



Crime-related exposure to violence and prosocial behavior: Experimental evidence from Colombia

Francesco Bogliacino^{a,*}, Camilo Gómez^b, Gianluca Grimalda^c

^a Università degli Studi di Bergamo, Via dei Caniana, No 2, Bergamo, Italy

^b CERGE-EI, Czechia

^c Kiel Institute for the World Economy, Germany

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ABSTRACT

Victims of violence appear hypersensitive to cues and their brain reacts to triggers as if the past events were happening in the present. We assess to what extent recalling these negative experiences increases prosociality. We conduct two artefactual field experiments in Bogotá (Colombia) to test this hypothesis. Our methodological strategy is to experimentally manipulate the recall of violence, either through a direct question or through a monetary loss in participants' experimental endowment. We interact these treatments with the degree of exposure to violence. We find that victims recalling experiences of urban violence act more prosocially in terms of trust, trustworthiness, and cooperation. The increase in prosociality favors residents in the same city district as the participant (ingroup bias). However, the ingroup bias holds in trust decisions but not in cooperation games decisions.

1. Introduction

Collective and interpersonal violence represent a major hindrance to economic development and individual wellbeing. According to the Institute for Economics & Peace (2019), violence costs world economies the staggering amount of \$14 trillion in 2018, equivalent to 11.2% of world GDP (Hoeffler, 2017 obtains similar estimates). This sum includes both the direct costs of violence – in terms of human lives lost, lost earnings, capital destruction, psychological trauma, etc. – and the opportunity costs of allocating financial resources to violence prevention rather than more productive activities such as investment in education or business development. The same Institute estimates that every one index-point improvement in their Positive Peace Index is associated with a tenfold rise in GDP per capita (Institute for Economics & Peace, 2022). In countries having been affected by persistent civil war such as Colombia, violence has been estimated to cost 25% of GDP during the 1990s (Institute of Medicine & National Research Council, 2012).

Violence is likely to create negative feedback loops, because GDP losses create poverty and inequality, and these in turn increase crime rates and homicides (Ouimet, 2012; Pare & Felson, 2014; Rennó Santos et al., 2017). Moreover, violence can have long-term effects in reducing cognitive skills, thus impairing individual productivity (Bogliacino et al., 2017).

The impact of violence is not limited to its material consequences or psychological trauma, but may also affect individual social attitudes. Perhaps surprisingly, extensive empirical evidence has shown that exposure to warfare and armed conflict strengthens community spirit and increases prosociality. Prosociality has been defined as the willingness to sacrifice individual interests for the benefit of others. Prosociality manifests itself in donating individual resources to collective causes, paying taxes to help needier citizens, following social norms when they are not in one's interests, and more generally partaking in cooperative behavior in social interactions (Bowles & Polania-Reyes, 2012). These characteristics contribute to what has been termed social

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* Corresponding author.

E-mail address: francesco.bogliacino@uni.bg.it (F. Bogliacino).

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capital (Putnam, 2000), which in turn has been shown to be positively correlated with economic development (Knack & Keefer, 1997; Guiso et al., 2008; Tabellini, 2010). Therefore, violence may also have long terms consequences through its effect on prosociality and social capital.

A meta-analysis by Bauer et al. (2016) over 23 different studies (mostly war-related) shows that people exposed to violence are more willing to engage in voting and participation in civic-oriented activities, have greater respect for social norms, and act more cooperatively in experimental games. Although causal interpretation is always debatable with naturally occurring variations, these studies use a variety of plausible research designs such as controlling for a vast array of observables, using instrumental variables, or performing sensitivity analysis. This plausibility prompts the authors of these studies to claim that Exposure to Violence (ETV) causes heightened pro-sociality. A reverse causal link would occur if, for instance, pro-social individuals were more likely to be targeted by violence.

Within those outcomes, trust is of particular interest. In fact, within the meta review by Bauer and coauthors, the impact of exposure to war is positive when trust is measured in the investment game by Berg et al. (1995) but null when the level of trust is measured through validated questions in surveys. Additional contrasting evidence is brought by studies in Uganda, Tajikistan and Spain (Cassar et al., 2013, Rohner et al., 2013, Valencia and Tur-Prats, 2020), where the authors found negative effects, but in one of them, they measure trust in institutions (Spain) and in the other, the main sample includes ex combatants (Tajikistan) where victimhood confounds with having been a perpetrator.

A second general conclusion of Bauer et al. (2016)'s meta-analysis is that pro-sociality is parochial, i.e., it favors members of one's own in-group, at the expense of the out-group. The social psychology literature conceptualizes an 'in-group' as a group to which individuals perceive they belong to, on the basis of some communality of fate, culture, and heritage (Romano et al., 2021; Weisel & Böhm, 2015). Such a sense of belonging is associated with the perception of a shared identity and attachment (Brewer, 1999). The definition of the out-group is inconsistent across the studies surveyed in Bauer et al. (2016). This aspect may not be problematic, though, as a meta-analysis by Balliet et al. (2014) shows that having a salient outgroup is not necessary to induce ingroup favoritism.

One peculiar aspect of exposure to violence is that the memory is "stored" in the body. Victims of traumatic events and patients diagnosed with PTSD are hypersensitive to cues that may remind them of their traumatic episodes, and once the memory kicks in, the brain reacts as if the traumatic event were happening in the present (Van Der Kolk, 2014). This may lead to behavioral consequences on decision making well beyond the original exposure. The first question we address in this paper is whether reminding victims of violence of their negative experience affects their other regarding behavior. We focus on victims of crime and not war. This question is relevant because the cost of interpersonal violence, that is, homicides and assault, are estimated to be about 7.5 times higher than the cost due to war and terrorism (Hoeffler, 2017). Moreover, we focus on urban violence in Colombia, a country with relatively high levels of both collective and interpersonal violence. A second research question is whether prosociality is parochial even in the context of urban violence as it is for war-related conflict. While warfare strengthens the demarcation between ingroup and outgroup to the highest possible degree, it is not obvious that the same will happen with urban violence.

Why does recalling exposure to violence may lead to prosociality? The literature claiming a causal effect of war on cooperation suggests different channels.

The first of such mechanisms proposed by Bauer et al. (2016) is what may be called a "rational" channel in terms of preferences, beliefs, and constraints. Lower opportunities to invest in physical and human capital in the aftermath of war induce rational agents to invest more in social capital (Knack & Keefer, 1997; Putnam, 2000), making cooperation the

equilibrium response. Furthermore, evolutionary models suggest that parochial norms are more likely to promote survival under group competition. As a result, complying with such norms increases individual fitness, making the norms more likely to be transmitted through social learning or intergenerational acculturation processes. In some versions of these models, called gene-culture co-evolutionary models, all pro-social norms are more likely to be shared (Böhm et al., 2016). Finally, a psychological explanation posits that survivors of a conflict are more optimistic about their future after recovery, thus becoming more likely to engage in communitarian activities.¹

It may be argued that both the social capital and the psychological mechanism may apply to both exposure to war and exposure to urban violence, with possible differences in intensity and duration of exposure. In general terms, war and conflict are likely to be more endangering than urban crimes, thus providing stronger psychological traumas. Nevertheless, urban violence can also be life-threatening and be highly traumatic. Both the duration of war and the police control over criminal gangs are highly variable, thus it is unclear which of the two forms of violence lasts longer. Overall, even if exposure to war conflict or urban violence may differ in intensity and duration, such two mechanisms seem to be applicable to both contexts, albeit with a difference in degree. Whether an evolutionary explanation carries over from exposure to war to exposure to crime is questionable. It is not implausible though: if our brain evolved to react to a threat, it is unlikely that the physiological reaction differs because of a variation in the source of the threat. One key difference between crime and war victims is the fact that the former often puts one's in-group in direct conflict with an out-group (Hardin, 1995; Sumner, 1906), while this tension between in-group and out-group is normally absent for crime-related ETV. This suggests that there is a stronger case to observe ingroup bias in war-related ETV than in crime-related ETV.

To accomplish the first goal of our paper we collect, as outcomes, measures of trust, cooperation, and altruism as indicators of prosociality in laboratory experiments involving victims with different degrees of ETV. We ask people to recall negative experiences of fear and anxiety, cueing violence as one of the possible occurrences. We manipulate the text of the recall to have a meaningful control condition in terms of emotional state. Additionally, we compare the effect of recall on participants who were highly exposed (defined as above the median of exposure within the sample) with respect to the effect on those that score low in terms of exposure.

The plausibility of our results rests on two premises. First, we assume that the random administering of the recall of fear and anxiety makes violence salient and induces a psychological state akin to the original experience (Lerner et al., 2003). The second assumption states that, except for recalling trauma, the additional consequences of recalling negative experiences are negligible in terms of our outcomes, and thus balance out in the two groups, the highly exposed and the lowly exposed. To increase the plausibility of this assumption, we do not split between victims and non-victims (extensive margin) but consider instead as comparison group those whose victimization score falls below the median (which in our sample is strictly positive), thus reducing the degree of heterogeneity across groups.² Additionally, we use controls at the level of the individual and of the district of residence to have meaningful comparisons along the margin of exposure. As a proof of

¹ Vélez et al. (2016) propose an alternative channel using Structural Equation Modelling, based on the perception of insecurity. Although the argument is mainly data driven, it is reasonable to assume that the perception of insecurity shapes the expected trustworthiness of the strategic counterpart, leading to change in pro-social behavior.

² It also weakens the concern for Social Desirability Bias in the self-reported exposure to violence. Since most of the participants have a non-zero exposure, in principle misreporting should systematically vary over the support of the variable.

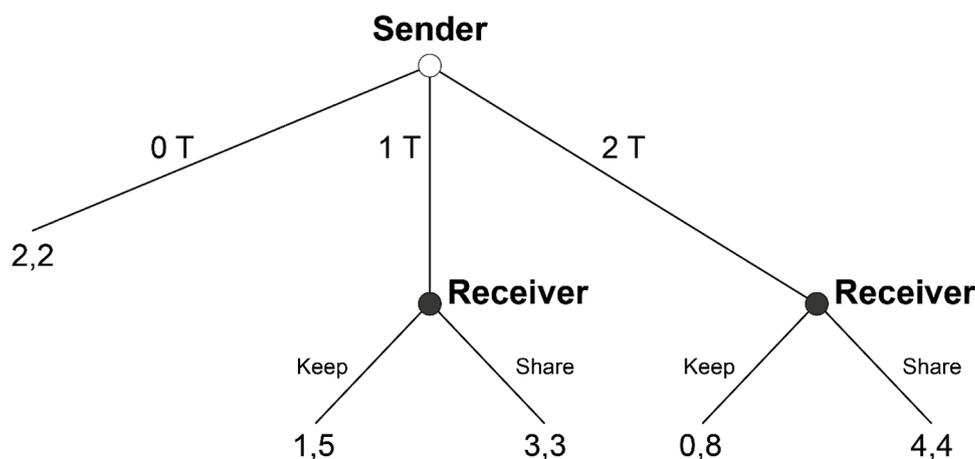


Fig. 1. The stage game.

concept, we will provide some support for both using the actual responses to the recall.

Our first study is an artefactual field experiment (Harrison & List, 2004) with a sample of young people covering nearly all districts of Bogotá, Colombia. Since urban ETV differs substantially across districts, this ensures considerable variation in ETV. We measure ETV through a questionnaire and we randomly administer either a recall of an experience that generated fear or anxiety (Fear and Anxiety Recall), or an emotionally neutral experience (Neutral Recall). We then split the sample according to the participants' degree of ETV and the assignment to the Recall. In this experiment, participants interact in a trust game (Berg et al., 1995).

Based on the results of the first experiment, we designed a second experiment where we manipulated the salience of violence through two different channels. The first manipulation is again the direct fear and anxiety recall, while the second is the re-experience of a loss of wealth, which is associated with many instances of crime. The loss of material assets poses severe constraints and can permanently affect the victims' wellbeing as well as their households' (Ibáñez & Moya, 2010; Moya & Carter, 2019). Experiencing losses induces negative emotions (Delgado et al., 2006a, 2006b) and can work as an emotional recall. The sample includes young participants from Bogotá, covering all districts, both university students and non-students. The measurement of self-reported ETV is performed as in the previous experiment. In this second experiment, participants make decisions in a prisoner dilemma (PD), as a measure of cooperation.

As for our second research question concerning the parochial bias in prosociality, we already noticed that group belonging is less salient in an urban environment. Generally, the "community" cannot be invoked to discriminate between locals and outsiders. Nevertheless, Falk and Zehnder (2013) did find significant in-group bias at the district level in Zürich, Switzerland. One can then conjecture that ingroup bias may also be relevant in an urban context and be affected by exposure to crime. Using a similar design to Falk and Zehnder (2013), we collect district-contingent decisions in both experiments to analyze whether the in-group bias varies by ETV.³

This article makes several contributions to the existing literature. Although a sizable literature studying victims of war conflicts exists, the evidence for victims of crime is limited and inconclusive, mainly because it is descriptive in character and lacks a precise identification strategy. While some contributions document a positive relationship

³ We also measure beliefs, social norms, aspirations, and perception of insecurity, to assess the mediating mechanisms proposed in Bauer et al. (2016). We perform this analysis in both our experiments. We discuss the evidence on the mechanisms in the Supplementary Online Materials (SOM).

between urban ETV and prosociality (Bateson, 2012; Dorff, 2017; Kochel, 2018; Rojo-Mendoza, 2013), other works find the opposite or argue for lack of any relationship (Littman et al., 2019; McMahon et al., 2013). We provide evidence focusing on recalling violence and through two different experiments.

A large multidisciplinary literature on the social psychology of group identity exists (Tajfel & Turner, 1986), as well as large experimental work on ingroup bias (Charness et al., 2007; Chen & Li, 2009; Hargreaves Heap & Zizzo, 2009; Ruffle & Sosis, 2006). Our contribution consists in discussing a potential new source of ingroup bias and assessing a methodological strategy to measure in group in urban contexts.

This paper adds to the literature on priming in economics. Economists relied extensively on priming to understand the impact of group identity (Bicchieri et al., 2019; Burszty et al., 2020; Kranton et al., 2020 just to name a few recent papers): the priming increases the weight of the social identity while the subject makes the decision. In general, Cohn and Maréchal (2016) suggest that priming allows researchers to perform causal inference of environmental factors when manipulation is either unethical or impractical. Violence is clearly an instance where the latter claim applies: Callen et al. (2014) and Bogliacino et al. (2017) use priming to understand the impact of violence on risk preferences and cognitive control. This manuscript will return in Section 5 below to some of the critiques to priming, such discussion is relevant to interpret the results that we are presenting in this paper.

2. Experiment I

2.1. Experimental design

We run our first experiment in Bogota. Although currently less violent than in the past, the capital of Colombia is still a dangerous place to live in comparative terms and provides a significant variation in terms of exposure to violence across its territory. In fact, according to data from the National Statistical Office (DANE), in 2014 it had 16 homicides per 100 thousand inhabitants, but in part of the city it can be even five times higher.⁴ Administratively, it is divided into 20 districts; one of which is completely rural. Districts are large administrative units, subdivided in neighborhoods. A map of the districts is in SOM Fig. A1.

We recruit participants aged 18–24 from the urban districts. We

⁴ In 2019, when we run Experiment II, the rate decreased two points, withholding the same level of heterogeneity. Most of High Income Countries have rates well below 10. In the US, one of the most violent cities is Chicago which in 2014 and 2019 had slightly higher rates than Bogotá (17 and 18, according to the Police Department).

Table 1
Experimental conditions and sample: Experiment 1.

	Fear and Anxiety Recall	Neutral Recall
TPP	N = 92	N = 125
No TPP	N = 30	N = 34

restrict age to this range because it reduces the likelihood of self-selection into a neighborhood.

The stage game is a version of the trust game (TG) by Berg et al. (1995) and proceeds as follows (Fig. 1): A sender and a receiver are endowed with two tokens and interact in a sequential game. Each token is worth 4000 Colombian pesos (COP, around USD 2.00 at the time). In the first stage, the sender can send either zero, one or two tokens to the receiver. The amount sent is tripled by the experimenter. The receiver's choices (trustworthiness) are collected through the strategy method (Selten, 1967). For each possible amount sent by the sender (one or two tokens), the receiver can decide whether to send back. If the receiver sends back, then the two parties end up with an equal number of tokens (three each in the first case and four each in the second case).

Each party plays in turn as sender and receiver, the order being randomized at the individual level. One of the two choices is randomly selected with equal probability and determines the final payoffs. The matching is perfect stranger; that is, no one interacts with the same person twice.

While acting as a sender, the participant makes contingent decisions on the (urban) district of the receiver for a total of 19 decisions. We use participants' own answers to the questionnaire to assign them to a district.

We have a 2 × 2 design. One treatment is the Fear and Anxiety Recall versus the Neutral Recall. In the former, participants are asked to recall an experience generating fear or anxiety that occurred during last year, whereas in the latter we ask for a neutral experience that has occurred. The other treatment is given by third-party punishment (TPP) or its absence (no TPP). TPP is instrumental to measuring the existence of social norms of anti-social or pro-social punishment.⁵ Randomization of treatments occurs at the session level. We summarize the design in Table 1, with sessions and participants per condition.

The Fear and Anxiety/Neutral Recall occurs just after explaining the stage game and prior to making the first decision. Participants are prompted to write down a report of their experiences and are given five minutes to do so. The prompts are the following and are adapted from Callen et al. (2014):

- We are interested in understanding your daily experiences. Could you describe an event from the past year?
- We are interested in understanding your daily experiences that may make you fearful or anxious. This could be anything; for example, getting sick, experiencing violence, losing a job, etc. Could you describe one event in the past year that caused you fear or anxiety?

There are two issues worth noting for the priming: (a) the neutral priming does not evoke daily experience to avoid generating any emotion. In other words, it is a real placebo. (b) The fear and anxiety recall refers to fear and anxiety which may not be related with violence, but we look precisely at the interaction with victimization. Notice that the experiment is conducted pen and paper and participants can write freely as the response is not linked to their code.

In the TPP sessions, we recruit 50% more participants and randomly assign one third of them to another room to play the third-party role. Third parties decide how much to spend from their endowment to reduce the endowment of the receiver in the TG, conditional on the choices being made. The punishment has a multiplier of five, that is, for

every token spent by the third-party, the receiver's payoffs are reduced by a factor of five. We use this ratio mainly for computational facility. Through the strategy method, we collect four choices of the third-party: if he/she decides to punish when the receiver decides to split tokens equally (anti-social punishment) for one and two tokens transferred and if he/she decides to punish when the receiver keeps all the tokens (altruistic punishment) for one and two tokens transferred. The third party receives an endowment equal to half the surplus of the transaction between the sender and the receiver (three tokens in the case of transfer of half the endowment and four tokens in the case of transfer of two tokens). For the third party, the amount is in COP, and he/she can spend zero or any multiple of 100 COP, up to the maximum amount that would leave the receiver with zero endowment. We collect a set of beliefs for all participants.

After finishing this stage, all subjects participate in a dictator game (DG) with NGOs as recipients. This applied to trustors and trustees in the TG and, if the session included TPP, the monitors.

The final payment was the sum of the following payoffs: a) a randomly chosen decision between the two made in the Trust Game, b) the beliefs, and c) the DG. For the participants who acted as monitors, the payoff was the sum of (a) the relevant decision in the TPP, (b) the beliefs elicitation task, and (c) the DG.

2.2. Experimental procedures

Experimental sessions were conducted in the Fundación Universitaria Konrad Lorenz in Bogotá, from August 2013 to September 2013. The institution granted ethical approval. All participants provided informed consent. We used oral and written instructions as well as pictures drawn on the whiteboard. All decisions were made with pen and paper. One of the two TG decisions was randomly selected for payment by one of the students at the end of the session. All participants had to answer a comprehension test before making their decisions.

Beliefs on the counterpart's actions were elicited at the end of each decision. The monetary incentive was relatively small (250 COP) and paid if the prediction was correct up to a margin of error of 400 COP. We believed that more complex incentivization rules, such as a quadratic scoring rule, would have proved too complex for our sample. The use of small payment reduced the saliency of the decisions to minimize the likelihood of hedging by participants (Blanco et al., 2010).

After the beliefs were collected, the last decision was a DG with 4000 COP that consisted of sending zero, or any multiple of 400 COP, to one of three NGOs (Eckel & Grossman, 1996). While we prepare the envelopes with payments, participants answer a post experimental questionnaire.

We conducted nine sessions with 281 participants. The participants received on average COP 16,000 (with 7050 standard deviation). The sessions lasted two hours with minimal variation. More details of procedures may be found in the SOM, with an English version of all the materials and instructions. In SOM – A4 there is a picture of the lab setting.

2.3. Identification strategy

We run the following estimation:

$$y_{ij} = \alpha + \beta_0 FEAR_i + \beta_1 Victim_i + \beta_2 FEAR_i \cdot Victim_i + X_{ij}\gamma + Z_j\delta + \varepsilon_i \quad (1)$$

where i indicates participant, and j the district. y_{ij} is our dependent variable, $FEAR$ is the dummy for the Fear and Anxiety Recall versus Neutral Recall, $Victim$ a dummy identifying those highly exposed to violence, X is a set of individual level controls (including the dummy for TPP), Z is a set of district level controls, and ε are individual level unobservables.

Individual level controls are mostly income and education-related:

⁵ The discussion of the TPP data is in the SOM.

besides age and gender, we use both the Socio-Economic Strata (SES) of the household dwelling,⁶ an indicator for Body Mass Index, computed from self-reported height and weight in the post experimental questionnaire, and the father’s educational level, again self-reported in the questionnaire.

We use a battery of district-level controls from the Bogotá Household Survey (*Encuesta Multiproposito de Bogotá, EMB 2014*) to control for district level heterogeneity. ‘Income’ is the average per capita income in the district; ‘Education’ is to the share of people aged 18–25 who attend college; ‘Heterogeneity’ is the percentage of Socio-Economic Status (SES) 1 and 2 households; ‘displacement rate’ is the rate of Internally Displaced Population as a share of the population of the district, ‘Mobility’ is the number of households in that district that moved to another location in the last three years; ‘Home ownership’ is the share of households holding the property of the house in which they live. These are the equivalent to the variables used in [Falk and Zehnder \(2013\)](#), adapted to Colombia. We also run estimation using district level fixed effects.

Victim is a dummy based on self-reported ETV. We recover these data from the post-experimental questionnaire. We use the following questions:

- Have you been subjected to a physical assault in the last twelve months?
- Have any of your family members been subjected to a physical assault in the last twelve months?
- Have you found yourself in the middle of a confrontation involving the use of guns or other firearms in the last five years?
- Have you been subjected to direct violence in the last twelve months?

From these data, using a principal components analysis, we retain the first component⁷ and compute a composite index of ETV. The median level of ETV is bounded away from zero (median = 0.12), thus the coefficient should be interpreted as an intensive margin. In SOM, Figure A2, we show the histogram for this composite index.

Our coefficient of interest is β_2 . This is the effect of recall in victims with respect to recall in the comparison group. The identifying assumptions are two. The first assumption is the existence of a first stage (we use the same terminology as in IV research design): the recall of fear and anxiety increases the weight of the traumatic experience on the mental state. The first assumption seems plausible because memories of trauma are well stored within the body and can be easily retrieved with a variety of instruments ([Hopper & Van Der Kolk, 2013](#)).

The second assumption is the following. Following an argument from the literature on psychological priming, the mental mechanism activated by the recall may not be unique ([Cohn & Maréchal, 2016](#)). We assume that conditional on individual and district level controls, the “trigger” effect on other classes of experiences but violence, generated by the fear-anxiety recall is negligible for both those lowly exposed and those highly exposed. Given that, beyond the effect of trauma, the effect of recalling negative experiences is likely to be negligible (priming is a mild manipulation), this assumption seems to be plausible.

⁶ In Colombia each dwelling is assigned one of six tax tiers, depending on the market value and other characteristics of the dwelling, as well as the area in which the dwelling is located. There exist six tax tier levels, with 1 (6) indicating the lowest (highest) tax tier, and thus lowest (highest) Socio-Economic Status. Utility bills are differentiated per tier, being *ceteris paribus* higher for dwellings estimated as having higher market value. People do not expect to be stigmatized according to SES, which ensures the truthfulness of this construct as a measure of socio-economic status ([Bogliacino et al., 2018](#)).

⁷ Kaiser-Mayer-Olkin test of accuracy of the sample, KMO = 0.50, the eigenvalue of the first component is 1.28, and the share of the variance explained is 32%.

We now come to the dependent variables. From the TG, we collect four measures. The sender decision is the level of trust, computed as the percentage of the available endowment. We calculate the average amount sent across districts and we also look at separate decisions per district. The two decisions by the receiver are our measures of trustworthiness: return upon one token sent and return upon two tokens sent, labelled *Return (1T)* and *Return (2T)*. Finally, we estimate the share sent to NGOs, as a measure of altruism.

To avoid the distortion of multiple hypotheses testing, we normalize the variables in z-score (subtracting the average and dividing by the standard deviation) and run regressions clustering standard errors at the individual level.

2.4. Results: impact on pro-social behavior

In the SOM Tables A2 – A3, we provide descriptive statistics of the sample. We also report the mean and standard deviation of the outcome variables by experimental condition, in SOM Table A4.

On average, participants send 54% of their endowment in the TG; they share back in 67% (74%) of the cases, when one token (two tokens) is (are) transferred; they donate 42% of their endowment to charities.

With regards to exposure to violence, we tabulate the questions in [Table 2](#). One third of the sample experienced a physical assault in the year before the data collection, almost half experienced violence within the close family, one fourth has been in the middle of gun confrontation, and 21% experienced other forms of direct violence. Around 27% did not report exposure to violence, whereas 73% answered yes to at least one of the questions.

In [Fig. 2](#), we plot the average unconditional mean. Subjects less exposed to violence are more likely to behave prosocially (around 10% of a standard deviation), but less so when primed on fear and anxiety. On the contrary, victims are less pro social than the average in the sample, but increase their level of trust, trustworthiness, and altruism when recalling violence.

We move to the regression analysis. The regression is reported in [Table 2](#). We report the results for each individual measure and the pro-social index to control for multiple hypotheses testing.

As can be seen In [Table 3](#) column 1, the differential effect of the priming in victims with respect to non-victims is positive and significant. The effect size is 0.53 of a standard deviation on the index of pro-sociality ($t = 2.82, p = 0.005$). The same effect is statistically significant for trust ($t = 1.89, p = 0.062$) and Return (1T) ($t = 2.82, p = 0.005$). It is not statistically significant for Return (2T) ($t = 1.29, p = 0.200$) nor for altruism ($t = 0.97, p = 0.334$), although the sign is also positive. All the results are robust to the use of district level fixed effects (SOM Tables A5–A9).

2.5. Results: in-group bias

In 4, we report the estimation for in-group bias. We use the 19 decisions at the district level for the trust decision, to estimate a panel data model. We introduce a dummy equal to one when the participant is sending towards a resident in their own district and zero for the other 18

Table 2
Exposure to violence.

Variable	Mean	Std. Dev.	% Answered No
Have you been subject to physical assault in the last twelve months?	0.32	0.47	67.6
Have any of your family members been subject to physical assault in the last twelve months?	0.46	0.50	53.4
Have you found yourself in the middle of a gun confrontation in the last five years?	0.27	0.45	72.6
Have you been subjected to direct violence in the last twelve months?	0.21	0.41	79.0

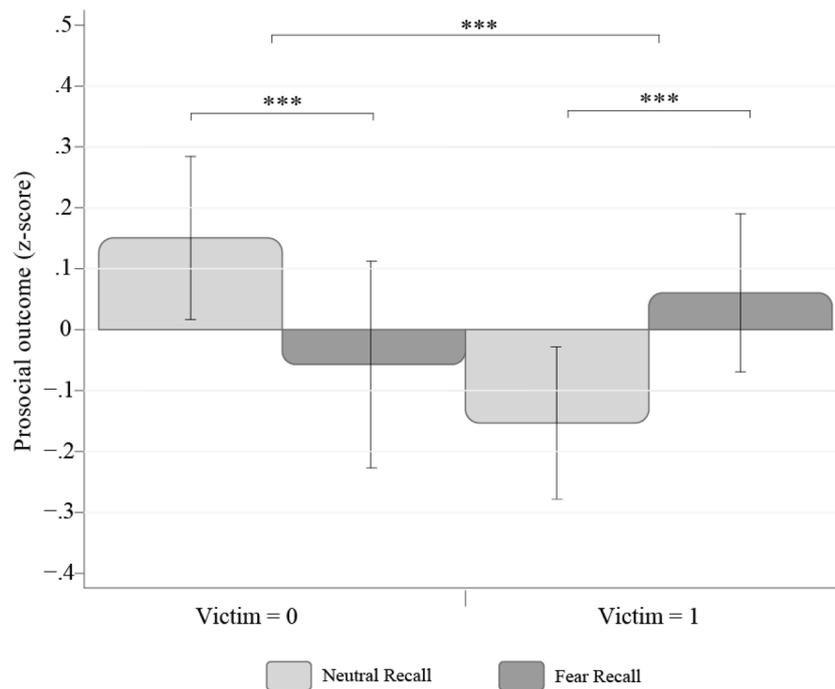


Fig. 2. Comparing the four groups defined by ETV and FEAR on prosocial behavior for experiment I
 Note: The dependent variable ‘Prosocial’ is the normalization in z-score of the trust, trustworthiness (1T), trustworthiness (2T), and altruism measure. This variable is centered in zero and in point of standard deviation. The graph shows the confidence interval at 5% significance level.

Table 3
 Difference-in-differences estimation of the impact of self-reported ETV on prosocial outcome.

VARIABLES	(1) Prosocial behavior (normalized)	(2) Trust	(3) Trustworthiness (1 T)	(4) Trustworthiness (2 T)	(5) Altruism
FEAR ·Victim	0.53*** (0.19)	0.13* (0.07)	0.41*** (0.15)	0.18 (0.14)	0.08 (0.08)
FEAR	-0.30** (0.14)	-0.04 (0.05)	-0.45*** (0.11)	-0.05 (0.10)	0.01 (0.06)
Victim	-0.28** (0.13)	-0.07 (0.05)	-0.20** (0.09)	-0.11 (0.10)	-0.04 (0.05)
Individual level controls	YES	YES	YES	YES	YES
District level controls	YES	YES	YES	YES	YES
Constant	-4.57*** (1.50)	-1.14 (0.65)	-0.86 (1.27)	1.26 (1.23)	-1.82** (0.71)
Observations	735	184	184	183	184
R-squared	0.06	0.09	0.17	0.08	0.12
F test	3.188	1.741	4.018	1.279	1.721

Note: The dependent variable ‘Prosocial’ is the normalization in z-score of the trust, trustworthiness (1T), trustworthiness (2T), and altruism measure. This variable is centered in zero and in point of standard deviation. Clustered standard errors at individual level in parentheses for column (1) and robust standard errors for Columns (2)-(5).. (See the detail definition of the variables in Table A1 in SOM and the complete regressions in Tables A5-A9 in SOM).

*** $p < 0.01$,
 ** $p < 0.05$,
 * $p < 0.1$.

districts. We add interactions with Victim and with FEAR to detect if there is a differential impact of ETV on in-group bias.

The participants send on average 75% of their endowment to participants of their own districts, while they send on average 55% of the endowment to participants outside their district (Tables 4 and 5).

As can be seen, this 20% differential transfer to other participants in the same districts in excess of what they sent to those from other districts is not different across levels of ETV. In particular, our coefficient of interest (the triple interaction) is not statistically significant ($t = 0.39, p =$

0.696). This result is robust to different estimations. In SOM Table A10, we report estimations with fixed effects, random effects, and alternative sets of controls.

Notably, this result is not driven by wealthy participants. One could argue that due to the decreasing marginal utility of income, it is rational to expect more trustworthiness by rich people. Since Bogotá is largely segregated by socioeconomic level, as are most Latin-American capitals (Bogliacino et al., 2018), rich people are more trusting towards those who live in the same neighborhood because they are of a comparable

Table 4
In-group bias.

VARIABLES	Amount sent
In-group	0.21*** (0.03)
FEAR	-0.04 (0.05)
Victim	-0.07 (0.05)
FEAR · Victim	0.13* (0.07)
In-group · FEAR	-0.02 (0.07)
In-group · Victim	-0.04 (0.05)
In-group · Victim · FEAR	0.03 (0.09)
Individual level controls	YES
District level controls (sender)	YES
District level controls (receiver)	YES
Constant	-1.08* (0.64)
Observations	3465
Number of id	184
Individual random effects	YES

Note: The dependent variable ‘Amount Sent’ is the transfer by senders as a share of the initial endowment. Clustered standard errors at individual level in parentheses., ** $p < 0.05$. (See the detail definition of the variables in Table A1 in SOM and the supporting regression in Table A10 in SOM).

*** $p < 0.01$.
* $p < 0.1$.

socioeconomic level. To assess this alternative explanation, we split the sample according to income status: we separate both rich and poor districts according to the average income of the district, and rich and poor participants according to their self-reported SES. Results are reported in 5. As can be seen, in-group bias holds for each of these groups: participants from rich districts transfer on average 23% more within the district ($t = 4.22, p = 0.000$) whereas those from poor district 19% more ($t = 4.22, p = 0.000$) and participants from high SES transfer 22% more within their district ($t = 4.12, p = 0.000$) compared to 20% of those from low SES ($t = 4.38, p = 0.000$).

3. Experiment 2

3.1. Design

The second experiment was also conducted in Bogotá and was designed upon the results of Experiment I. Participants were recruited from all districts in Bogotá, excluding the rural one.

We collected data from three incentivized choices and from a questionnaire. Decision one was a Prisoner’s Dilemma (PD). We used a PD instead of a TG to assess robustness with respect to the measurement of pro-sociality and to make the shock meaningful and independent from the sequence of the game (as it would be for the trustee in the trust game). The PD was followed by a belief elicitation task on the counterpart’s decision. Decision three was a DG with NGOs as donations’ recipients, similar to Experiment I.

The final payment was the sum of a show-up fee (2000 COP), the payoff from the PD, and the payoff from one randomly chosen decision between the DG and the belief elicitation task. We chose to always pay the PD because it was our main decision, and it was necessary to make the Negative Wealth Shock (NWS) salient (see below). We randomly paid one of the other two decisions to avoid deception and possible income effects. The maximum payment assigned to DG and the belief task was 4000 COP.

The PD was based on Holt and Capra (2000), with a loss framing. Participant received an initial endowment of 20 points (20,000 COP)

Table 5
In-group bias by poor and wealthy district and individual characteristics.

VARIABLES	(1) Trust (poor district)	(2) Trust (wealthy district)	(3) Trust (poor individual, by SES)	(4) Trust (wealthy individual, by SES)
In-group	0.23*** (0.05)	0.19*** (0.05)	0.22*** (0.05)	0.20*** (0.05)
FEAR	0.04 (0.07)	-0.09 (0.06)	0.10 (0.06)	-0.12* (0.06)
Victim	-0.03 (0.07)	-0.13** (0.06)	0.06 (0.08)	-0.15*** (0.05)
FEAR·Victim	0.17* (0.10)	0.11 (0.08)	0.04 (0.10)	0.20** (0.08)
In-group · FEAR	-0.09 (0.11)	0.03 (0.08)	-0.13 (0.10)	0.06 (0.08)
In-group · Victim	-0.07 (0.08)	-0.01 (0.07)	-0.04 (0.08)	-0.03 (0.07)
In-group · Victim · FEAR	0.06 (0.14)	0.03 (0.11)	0.14 (0.15)	-0.03 (0.11)
Individual level controls	YES	YES	YES	YES
District level controls (sender)	YES	YES	YES	YES
District level controls (receiver)	YES	YES	YES	YES
Constant	-0.33 (0.54)	-0.08 (0.34)	0.04 (0.47)	0.17 (0.35)
Observations	1589	1876	1382	2083
Number of id	84	100	74	110
Random effects	YES	YES	YES	YES

Note: The dependent variable ‘Trust’ is the transfer of tokens by senders as a share of the initial endowment. ‘In-group’ is a dummy equal to one when the transfer is towards the same district of the participant. ‘FEAR’ is a dummy for fear and anxiety recall. ‘Victim’ is a dummy for above-the-median exposure to violence index. Individual level controls are age, gender, SES, father’s education and body mass index. District level controls are income, education, displacement rate, heterogeneity, mobility, and home ownership. Clustered standard errors at individual level in parentheses. (See the detail definition of the variables in Table A1 in SOM and the complete regression in Table A11 in SOM).

*** $p < 0.01$.
** $p < 0.05$.
* $p < 0.1$.

and choose between two options. Each of the possible outcomes involved losses, as per Fig. 3. The loss framing was necessary to introduce the NWS through manipulation of the initial endowment. de Heus et al. (2010) find that the behavior in the PD is not statistically different in the loss framing with respect to the standard framing, thus the decision has external validity as a measure of cooperation.

The experimental questionnaire was divided into two blocks. The first block of questions was administered at the beginning of the experiment before the experimental tasks. This block included questions

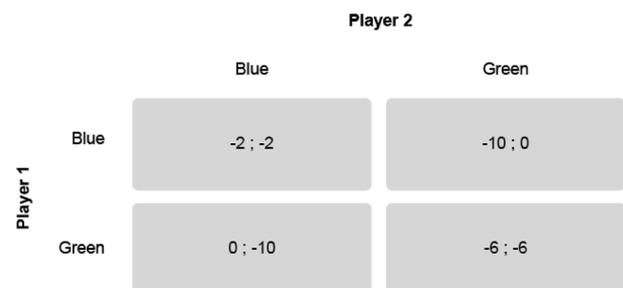


Fig. 3. The normal form of the Prisoners’ Dilemma. Source: Author’s elaboration.

Table 6
Experimental conditions and sample: Experiment 2.

	Fear and Anxiety Recall	Neutral Recall
NWS	N = 51	N = 60
No NWS	N = 51	N = 61

about gender, age, civil status, residence district, and SES. It was necessary to ask these questions at the beginning to avoid ex post manipulation of the residential district. The second block was asked at the end. The latter included questions about aspirations, the frequency of political participation and a set of questions to measure ETV and perception of insecurity (following the questionnaire format of Vélez et al., 2016).

We have a 2 × 2 design. One factor is the Recall, with two levels: Fear and Anxiety Recall versus Neutral Recall, as in the first experiment. The other factor is an NWS. The NWS treatment was a reduction of 50% over the initial endowment (i.e., shocked participants are left with an initial endowment of 10 points instead of 20 points).⁸ The loss occurred before a participant made the PD decision. Each participant was informed of her own loss but was not informed on whether the counterpart suffered a loss or not. We made this design choice to soften the concern on inequity aversion motives. Randomization occurred at the individual level. Table 6 summarizes the design and includes the number of observations per condition.

3.2. Procedures

Experimental sessions were conducted at the Universidad Nacional de Colombia in Bogotá, from September 2018 to November 2018. The average payment was COP 14.000 (USD 5.00, with COP 6779 standard deviation). After the assignment of the experimental condition, the participants made 19 district-contingent decisions in the PD task, followed by the belief elicitation task and the DG. No feedback of the PD results was given prior to the two other decisions. In total, we had 17 sessions, with 223 participants.

Ethical approval was granted by the Faculty of Medicine of Universidad Nacional de Colombia. All participants provide informed consent. An English version of all the materials and instructions are included in the SOM. In SOM – A4 there is a picture of the lab setting.

3.3. Identification strategy

We run the following estimation:

$$\begin{aligned}
 Y_{i,j} = & \alpha + \beta_1 FEAR_i + \beta_2 NWS_{i,j} + \beta_3 FEAR \& NWS_{i,j} + \beta_4 Victim_{i,j} \\
 & + \beta_5 FEAR_{i,j} \cdot Victim_{i,j} + \beta_6 NWS_{i,j} \cdot Victim_{i,j} + \beta_7 FEAR \& NWS_{i,j} \cdot Victim_{i,j} \\
 & + \gamma' X_{i,j} + \delta' Z_j + \varepsilon_{i,j}
 \end{aligned}
 \tag{2}$$

where *i* indicates participant and *j* the district. *Y* is our dependent variable, *FEAR* is the dummy equal to one for the condition Fear and Anxiety Recall-No NWS, *NWS* is the dummy equal to one for the condition Neutral Recall-NWS, *FEAR & NWS* is the dummy equal to one for the condition Fear and Anxiety Recall-NWS, *Victim* is a dummy identifying those highly exposed to violence, *X* is a set of individual level controls, *Z* is a set of district-level controls, and ε are individual level unobservables.

Individual-level controls are measured in the post-experimental questionnaire: we use Socio-Economic Strata, age, gender, the number

⁸ Delgado et al (2006a, 2006b) show that negative shocks induce an emotional shock, and we built on this result to manipulate recall in an alternative way. Exposure to violence is systematically associated to losses, but especially so in urban context where the most common case is robbery.

of wrong answers in the comprehension questions, dummies for students and members of the Idipron program,⁹ educational level, and a dummy for those born in Bogotá.

We used a battery of district-level controls from the Bogotá Household Survey (*Encuesta Multiproposito de Bogotá, EMB 2017*): average weekly expense, education, heterogeneity, mobility, and home ownership. These are the equivalent of the variables used in Falk and Zehnder (2013), adapted to Colombia, and match the variables used in Experiment I. 'Average Weekly expense' corresponds to the sum of household expenditure on beverages and non-alcoholic foods, transportation, mail, fax and parcels, fuel and parking, meals outside the home, bets and lotteries and internet café. This is used instead of income per capita for data availability. 'Education' corresponds to the average number of years of schooling; 'Heterogeneity' is the percentage of SES 1 and 2 households in the district; 'Mobility' is the number of households in the district that moved to another location in the last 5 years; 'Home ownership' is the percentage of households that have their own home in the locality.

As in Experiment I, HETV is a dummy based on self-reported ETV. We recover these data from a larger set of questions than in Experiment I (see Section 2.1):

In the last 12 months, have you been victim of _____

- A Non-armed robbery
- B Armed robbery
- C Assault or sexual violence
- D Kidnapping
- E Street fight
- F Domestic violence
- G Damage to your property, crops, or animals
- H Extortion
- I Verbal aggression
- J Harassment
- K Persecution
- L Psychological abuse
- M Death threats?

From these data, using a principal components analysis, we retain the first component¹⁰ and we compute a self-reported violence index. The median level of ETV is again bounded away from zero (median = 0.10), thus the coefficient can be interpreted as an intensive margin. In SOM, Figure A3, we show the histogram for this composite index.

Our coefficients of interest are β_5 , β_6 and β_7 . As in Experiment I, these parameters capture the effect of recalling violence in high ETV with respect to low ETV. The plausibility of the empirical evidence rests on the same premises discussed in Section 2.3.

Our outcome is cooperation, measured using the decisions in the PD. We calculate the average level of cooperation over all the possible district-level decisions.

3.4. Results: impact on pro-social behavior

In the SOM Table A13, we provide descriptive statistics of the sample, by experimental condition. On average, 47% of the participants decide to cooperate in the Prisoner Dilemma, 52% believe that the counterpart will cooperate, and they donate 47% of their endowment to charities. As expected, cooperation is highly correlated with the expectation (Pearson's rho = 0.31, $p < 0.01$).

We tabulate the level of ETV in Table 7. Around 70% of the subjects

⁹ To include participants from two of the most dangerous districts, we recruited youngsters who belong to a special program which helps victims of violence, the homeless and former drug addicts.

¹⁰ KMO = 0.63, the eigenvalue of the first component is 2.49, and the variance explained accounts for 19% of the total.

Table 7

Exposure to violence.

Variable	Mean	Std. Dev.	% Answered No
Non-armed robbery	0.26	0.44	73.5
Armed robbery	0.14	0.35	85.7
Assault or sexual violence	0.01	0.11	98.7
Kidnapping	0.01	0.07	99.6
Street fight	0.16	0.36	84.3
Domestic violence	0.06	0.23	94.2
Damage to your property, crops, or animals	0.18	0.39	81.6
Extortion	0.02	0.15	97.8
Verbal aggression	0.50	0.50	50.0
Harassment	0.23	0.42	77.1
Persecution	0.08	0.27	92.4
Psychological abuse	0.11	0.32	88.8
Death threats?	0.03	0.16	97.3

report having been a victim at least once. Aggression and robbery are the most common cases.

Fig. 4 replicates the main findings of Fig. 2 by comparing the unconditional means of the various groups. On average, control participants reporting low exposure cooperate on average 55% of the time. The treatments reduce the cooperation: the fear and anxiety recall and the NWS by around 10%, whereas the recall and the NWS by around 5%. Victims on the other hand are 15% less likely to cooperate in the baseline, but the treatment increases the rate by 10% (recall), 5% (NWS), and 3% (recall and NWS).

We report the main results in Table 8. We find the same effect as in Experiment I concerning the differential effect of the recall in victims (24 percentage points, pp) ($t = 1.95, p = 0.052$).

Something similar happens for the NWS. The difference-in-differences effect is 21 percentage points (pp) and is again weakly significant ($t = 1.92; p = 0.056$). All the results are robust to the use of district level fixed effects (SOM Table A-14).

The case of the joint manipulation of NWS and Fear is not statistically significant. However, the sign is the one we expected. The lack of significance may be due to low statistical power.¹¹

When we run a separate regression for our measure of altruism, we cannot find any effect, confirming the result from Experiment I. The regression is reported in Column (2), Table 8.

Experiment II allows us to conduct a proof of concept. As we said, we have been assuming that the recall leverages the memory of violence. Since this experiment is computerized, we can associate the written text in the recall to the subject who wrote it. This very fact also increases scrutiny and thus underreporting but this does not concern the validation of the recall as long as the underscrutiny does not interact with the treatment.¹²

A research assistant not privy to the research question was asked to codify the text. The share of participants who wrote a violence related text was 6.7% in the control, increasing to 23.5% under the fear and anxiety recall. As expected, those individuals that self-report stronger exposure to violence are also more likely to talk about violence in the recall (twice as much with respect to lowly exposed, 18.8% vs 9.9%). Finally, the fear and anxiety recall increases the likelihood of reporting a description related with violence by 4% (from 8% to 12%) among those self-reporting low exposure, whereas the increase reaches 30% (from 5% to 35%) among those recording high exposure.

¹¹ According to an *ex post* Monte Carlo approach (Moffatt, 2015), the power is less than the canonical 80%. Notice that by *ex post*, we meant recalculating the power using the empirical value of the standard deviation and still assuming the original minimum detectable effect, and *not* deriving the power from the *p*-values, which is severely criticized in the literature.

¹² It should be noted that the effect of the recall on the mental state is produced as long as the subject thinks at the experience even if she is scared to materially write it.

In Fig. 5, we plot the average cooperation distinguishing between those whose textual account of the negative experience was violence related and those for whom it was not. The level of cooperation increases even more for violence related written texts than for other written text. Notice that we do not split within the groups of neutral recall or control because the prevalence of the account of violence is minimal and the confidence intervals widen excessively.

3.5. Results: in-group bias

On average, participants cooperate with a probability of 47.2% with someone from the same district, while they do it 42.6% of the times with participants outside their own district. The difference is not statistically significant.

Table 9 reports the results for in-group bias. We use the 19 decisions at the district level to estimate a panel data model. We introduce a dummy for those decisions that involve cooperation towards a resident in the same district and we look at all potential interactions. The Table for this regression is reported in the SOM, Table A16. The coefficients of interest are the triple interactions between the treatment, the dummy for victim and the dummy for the same district; we cannot reject the null for the FEAR treatment ($t = 0.48, p = 0.63$), for the NWS treatment ($t = -0.23, p = 0.81$), but is marginally significant for the FEAR & NWS treatment ($t = -1.84, p = 0.06$).

It is worth mentioning that participants did differentiate their choices across districts. Decomposing variance shows that 66% of the variation occurs within choices at the participant level, while 34% between participants. There is no evidence of discrimination according to income either, as shown by the lack of correlation between likelihood to cooperate and average district income (Spearman rho=0.01, $p = 0.87$).

4. Discussion and concluding remarks

In this article, we addressed two research questions: A) does recalling crime related ETV increase pro-social behavior? B) does recalling ETV affect in group bias?

To answer research question A, we conducted two controlled experiments in Bogotá (Colombia). In both cases, we use experimental manipulations of recall (directly through a question or indirectly through a shock) combined with self-reported victimization to causally assess the impact of ETV on different dimensions of pro-sociality. Our evidence suggests that recalling traumatic experiences makes subjects act more pro-socially.

The use of two experiments to replicate and extend the empirical evidence is a major contribution of this article. In experimental research, a major source of discussion is external validity. Although the samples were convenience ones, we were able to cover all the districts of the city. Additionally, showing robustness and heterogeneity across tasks is an important contribution to the existing literature.

To answer research question B, we had to assess a methodological tool to induce in group bias in urban context, given that we cannot use the community as in rural studies on conflicts.

We show, as Falk and Zehnder (2013) in Zurich, that people discriminate in trust decisions across districts, with a bias towards their in-group. However, this result does not replicate to a cooperation game. This lack of replication may be due to the difference in our design, which included one extra random manipulation, that is, the NWS, in Experiment II compared to Experiment I. Nonetheless, we believe that an NWS is a constitutive part of the mechanisms accounting for the relationship between trauma and pro-sociality, as demonstrated in Experiment II

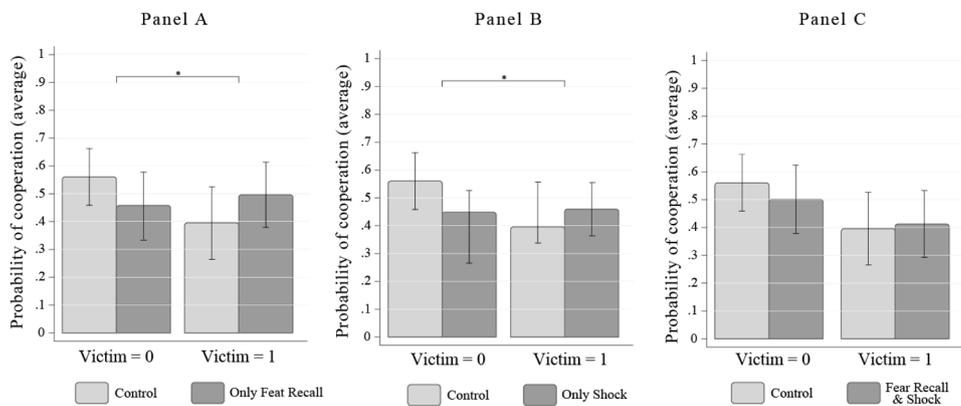


Fig. 4. Comparing the groups defined by ETV, FEAR and NWS on average cooperation for Experiment II

Note: Mean cooperation and confidence interval at 5% significance level. Panel A include low exposed, low exposed recalling, high exposed and high exposed recalling. Panel B include low exposed, low exposed with NWS, high exposed and high exposed with NWS. Panel C include low exposed, low exposed recalling and receiving NWS, high exposed and high exposed recalling and receiving NWS.

Table 8

Difference-in-differences estimation of the impact of self-reported ETV on cooperation and altruism.

VARIABLES	(1) Probability of cooperation (average)	(2) Altruism
FEAR · Victim	0.24* (0.12)	-0.02 (0.09)
NWS · Victim	0.21* (0.11)	0.09 (0.10)
FEAR & NWS · Victim	0.10 (0.12)	0.09 (0.14)
FEAR	-0.13 (0.08)	-0.04 (0.06)
NWS	-0.13* (0.07)	-0.09 (0.08)
FEAR & NWS	-0.08 (0.08)	-0.06 (0.09)
Victim	-0.20** (0.08)	-0.09 (0.08)
Individual controls	YES	YES
District controls	YES	YES
Constant	0.25 (0.51)	0.35 (0.58)
Observations	223	223
R-squared	0.08	0.13
F test	0.771	-

Note: The dependent variable probability of cooperation is the average across the 19 decisions (one per district) by participants. The dependent variable 'Altruism' is the amount sent in the DG to the NGOs. Robust standard errors in parentheses. *** $p < 0.01$, (See the detail definition of the variables in Table A1 in SOM and the complete regression in Tables A15 and A16 in SOM).

** $p < 0.05$.

* $p < 0.1$.

itself.¹³ We believe that another reason may be due to the intrinsic differences for ingroup bias to be manifested between a dynamic game like the TG and a simultaneous-move game like the PD (Yamagishi & Kiyonari, 2000). It may be conjectured that the incentives to display ingroup favoritism are higher in a TG than in the PD. Since the first mover chooses in isolation from the second-mover, an act of ingroup favoritism can be more easily identified as welfare improving for a fellow district resident than a similar act in a simultaneous game. This heightened facility to identify one's action as welfare-enhancing for one's ingroup may increase self-esteem (Bénabou & Tirole, 2002). This

¹³ Violence is always accompanied by losses, which sometimes are the direct goal of the violent act, and sometimes the indirect consequences of injuries or other more direct effects. Additionally, losses can work as a mechanism to recall the experience, especially given that they activate negative emotions (Delgado, Labouliere, et al., 2006; Delgado, Olsson, et al., 2006b).

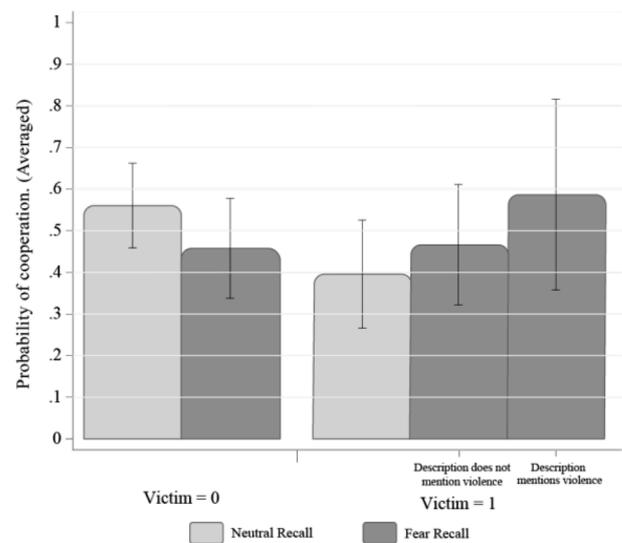


Fig. 5. Comparing the groups defined by ETV, FEAR and violence related text on average cooperation for Experiment II.

post-diction interpretation and whether urban segregation mirrors patterns of discrimination in trust should be investigated in other cities and may have important policy implications. A limitation of this result is that the reference group of an urban residence may be imperfectly measured by the district. Nonetheless, the results of the trust game are very robust and contribute to the discussion initiated by Falk and Zender (2013) on trust and discrimination in urban environment.

Identifying the relevant mechanism behind this relationship remains an open question. In the SOM, Section A6, we tried to assess the explanations proposed by the literature on conflicts, namely that beliefs, social norms, psychological recovery, or perception of insecurity drive the causal effect of ETV on pro-social behavior. Some limited evidence comes from beliefs, but only from the first experiment. The rest of the assessment is inconclusive. Not all the four variables relevant for the mechanisms analysis have been incentivized, because for some of them inducing value is problematic. Our result does not undermine the validity of these explanation for conflict related studies, where the group framing is extremely salient (Hardin, 1995; Sumner, 1906). In fact, it is possible that exposure to war and crime may be associated with different mechanisms. Nevertheless, if evolution made humans hardwired to react in a certain way to inter-group violence, it is plausible that the same reaction carries over to violence originated from different sources. Nevertheless, we cannot reject the null for the crime related ETV case. This paper has implications for the literature on changing preferences after being victims of shocks. Shocks have been studied in the context of

Table 9
In-group bias for cooperation.

VARIABLES	(4) Cooperation
In-group	-0.12 (0.08)
FEAR	-0.02 (0.06)
NWS	-0.04 (0.06)
FEAR & NWS	-0.04 (0.06)
HETV	-0.07 (0.04)
In-group · FEAR	0.09 (0.12)
In-group · NWS	0.07 (0.12)
In-group · FEAR & NWS	0.18 (0.12)
In-group · Victim	0.09 (0.12)
In-group · FEAR · Victim	-0.08 (0.18)
In-group · NWS · Victim	-0.04 (0.17)
In-group · FEAR & NWS · Victim	-0.32* (0.17)
Individual controls	YES
District controls	YES
Constant	0.07 (0.49)
Obs	4237
Individuals	223
Random Effects	Yes

Note: The dependent variable probability of cooperation is the average across the 19 decisions (one per district) by participants. Robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$. (See the detail definition of the variables in Table A1 in SOM and the complete regression in Table A 16 in SOM).
* $p < 0.1$.

violence, poverty, natural disasters, and there is compelling evidence of behavioral change and shift of preferences (Bauer et al., 2016; Bejarano et al., 2018; Cassar et al., 2017). The possibility that a common mechanism lies behind all this evidence deserves further investigation.

The use of recall and endowment shocks to prime participants deserves some discussion. Priming is a psychological technique used to increase the weight of a mental concept through situational cues. Priming has been vastly used in cognitive and social psychology, to understand the impact of stereotypes (Bargh et al., 1996) or whether religious priming increases prosocial behavior (Shariff & Norenzayan, 2007). These strands of research have been strongly criticized, due to the lack of replicability and the inference out of underpowered experiments (Rivers & Sherman, 2018; van Elk et al., 2015; Yong, 2012). Lack of replication have been observed elsewhere (Open Science Collaboration, 2015) and should not be used as an argument to discard a methodological tool especially given the heterogeneity of the research questions. Running two experiments is per se a proof of our willingness to rely on replication to assess our claims, although this is not an exact replication but a conceptual one (Hudson, 2021). Recalling violence is different from priming on age or religion, it is plausible to assume that violence is much more salient than other environmental factors or personal experience. In our previous work (Bogliacino et al., 2017), we documented that exposure to displacement was so salient to the point of being recalled even by a neutral question. This phenomenon was so prevalent that to maintain the validity of the experimental manipulation, a joy recall had to be used as a comparison group with the fear and anxiety recall. Experiencing crime may be less extreme than displacement, but urban violence can also be life-threatening and highly traumatic.

Another challenge to the use of recall is that it may tap into different

mechanisms and thus confound the main effect with other channels (Cohn & Maréchal, 2016). This is a valid concern, which also applies more generally to any study using intention to treat or instruments. For our study, the concern is originated by the recall mentioning different experiences, besides violence. This phrasing of the text of the recall partly responded to the desire to soften experimenter demand and partly to closely follow other validated recalls, such as that used by Callen et al. (2014). Nonetheless, our difference in differences approach partly addresses these possible confounds, under the second identifying assumption (see Section 2.3).

The main limitation of our research is that both the recall and the endowment shocks are mild manipulations. Using strong emotional stimuli in victims would certainly provide more compelling evidence on causal impact and mechanisms. Our estimations represent a lower bound of the actual effect.

Overall, we believe that the evidence produced in this study supports the view that violence may have consequences on other regarding behavior, which operate alongside the negative feedback loops sketched out in the introduction. Although the quantification of the two effects is beyond the scope of the present paper, it is plausible that the two effects are partly affecting in opposite directions the process of economic development. It is nonetheless important to ascertain their existence and quantify their effects, as we have sought to do in the present study.

Data availability

Data and Do-files for replicating the analysis are available at the OSF page of the companion working paper: <https://osf.io/a9ghu/>.

Supplementary materials

Supplementary material associated with this article can be found, in the online version, at [doi:10.1016/j.socec.2023.102023](https://doi.org/10.1016/j.socec.2023.102023).

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