Does exposure to violence affect reciprocity? Experimental evidence from an ongoing conflict

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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 find that exposure to violence affects reciprocity of Palestinian adolescents: those 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 their peers' behavior. A re-analysis of micro-datasets from other conflict contexts provides evidence of the generalizability of our results.

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

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

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1 Introduction

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, 2020) 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). Both the Torah (e.g., Deuteronomy 19:21) and the Qur'an (e.g., Surah al-Nahl 16:126) invoke the concept of behavioral reciprocity as 'measure-for-measure'. The flip side of reciprocity in the domain of cooperation is however reciprocity in the domain of aggression: Bolle, Tan and Zizzo (2014) 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. Depending on the setting, perverse social capital can result (Pelling and High, 2005; Rubio, 1998).

Since reciprocity is key to understand the emergence of cooperation and cycles of retaliation, it is a crucial element of models of conflict resolution in economics and political science. Studies of violent conflicts highlight that their relapse over time is a common empirical regularity (Walter, 2011). Several studies in political science have highlighted how violent acts, political assassinations and terror actions derail protracted peace efforts by invoking more violence (e.g. Dugan and Chenoweth, 2012; Iqbal and Zorn, 2008). A reason for this, which we test, is the impact of exposure to violence induced by political conflict on reciprocal preferences.

In this paper, 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. We provide evidence on a group of adolescents, a key period of life for the formation of reciprocal preferences (Blakemore,

2018), from a context of ongoing conflict. The experiment was conducted with 1,172 Palestinian adolescents in secondary education schools in the West Bank region of the Palestinian territories, some of which are more exposed to violence than others. We will explain below our strategy to identify exogenous variation in exposure to violence in their lives. 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, two player interactive tasks where retaliation can but need not arise, as a natural and simple way to measure reciprocity in the domain of aggression. Our hypotheses on the effects of violence on reciprocity are informed by established findings in social psychology: 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). Our key finding is that that adolescents more exposed to violence engage in more reciprocal behavior in both the domain of cooperation and the domain of aggression.

We are aware of one paper that come closest to our research on reciprocity, albeit with a different game (a trust game) and a sample of 50 senior citizens non-exposed to the violence arising from the conflict (Gneezy and Fessler, 2012). Our results complement theirs as we find that the effect on reciprocity comes from the individual experience of increased exposure to violence (in addition to the possible exposure to conflict in the environment, which they evidenced). The fact that all Palestinians children are exposed to the ongoing political conflict (and potentially exposed to some level of baseline violence) is a strength of our case study: the design provides a conservative test of the effect of direct exposure to (exogenously increased) violence, netting out the influences arising from living in a context of ongoing conflict, which are common among all participants.

Four additional papers provide background knowledge on the likely effects of violence on cooperation using trust games. Specifically, Gilligan, Pasquale and Samii (2014), Cassar, Grosjean and Whitt (2013), Bauer, Fiala and Levely (2017) and Bogliacino, Gómez and Grimalda (2019) look at back-transfers by receivers to senders in trust games, as

an indicator of cooperation, with mixed results on the effects of exposure to violence on back-transfers.[1] The focus of these papers is different from ours: differences in levels of back-transfers by violence exposure is not the same as reciprocity. Higher back-transfers in violence-affected individuals can arise because of a general shift in pro-social preferences, a 'level effect', or because of a shift in reciprocity – a person's reaction in response to an action by someone else, a 'slope effect'. Most of the existing studies in post-conflict settings, surveyed in Bauer et al. (2016)'s meta-analysis, focus on the level effect: differences in unconditional behavior (i.e. whether agents are more or less pro-social in general, regardless of the actions of others). Contrary to the general message from Bauer et al. (2016), we find that violence does not change unconditional cooperation and unconditional aggression among adolescents in conflict, but it changes their sensitivity to other's people actions.[2]

Our study makes four main contributions relevant to the study of human behavior in conflict settings in economics and political science. First, as far as we know, this is the first study that, using a controlled experimental methodology, identifies what difference exposure to violence makes on reciprocal behavior, defined as conditional behavior: one's action in response to and as a function of the actions of others, independently from

[1]Gilligan, Pasquale and Samii (2014) and Bauer, Fiala and Levely (2017) find an increase in back transfers among subjects more exposed to war violence (Nepalese adults in war-affected communities in Gilligan, Pasquale and Samii (2014) and former soldiers in Uganda in Bauer, Fiala and Levely (2017)). Gilligan, Pasquale and Samii (2014) also elicit unconditional cooperation in a public-good game and find a positive association with violence exposure. On the contrary, Cassar, Grosjean and Whitt (2013) find a negative (yet insignificant) association between (self-reported) victimization and back-transfers among Tajik people. A negative (and significant) correlation is also found in Bogliacino, Gómez and Grimalda (2019) using a combined index from sender and receiver behavior among Colombians.

[2]Bauer et al. (2016)'s meta-analysis suggests an increased in cooperation in people exposed to violence in post-conflict settings. There are some important aspects that distinguish our study from the settings of the papers in Bauer et al. (2016)'s meta-analysis. First, all the papers in Bauer et al. (2016)'s meta-analysis, except that by Gneezy and Fessler (2012), come from post-conflict settings, with data often collected several years after the conflict ended. In post-conflict settings peace building, reconciliation and social cohesion initiatives are common, e.g. inter-group sport events (e.g. Mousa, 2020), and they can be targeted to areas where conflict was more intense. These strategies may help catalyze attitudes towards pro-sociality and cooperation and may in part explain the increased level of social group participation and community leadership observed in post-conflict settings. Such influences are less present in our setting of ongoing conflict. Second, our evidence come from two sets of games, public good games and vendetta games, that, while appropriate for understanding reciprocity, are not surveyed in the meta-analysis (only one study uses a public good game). The meta-analysis' evidence of increased (in-group) pro-sociality in experimental games (8 studies in total) comes from sharing games (2), envy games (2), trust games (4), dictator games (2), ultimatum game (1) and public good game (1).

possible changes in unconditional behavior.

Second, existing studies using lab-in-the-field experiments in post-conflict zones have adult samples. Twin heritability studies show that, while some heterogeneity in reciprocity across people is genetic, the role of environmental factors and life experiences in the development of heterogeneous reciprocal preferences is important (Cesarini et al., 2008; Kosse et al., 2020; Wallace et al., 2007; Zizzo, 2003). Adolescence is a key time for the development of reciprocal preferences and other social behaviors (Blakemore, 2018; van den Bos et al., 2011) and this explains why, as a first study, we find it optimal to focus on adolescents.

Third, by focusing comprehensively on the two domains of reciprocity – cooperation and aggression – we complement the existing evidence on the effects of exposure to political violence on social behavior, which focused exclusively on the cooperation side of behavior (Bauer et al., 2016). A design choice for a first study was to focus on the exposure of violence on reciprocity *among* Palestinian adolescents. This helps us to identify the effect of exposure to violence on reciprocity without having to worry about additional confounding effects. An example of these might be the presence of Israeli adolescents, should Palestinian adolescents specifically wish to retaliate towards them, interesting though undoubtedly this would be to explore in future research.

Fourth, we add to the existing literature by providing evidence on behavioral change from recent episodes of violence from a context of ongoing conflict. This distinguishes our paper from available evidence that is frequently collected several years after the end of the hostilities that are thought to produce behavioral change (e.g., see Table 1 in Bauer et al., 2016). This is an important contribution to advance our understanding of the legacies of political violence over time. While efforts to achieve conflict resolution incorporate a wide range of responses, knowledge of behavioral change induced by violence can inform the design of 'bottom-up' policy interventions aimed at fostering cooperation.

The remaining of the paper is as follows. 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.[3]

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

[3]Several studies in social psychology have highlighted how violence may affect a number of aggression-promoting cognitive schemas that reinforce violent responses. Examples are: increased perception of threat (Novaco and Chemtob, 1998), on which we focus; the endorsement of normative beliefs that aggression is acceptable (Guerra, Rowell Huesmann and Spindler, 2003) or an appropriate response to ambiguous peer provocation (Schwartz and Proctor, 2000); or the belief that little can be done to avoid violent confrontations (see Kirk and Hardy (2014). Changes in cognitive schemas have to do with individuals beliefs, which motivates our focus on beliefs about others in section 4.

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.[4] This effect may have neurocognitive bases. Neuroscience studies based on functional MRI[5] 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 typical inhibitory controls of aggressive behavior. In other words, the activation of hostile appraisal schemas strengthens aggressive reactions.[6] 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 been tested yet.

To further study the impact of exposure to violence on retaliatory behavior, we focus on 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

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

[5]Functional magnetic resonance imaging.

[6]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).

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.[7]

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.

3.1 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 and therefore are more exposed to episodes of violence. We use the exogenous variation in the obligation to regularly cross a military checkpoint to study the otherwise

[7] 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 empirical findings. 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.

endogenous effect of being exposed to violence on reciprocity. 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.[8]

Using the variation in checkpoint crossing to establish a causal link between exposure to violence and reciprocity rests on four assumptions: (i) the subjects cannot influence their own obligation to regularly cross a checkpoint, and one can therefore consider 'crossing a checkpoint' exogenous from the point of view of the adolescents. This implies that the school selection process is not influenced by the intention to avoid crossing a checkpoint. (ii) The location of checkpoints that our subjects might be crossing does not depend on pre-existing violence in the area, which might have already influenced reciprocity. (iii) Having an obligation to cross a checkpoint increases the likelihood of being exposed to violence. (iv) The hypothesized correlation between obligation to cross a checkpoint and reciprocity cannot be explained by a common factor causing both, rather than being causal.

We address each of these assumptions in turn.

(i) **Exogeneity of the obligation to cross a checkpoint and school selection.** Interviews with representatives of the Palestinian Ministry of Education indicated that it is customary for children to enrol in the nearest school to their locality of residence. This was confirmed by quantitative survey evidence. In an independent online survey open to all Palestinian students in grade 9 to 12 living in the West Bank, we collected information on whether the school is the closest school to their home among all schools, whether it is the closest offering the academic track the student wanted to study, or whether it is not the closest. Furthermore, we asked students to rank a list of reasons that influence the choice to attend their current school. The task gave students 10 points and asked them to allocate the points to any reason that affected their choice (or the choice of who decided for them): the highest the number of points allocated, the more important the

[8]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?".

reason was compared to others. The list included 7 reasons,[9] presented in random order, and the possibility for students to write their own reason.

The survey was accessed and completed by 5,732 students.[10] The school currently attended is the closest to home among all schools for 68% of students and the closest to home offering the desired academic track for 25%. Only 7% of students indicated the school is not the closest offering the desired academic track. Among this 7%, only 0.2% of students (9 out of 5,732) listed avoiding checkpoints as their single primary reason for choosing the school; and only 1.04% (60 out of 5,732) listed avoiding checkpoints as a primary reason tied with others, though within this set the average number of ties is 5.2, which implies 2 points at most were given to any one reason, suggesting a low importance.[11]

Overall, these figures indicate that checkpoint crossing was not what drove school choice for 98.9 to 99.8% of the student population, with the process of school selection being best predicted simply based on proximity to their home.

(ii) **The location of checkpoints does not depend on pre-existing violence.** The location of military checkpoints, at the discretion of the Israeli Defence Force, is likely to be targeted. A concern for the research design is whether checkpoints are purposefully located in areas that were more violent to start with or saw more episodes of violence in the past. If violence influences reciprocity and checkpoints are located in violent areas, we might be picking up initial differences in reciprocity.

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 and Palestinians. Simply sampling schools differently

[9]The list of reasons was: 1. The school is the nearest to your home; 2. Good school reputation; 3. Public transport to the school; 4. Avoid checkpoints; 5. Your older brothers have attended this school; 6. It's easy for my parents to get me to school; 7. Activities offered by the school; 8. Write your own reason; 9. Write your own reason.

[10]The survey was conducted in September 2020. At the time of the data collection of experimental tasks we did not have the budget to survey the entire sampling frame of students in the way we present here. In an attempt to keep the experimental sessions short and given the large anecdotal evidence that the process of school selection is based on proximity to the student home, we decided at the time not to include the questions of school choice and their reasons in the end-of-session-questionnaire.

[11]Among those who wrote their own reasons, none among the 7% mentioned the words 'checkpoint', 'occupation', 'barriers'; only 0.3% of students mentioned them in the entire sample.

distant from checkpoints is inappropriate since it might be prone to reverse causality of violence. Our sampling frame of schools is chosen to circumvent this potential exogeneity concern.

First of all, our sampling strategy only considers checkpoints 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 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.

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). Calculations on data from BTselem, an Israeli human rights organization, show that, out of all settlements pre-dating the First Intifada, 91% of existing Israeli settlements were built at least 2 years before the start of the uprising. This is supportive of our assumption that the location of Israeli settlements in our sample is unlikely to be related to the violence that occurred in later years. In the early 1990s, checkpoints followed the location of settlements: 71% of pre-1987 settlements had a checkpoint placed within 3km.[12] Historical records are also not consistent with the location of settlements being based on local cultures of reciprocity with Palestinian communities (e.g., Troen (2011) and B'Tselem (2012)). 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 these schools for which we are confident that the closeness by checkpoint was placed independently of the extent of violence during the Intifada, which could otherwise be a source of concern.[13]

[12]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 11). 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.

[13]Estimating the effects of mobility restrictions on the labour market, Fratto (2019) uses an identification strategy based on the fact that mobility restrictions in the West Bank are more likely to be placed near pre-existing Israeli settlements. These arguments support the choice of our sampling frame.

We had information from the Palestinian Ministry of Education on the location of 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.[14] The location of the sample schools can be found on a West Bank map provided in the online appendix (Figure 2).

(iii) **The obligation to cross a checkpoint increases the likelihood of being exposed to violence.** This is a testable assumption for which we observe evidence in our data (see Table 10 in the online appendix). Regularly crossing a checkpoint is correlated with higher exposure to direct violence, such as being hit or beaten, verbal abuse or being a witness of harm or frightening situations. The increased frequency of violence exposure among children regularly crossing checkpoints is quantitatively large and ranges from a 22% increase in reported verbal abuse to a 175% increase in reported threats of harm. There could be other, more subtle, forms of perceived psychological victimization, such as the frustration of being checked. As additional robustness analysis, the online appendix presents the effects of direct and individual measures of violence on reciprocity. That said, while validating our use of checkpoints as a change in exposure to violence, measures of individual violence exposure are not a good measure to directly analyze the effect of exposure to violence on reciprocity, due to their endogeneity.

(iv) **An alternative common cause**. If the hypothesized relationship between obligation to cross a checkpoint and reciprocity can be explained by a common factor causing both, we would have a correlation but not a causal link between the two. We control

Fratto (2019) identification uses village-level variation on the fraction of time spent on roads in 1997 (which would be an unsuitable instrument for exposure to violence in our subjects who are born after 1997). Our identification strategy differs as it is based on the individual's obligation to cross the mobility restrictions.

[14]The choice of a 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.

for specific potential common factors - such as migration, household income or gender - in our analysis in section 4 and where relevant in the online appendix. Section 4 also presents instrumental variable (IV) estimation as well as a general sensitivity test for omitted variables based on Altonji, Elder and Taber (2005). These tests support the robustness of our results. One might also postulate that higher frequency of being required to cross checkpoints may be explained by subjects who have stronger in-group identity, stronger social network, or more broadly greater pro-sociality. If any of these mechanisms were to hold, however, we would expect higher cooperation and lower aggression in our experiment, and, as it will turn out, this is not the case (see section 5 for a lengthier discussion).

Our research design lends itself to the following empirical bivariate Probit model that considers two binary outcomes simultaneously: each subject i's conditional behavior in the domain of cooperation (Y_{1i}) and aggression (Y_{2i}), which may be correlated. We define how we classify subjects as exhibiting conditional behavior in the two domains in section 4. The expected probability of the two outcomes is determined by:

$$E[Y_{1i}] = \Phi_{1i}[\alpha + \delta_1 must\ cross\ checkpoint_i + \beta_1' X_{1i} + \epsilon_{1i}] \quad (cooperation\ domain)$$
$$E[Y_{2i}] = \Phi_{2i}[\alpha + \delta_2 must\ cross\ checkpoint_i + \beta_2' X_{2i} + \epsilon_{2i}] \quad (aggression\ domain)$$

where *must cross checkpoint* is the variable that provides the exogenous variation in exposure to violence, X_1 and X_2 are vectors of socio-demographic characteristics including sex, age (in months), class size and a data collection period indicator and where the error terms, ϵ_{1i} and ϵ_{2i}, are jointly normally distributed with mean zero, variance 1 and correlation ρ. The identification comes from the exogenous variation in *must cross checkpoint*, whose parameter $\delta = (\delta_1, \delta_2)$ can be interpreted as an Intention-to-Treat effect, and the non-linear Probit functional form. The parameters are estimated by maximum likelihood.

3.2 Measurement: 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 and main data collection took place in September and October 2017 (*phase 2*: 40 schools).[15]

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 was the Voluntary Contribution Mechanism game; Part 2 was the vendetta game; Part 3 was a 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). The experimental instructions can be found in the online appendix, section B.

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:

[15]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 includes 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 those in Phase 1.

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

where c_i is subject i's 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.

Following Fischbacher, Gächter and Fehr (2001), 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 and employs a strategy method, that is each subject is 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 our set up, the average contribution from other group members can be a (integer) number from 0 to 5 tokens. In other words, the subject reports her own's contribution schedule given the other group members' average contributions. This helps us identify clear strategy profiles for all subjects in a one-shot setting, including specifically whether they are conditional cooperators or not.[16]

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 include a number of examples. To ensure thoughtful decisions, no time limit was imposed. The game was played once and subjects knew it.

[16]Since the experiment took place during school hours and could only be conducted using paper-and-pencil, the strategy method allowed us to collect more and better data in less time. In a survey of experimental evidence, Brandts and Charness (2011) found no evidence that treatment effects identified with a strategy method are not replicated with a direct response method.

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 (2014).

In this game, students are randomly paired with a co-player and the game proceeds sequentially across a finite number of periods.[17] 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 do not steal. Figure 1 illustrates the decision tree.

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". Stealing should not occur.[18] 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. Subject's stealing behavior in the First player role is used to classify *unconditional attackers.*[19]

We implement the game in strategy method, eliciting subjects' decisions at each node

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

[18]The equilibrium is the same if one assumes inequality averse preferences, linear altruism or moderate levels of spite: see Bolle, Tan and Zizzo (2014).

[19]The payoffs of the game were designed so that cooperating (in the VCM) and stealing (in the vendetta game) are best responses to cooperative or stealing actions respectively under a range of different preferences typically found in adolescents, such as inequality aversion and intention-based reciprocal preferences Almås et al. (2010); Fehr, Glätzle-Rützler and Sutter (2013). However, for self-interested agents the predictions in the two games are different: self-interested agents will be reciprocators in the vendetta games but not in the VCM. Section 5 contains a discussion of whether this is a problem for the interpretation of our findings: it turns out it is not, as Section 4 shows that the evidence rules out the possibility that observed changes in reciprocity are due to changes in the proportion of self-interested subjects as a result of crossing checkpoints. The different properties of the two games provides an additional dimension of robustness to our findings.

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 data collection) 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 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.[20] 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 (see Table 1 for descriptive statistics). We pool for the data from the two phases for the analysis, introducing a phase 2 indicator in the regression analysis. 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. The difference is quantitatively large: 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 10 in the online appendix). This provides support to our

[20]The experimental tasks were presented in fixed order to minimize enumerators' confusion during the sessions. No feedback on earned payoffs nor on the behavior of other players was provided during Part 1 and 2, thus minimizing the emergence of spillover or other order effects from the fixed sequence. Also note that, since we compare differences between crossers and non-crossers who were all subject to the same design, if any order effect were present, it would be constant between control and treatment group and therefore not affect our findings.

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 in 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 3, column 1). While a bivariate test suggests a difference in the frequency of unconditional attackers, this disappears in the regression analysis allowing for controls (see Table 3, 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.[21] 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. There are 33% of 'conditional attackers' in the sample.

We analyze reciprocal behavior in the domain of cooperation and aggression jointly

[21]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%. We also obtain similar results using a classification based on a Pearson's correlation coefficient greater than 0.5 as suggested by Thöni and Volk (2018). See Table 12 in the online appendix.

using a bivariate probit model.[22] 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 4.[23] 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 (=0.536-0.464) increase in conditional behavior (corresponding to a 15% increase from the control group's rate of conditional cooperation) and a 8.1 percentage points (=0.383-0.302) increase in retaliation (corresponding to a 27% increase from the control group's rate of retaliation).[24] These magnitudes are in line with previous results in different contexts and samples.[25]

A possible confounding variable in our analysis is movement into or away from the sampling sites, since migrants' preferences may differ from non-migrants'. We address these concerns using questionnaire data. Table 5 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).[26] 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

[22]While the Spearman's correlation coefficient obtained from the VCM game can be used in estimation as a measure of reciprocal behaviour, the vendetta game by construction leads to a binary indicator of reciprocal behaviour. Using a binary classification of reciprocal behavior for both games has the advantage of allowing joint estimation as well as comparability.

[23]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.

[24]The increase in frequencies of conditional cooperation is reflected in an increase in the value of the Spearman correlation between others' and own contribution among crossers (0.45) relative to non-crossers (0.38). We do not find significant gender differences in reciprocity behavior.

[25]For example, the increase of returned-transfers in Gilligan, Pasquale and Samii (2014)'s trust game among Nepalese adults in war-affected communities relative to not-affected communities is 7 percentage points. The results in Bauer, Fiala and Levely (2017) indicate that former soldiers in Uganda return on average 5 percentage points more in the trust games than their not-abducted peers.

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

other localities. Table 6 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 4 are robust to a number of additional robustness tests which we present in the online appendix. First, the results remain when we include a proxy for household income and an indicator for migrant subject (see Table 13).[27]

Second, our results maintain when we look at the impact of specific types of violence witnessed or directly experienced which we measured in our questionnaire. For the reasons explained in section 3.1, direct measures of exposure to violent incidents are plagued by endogeneity bias due to selection issues. The checkpoint crossing indicator minimizes this problem and provides estimates of the Intention-To-Treat effect (ITT) of violence exposure on behavior. An alternative strategy is to try to correct the endogeneity of violence exposure measures using systems of equations or two-stage methods and calculate the Average Treatment Effect on the Treated (ATT). Since our outcome is binary and the (endogenous) violence exposure regressor is also binary, we use a system of two probit regressions (bivariate probit) to correct for endogeneity.[28] One equation predicts violence exposure using the exogenous variation in checkpoint crossing and the second equation estimates the effect of violence exposure on reciprocal behavior. Table 17 in the online appendix presents the estimates from bivariate probit models for different types of violent incidents reported by the subjects. The results confirm our main conclusion: violence exposure predicts more reciprocal behavior. As expected, estimates of the ATT are bigger than the magnitudes of the ITT estimates in Table 4.

Third, as an additional general robustness test for unobserved endogenous variation, we instrument the obligation to cross a checkpoint using the distance (in km) from the

[27]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.

[28]Two-stage methods, like Instrumental Variable, are suited for linear models but do not yield consistent estimates in non-linear models with endogenous binary variables, as in our case.

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 (F test=5.15, p-value 0.000, see Table 14). 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 (Table 14).

Fourth, 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 estimates, shown in Table 15, 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.

Fifth, our findings are also robust to whether we control for the presence of a belief elicitation mechanism (Table 16).[29]

Sixth, 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 18 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 18 suggests that the increase in reciprocators among crossers broadly comes from all other categories. Tables 19 and 20 show that the increase in the number of reciprocators are drawn proportionally from the typology based on unconditional behavior. For example, the ratio between conditional attackers who attack when they are first movers, and those who do not, remains approximately 2.

Seventh, while small and insignificant when allowing for control variables, the dif-

[29]Table 16 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 (2014).

ference in the frequency of unconditional attackers in the vendetta game may however suggest the possibility that, under exposure to violence, stronger reciprocity is driven by self-interest in this game. Specifically, we may be concerned that having to cross checkpoints leads to a greater proportion of self-interested teenagers. The evidence however speaks against this possibility. First, Table 2 shows that in the VCM the percentage of self-interested adolescents (as proxied by the percentage of free riders) is virtually identical (within less than one percentage point) between adolescents who must cross checkpoints and adolescents who must not. Second, in the vendetta game a self-interested agent would be a conditional attacker but not an unconditional attacker. If a greater proportion of self-interested adolescents is what is driving our stronger reciprocity effect under exposure to violence, then Table 19 (in the online appendix) should not show, under exposure to violence, any increase in the percentage of conditional attackers for adolescents who are unconditional attackers, since self-interest cannot explain them being unconditional attackers. Instead, this percentage goes up from by 7 percentage points within this cohort. This increase is quantitatively as large as or larger than the increase (5 percentage points) in the percentage of conditional attackers we observe among adolescents who are not unconditional attackers. On both grounds, the evidence indicates that, when there is exposure to violence, the stronger reciprocity is not driven by increased self-interest. Our findings are therefore robust not only to the difference of domain (cooperation vs. aggression) of the VCM and vendetta games, but also to the asymmetry of theory predictions under self-interest for the two games.[30]

Eighth, as an additional sensitivity test for omitted variables, we compute the statistics suggested by Altonji, Elder and Taber (2005). These statistics indicate the 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

[30]It is also worth noting that, while reciprocal behavior is a best response under a wider range of preferences in the vendetta game than in the VCM, Table 18 in the online appendix shows no evidence that the proportion of reciprocators is higher in the vendetta game than in the VCM game. If anything, the reverse is true (33% vs. 49%), due to the pull that always taking away (adopted by 55% of subjects) has as a strategy in the vendetta game.

linear regressions. We compute the impact of checkpoint crossing without control, β_0, and 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 21 shows that the Altonji et al.'s ratios are 6.3 for conditional attackers and 20.7 for conditional cooperators. 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 attackers (conditional cooperators) to disappear. In the restricted sample of schools 'close by' pre-1987 settlements, these ratios are 3.9 for conditional attackers and -11.5 for conditional cooperators. 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 for conditional cooperators.

Table 7 shows that there is a positive relationship between one own's conditional behavior and the beliefs about the behavior of other players. Specifically, subjects who expect a comparatively high number of conditional attackers in the room are more likely to be conditional attackers themselves. Similarly, subjects who believe it more likely to interact with a conditional cooperator are similarly more likely to be conditional cooperators. Do changes in beliefs about the behavior of others provide a mechanism to explain 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 8 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 8 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 (since this is the only difference between the two groups in the counterfactual exercise). This counterfactual simulation exercise, reported in Table 22, 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 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 military checkpoints for Palestinian students in schools sampled based on distance from pre-1987 Israeli settlements. This strategy allows to overcome the challenge of finding exogenous variation in exposure to violence. The assumption that more violence occurs from regu-

larly crossing checkpoints is corroborated by a strong correlation between being required to regularly cross checkpoints and stated measures of exposures to violence. One would not be able to rely on the self-reported measures directly as measures of exposures to violence because individual stated measures of exposure to violence (such as verbal abuse) are likely to be endogenous.

Using a natural proxy for exposure to violence has advantages of external validity, while presenting limitations from the viewpoint of internal validity. In the previous sections we have considered how our identification strategy handles concerns of selection related to school choice (section 2: adolescents generally go to school near by), the 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) and checkpoint location (sections 3.1, 4: using pre-1987 settlements and, also, results are robust to just focusing on schools near pre-1987 settlements). The result that exposure to violence leads to more reciprocal behavior is robust to 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), potential omitted variables (section 4: results robust to controlling for household migration decisions, household income or presence of a belief elicitation mechanism), unobserved effects related to specific types of violence exposure and more general unobservable potential endogenous variation (section 4: results by type of violence episodes experienced or witnessed, IV approach and Altonji et al.'s ratio test), as well as the different games (section 4: same key finding with VCM and vendetta games, notwithstanding different theory predictions under self-interest as well as different domains in terms of cooperation vs. aggression).

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. Similarly, while this should be largely controlled for by our stress on *obligation* to cross checkpoints, one might hypothesize that adolescents who cross the checkpoints are more likely to be socially networked. However, both these inter-

pretations imply greater pro-sociality in the adolescents exhibiting this behavior. 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 these interpretations.

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: one could potentially argue that greater interaction with Israeli soldiers at checkpoints would lead crossers to feel stronger in-group identity than non-crossers. 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.

Being this a first case-study specifically studying reciprocity in a conflict context, replication of our results in other contexts will clearly be useful to provide additional support to the correctness of our interpretation.To further substantiate whether our results have wider validity beyond this particular setting, we re-analyzed data from a number of existing studies in conflict contexts. While our study is unique in adopting a VCM in strategy method and a vendetta game to specifically measure reciprocity, a handful of other studies have studied social preferences using 'trust games'.[31] We identify two published studies with accessible data that (i) use the strategy method so that behavior of Receivers *conditional* on Senders' behavior is observable; (ii) contain two distinguished groups with different levels of exposure to violence; (iii) include a sample of young people (younger than 25 years old). One study is conducted in Uganda (Bauer, Fiala and Levely, 2017); the other in Tajikistan (Cassar, Grosjean and Whitt, 2013). One additional study includes older subjects in Israel (Gneezy and Fessler, 2012). We re-analyzed these data by regressing the percentage amount that Receivers return to Senders on three key variables: the sum originally sent by Senders, an indicator for violence exposure and an interaction

[31]In the trust game, one player (the Sender) first decides how much of an endowment to give to the second player (the Receiver). This amount is multiplied by the experimenter and delivered to the Receiver who, in turn, decides how much of this increased endowment to return to the Sender (Berg, Dickhaut and McCabe, 1995).

term between these two variables (plus any additional control variables as in the original publication).[32] To allow for comparability with our results, we restrict the analysis to samples of young people, defined as subjects younger than 25 years old. These results are reported in Table 23 in the online appendix. The table shows an overarching pattern: conflict exposure increases the sensitivity in reciprocal interactions as indicated by a positive interaction term's coefficient. We are aware that there are differences from our setting and the number of observations in these studies is small. Nevertheless, this exercise provides indicative evidence that the behavioral pattern identified in this paper is present in other settings.[33]

Our results contribute to the debate about the formation of social capital in contexts of conflict. The economic literature so far has focused on cooperative behavior (as opposed to aggression and revenge) and highlights that the pro-sociality that emerges from wars is focused towards in-group interactions. Our results suggest that there might also be changes in revengeful behavior towards others.[34]

The existing literature has put forward competing theories concerning the explanations of the observed changes in cooperative behavior after war (changes in constraints and economic incentives; changes in beliefs; changes in norms; changes in preferences and psychological explanations rooted in personal growth), but empirical evidence remains scarce. Our results help to shed light on these explanations by providing some evidence in favor of a belief mechanism.

Specifically, in the domain of aggression, we find that subjects with an obligation to

[32]This analysis is different from the analysis reported in the original papers and summarized in the introduction. The analysis reported in Table 23 provides estimates of the *reciprocal* behavior of Receivers conditional on the amount received by Senders, in other words a *slope* parameter. This is different than estimating the difference in average amount sent or returned between victimized and non-victimized subjects, which is a *level* parameter. For example, Cassar, Grosjean and Whitt (2013) report that Receivers return on average a smaller amount if they are victimized (a negative level parameter). The re-analysis of data finds a positive reciprocity parameter: victimized Receivers return a higher amount when Senders send higher amounts. The two results are not in contradiction.

[33]The reader interested in older subjects should find Gneezy and Fessler (2012) of particular interest. Their conclusion with their sample is consistent with ours in finding enhanced behavioral reciprocity as a result of conflict exposure.

[34]A less related literature worthwhile mentioning is that on exposure to terrorism ((Berrebi and Klor, 2006, 2008; Getmansky and Zeitzoff, 2014; Gould and Klor, 2010; Grossman, Manekin and Miodownik, 2015)).

cross a checkpoint believe that there are more conditional attackers in the room compared to subjects without obligation. Some suggestive evidence indicates that this difference might explain around half of the observed change in conditional aggression behavior. The belief mechanism is consistent with an interpretation of our results based on psychologists Novaco and Chemtob's (1998) idea of enhanced sensitivity to perceived threats in traumatized people. Perceived enhanced salience of conflict or fear of future conflict could be part of this and be the result of exposure to violence. We do not find evidence of the same mechanism in the domain of cooperation and it is unclear why. 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.[35] 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.

More generally, these results suggest an increased potential for conflict resolution or entrenchment based on the feedback loops that follow from initial interactions involving new generations exposed to violence. The strengthening of reciprocal behavior implies that the initiating event in a two party interaction is key. It is therefore important to foster opportunities for initiating cooperative actions and to minimize the possibility of acts of aggression that are likely to invite retaliatory behavior.

[35] 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.

Figure 1: 'Vendetta' game (VG)

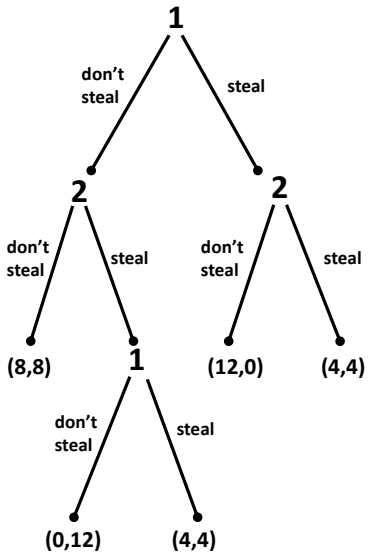

Table 1: Sample descriptive statistics by data collection phase

	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⋆**	**Total**
N	431	734		1,165
girl⋆	0.49	0.57	0.007	0.54
class size	27.32	27.60	0.538	27.49
migrant household⋆	0.20	0.17	0.190	0.18
income (index, PCA)	-0.03	0.02	0.665	0.00
total earnings (NIS)	32.15	32.41	0.704	32.32
mistakes in part 1	2.352	2.313	0.788	2.327
mistakes in part 2⋆	0.378	0.476	0.200	0.440
locus of control scale	2.99	2.92	0.416	2.94
social desirability scale	12.03	12.26	0.098	12.18
unconditional cooperation (mean tokens)	2.42	2.48	0.455	2.46
unconditional cooperators (%)	46.6	47.8	0.695	47.3
unconditional attackers (%)	71.2	76.7	0.038†	74.5

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; girls: 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 to the public pot in the VCM game; unconditional cooperators: percentage of subjects classified as unconditional cooperators in the VCM game (a subject is classified as an unconditional cooperator if they contribute at least 3 tokens in the unconditional decision, see Section 4); unconditional attackers: percentage of subjects classified as unconditional attackers (a subject is classified as an unconditional attacker if they steal tokens when playing in the First player role in the vendetta game, see Section 4). ⋆: 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 3.

Table 3: 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 $y_1 = 1$ (column 2) if the subject contributes unconditionally at least 3 tokens to the public pot and is thus classified as an 'unconditional cooperator'. $y_2 = 1$ (column 3) if the subject takes away tokens from the co-player when acting as first Player and is thus classified as 'unconditional attacker'. Standard errors are clustered at the school class level. $*p < 0.10$; $**p < 0.05$; $***p < 0.01$.

Table 4: 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. $*p < 0.10$; $**p < 0.05$; $***p < 0.01$.

Table 5: 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 the Part 3 questionnaire.

Table 6: 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. $*p < 0.10$; $**p < 0.05$; $***p < 0.01$.

Table 7: Beliefs about others and conditional behavior by crossing

Conditional cooperator frequencies by beliefs about others' behavior			
	Belief measure: As the average tokens in the public pot increase, 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 by beliefs about others' behavior				
	Belief measure: Believed number of conditional attackers in the class room			
	Few (<25%)	Some (25-50%)	Several (50-75%)	Very many (>75%)
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 cooperators and conditional attackers by different levels of beliefs about others. The measure of belief about others being conditional cooperators is the subject own's belief that other players increase their contribution while the average contribution increases. The measure of belief about others being conditional attackers is the subject's reported (believed) number of conditional attackers in the class room. Categories 'few', 'some', 'several' and 'very many' are based on the quartiles of the distribution of the number of conditional attackers in the class. 's.e.' stands for standard errors.

Table 8: Impact of crossing on subjective beliefs about others

	Conditional beliefs	
	Conditional Cooperator	**Conditional Attacker**
Must cross checkpoint	-0.077*	2.234**
	(0.043)	(0.877)
Class size	0.005	0.027
	(0.006)	(0.114)
Girl	-0.014	2.970*
	(0.079)	(1.486)
Constant	0.393**	-1.384
	(0.174)	(2.618)
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. $*p < 0.10$; $**p < 0.05$; $***p < 0.01$.

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Online Appendix

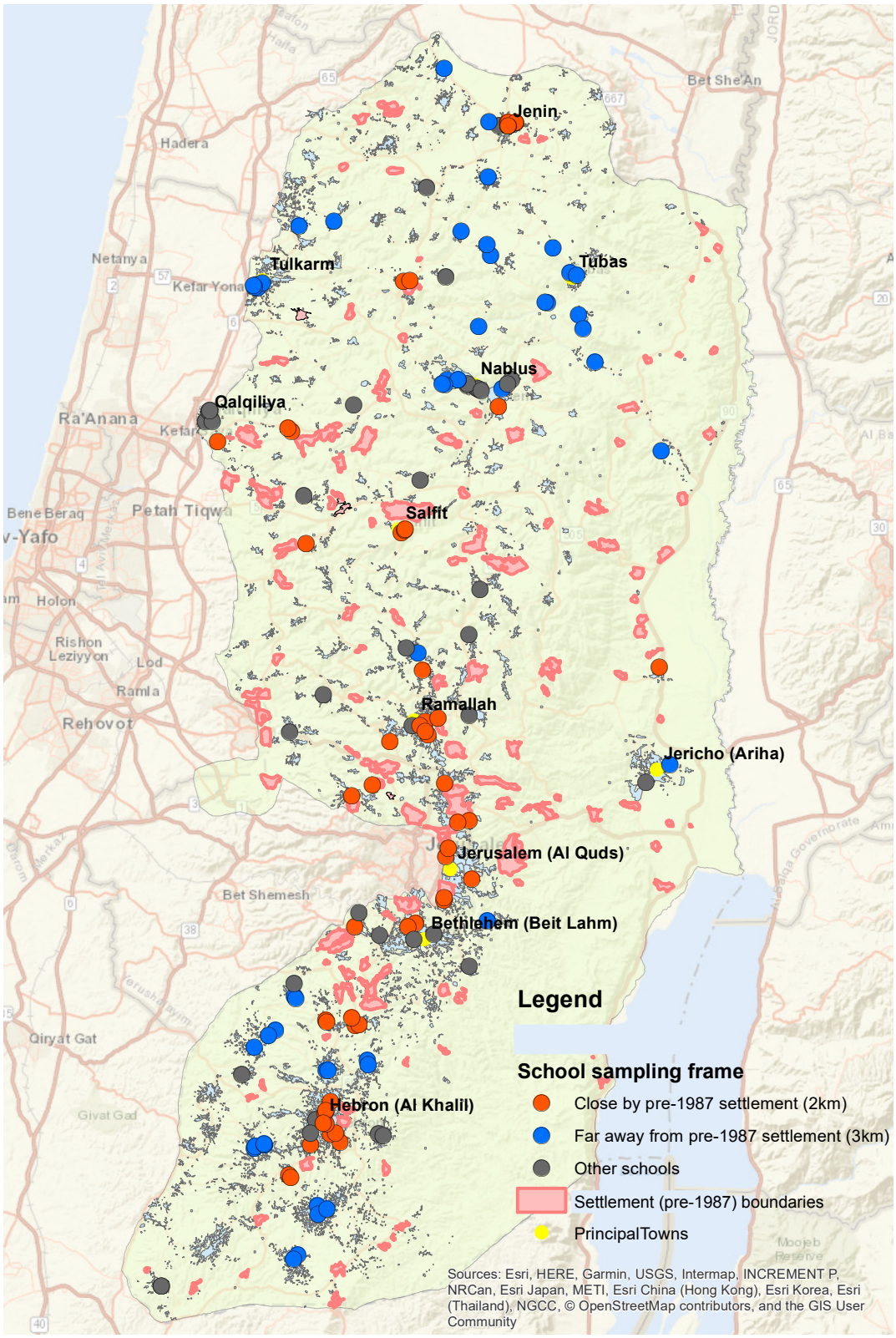

Figure 2: Sampling frame for school selection

Table 9: Location and reasons for choosing the current school

Panel A	
Is the secondary school you are currently attending the (geographically) closest school to your home? (n=5,732)	
Yes, the school is the closest school to my home among all schools	68.02%
Yes, the school is the closest offering the academic track I wanted	24.88%
No, the school is not the closest school offering the academic track I wanted	7.10%
Panel B: if the school is not the closest school:	
% reporting avoiding checkpoint among the top reasons (with ties)	14.7
average number of reasons ranked on top when avoiding checkpoint is selected (♯ ties)	5.02
% reporting avoiding checkpoint as the top reason (no ties)	3.4
number of students mentioning words 'checkpoint'/'barrier'/'occupation' while writing own reason	0

In Panel A, the table shows the percentage of students whose current school is closest, closest among the schools offering the academic track they wanted to study and not the closest school to their home. In Panel B, it shows frequencies of interest among the group of students whose school is not the closest to their home. The data come from a nationwide survey of students conducted by one of the authors. The sample of 5,732 includes only students from the West Bank region.

Table 10: Violence exposure by obligation to cross a checkpoint

	Hit, beaten up	Verbally abused	Threat of harm	Seen serious incident	Frightening situation
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 who do not have. The questionnaire module refers to the past year and violence incidents are self-reported.

Table 11: Number of people 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 average number of people in settlements 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 12: Robustness: alternative definitions of conditional cooperator (Spearman's correlation set at 10% significance level; Pearson's correlation)

	Spearman correlation (10%)		**Pearson correlation > 0.5 (20%)**	
	Conditional behavior		**Conditional behavior**	
	Conditional Cooperator	**Conditional Attacker**	**Conditional Cooperator**	**Conditional Attacker**
must cross checkpoint	0.115*	0.191**	0.195***	0.192**
	(0.071)	(0.088)	(0.070)	(0.096)
girl	-0.021	0.021	-0.124	0.021
	(0.091)	(0.136)	(0.085)	(0.143)
age	-0.005	0.004	-0.011	0.004
	(0.008)	(0.006)	(0.008)	(0.007)
class size	-0.006	-0.007	-0.005	-0.007
	(0.008)	(0.010)	(0.006)	(0.010)
phase 2 data (=1)	-0.035	-0.202	-0.041	-0.202
	(0.183)	(0.176)	(0.168)	(0.158)
constant	1.045	-1.154	2.418	-1.148
	(1.851)	(1.566)	(1.884)	(1.794)
ρ between equations	0.0587		0.0964	
	(0.066)		(0.051)	
N	1165		1165	

The table shows the estimates from a bivariate probit regressions with alternative definitions of 'conditional cooperator'. Column 1 categorizes $y_1 = 1$ 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). Column 3 categorizes $y_1 = 1$ if the subject is classified as a 'conditional cooperator' using a Pearson' correlation greater than 0.5 at 20% significance level. The 'conditional attacker' classification remains unchanged: $y_2 = 1$ if the subject is classified as 'conditional attacker'. Standard errors are clustered bootstrapped at the school class level (100 replications). $*p < 0.10$; $**p < 0.05$; $***p < 0.01$.

Table 13: Robustness: adding income and indicator for migrant subject

	Unconditional behavior		**Conditional behavior**	
	Contribution $\geq$ 3	**Attacking**	**Conditional Cooperator**	**Conditional Attacker**
must cross checkpoint	-0.0241	-0.1692	0.1724**	0.1816*
	(0.087)	(0.106)	(0.081)	(0.101)
girl	-0.0889	0.1236	-0.1343	0.0266
	(0.109)	(0.144)	(0.088)	(0.143)
age	0.0032	-0.0022	-0.0107	0.0052
	(0.006)	(0.007)	(0.007)	(0.006)
class size	0.0033	-0.0047	-0.0055	-0.0073
	(0.006)	(0.011)	(0.006)	(0.010)
maindata (=1)	0.1838	-0.0454	-0.1295	-0.1729
	(0.174)	(0.235)	(0.140)	(0.145)
moved since 1986 (=1)	-0.0743	-0.1898	-0.1740*	0.0543
	(0.088)	(0.145)	(0.100)	(0.076)
income (PCA)	0.0430***	-0.0133	0.0129	0.0084
	(0.016)	(0.025)	(0.020)	(0.024)
constant	-0.9062	1.3189	2.5192	-1.2584
	(1.327)	(1.484)	(1.536)	(1.291)
obs	1124		1124	

The table shows the estimates from bivariate probit regressions with $y_1 = 1$ if the subject is classified as a 'unconditional cooperator' and $y_2 = 1$ if the subject is classified as 'unconditional attacker' (under columns 'Unconditional behavior') and 'conditional cooperator' and 'conditional attacker' (under columns 'Conditional behavior'). Standard errors are clustered at the school class level. $*p < 0.10$; $**p < 0.05$; $***p < 0.01$.

Table 14: Instrumenting checkpoint crossing: IV regressions

	IV regression (LPM)	
	First stage: must cross checkpoint	
distance to nearest checkpoint (km)	-0.0751***	
	(0.016)	
distance (squared)	0.004***	
	(0.001)	
F test (2, 1158)	22.52***	
F test's p-value	0.000	
	Conditional behavior	
	Conditional Cooperator	**Conditional Attacker**
(IV) must cross checkpoint	0.163	0.484***
	(0.135)	(0.157)
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	1.460***	-0.1073
	(0.597)	(0.659)
obs	1165	1165

The table shows the results of an IV (linear) regression in which the obligation to cross a checkpoint is instrumented with the distance (in km) between the school attended and the nearest checkpoint. Standard errors are calculated by clustered bootstrapping (150 replications) at the school class level. $*p < 0.10$; $**p < 0.05$; $***p < 0.01$.

Table 15: 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. $*p < 0.10$; $**p < 0.05$; $***p < 0.01$.

Table 16: 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 attacker', including a control variable for the presence of belief elicitation questions. Standard errors are clustered at the school class level. $*p < 0.10$; $**p < 0.05$; $***p < 0.01$.

Table 17: Impact of violence exposure measures on reciprocity: bivariate probit estimates

	Hit, beaten up		**Verbally abused**		**Threat of harm**		**Seen serious incident**		**Frightening situation**	
	PG	VG	PG	VG	PG	VG	PG	VG	PG	VG
Eq 1: Predicting violence exposure using checkpoint crossing										
Must cross checkpoint	0.3194***	0.3345***	0.1852***	0.1973***	0.5398***	0.5742***	0.4038***	0.4097***	0.5616***	0.5757***
	(0.063)	(0.088)	(0.054)	(0.068)	(0.117)	(0.107)	(0.087)	(0.078)	(0.094)	(0.099)
Girl	-0.3754***	-0.3651***	-0.2592***	-0.2597***	-0.7000***	-0.6670***	-0.6937***	-0.6951***	-0.1657	-0.1544
	(0.102)	(0.104)	(0.086)	(0.086)	(0.137)	(0.145)	(0.088)	(0.087)	(0.105)	(0.108)
Age	-0.0068	-0.0114	-0.0025	-0.0041	-0.001	-0.0001	-0.0147*	-0.0152**	0.004	0.0016
	(0.008)	(0.009)	(0.005)	(0.006)	(0.01)	(0.012)	(0.008)	(0.007)	(0.007)	(0.007)
Class size	-0.0079	-0.0075	-0.0032	-0.0031	-0.0088	-0.0102	-0.0091	-0.009	0.001	0.0003
	(0.006)	(0.006)	(0.006)	(0.006)	(0.008)	(0.008)	(0.006)	(0.006)	(0.009)	(0.009)
Phase 2 data	0.0382	-0.005	-0.0716	-0.1049	-0.0992	-0.0814	-0.0454	-0.0316	0.144	0.1349
	(0.116)	(0.146)	(0.148)	(0.156)	(0.199)	(0.207)	(0.132)	(0.123)	(0.184)	(0.182)
Constant	0.7214	1.707	0.4438	0.797	-0.9505	-1.1357	3.1959*	3.2774**	-2.1999	-1.6841
	(1.681)	(1.944)	(1.156)	(1.463)	(2.327)	(2.678)	(1.873)	(1.63)	(1.556)	(1.656)
Eq 2: Impact of violence exposure on behavior										
Violence exposure (see columns)	1.4784***	1.0051***	1.4090***	1.4554***	0.9246**	1.3232***	0.9927***	1.1702***	1.1717***	0.8210**
	(0.198)	(0.369)	(0.085)	(0.115)	(0.365)	(0.355)	(0.347)	(0.233)	(0.316)	(0.353)
Girl	0.0427	0.0973	0.0577	0.1545	-0.0752	0.0973	0.121	0.3058**	-0.0944	0.0343
	(0.097)	(0.14)	(0.086)	(0.104)	(0.094)	(0.141)	(0.155)	(0.135)	(0.095)	(0.136)
Age	-0.0066	0.0068	-0.0079	0.0051	-0.0121*	0.0057	-0.0073	0.0092*	-0.0125*	0.0051
	(0.007)	(0.006)	(0.005)	(0.006)	(0.007)	(0.005)	(0.006)	(0.005)	(0.007)	(0.006)
Class size	-0.0005	-0.0046	-0.0019	-0.0027	-0.0034	-0.0048	-0.0014	-0.002	-0.0045	-0.0054
	(0.006)	(0.009)	(0.006)	(0.008)	(0.006)	(0.009)	(0.006)	(0.008)	(0.007)	(0.009)
Phase 2 data	-0.0914	-0.2029	-0.0364	-0.0981	-0.1112	-0.1748	-0.098	-0.1527	-0.1496	-0.2143
	(0.131)	(0.152)	(0.122)	(0.139)	(0.132)	(0.151)	(0.138)	(0.133)	(0.125)	(0.154)
Constant	1.2047	-1.7237	1.1594	-1.8415	2.6606*	-1.4423	1.1804	-2.6695**	2.7218*	-1.2685
	(1.479)	(1.332)	(1.18)	(1.363)	(1.518)	(1.227)	(1.509)	(1.273)	(1.543)	(1.29)
N	1163	1163	1161	1161	1162	1162	1161	1161	1148	1148
Frequency of violence	0.18		0.38		0.07		0.37		0.14	
Pr(y=1| no violence exposure)	0.408	0.2863	0.2978	0.1982	0.4689	0.3105	0.3556	0.2288	0.42999	0.3011
Pr(y=1| violence exposure)	0.8924	0.6625	0.8079	0.7218	0.8106	0.7936	0.7225	0.6262	0.8407	0.617
ATT	0.4844	0.3762	0.5101	0.5236	0.3417	0.4831	0.3669	0.3974	0.41071	0.3159

The table shows from bivariate probit regressions of equation 1: $y_2 = 1$ if the subject is classified as 'conditional cooperator' (column PG)/'conditional attacker' (column VG) and equation 2: $y_2 = 1$ if the subject reports to be exposed to at least one violence incident in the last year. Standard errors are clustered at the school class level. Pr(Y=1| no violence exposure) indicates the marginal probability of being a 'conditional cooperator'/'conditional attacker' ($y = 1$ in equation 1) given the subject is not exposed to violence. Pr(y=1| violence exposure) indicates the marginal probability of being a 'conditional cooperator'/'conditional attacker' given the subject is exposed to violence. ATT indicates the estimated Average Treatment effect on the Treated (the differences in marginal probabilities).

Table 18: 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 19: Vendetta Game: Classification by unconditional behavior and obligation to cross a checkppoint

	Must not cross a checkpoint					Must cross a checkpoint				
Unconditional attacker (i.e., when 1st mover)	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 choice whether to attack when first mover, 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 20: VCM Game: Classification by unconditional behavior and obligation to cross a checkpoint

	Must not cross a checkpoint						Must cross a checkpoint					
Unconditional contribution	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 contribution choice 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 21: Omitted variable bias: Altonji (2005) ratios

	Conditional attacker	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 4 of the paper.

Table 22: 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. $*p < 0.10$; $**p < 0.05$; $***p < 0.01$.

Table 23: Re-analysis of existing data from conflict contexts

	Bauer, Fiala and Levely (2017)	**Bauer, Fiala and Levely (2017)**	**Cassar et al. (2013)**	**Cassar et al. (2013)**	**Gneezy and Fessler (2012)**
Country	Uganda	Uganda	Tajikistan	Tajikistan	Israel
Interaction term: violence exposure × amount sent by Sender	0.287*	0.330***	0.121	0.162	0.661** ♭
(standard error of coefficient)	(0.067)	(0.000)	(0.509)	(0.406)	(0.011)
Control variables	Yes	Yes	Yes	Yes	No
Definition of youths	$\leq$ 20 yrs old	$\leq$ 25 yrs old	$\leq$ 25 yrs old	$\leq$ 25 yrs old	senior subjects♮
Number of observations	72	169	67	67	50
Violence exposure definition	Length of abduction (in years)	Length of abduction (in years)	Injured or killed	Injured and killed	Israel-Hezbollah war period

The table shows the coefficient of interest of a regression analysis of existing data from different conflict contexts. The dependent variable is the fraction (in percentage) of the amount Receivers return to Senders (divided by the amount received from Senders after multiplication by the experimenter). The explanatory variables are: the amount originally sent by Senders, an indicator for violence exposure, an interaction term between these two variables (shown in the table) and the control variables included in the original study if there were any (indicated by a Yes in the row 'Control variables'). The coefficients' standard errors are clustered at the individual level. The row 'Definition of youths' specifies the age range adopted in the re-analysis of the data; the range is selected based on considerations of comparability with our study and sample size considerations. ♭: the estimated coefficient differs from the value of 0.024 reported in Gneezy and Fessler (2012) because in the original study the observations are standardized. ♮: the original study does not provide age information. $*p < 0.10$; $**p < 0.05$; $***p < 0.01$.

Online Appendix - Section B

This section presents the survey questions on school choice and the experimental instructions. The survey was conducted in September 2020 and received IRB approval from King's College London (LRS-19/20-19495). The experiments were conducted in March (pilot) and September-October 2017 and received IRB from Qatar University and King's College London (QU-IRB 628-EA/16).

Survey on school selection: questions