and others to anger and fight reactions (i.e. externalizing problems). In as much as fear and flight reactions result in less retaliatory behavior, our empirical results would be attenuated downwards by the presence of flight reactions. The share of people leaning towards flight as opposed to fight reactions may vary and be partly a byproduct of the history of violence. Studies show that repeated violence may desensitize adolescents to the emotional distress that violence generates and desensitization may make flight reaction less likely to occur as compared to fight reactions (Huesmann and Kirwil, 2007).

[4] We focused on *descriptive* beliefs – the beliefs about what others people are likely to do – rather than *normative* beliefs – the beliefs about what one ought to do. Normative beliefs about aggression are an alternative mechanism. The choice to focus on descriptive as opposed to normative beliefs was informed by the findings of recent research. For example, research by Bicchieri and Xiao (2009) suggests that descriptive beliefs are more important than normative beliefs in influencing norm behavior. Among studies investigating the role of normative beliefs to explain aggression, Guerra, Rowell Huesmann and Spindler (2003) find that normative beliefs only explain a small share of the effect of exposure to violence on behavior.

3.1 Sampling frame and research design

A notorious challenge in studying the effect of violence exposure is that violence incidents are generally not random. On the one hand, the aggressor can target certain types of individuals (e.g. the more/less aggressive); on the other hand, the potential target may try, to different degrees, to avoid being exposed to violence (self-selection). Either of these issues introduces selection bias. In the context of the West Bank, the majority of violent incidents occur in proximity of military checkpoints. These are the sites, generally manned by Israeli soldiers, where interactions between Palestinians and Israelis are forced to take place. A noteworthy feature of our design is that we are able to identify adolescents who have an *obligation* to regularly cross a military checkpoint to go to school or for other reasons. Having the obligation to cross a checkpoint increases the likelihood of being exposed to violence, something we observe in our data (see Table 8 in the online appendix). We specifically focus on a student's obligation to *regularly cross* a checkpoint, as opposed to simply attend to (e.g. to demonstrate) a checkpoint to minimize concerns about choosing to go to a checkpoint or about seeking violence by attending a checkpoint.[5] Inasmuch as the adolescents cannot influence their own obligation to cross a checkpoint, we can therefore consider 'crossing a checkpoint' exogenous from the point of view of the adolescents.[6]

Our sampling strategy implies that all checkpoints subjects must cross in our analysis are located within the interior of the West Bank, as opposed to checkpoints located on the borders with Israel. Since most political demonstrations take place at border checkpoints, selectively including only interior-checkpoints further minimizes the likelihood that subjects reporting to 'cross a checkpoint' are referring to checkpoints which people choose to attend to engage in actions that may trigger exposure to violence, hence avoiding this potential source of endogeneity.

The location of checkpoints is however potentially targeted. Military checkpoints in the West Bank first appeared in the 1990s following the first Palestinian uprising, known as the First Intifada (1987-1993), a period that saw widespread violence between Israelis

[5]The relevant questions specifically ask "Do you normally have to cross a checkpoint on your way from home to school?" and "Do you regularly have to cross a checkpoint (or checkpoints) for reasons other than going to schools?".

[6]Secondary schooling in Palestine is compulsory. It is a policy of the Palestinian government to provide access to secondary education within short distance and anecdotal evidence suggests that children tend to enrol in the school closest to their locality of residence (interviews with Dr Shukri, Director of the Research and Development Department of the Palestinian Ministry of Education). Analyzing the year of foundation of existing Palestinian secondary schools in the West Bank, the data shows that the average founding year is 1977 and the median year is 1980. This evidence speaks against the potential concern that schools are built in locations to circumvent checkpoint crossing.

and Palestinians. Checkpoints might be built in areas where they are thought to be most needed, such as in areas that saw several episodes of violence. Simply sampling schools differently distant from checkpoints may therefore bring about a potential selection bias due to pre-existing violence. Our sampling frame of schools is chosen to circumvent this potential reverse causality of violence.

One of the functions of interior checkpoints is to provide protection to Israeli settlements. Our sampling frame of schools is based on the school proximity to Israeli settlements built *prior* to the widespread violent events during the First Intifada (1987-1993). We select schools close to and further away from Israeli settlements pre-dating the First Intifada (preceding year 1987). Using data from BTselem, an Israeli human rights organization, we calculate that 41% of existing Israeli settlements pre-dating the Intifada were built in the 1970s: among all settlements, the average foundation year is 1975. This suggests that the location of Israeli settlements pre-dating 1987 is unlikely to be related to the violence that occurred over ten years later. In the early 1990s, checkpoints followed the location of settlements: 71% of pre-1987 settlements had a checkpoint placed within 3km. [7] Having half of our schools close to pre-1987 Israeli settlements will therefore be useful to check whether our results hold if we restrict our sample to schools for which we are confident that the close by checkpoints were placed independently of the extent of violence during the Intifada, which could otherwise be a source of concern.

We had information from the Palestinian Ministry of Education on the location of the 145 secondary schools in the West Bank. Within this sampling frame, we first randomly selected 24 schools within a distance of 2 km from the boundaries of pre-1987 Israeli settlements. Secondly, we sampled 24 pairing schools from comparable schools within the same geographical district and with student body of the same sex located at least 3 km away from any pre-1987 settlement.[8]

[7] While not all settlements had a checkpoint in their proximity, the data show that those who did not have a checkpoint built close by are very small settlements, with a population of almost five times smaller than the average settlement (and the ratio remains constant over time, see Table 9). If we exclude those settlements in the bottom quartile of population size (using the 1996 population data, the earliest year available), the percentage of settlements with a close by checkpoint is over 77%. Therefore it is reasonable to argue that the placement of checkpoints follows existing settlements.

[8] The choice of 2km threshold to define schools 'close by' (and 2+1km to define 'further away' from a settlement) was deemed reasonable based on the geography of schools and sampling considerations. The 2km (2+1km) threshold gives a balanced classification between schools classified as 'close by' and 'further away' from a settlement and allows us to make meaningful random sampling within these groups. An alternative 2km (and 2+2km) threshold for example gives an unbalanced classification with much fewer schools classified 'further away' relative to 'close by' schools, complicating finding comparable schools in each group.

3.2 Experimental tasks and survey data

The data for this study were collected in two phases: the first data collection took place in March 2017 and served as a pilot of the instruments (*phase 1*: 7 schools). The second phase took place collected in September and October 2017 (*phase 2*: 40 schools).[9]

A total of 1,172 subjects participated in the experiment, which took place in the classrooms during school hours. There was no attrition. Each experimental session was made of three parts. Part 1: the Voluntary Contribution Mechanism game; Part 2: the vendetta game; Part 3: survey questionnaire. Each subject was allocated a random number which is used to identify them and calculate the payment. Subjects knew they were paid at the end of the session according to the number of tokens they earned during the session, exchanged for local currencies at the rate of 1 token to NIS 5 (€1).

Part 1: Voluntary Contribution Mechanism game – To elicit reciprocity in the domain of cooperation we use a version of the Voluntary Contribution Mechanism game (VCM) which elicits directly the willingness for conditional cooperation ((Fischbacher, Gächter and Fehr, 2001)). Students are randomly matched in groups of four members. Each member is given 5 tokens and decides how many tokens to contribute to a 'public pot' and how many tokens to keep for himself. After everyone contributed, each member receives a third of the total amount of tokens contained in the 'public pot' plus any tokens s/he kept for himself. The following equation describes each subject's payoff function which was explained in simple terms to the students:

$$\pi_i = 5 - c_i + \frac{1}{3}\sum_{j=1}^{4} c_j \tag{1}$$

where c_i is subject's i contribution to the 'public pot'. For simplicity, contributions could only be in integer numbers of tokens. Under standard assumptions of rationality and selfishness the prediction is complete free riding by all subjects.

[9]Phase 2 has three additional features as compared to Phase 1. i) As it is explained later, belief elicitation questions were introduced in half of the selected schools. The introduction of belief elicitation questions was done for pairs of 'close by' and 'further away' schools selected randomly. ii) The payment structure in schools with beliefs elicitation questions include incentives for reporting correct beliefs. The experimental instructions for this group were modified to incorporate explanations of the payments related to the belief questions. The understanding check questions for this group included one additional question related to payments for correct beliefs. The remaining instructions were identical to Phase 1 and the other half of Phase 2's sample. iii) The questionnaire included a Parenting Style module placed at the end of the Part 3 questionnaire. The Parenting Style module is based on Robinson et al. (2001). We did not find it having explanatory power on our key variables and is not referred to below. Other than these differences, we kept experimental instructions, questionnaire and incentives identical to Phase 1 sample.

We implement the game in strategy method.[10] Subjects are asked to make two types of contribution decisions. The first decision concerns how many of the 5 tokens to contribute to the public pot. This is an unconditional contribution, as it does not depend on the decision of other players, and proxies the willingness for *unconditional cooperation*. The second decision concerns conditional contributions. Each subject was asked how many of the 5 tokens s/he would contribute to the public for *each of the possible* average contribution levels of the other group members (rounded to integers). In other words, the subject reports the contribution schedule given the other group members' contributions. This elicits the willingness for *conditional cooperation* (reciprocity in the domain of cooperation).

To ensure that both decisions are potentially payoff relevant for all subjects and to give participants a monetary incentive to take the decision seriously and report them truthfully, we adopt the following procedure. Subjects are told that, at the end of the session, a random mechanism will determine which of the two types of decisions will become relevant to compute their actual payment. In each group, earnings are calculated by taking the decision in the conditional contribution table for *one* randomly selected group member and the decisions in the unconditional contribution table of the other group members. The instructions includes a number of examples. To ensure thoughtful decisions, no time limit was imposed. The game was played once and subjects knew it. Questions are provided to check the understanding of the game.

Part 2: 'Vendetta' game – To elicit reciprocity in the domain of aggression we use a simplified version of the 'vendetta' game developed in Bolle, Tan and Zizzo (2104).

In this game, students are randomly paired with a co-player and the game proceeds sequentially across a finite number of periods.[11] Define the players endowment as (e_1, e_2) where e_i are player 1 and player 2's endowments respectively. At the initial stage of the game each player has 8 tokens, (8,8). Player 1 moves first and can choose whether to steal 8 tokens from the co-player or do not steal. If player 1 does, he gains 4 tokens. Thus, stealing results in an efficiency loss. Upon an act of stealing, the second period starts with a pair endowment of (12,0), otherwise it starts with a pair endowment of (8,8). In the second period, Player 2 can decide whether to steal back 8 tokens (retaliate) or don't steal. Figure 1 illustrates the decision tree.

[10] In a survey of experimental evidence, Brandts and Charness (2011) found no evidence that treatment effects identified with a strategy methods are not replicated with a direct response method.

[11] Subjects were randomly matched anew with other subjects in the same classroom, independently of their previous group matching.

The game ends when both players have not stolen for two consecutive periods, or when both cannot steal 8 tokens any longer. Assuming standard rationality and selfishness, the equilibrium of the game, derived by backward induction, is the strategy "Player 1 does not steal, and Player 2 does not steal".[12] Stealing should not occur. We use subject behavior in the Second player role to elicit reciprocity in the aggression domain. Specifically, a *conditional attacker* steals when stolen from and does not steal when not stolen from, whereas an *unconditional attacker* steals in both cases.

We implement the game in strategy method, eliciting subjects' decisions at each node of the sequential decision tree. Subjects know that, at the payment stage, they can be assigned to be First or Second player and we compute their earnings based on their decisions in the selected role. The game is played once and has no time limit. The instructions use a neutral frame using the term 'taking away tokens' (rather than 'stealing tokens'). Again, questions are provided to check the understanding of the game.

Part 3: Survey questionnaire – Part 3 is the last part of the session. The survey includes socio-economic questions, whether the student has an obligation to regularly cross a checkpoint, exposure to different kind of violent incidents in the last year and a detailed account of all household's domicile movements (of a duration of at least 1 month) since 1986. Subjects' time to answer Part 3 was compensated with NIS 3.

Beliefs manipulation. In half of the sample (N=500, excluding the pilot experiments) we inserted incentivized questions eliciting beliefs about the behavior of other subjects in the same session. To elicit the beliefs about the likelihood of interacting with a conditional cooperator, we ask students to imagine that one student from the class was selected at random. Each subject is then asked to report whether it was more likely that the selected student's contribution would decrease/stay the same/increase as the tokens in the public pot increase. To elicit beliefs about the likelihood to interact with a conditional attacker, we ask each subject to report the number of classmates he or she thought would take 8 tokens away as Second player when they were stolen from and when they were not by the first Player. Each answer was incentivized by an extra earning of 2 tokens if the subject's answer coincided with the actual decision of the selected student or the actual number of people stealing as Second players in any given situation.

Procedures and payments. Subjects are given the experimental instructions for one game at a time, followed by a control questionnaire to check understanding of the

[12]The equilibrium is the same if one assumes inequality averse preferences, linear altruism or moderate levels of spite: see Bolle, Tan and Zizzo (2104).

instructions after each game. The enumerators overseeing the session correct the control questions and discuss any incorrect answer in one-to-one conversations with the subject. Subjects then proceed to answer the first task. After everyone completes the first task, enumerators distribute the instructions for the second task. Once again students answer a control questionnaire, their answers are checked and discussed before proceeding with the task. Part 3, the survey questionnaire, follows. The experimental session concludes with the payments. Student are called one by one at the experimenter desk and are given the payments, together with a short explanation of the payment sum. The experimental instructions in English are provided in the online appendix. Subjects read experimental instructions translated into local Arabic.

4 Results

Our sample includes subjects who took part in both phase 1 and 2 of the data collection; there are no major differences among the two samples (see Table 1). The descriptive statistics of participants who have an obligation to cross a checkpoint and those who have not such obligation are shown in Table 2. Demographic and common socio-economic characteristics are balanced. One exception is the percentage of girls among the non-crossers. This difference is due to class sizes being larger in female schools. A regression of the obligation to cross a checkpoint on gender and class size yields no significant differences in the propensity of crossing between boys and girls. All our regressions control for gender, age, class size and an indicator for phase 1 vs. phase 2 data collection; similar key results can be obtained without these controls.

Our data show that the frequency of (self-reported) violent incidents is higher among the sample of adolescents who regularly must cross a checkpoint. They report to have been threatened almost three times as often as those who must not cross, and to have been hit and verbally abused 1.5 times more frequently than the other group (see Table 8 in the online appendix). This provides support to our identification strategy and specifically to our conjecture that being required to regularly cross a checkpoint is a proxy for exposure to violence.

Table 2 shows no difference in the frequency of unconditional cooperators (defined as contributing at least 3 tokens in the unconditional VCM game) nor the average contribution between adolescents who regularly must cross a checkpoint and those who do not. The result is robust if a regression is run simply on level of contribution in the unconditional VCM game (see Table 10, column 1, in the online appendix). While a bivariate test

suggests a difference in destruction rates, this disappears in a regression analysis allowing for controls (see Table 10, column 3). .

We classify 'reciprocal' subjects based on their answers in the VCM and the vendetta games. Our methodology to classify reciprocal subjects in the VCM game, and so in the domain of cooperation, is based on Fischbacher, Gächter and Fehr (2001). In the VCM game, subjects whose contribution schedule has a positive and significant Spearman correlation (set at 20% significance level) with the other group members' contributions are classified as 'conditional cooperators'. There are 49.1% of conditional cooperators in the sample.[13] The group of non-conditional cooperators includes subjects whose schedule is positive and does not change (constant pattern, 3.4%); subjects whose schedule is flat at zero (free-riders, 2.4%); whose schedule increases and then decreases (hump-shaped, 6.8%) and other patterns (38.3%). In the vendetta game, 'conditional attackers', that is reciprocators in the domain of aggression, can be simply identified as those subjects whose second player decision is to take away tokens when the first player takes tokens away and not to take away tokens when the first player does not take away.

We analyze reciprocal behavior in the domain of cooperation and aggression jointly using a bivariate probit model. We cluster the standard errors at the class level within the school to account for intra-class correlation among subjects in the same class. The results are shown in Table 3. [14] The obligation to regularly cross a checkpoint significantly increases reciprocal behavior: in support of both Hypotheses 1 and 2, it increases the propensity to engage in greater reciprocity in the domain of aggression and the domain of cooperation. The magnitude of the effect is sizeable: checkpoint crossing generates a 7.2 percentage points increase in conditional behavior (corresponding to a 15% increase from the control group's rate of conditional cooperation) and a 8.1 percentage points increase in retaliation (corresponding to a 27% increase from the control group's rate of retaliation).[15]

A possible confounding variable in our analysis is movement into or away from the

[13]The significance level is set at 20% to allow for small deviations from the theoretical conditional cooperation schedule which would predict a Spearman correlation of 1. Fischbacher, Gächter and Fehr (2001) use a more conservative significance level but they have 20 observations per schedule rather than 5 and their experiments is a laboratory study with university students, and so obviously one with less noise than can be expected in our lab-in-the-field setting with adolescents. That said, we get broadly the same qualitative results (at $p< 0.1$) if we employ a significance level set at 10% as opposed to 20% (see Table 11 in the online appendix).

[14]Seven subjects returned the questions on obligation to cross a checkpoint blank. As a result, the empirical sample in our analysis includes 1,165 subjects.

[15]We do not find significant gender differences in reciprocity behavior, which is in line with previous results Fischbacher, Gächter and Fehr (2001).

sampling sites, since migrants' preferences may differ from non-migrants'. We address these concerns using questionnaire data. Table 4 shows that merely 18% of subjects come from households who changed domicile for at least 1 month since 1986. The number of moves in the interim period is 1.12 on average. These movements are predominately driven by work and marriage reasons (79.5% of all movements).[16] The average residency duration in the sampled localities of subjects from households who moved at least once is 16.7 years. For most subjects this corresponds to as long as their entire life. They also do not report to having to cross checkpoints more often than their non-migrant peers, which one might expect because of more frequent visit to extended family members in other localities. Table 5 reports the regression results estimated on a restricted sample of subjects whose household has never moved from the sampled locality. The results are qualitatively identical and, if anything, the impact is slightly larger. Overall, these results suggest that the impact on reciprocal behavior is not driven by omitted characteristics of migrant subjects.

The results shown in Table 3 and 5 are robust to a number of additional robustness tests. First, the results maintain when we include a proxy for household income and an indicator for migrant subject.[17] Second, we instrument the obligation to cross a checkpoint using the distance (in km) from the adolescent's locality of residence to the nearest checkpoint. The first stage significantly predicts an increase in the obligation to cross when checkpoints are closer. The IV estimates show a picture consistent with the main results: the obligation to cross increases reciprocity. For reciprocity in the domain of aggression, the IV estimates yield stronger results. For reciprocity in the domain of cooperation, the magnitude of the impact is unchanged although the statical significance is weaker (see Table 13 in the online appendix). Third, since schools 'further away' from settlements are also away from exogenous checkpoints – those unrelated to possible pre-existing violence – one cannot exclude that other checkpoints may have been placed close to these schools for other reasons such as greater incidence of violence. We therefore replicate the analysis restricting the sample to schools 'close by' pre-1987 settlements which are not prone to this potential confound. The results, shown in Table 12, offer a picture consistent with our main results. The coefficient magnitudes are qualitatively similar, although restricting the sample to 20 schools requires bootstrapping the standard errors. Fourth, our findings are also robust to whether we control for the presence of a belief elicitation

[16] In the Palestinian society, it is customary that the wife joins the husband's family residence location).

[17] Household income is proxied by the first factor in a Principal Component Analysis of a battery of household assets. Household income is potentially endogenous to the obligation to cross a checkpoint and therefore we exclude income from the main model equation.

mechanism.[18] Fifth, the differences in the frequency of reciprocators we observe by obligation to cross a checkpoint are not an artefact of differences in some other category of 'behavioral type'. Table 14 provides the distribution of behavioral types in both games by obligation to cross. Since categories are mutually exclusive, an increase in the frequency of one category must be drawn from other categories. Allowing for the fact that some of the behavioral type categories have few subjects, Table 14 suggests that the increase in reciprocators among crossers comes from all other categories. Tables 15 and 16 shows that the increase number of reciprocators are drawn proportionally from the typology based on unconditional behavior. For example, the ratio between conditional attackers who *unconditionally* attack and those who do not remain approximately 2. Sixth, as an additional sensitivity test for omitted variables, we compute the statistics suggested by Altonji, Elder and Taber (2005). These statistics indicate the relative amount of selection on unobservables relative to observables that would be required to explain away the impact of checkpoint crossing. To gauge the role of selection bias in a simpler way, we ignore the bivariate probit structure of outcomes and we show the effect using separate linear regressions. We compute the impact of checkpoint crossing without control, β_0, when controls are added,β_C . Under the assumption that the relationship between crossing a checkpoint and the unobservables is the same as the relationship between crossing and the observables, the ratio $\beta_C/\beta_0 - \beta_C$ gives an indication of how large the selection on unobservables have to be relative to the selection on observables to explain away the checkpoint effect. Table 18 in the online appendix shows that the Altonji's ratios range are 6.3 for conditional aggressor and 20.7 for conditional cooperator. These ratios imply that the shift in the distribution of the unobservables would have to be 6.3 (20.7) times as large as the shift in the observables for the impact of checkpoint crossing on conditional aggressor (conditional cooperator) to disappear. In the restricted sample of schools 'close by' pre-1987 settlements, these ratios are 3.9 and -11.5. A negative ratio occurs when the observable controls are on average negatively correlated with the treatment and positively with the outcome (or vice-versa). This implies that in this restricted sample our estimate is attenuated towards zero by unobservable variables.

Table 6 shows that there is a positive correlation between beliefs and behavior. Specifically, subjects who expect a comparatively high number of conditional attackers in the room are more likely to be conditional attackers themselves and subjects who expect to interact with a conditional cooperator are similarly more likely to be conditional coop-

[18]See the online appendix for the regression analysis. It also shows a negative effect of having a belief elicitation mechanism on the likelihood of conditional aggression, which replicates a similar finding in Bolle, Tan and Zizzo (2104)

erators. Could a belief mechanism mediate the effect of regularly crossing a checkpoint on the degree of reciprocity? Consistently with Novaco and Chemtob (1998), exposure to averse interactions occurring at checkpoints may lead to strengthening the belief of reciprocation, whether in the domain of cooperation or aggression. Table 7 shows the impact of crossing a checkpoint on the subjective belief about the number of conditional attackers in the classroom and on the propensity to interact with a conditional cooperator. The results show that the obligation to cross a checkpoint does not predict beliefs of conditional cooperation being more likely in the expected positive direction; we will come back to this in the next section.

In relation to reciprocity in the domain of aggression, subjects with an obligation to cross believe that there are two additional conditional attackers in the room compared to the group without such obligation. The magnitude of this effect is sizeable since the baseline amount of conditional attackers was believed to be 1 in the control group. The combination of evidence in Tables 7 and 6 suggests that, in relation to reciprocity in the domain of aggression, changes in beliefs are a mechanism behind the impact of checkpoint crossing on behavior.

To gauge some evidence of how much the change in beliefs explains the observed gap in the frequencies of conditional attackers between crossers and non-crossers we carry out a counterfactual simulation exercise. We simulate the distribution of beliefs that the control group would have if they had crossed a checkpoint. In other words, the counterfactual uses data from the control group to generate a distribution of beliefs statistically identical to that of the group who crosses a checkpoint. Comparing the frequencies of conditional attackers that would arise in the simulated distribution versus the original control group would proxy the change in frequency due to beliefs changing. This counterfactual simulation exercise, reported in Table 19, provides some suggestive evidence that in the case of conditional attackers the change in beliefs explains approximately 50% of the observed gap in the frequencies of conditional attackers between crossers and control group.

5 Discussion and Conclusions

We have shown how our proxy for exposure to violence leads to more reciprocal behavior for Palestinian adolescents, both in the domain of cooperation and in the domain of aggression. Our proxy for exposure to violence is the students' stated *obligation* to cross checkpoints for students in schools sampled based on distance from pre-1987 settlements. Using a natural proxy for exposure to violence has obvious advantages of external validity,

while obviously presenting limitations from the viewpoint of internal validity. In the previous sections we have considered how our identification strategy handles concerns such as choice whether to cross a checkpoint or propensity to engage in violence (sections 1, 3.1: the key question is about obligation to cross checkpoint and, also, it is not about going to a checkpoint by itself), checkpoint location (sections 3.1, 4: interior checkpoints and robustness to just focusing on schools near pre-1987 settlements), relationship between checkpoint crossing obligation and stated exposure to violence proxies (section 4: data supports relationship), household migration decisions, household income or presence of a belief elicitation mechanism (section 4: results robust to controlling for these factors), potential school choice (section 2: adolescents generally go to school near by), more or less conservative definitions of reciprocators in the domain of cooperation (section 4: significance level of 10% or 20% applied to Spearman correlations of own contribution and the average contribution of others), and unobservable potential endogenous variation (section 4: IV approach). We have also seen how changes in the proportion of reciprocators relates to changes in the proportion of other types (section 4).

One alternative potential interpretation of the regular obligation to cross the checkpoint is to postulate that adolescents who have a greater obligation to cross the checkpoints perceive that they are required more to visit other people due to their higher degree of pro-social preferences. However, more pro-social adolescents should cooperate more in the public good game and destroy less in the vendetta game, and we do not find clear evidence of either, which runs counter to this interpretation.

Our study looks at the effect of exposure to violence on reciprocal behavior with respect to peers. This leads to another potential alternative interpretation of the obligation to cross a checkpoint: it could be that greater interaction with Israeli soldiers would lead to stronger in-group identity. In this case, once again we should observe more cooperation in the public good game and less destruction in the vendetta game, which we do not. It would nevertheless be interesting to compare within-group with inter-group effects in future research, involving both Palestinian and Israeli students.

Further research would clearly more broadly be useful to provide additional support to the correctness of our interpretation. When we presented our work to a sample of approximately 20 schoolmasters and officials at the Palestinian Ministry of Education in the West Bank, it was a reassuring validation of our interpretation that their experience of students who had been more exposed to violence was that they were indeed more reciprocal.

The assumption that more violence occurs from regularly crossing checkpoints is rea-

sonable and corroborated by the relationship between being required to regularly cross checkpoints and stated measures of exposures to violence. We would not, however, be able to rely on these measures directly as measures of exposures to violence because, while we can make a case for the exogeneity of regularly having to cross a checkpoint, individual stated measures of exposure to violence (such as verbal abuse) are likely to be endogenous. It would nevertheless be valuable in future research to determine the robustness of our findings to different exogenous measures of exposure to violence.

There is some partial evidence of a belief mechanism, which would be consistent with the Novaco and Chemtob (1998)'s interpretation for why we find the results we find, as it is based on an enhanced sensitivity to perceived threats. Specifically, we find evidence for a belief mechanism in the domain of aggression. It is unclear why we do not find it in the domain of cooperation. We recognize that there are limitations to our incentivized belief elicitation mechanism in the domain of cooperation due to its unavoidable complexity. This may have diluted the likelihood of significant findings. Our incentivized belief elicitation mechanism in the domain of cooperation is also different from the one used in the vendetta game in that it picks a direction (contributions going up, staying the same or going down depending on the behavior of others) rather than how many engage in a specific action. While in a first study we felt that incentivization was essential, future research may wish to go for simpler unincentivized belief elicitation mechanisms, particularly in the domain of cooperation.

Being more sensitive in reciprocal interactions as a result of exposure to violence can bring about positive as well as negative feedback loops in social interactions. This has policy implications. For example, students exposed to violence could be more sensitive to positive rewards mechanisms: the use of praise and rewards in teaching practices may lead to an increase in student cooperation because it brings about more positive reciprocity; the use of punishments instead may induce less cooperation as it brings about negative reciprocity.

Figure 1: 'Vendetta' game (VG)

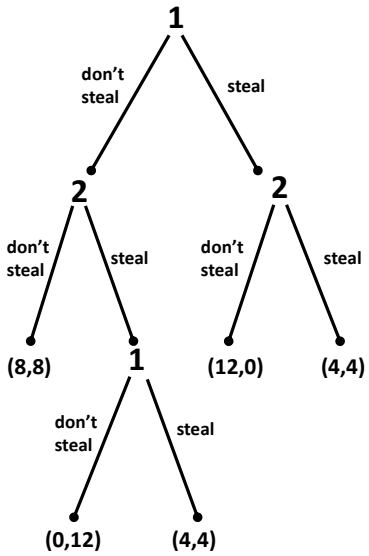

Table 1: Sample descriptive statistics by data collection phase

	Phase 2 (main)		**Phase 1 (pilot)**	
	Far away from settlement	Close to settlement	Far away from settlement	Close to settlement
N	532	468	84	88
Number of schools	20	20	3	4
girls	0.56	0.55	0.43	0.55
age (months)	205.71	205.02	217.95	217.60
class size	28.42	26.84	29.52	23.64
must cross checkpoint	0.30	0.38	0.45	0.63
Total earnings (NIS)	32.27	32.36	33.50	33.60
Earnings Part 1 (VCM)	34.44	34.70	33.50	33.58
Earnings Part 2 (VG)	28.41	29.62	not occurred	29.31
Part 1: ♯ mistakes (max 13)	2.084	1.993	4.023	3.897
Part 2: ♯ mistakes (max 4)	0.443	0.318	0.714	0.818

The tables shows the sample descriptive statistics for Phase 1 (pilot) and Phase 2 (main) data collection. Payments were done in Israeli shekels (NIS), the local currency. As explained in Section 3.2, subject payments included Part 3 and either Part 1 or Part 2. The table shows the average total earnings delivered and the earnings in the Voluntary Contribution Mechanism (VCM) game and Vendetta game (VG) separately. 'Part 1: ♯ mistakes (max 13)' indicates the average number of mistakes in the Part 1's thirteen understanding check questions. 'Part 2: ♯ mistakes (max 4)' indicates the average mistakes in the Part 2's four understanding check questions.

Table 2: Sample descriptive statistics by obligation to cross

	Must cross checkpoint	**Must not cross**	**p-value of test⋆**
N	431	734	
girl⋆	0.49	0.57	0.007
class size	27.32	27.60	0.538
migrant household⋆	0.20	0.17	0.190
income (index, PCA)	-0.03	0.02	0.665
total earnings	32.15	32.41	0.704
mistakes in part 1	2.352	2.313	0.788
mistakes in part 2⋆	0.378	0.476	0.200
locus of control scale	2.99	2.92	0.416
social desirability scale	12.03	12.26	0.098
unconditional cooperation (tokens)	2.42	2.48	0.455
unconditional cooperators (%)	0.47	0.46	0.695
unconditional aggression (%)	0.71	0.76	0.038†

The tables shows the count, average or percentage values of different variables as appropriate, in relation to subjects who have an obligation to cross a checkpoint and those who do not have such obligation. The variables are as follows. N: observations; gils: proportion of female subjects; migrant household: proportion of subjects from households who moved into the locality from somewhere else; income (index, PCA): average of the first factor in a Principal Component Analysis on a battery of household assets; mistakes part 1(2): average mistakes in Part 1(2)'s understanding checks; locus of control scale: average of the Internality, Powerful others and Chance scale (Levenson, 1981); social desirability: average of the social desirability scale-17 (Stöber, 2001); unconditional cooperation: average tokens contributed; unconditional cooperators: percentage of subjects classified as unconditional cooperators (see Section); unconditional aggression: percentage of aggression rates (see Section). ⋆: The differences by group are evaluated using a T-test for continuous variables and a Pearson's χ^2 test for discrete variables. †: the significance disappears when adding controls, see Table 10.

Table 3: Impact of crossing on reciprocity: bivariate probit estimates

	Conditional behavior	
	Conditional Cooperator	**Conditional Attacker**
Must cross checkpoint	0.1729**	0.1918**
	(0.080)	(0.096)
Girl	-0.1476	0.0218
	(0.090)	(0.140)
Age	-0.0130*	0.0048
	(0.007)	(0.006)
Class size	-0.0054	-0.0071
	(0.006)	(0.010)
Phase 2 data(=1)	-0.1198	-0.2017
	(0.142)	(0.151)
constant	2.9312*	-1.156
	(1.608)	(1.311)
ρ between equations	0.0873*	
	(0.048)	
N	1165	
Pr[Y=(1,1)—must not cross]	0.152	
Pr[Y=(1,1)—must cross]	0.218	
Pr[marginal(Y=1)—must not cross]	0.464	0.302
Pr[marginal(Y=1)—must cross]	0.536	0.383

The table shows the estimates from a bivariate probit regression with $y_1 = 1$ if the subject is classified as a 'conditional cooperator' and $y_2 = 1$ if the subject is classified as 'conditional attacker'. Standard errors are clustered at the school class level.

Table 4: Migration and reasons for migration into current locality

moved since 1986	18.07 %
average number of moves	1.12
average residence duration (years)	16.7
% must cross among migrants	40.9%
% must cross among non-migrants	36.1%
The main reason was:	
building of wall	1.05 %
checkpoints	2.62 %
settlements	4.71 %
harassment by IDF	12.04 %
none of the above	79.58 %
If none, the main reason was:	
work reasons	32.74 %
study reasons	6.55 %
marriage	19.64 %
other reasons	39.29 %

The table reports the frequency of subjects who report at least one change in their household domicile for at least 1 month since 1986. It reports the average average duration of residence in the current domicile and the main reasons for the move. Reasons for movements were asked for each reported move in a questionnaire module (part 3).

Table 5: Impact of crossing on reciprocity among subjects who never moved

	Conditional behavior	
	Conditional Cooperator	**Conditional Attacker**
Must cross checkpoint	0.226**	0.251**
	(0.092)	(0.110)
Girl	-0.124	0.016
	(0.100)	(0.141)
Age	-0.011	0.003
	(0.009)	(0.008)
Class size	-0.006	-0.008
	(0.007)	(0.010)
Phase 2 data(=1)	-0.089	-0.191
	(0.173)	(0.160)
constant	2.515	-0.795
	(2.080)	(1.779)
ρ between equations	0.077	
	(0.051)	
N	955	

The table shows the estimates from a bivariate probit regression with $y_1 = 1$ if the subject is classified as a 'conditional cooperator' and $y_2 = 1$ if the subject is classified as 'conditional attacker'. The model is estimated on data from the sample of subjects whose household has never moved from the sampled locality. Standard errors are clustered at the school class level.

Table 6: Beliefs about others and conditional behavior by crossing

Conditional cooperator frequencies			
	Beliefs about the contribution by the randomly selected participant		
	decreases	stays the same	increases
Must cross	0.406	0.480	0.591
(s.e.)	(0.088)	(0.071)	(0.061)
Must not cross	0.378	0.543	0.541
(s.e.)	(0.057)	(0.052)	(0.037)

Conditional attacker frequencies				
	Distribution of believed number of conditional attackers in the class room			
	bottom 25%	25-50th	50-75th	top 25%
Must cross	0.184	0.146	0.314	0.647
(s.e.)	(0.064)	(0.056)	(0.080)	(0.083)
Must not cross	0.111	0.131	0.191	0.361
(s.e.)	(0.032)	(0.034)	(0.048)	(0.053)

The table shows the frequency of conditional cooperation and conditional attackers by different levels of beliefs about others. 's.e.' stands for standard errors.

Table 7: Impact of crossing on subjective beliefs about others

	Conditional beliefs	
	Conditional Contributor	**Conditional Attacker**
Must cross checkpoint	-0.077*	2.128**
	(0.042)	(0.956)
Class size	0.0047	0.1115
	(0.006)	(0.134)
Constant	0.396**	-2.0021
	(0.172)	(3.003)
adj.R^2	0.010	0.020
N	497	497
mean in Control group	.524	1.042

The table shows the estimated impact of checkpoint crossing on the belief about the probability of interacting with a conditional cooperator (linear probability model) and the number of conditional attackers (OLS) in the classroom. Standard errors clustered at the school class level. The sample is the sub-sample of subjects for whom beliefs were elicited.

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Online Appendix

Table 8: Violence exposure by obligation to cross a checkpoint

	Hit, beaten up	Verbally abused	Threat of harm	Seen serious incident	Frightening situation
Frequency	0.18	0.38	0.07	0.37	0.14
Must cross checkpoint	0.23	0.43	0.11	0.47	0.23
Mustn't cross checkpoint	0.15	0.35	0.04	0.31	0.10
Fisher test p-value	0.001	0.012	0.000	0.000	0.000

The table shows the frequency of violent incidents among participants who have an obligation to cross a checkpoint and those how do not have. The questionnaire module refers to the past year and violence incidents are self-reported.

Table 9: Population in settlements with and without a checkpoint close by

	Years:			
settlement has	1996	1997	1998	1999
a checkpoint (=1)	1528.29	1652.48	1796.05	1873.06
no checkpoint (=0)	324.32	343.90	360.00	365.96

The table shows the settlement population over time (years) by group of settlements with (and without) at least one checkpoint closer than 3km. Settlement population data are taken from Peace Now.

Table 10: Impact of crossing on unconditional cooperation and aggression

	Unconditional behavior		
	Tokens contributed	**Contribution $\geq$ 3**	**Attacking**
	(1)	(2)	(3)
must cross checkpoint	0.048	0.022	0.159
	(0.093)	(0.087)	(0.105)
girl	0.079	0.113	0.148
	(0.124)	(0.111)	(0.140)
age	0.002	0.003	0.003
	(0.006)	(0.006)	(0.007)
class size	0.002	0.003	0.005
	(0.008)	(0.007)	(0.011)
phase 2 data (=1)	0.157	0.187	0.070
	(0.178)	(0.187)	(0.216)
constant	1.878	0.914	1.665
	(1.423)	(1.268)	(1.450)
ρ between equations		-0.093*	
		(0.052)	
N	1165	1165	
Pr[Y=(1,1)—must not cross]		0.354	
Pr[Y=(1,1)—must cross]		0.319	
Pr[marginal(Y=1)—must not cross]		0.478	0.766
Pr[marginal(Y=1)—must cross]		0.466	0.712

The table shows the estimates from an OLS regression of number of tokens contributed unconditionally in the VCM game (column 1) and from a bivariate probit regression (columns 2 and 3) with $y_1 = 1$ if the subject contributes unconditionally at least 3 tokens to the public pot and is thus classified as an 'unconditional cooperator'. $y_2 = 1$ if the subject takes away tokens from the co-player when acting as first Player and is thus classified as 'conditional attacker'. Standard errors are clustered at the school class level.

Table 11: Robustness: restrictive definition of conditional cooperator using Spearman's correlation set at 10% significance level

	Conditional behavior	
	Conditional Cooperator	**Conditional Attacker**
must cross checkpoint	0.115*	0.191**
	(0.070)	(0.090)
girl	0.021	0.021
	(0.090)	(0.145)
age	0.005	0.004
	(0.008)	(0.006)
class size	0.006	0.007
	(0.008)	(0.010)
phase 2 data (=1)	0.035	0.202
	(0.182)	(0.166)
constant	1.045	1.154
	(1.739)	(1.457)
ρ between equations	0.0587	
	(0.066)	
N	1165	

The table shows the estimates from a bivariate probit regression with $y_1 = 1$ if the subject is classified as a 'conditional cooperator' using a positive Spearman correlation between a subject contribution schedule and that of others set at 10% significance level (as opposed to 20% significance level as in the main analysis). The 'conditional attacker' classification remains unchanged: $y_2 = 1$ if the subject is classified as 'conditional aggressor'. Standard errors are clustered bootstrapped at the school class level (100 replications).

Table 12: Robustness: restricting to schools close by settlements

	Schools close by settlements		**Schools close by settlements**		**Full sample**	
	Clustered s.e.		Clustered bootstrapped s.e.		Clustered bootstrapped s.e.	
	Conditional cooperator	Conditional Attacker	Conditional cooperator	Conditional Attacker	Conditional cooperator	Conditional Attacker
must cross checkpoint	0.2530**	0.227	0.2530**	0.2268*	0.1729**	0.1919**
	(0.101)	(0.149)	(0.110)	(0.126)	(0.074)	(0.088)
girl	-0.147	0.002	-0.147	0.002	-0.148	0.022
	(0.140)	(0.218)	(0.136)	(0.246)	(0.097)	(0.148)
age	-0.0195*	0.009	-0.0195*	0.009	-0.013	0.005
	(0.011)	(0.006)	(0.011)	(0.010)	(0.008)	(0.006)
class size	-0.008	0.002	-0.008	0.002	-0.005	-0.007
	(0.010)	(0.013)	(0.010)	(0.014)	(0.007)	(0.010)
phase 2 data (=1)	-0.073	-0.279	-0.073	-0.279	-0.121	-0.203
	(0.185)	(0.170)	(0.203)	(0.222)	(0.176)	(0.167)
constant	4.3069*	-2.119	4.3069*	-2.119	2.9323*	-1.154
	(2.403)	(1.395)	(2.347)	(2.312)	(1.763)	(1.486)
ρ between equations	0.0384		0.0384		0.0876*	
	(0.082)		(0.075)		(0.053)	
N	555		555		1165	

The table shows the results from bivariate probit regressions. Column (1) shows the estimates from a sample restricted to subjects attending a school close by an exogenous settlement; standard errors are clustered at the school class level. Column (2) shows the estimates from a sample restricted to subjects attending a school close by an exogenous settlement; standard errors are clustered bootstrapped at the school class level (100 replications). Column (3) shows the estimates with school class level clustered bootstrapped standard errors using the full sample.

Table 13: Instrumenting checkpoint crossing: IV regressions

	IV regression (LPM)	
	First stage: must cross checkpoint	
distance to nearest checkpoint (km)	-0.0751***	
	(0.016)	
distance (squared)	0.003***	
	(0.001)	
	Conditional behavior	
	Conditional Contributor	**Conditional Attacker**
(IV) must cross checkpoint	0.163*	0.4840***
	(0.092)	(0.147)
girl	-0.0509	0.0387
	(0.033)	(0.032)
age	-0.0045*	0.0016
	(0.003)	(0.003)
class size	-0.0023	-0.0031
	(0.002)	(0.002)
phase 2 data (=1)	-0.0203	0.0023
	(0.052)	(0.058)
constant	1.4602***	-0.1073
	(0.548)	(0.639)
obs	1165	1165

The table shows the results of an IV regression in which the obligation to cross a checkpoint is instrumented with the distance between the school attended and the nearest checkpoint. Standard errors are calculated by clustered bootstrapping (150 replications) at the school class level.

Table 14: Distribution of types by obligation to cross a checkpoint

	Vendetta game (conditional behavior)					VCM game (conditional behavior)					
	Always take away 1|a, 1|not a	Never take away 0|a, 0|not a	Conditional attacker 1|a, 0|not a	Contrarian attacker 0|a, 1|not a	Total	Conditional cooperator	Free-rider	Hump-shaped	Constant patterns	Other patterns	Total
Mustn't (N)	423	41	222	48	734	341	16	44	22	311	734
cross (%)	57.63	5.59	30.25	6.54	100	46.46	2.18	5.99	3	42.37	100
Must cross (N)	212	32	165	22	431	231	12	35	17	136	431
(%)	49.19	7.42	38.28	5.1	100	53.6	2.78	8.12	3.94	31.55	100
Total (N)	635	73	387	70	1165	572	28	79	39	447	1,165
(%)	54.51	6.27	33.22	6.01	100	49.1	2.4	6.78	3.35	38.37	100

The table tabulates the classifications of subject 'types' based on the behavior in the games by obligation to cross a checkpoint. Vendetta game: the classification is based on the 2nd mover behavior. '1|a, 1|not a' indicates strategy 'attack if attacked, attack if not attacked'; '0|a, 0|not a' indicates strategy 'don't attack if attacked, don't attack if not attacked' etc. VCM game: The classification follows Fischbacher, Gächter and Fehr (2001) and is described in section 4. (N) indicates observations.

Table 15: Vendetta Game: Classification by unconditional behavior and obligation to cross a checkppoint

	Must not cross a checkpoint					Must cross a checkpoint				
Unconditional Attacker	Always take away 1|a, 1|not a	Never take away 0|a, 0|not a	Conditional attacker 1|a, 0|not a	Contrarian attacker 0|a, 1|not a	Total	Always take away 1|a, 1|not a	Never take away 0|a, 0|not a	Conditional attacker 1|a, 0|not a	Contrarian attacker 0|a, 1|not a	Total
No	46	23	91	11	171	20	25	72	7	124
(%)	26.9	13.45	53.22	6.43	100	16.13	20.16	58.06	5.65	100
Yes	377	18	131	37	563	192	7	93	15	307
(%)	66.96	3.2	23.27	6.57	100	62.54	2.28	30.29	4.89	100
Total	423	41	222	48	734	212	32	165	22	431
(%)	57.63	5.59	30.25	6.54	100	49.19	7.42	38.28	5.1	100

The table tabulates the classifications of subject 'types' based on the 2nd mover behavior in the Vendetta game by the unconditional behavioral types and by obligation to cross a checkpoint. '1|a, 1|not a' indicates strategy 'attack if attacked, attack if not attacked'; '0|a, 0|not a' indicates strategy 'don't attack if attacked, don't attack if not attacked' etc. Figures in rows marked with (%) are percentages, otherwise they are number of observations.

Table 16: VCM Game: Classification by unconditional behavior and obligation

	Must not cross a checkpoint						Must cross a checkpoint					
Unconditional contributor	Conditional cooperator	Free-rider	Hump-shaped	Constant patterns	Other patterns	Total	Conditional cooperator	Free-rider	Hump-shaped	Constant patterns	Other patterns	Total
No	180	13	28	14	148	383	125	10	24	12	59	230
(%)	47	3.39	7.31	3.66	38.64	100	54.35	4.35	10.43	5.22	25.65	100
Yes	161	3	16	8	163	351	106	2	11	5	77	201
(%)	45.87	0.85	4.56	2.28	46.44	100	52.74	1	5.47	2.49	38.31	100
Total	341	16	44	22	311	734	231	12	35	17	136	431
(%)	46.46	2.18	5.99	3	42.37	100	53.6	2.78	8.12	3.94	31.55	100

The table tabulates the classifications of subject 'types' conditional behavior in the VCM game by the unconditional behavioral types and by obligation to cross a checkpoint. The classification follows Fischbacher, Gächter and Fehr (2001) and is described in section 4. Figures in rows marked with (%) are percentages, otherwise they are number of observations.

Table 17: Robustness: controlling for belief elicitation

	Conditional Cooperator	**Conditional Attacker**
must cross checkpoint	0.180**	0.161*
	(0.080)	(0.096)
girl	-0.149	0.029
	(0.092)	(0.121)
age	-0.012*	0.001
	(0.007)	(0.006)
class size	-0.005	-0.009
	(0.006)	(0.008)
phase 2 data (=1)	-0.155	-0.019
	(0.149)	(0.151)
belief elicitation (=1)	0.091	-0.496***
	(0.085)	(0.127)
constant	2.7340*	-0.282
	(1.532)	(1.395)
ρ between equations	0.0974**	
	(0.049)	
obs	1165	

The table shows the estimates from a bivariate probit regression with $y_1 = 1$ if the subject is classified as a 'conditional cooperator' and $y_2 = 1$ if the subject is classified as 'conditional aggressor', including a control variable for the presence of belief elicitation questions. Standard errors are clustered at the school class level.

Table 18: Omitted variable bias: Altonji (2005) ratios

	Conditional aggressor	Conditional cooperator
Full sample	6.329	20.762
Schools close by	3.902	-11.502

The table shows the ratio of selection on unobservables relative to observables that would be required to produce a treatment effect of zero, using the method by Altonji (2005) in a linear model (OLS). A negative entry arises because the observable controls are on average negatively correlated with the treatment and positively with the outcome, suggesting a downward bias in the OLS estimates (provided the unobservables have similar correlation patterns with the outcome and the treatment as the included observables). 'Schools close by' refers to the restricted sample of schools 'close by' a pre-1987 settlement. The control variables included are those presented in Table 3 of the paper.

Table 19: Counterfactual exercise: Impact holding beliefs constant

Conditional Aggression

	Treatment Must cross	**Control Must not cross**	**Gap**
Frequency in sample with belief elicitation (n=500)	0.3108	0.1919	0.118***
Simulated beliefs as if:			
they were the same as the T group	0.3108	0.2471	0.063
Indirect effect (se)		0.055	(0.020)

The table shows the result of a counterfactual exercise predicting the frequency of conditional aggression that the control group would have if it had the same distribution of beliefs as the treatment group.