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On the behavioral impacts of violence: Evidence from incentivized games in Kenya☆

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ABSTRACT

Political violence is a major impediment to economic development, damaging social, physical and human capital. By contrast, the manner in which violence influences prosocial behaviors is less clear cut, even though these behaviors likely contribute to post-conflict outcomes at individual and aggregate levels. We propose that the standard routes through which the experience of violence is thought to increase prosocial behaviors offer different theoretical explanations under heterogeneous conflict exposure histories and for different behavioral domains. We test these hypotheses using incentivized behavioral experiments, collected in the context of electoral violence in Kenya. While we provide some evidence that exposure to violence increases prosocial behaviors, results display significant heterogeneities relating both to the dimensions of behavior analyzed and whether or not individuals were personally injured.

1. Introduction

In the aftermath of war, people often make investments in behaviors that positively affect others, termed prosocial behaviors (Bauer et al., 2016; Mironova and Whitt, 2014; Voors et al., 2012). The specific behaviors that can be considered prosocial by this definition are broad, ranging from voting behavior, to investment in public goods, to trust in government or other institutions, to charitable giving. From a rationalist perspective, such behaviors in conflict scenarios might reflect investments in social insurance (e.g. Grosjean, 2014) or other efforts to minimize the future risk of violence or its costs (e.g. Keeley, 1996). Psychological and political explanations, like “post traumatic growth” (Rosner and Powell, 2014); and “expressive preferences” (Wood, 2003), have similar implications that prosocial behavior is used to build stronger ties between individuals and groups following the experience of war.

In this article, we explore the effects of two understudied implications of theories built on this foundation. The first is that the type of victimization might drive subsequent behaviors. Those most harmed are likely to view returns from building stronger ties differently, and perhaps more positively, than those unharmed. Different kinds of exposure differently influence related domains like risk attitudes (Rockmore et al., 2016; Mironova et al., 2019; Gangadharan et al., 2022) and interpersonal trust (Ingelaere and Verpoorten, 2020), suggesting that it is logical to test if this also plays a role in determining prosocial behaviors. The second is that the type of decisions might be important, with cooperative behaviors potentially more analogous to building a stronger, more united, society than generous ones.

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To test these hypotheses, we collect data from a range of one-shot incentivized behavioral games that vary the degree to which actions are cooperative (towards increasing the size of the shared pool) – captured by the public goods and stag hunt games and the Player A role in the trust game; and the degree to which they are distributional (dividing a fixed prize) – captured by the dictator game and Player B role in the trust game. We match choices made in these games to data on exposure to electoral violence in Kenya in 2007–2008. Specifically, we test which – if any – behavioral domains are affected by: personal injury; having family members injured; and of witnessing violence perpetrated against strangers. Kenya affords an excellent case-study to answer these questions. Since independence, it has suffered a system of (partly) ethnic-based patronage, giving rise to electoral violence, which serves the purpose of "… [creating] a much wider sense of chaos linked to the hosting of elections" (Branch, 2011, p. 226). After electoral conflicts in the 1980s and 1990s, large-scale violence broke out in 2007, with over 1200 deaths over half a million people displaced.

The pattern of violence after the disputed 2007 election affords us the opportunity to study heterogeneous conflict experiences. While many were personally injured, some were more harmed than others, and others were left physically unharmed. Despite tense elections since, the transfer of power was broadly peaceful after 2007. Indeed, despite notable fragility, Kenya does not exhibit widespread conflict. This affords us the opportunity to isolate experiences of the 2007/2008 episode. Although ostensibly political, the conflict had an ethnic component due to the patronage system, which allows us to test and rule out (see: Barriga et al., 2020) parochial explanations of our findings (Whitt and Wilson, 2007; Bauer et al., 2014a, 2014b; Cecchi et al., 2015; Beekman et al., 2017).

We find no evidence that witnessing violence perpetrated against strangers, nor the severity of that violence, influences choices in any task. Having family members injured (or severely injured), too, does not affect choices in any task. We find no evidence that choices in cooperative tasks are affected by any kind of exposure, nor the severity of exposure. Having been personally injured, however, increases giving to one's partner in both the Player B role in the trust game and in the dictator game. In other words, we find evidence of an increase in prosocial giving in situations without immediate return to that giving, following the experience of violence. This, however, only arises as a consequence of personal injury and affects choices, only, in distribution tasks. A series of robustness checks confirm that this basic structure of results holds, even for different measures of exposure to conflict, such as variations in the salience of the episode in question and the severity of injury.

These results contribute to a broad literature that, to date, has produced somewhat messy results. Some work definitively shows positive prosocial outcomes in the aftermath of war (Bellows and Miguel, 2009; Voors et al., 2012; Hartman and Morse, 2015; Bauer et al., 2016). Other work shows the opposite, with reductions in prosocial behavior present (Rohner et al., 2013; de Luca and Verpoorten, 2015b; Grossman et al., 2015; Hager et al., 2019). What is missing in literature to date is an effort to place structure on these contradictory findings by looking at what drives particular sets of outcomes. We present both positive and null impacts of violence on prosocial behavior and show the role of exposure type and the behaviors under study in determining these outcomes. This ensures we provide important refinements to the literature to date.

The remainder of this article is structured as follows: In the next section, we consider the background literature and the factors that link the experience of violence to prosocial behavior, from which we build theory and derive hypotheses. Next, we discuss data and methods. We then present our results. Finally, we discuss our findings and offer discussion and conclusions.

1.1. Theoretical background

1.1.1. Does violence influence prosocial behavior?

With the exception of a small amount of work (Gibson and Gouws, 2005), literature tends to provide evidence of some sort of relationship between violence and prosocial behavior. However, the literature also casts significant doubt on the direction of the effect and its causes. In a meta-analysis, Bauer et al. (2016: 250) suggest that "people exposed to … violence tend to behave more cooperatively after." These findings are, broadly, supported within the wider literature (Bellows and Miguel, 2009; Blattman, 2009; Annan et al., 2011; Bateson, 2012; Voors et al., 2012; Hartman and Morse, 2015; Hopfensitz and Miquel-Florensa, 2014; Mironova and Whitt, 2014; Bauer et al., 2017).

However, a second set of articles directly contradicts this literature. Rohner et al. (2013) show reductions in trust and increases in the salience of ethnic identity in Uganda. Schubert and Lambsdorff (2014) show negative discriminatory behavior between Israelis and Palestinians. De Luca and Verpoorten (2015b) show reductions in trust in Uganda. Grossman et al. (2015) show that exposure to military action in Israel hardens attitudes towards rivals and reduces support for peaceful conflict resolution. Hager et al. (2019) show that neighborhoods worst affected by ethnic rioting in Kyrgyzstan exhibit lower levels of prosocial behavior; and Conzo and Salustri (2019) that exposure to World War II in early childhood lowers trust and social engagement in adulthood. Whitt and Wilson (2021) show almost no prosocial behavior (in the form of altruism) between formal rivals. Blanco (2013) shows that (violent) crime in Mexico is associated with reductions in trust in other people. Gangadharan et al. (2022) show increases in antisocial behavior as a result of historical exposure to genocide.

In between, a third cluster of findings are mixed or conditional. Improvements in one dimension – such as civic responsibility – are offset by reductions in others, like trust in government (Grosjean, 2014; Gilligan et al., 2014; de Juan and Pierskalla, 2016; Voors and Bulte, 2014; de Luca and Verpoorten, 2015a). Other qualifiers are present, too. Bauer et al. (2014a; 2014b) show effects only for exposure to violence in very specific age cohorts; Cecchi et al. (2015) show that the street footballers they study are more likely to receive in-game punishments. Gneezy and Fessler (2012) trade off increases in willingness to reward with increased preferences for costly punishments. Becchetti et al. (2014) show that victims of violence show higher trustworthiness but also respond more negatively when let down.[1]

It is surprising that, to date, the literature has not sought to analyze the structure to these apparently contradictory findings. In the next section, we seek to derive some of these potential predictors from evaluating the mechanisms mooted in literature to date.

1.2. Why might violence influence prosocial behaviors?

We cluster the mechanisms from the literature into two broad topics for expositional ease. First, are economic drivers, such as an increased value placed in social insurance. Here it is argued that the physical damage caused by violence increases returns on investments in social capital as people become more reliant on (informal) local systems. During episodes of violence, investments in social capital may have higher anticipated returns, or lower costs and risks, than those in human or physical capital. Such investments might also help to protect individuals during and after violence.

Second, are psychological explanations. While the experience of violence is linked to depression and trauma (e.g. Magruder and Yeager, 2009), people often tend to recover from these symptoms in time and that, in some cases, there is in fact "post traumatic growth" (Rosner and Powell, 2014). Here, at least some people respond to traumatic experiences by reflecting on what is important and reevaluating their lives. Consequent to this, the possibility that individuals place renewed value on relationships within their community arises, which can result in changes, for example, in other-regarding preferences, engagement in pro-social activities and so on.

In both cases, the logic broadly flows that war encourages individuals to build and invest in stronger (social) ties. In the next section, we theorize how and why these incentives might vary across two variables of interest: the nature of exposure to violence; and the behavioral domain in which violence is measured.

1.3. Do heterogeneous experiences interact with these mechanisms?

First, we consider economic motives. Here, the intuition is clear: individuals use prosocial behavior in order to invest in social capital, which can help them avoid being victimized by violence in the first place, or assist their recovery. From this, two things become clear. The first is that the nature of victimization, or anticipated victimization, matters. Those who have been most harmed, or expect to be most harmed, have greater incentives to "invest" in prosocial behaviors than those who were not, or who do not expect to be. Consequently, we expect variation in post-violence prosocial behaviors between those who were personally harmed and those who weren't; and amongst those who were most severely victimized. The second is that this mechanism appears to predict that people's activities are focused, most keenly, on cooperative processes that result, not only, in increased protection for individuals but across all involved individuals. In turn, this suggests that behavioral effects should be most keenly captured in cooperative activities, rather than across all forms of prosocial behavior. What is unclear form this observation is whether this implies that investments in prosocial behaviors increase overall, or if individuals substitute between forms of prosocial behavior. Consequently, it becomes possible that there might be increases in investment in some behaviors and corresponding reductions in others.

Post-traumatic growth implies the experience of trauma. The experience of living through an episode of violence is traumatic even for those who are not directly harmed but the nature of trauma delineates along the lines of private experiences. It is plausible to expect that those who are injured experience additional kinds of trauma not experienced by those who are not. Those who are worst injured might, too, face higher orders of trauma. Consequently, the degree and kind of trauma experienced, and thus the nature of post-traumatic growth, could also vary across individual exposure histories.

In turn, we theorize that the kinds of behavior studied and the type(s) of exposure most prevalent go some way to explaining the messy and contradictory, evidence presented in the literature to date. From these observations, we draw two specific hypotheses:

H1. Individuals who experience political violence directly, through being personally injured, will exhibit different responses compared to those who experience violence in other ways. The extent to which one experiences this violence delivers similar predictions.

H2. Effects are more likely to be captured in strategic settings that involve behaviors designed to maximize joint outcomes, such as cooperative tasks.

[1] An additional, and potentially relevant, strand of literature looks at group-based aspects of such results (Horowitz, 2001). Ingroups are often (but far from exclusively) the beneficiaries of increased prosocial behavior (Whitt and Wilson, 2007; Bauer et al., 2014a, 2014b; Cecchi et al., 2015; Beekman et al., 2017). Indeed, increases in such parochial social preferences (Fehr et al., 2013) could serve as a stimulus for further conflict (Halevy et al., 2008; Struch and Schwartz, 1989), substantially changing the interpretation of these findings. However, recent work explicitly shows effects that are not driven by ethnicity (Restrepo-Plaza and Fatas, 2022). Noting this potential, we have conducted analyses that rule out the presence of an ethic component in our data. These results are described in Barriga et al. (2020).

2. Data and methods

2.1. Data collection

We collect data on prosocial behaviors from a set of single-shot two-player incentivized behavioral games. We discuss the games played and the reasons for their selection in the next sub-section. We conducted experiments in four areas of Kenya: Kibera, Kawangware, Kisumu and Viwandani. We chose these places as all suffered extensive violence in 2007/2008, allowing us to draw a sample in which there is significant exposure to violence but also significant heterogeneity in exposure including individuals who were not harmed. None of these places have suffered structural conflictual violence since 2007/2008, allowing us to draw on experiences during this episode, rather than violence in day-to-day life.[2] Data were collected between 8 April and November 17, 2015, eight years after the episode. Theoretically, this opens up recall errors and a reduction in the salience of exposure to violence but such lags are common in the bulk of literature to date.[3]

To test the salience of the episode of violence, we "prime" some individuals by asking them to recall their experience of violence before completing a short survey and the behavioral games. The "unprimed" sample answered these questions at the end of the session. Those who were primed reported significantly lower perceptions of their own safety among members of other ethnicities than the unprimed group (two-sided Mann-Whitney test with $p < 0.001$) but do not report any differences in the level or severity of their experience of violence. From this, we conclude both that the salience of violence is high and that the elevated salience in the "primed" group does not lead to structural differences in recall. While we cannot definitively rule out broader recall biases, literature shows higher quality of recall of distinctive, rare, and major events, such as an episode of political violence (Waddill and McDaniel, 1998). To avoid gender effects, we collected data from 654[4] men aged between 18 and 73 (mean: 32 years).[5]

Our sample comprises: 99 individuals in Kawangware; 105 in Kisumu; 313 in Kibera; and 137 in Viwandani. Participants were recruited, only, from the Kikuyu and Luo populations. Recruitment took place as follows: First, advertisements were placed within the locations of interest. These advertisements included basic information about where and when the experiment would take place, the rewards on offer and contact information for the center where data were collected. Individuals self-selected their interest in participation. Callers were screened for eligibility (specifically: that they were male, over 18 years of age, had been resident in the respective neighborhoods in 2007 and 2008, were of either Kikuyu or Luo ethnicity and had not participated in earlier experiments involving the games we played). All eligible callers were invited to the sessions, with almost all invitees showing up. Due to this sampling approach, we make no claims about the representativeness of our data. Participants received a show-up fee of 250 Kenyan Shillings (KSH), approximately equal to one day's pay, in addition to payoffs from the games.

2.2. Game selection and descriptions

As noted in our derivation of H_2, theory suggests potential variations across different kinds of (prosocial) behavior. In order to test this, we require outcome measures that differ in the strategic space in which they sit. To this end, we asked participants to undertake five tasks: both roles in the trust game, the dictator game, the public goods game and the stag hunt game. These games delineate into cooperation tasks (where (joint) action maximizes social outcomes) and distribution tasks (where the payoff is fixed and the decision choses how it is split amongst the players).[6] In the first group are the public goods game, the stag hunt game and the Player A role in the trust game. In the latter are the Player B role in the trust game and the dictator game.

In the dictator game – commonly thought to measure altruism – individuals were asked to divide a fixed amount between themselves and a partner, about whom they were given a small amount of information. In the public goods game – assumed to capture cooperation – individuals could contribute to a public good along with a partner. All donations to the public good were multiplied by 1.5 and then divided equally between the two partners, while all sums not donated to the public good were kept by the player. In the

[2] In addition, all four places have significant Kikuyu and/or Luo populations, which facilitated the work on the ethnic components of our dataset, described in Barriga et al. (2020). Of course, these communities are not the only places which satisfy these criteria. From a practical perspective, they were selected as they were comparatively easy to enumerate.

[3] Annan et al. (2011) – 7 years after; Bauer et al. (2014a) – 8 years; Bauer et al. (2014b) – 5 years; Cassar et al. (2013) – 13 years. Cecchi et al. (2010) – 9 years. De Luca and Verpoorten (2015a; 2015b) – 12 years.

[4] The sample size was derived based on a 2:3 ratio of effect size and standard deviation, an anticipated 10% effect size and a desired power of 80%. These calculations suggested a minimum sample of 71 individuals per group in our analysis. Given the combination of locations, primes and treatments, our analysis has eight potential groups, requiring a minimum sample size of 568. Due to a policy of "over-inviting" individuals to ensure each session was full, hosting of additional sessions on the expectations that sessions would have spare capacity and the high attendance at our sessions, our targeting resulted in a final sample of 654.

[5] Men tend to behave different from women in at least two of the games we employ; the public goods game (Solow and Kirkwood, 2002) and the trust game (Buchan et al., 2008). Further, Garbarino and Slonim (2009) show that both men and women behave more generously towards women than they do towards men. While this approach enhances the narrow internal validity of our findings, results must be understood in this gendered context. Results from the literature to date "appear to hold for men and women, as well as children …" (Bauer et al., 2016: 250) but we cannot confirm that this strictly holds in our case.

[6] More completely, these games differ in two more specific dimensions. First is the degree of conflict of interest (Axelrod, 1967), which relates the gains from cooperation to the stakes of the game. The larger the gains from cooperation, the smaller the degree of conflict of interest. Second is the extent to which beliefs matter for equilibrium behavior.

stag hunt game – considered to capture coordination – individuals were asked if they would like to make an investment with a small, guaranteed, return; or a larger, riskier, prize that required – without conferral – that the partner make the same choice. In the Player A role of the trust game – capturing trusting behavior – individuals could send part of their endowment to their partner. What was sent was multiplied by 3 and given to the partner. Everything not sent was kept by the active player. In the Player B role of the trust game – capturing trustworthy behavior – the partner could return any amount of what was given to him to the partner, who made the choice in the Player A role.

2.3. Experimental set up

Participants play each game in a semi-random order, in order to minimize learning between games and across the experiment. As they are effectively the same decision, we impose that a participant should not play the Player B role in the trust game and the dictator game after each other. To accurately facilitate this requirement, players undertook the trust game first, with the first role played (Player A or Player B) selected randomly. Following the trust game, participants then randomly played either the public goods or stag hunt game. The order of the remaining games was fully randomized. To prevent other biases, such as individuals maximizing outcomes across all games, rather than within each, one game was randomly selected for payout after all tasks were completed. At no stage were participants made aware of the choices made by their partners, nor their outcome from any game. Payoffs were made at the end of the session, once all tasks and an associated survey had been completed. In order to ensure participants understood the games and the derivation of outcomes, a series of examples were given during the introduction to each game and participants asked to answer questions (e.g. how much they would need to choose to generate a particular outcome). We depict the experimental setup in Fig. 1.

The survey had two components. The first collected personal information; the second a 'conflict exposure module'. Individuals were asked questions relating to whether or not they were affected by the electoral violence in 2007 and how severely. We ask individuals the degree to which they were personally injured; the degree to which family had been injured; and whether or not they witnessed strangers being injured. Specific examples of injuries were given to remove frame of reference problems.[7]

Players were given an endowment of 250 KSH in each game and asked to select any amount as their choice, except in the stag hunt game, where they faced the choice of keeping or investing the endowment. To minimize noise, participants selected choices spaced at 50 KSH intervals. In the case of the public goods game, the dictator game and the Player A role in the trust game, the choice was enumerated directly. Data were elicited using the strategy method in the Player B role of the trust game, with the outcome variable derived as the mean proportion returned. The stag hunt game is enumerated as a dummy variable, taking the value of 1 if an individual chooses to invest. The full wording of each game can be found in the Annex, along with information on the broader experimental process and the surveys. Summary statistics for each choice, split by violence experience type, are shown in Table 1.

2.4. Methods

In order to identify causal effects, we follow the precedent of the literature in showing conditional unconfoundedness. While we note that it is nearly impossible to assume that all determinants of the experience of violence are observable, the settings we analyze and the specific combination of results we present suggest this approach is sufficient to establish causality. The factors that determine selection into a neighborhood could jointly define prosociality and exposure but we look at differential experiences within small neighborhoods. In this regard, we argue that some element of chance is involved in exposure type and even more so with exposure severity. It is, of course, possible that individuals with higher prosocial preferences sought to help those injured or to protect their communities and, in doing so, put themselves in danger, increasing their likelihood of being injured or, indeed, undertook acts of violence, and potentially defense, as a prosocial choice and becoming injured in the process.

To counter this concern, we first differentiate extent of injuries and repeat analyses on these indicators. While elevated underlying prosocial preferences could result in an individual being injured in the first place, some element of chance or (bad) luck is still involved that differentiates – for example – a bruised arm from a broken one. Similarly, by looking across different kinds of prosocial behavior (e.g. distribution and cooperation tasks), we further argue against such bias being a potential driver of our results. It is not *a priori* clear, for example, why more cooperative individuals would not place themselves in harm's way, while more altruistic individuals do, or vice versa. Consequently, delineation in our findings across these categories (see results section) challenges such interpretations. Similarly, if individuals are more likely to participate in violence as a result of underlying prosocial tendencies, they should not only be more likely to become injured themselves but also more likely to witness others becoming injured. Again, therefore, any delineation in results between being personally harmed and witnessing violence against strangers challenges the existence of bias of this form.[8] To

[7] The specific questions used are: "Now we would like to ask you about your experiences during the elections in 2007. Some of these experiences might be upsetting to think or talk about. Please remember that your answers are confidential. We are going to mention several events. Please tell us if you experienced any.1) Were you physically injured? (1) No; (2) Minor (e.g. being pushed around); (3) Some (e.g. cuts or bruises); (4) Significant (e.g. heavy beating); (5) Severe (e.g. leading to permanent disabilities).2) Were one or more of your kin physically injured? (1) No; (2) Minor (e.g. assault, being pushed around); (3) Some (e.g. cuts or bruises); (4) Significant (e.g. heavy beating); (5) Severe (e.g. leading to permanent disabilities); (6) Killed.3) Did you witness mob violence or mob justice? (1) No; (2) Physical harming/assault of others; (3) Killing of others.

[8] We note that other accusations could be laid against our approach. For the sake of brevity, we do not deal with them all here but note that they can be similarly refuted by the structure of our results across both the strategic space of the games studies and the delineation of different exposure types.

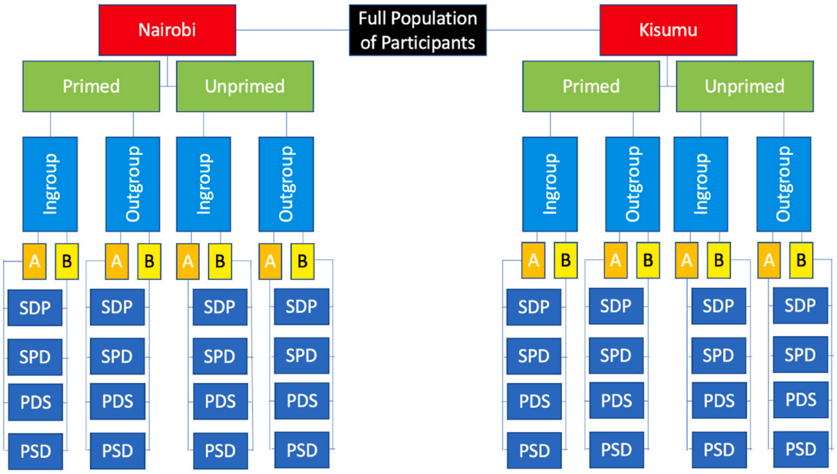

Fig. 1. Experimental Setup
Legend: A = trust player A; B = trust player B; S = stag hunt game; D = dictator game; P = public goods game.

Table 1
Basic summary stats on choices in games by kind of exposure.

	Personally Injured			Family Injured			Witnessing Violence Against Strangers		
	(1)	(2)	(3)	(1)	(2)	(3)	(1)	(2)	(3)
VARIABLES	N	mean	sd	N	mean	sd	N	mean	sd
Trust Game PA	273	96.34	58.67	371	97.44	59.28	258	98.64	59.89
Trust Game PB	273	44.59	22.60	371	43.25	23.05	258	43.00	23.05
Public Goods	269	134.9	71.41	363	136.6	73.26	253	135.4	76.66
Dictator	273	95.42	55.96	371	90.03	53.60	258	91.47	57.04
Stag Hunt	273	0.615	0.487	371	0.631	0.483	258	0.647	0.479

establish conditional unconfoundedness, we first test whether or not there are observable determinants of exposure type by running a simple regression on each violence exposure type on a range of survey measures:

$$ExposureType_{ij} = \gamma + \varphi_1 X_{ij} + \upsilon_{ij} \tag{1}$$

where: *ExposureType* is a binary variable that captures whether individual *i* in location *j* experienced that violence in that way; γ is the regression constant; X is a $k \times n$ matrix of survey variables; φ_i is a vector of regression coefficients; and υ is the idiosyncratic error. In the case of full conditional unconfoundedness, one would expect $\varphi_i = 0 \mid \forall_i$.

Results are shown in Table 2 and exhibit little sign of selection into any kind of exposure.[9] Importantly, past prosocial behaviors - enumerated via voting behavior in the 2007 election, balances across all exposure types. This indicator related to an activity undertaken before the episode of violence studied, and is not – therefore – an outcome indicator of the analysis. It is often used as a barometer of prosocial behavior (e.g. Cassar et al., 2013; Grosjean, 2014; de Luca and Verpoorten, 2015b). This further refutes the argument that prior preferences or behaviors could have resulted in injuries. More general summary statistics for each region are shown in Table 3.

Given the balance shown in Table 2 and the nature of the data we hold, we estimate a series of OLS regressions of the form:

$$Choice_{ij} = \alpha + \beta_1 Personal_i + \beta_2 Family_i + \beta_3 Witness + \beta_4 X_i + \varepsilon_i \tag{2}$$

where: *Choice* is the decision made by individual *i* in game *j*; α is the constant; *Personal*, *Family* and *Witness* are binary variables that capture individual exposure histories; X is a set of control variables pertaining an individual's location; and ε is the idiosyncratic error of the equation.

We run a series of additional analyses using variants of Equation (2). First, we include a dummy variable denoting whether or not an individual was "primed" (i.e. answered the conflict exposure module before completing the tasks) and its interaction with the exposure identifiers, to control for potential conflation of results with those of the experiment. Second, we replace binary exposure variables

[9] Across 13 × 3 variables and exposure types, we find significant effects in only three indicators (7% of the indicators). Of these, two out of three of those indicators are a feasible outcome of the experience of violence (Tenure Type).

Table 2
Analysis showing structural determinants of experiencing violence.

	(1)	(2)	(3)
VARIABLES	Personally Injured	Family Injured	Witnessed Violence
Dwelling Type	0.0114	0.0116	−0.00352
	(0.0127)	(0.0135)	(0.0117)
Tenure Type	0.125**	0.134**	0.0399
	(0.0419)	(0.0444)	(0.0387)
Tenure Duration	−6.18e-08	−7.22e-08	−5.84e-08
	(4.36e-08)	(4.63e-08)	(4.03e-08)
Literacy	−0.0493	−0.0213	−0.0118
	(0.0397)	(0.0421)	(0.0367)
No. Hours Slept Last Night	0.00174	0.000586	0.00368
	(0.00919)	(0.00975)	(0.00850)
Has Travelled Internationally	−0.0609	−0.0274	−0.0588
	(0.0375)	(0.0398)	(0.0347)
Size of Household	0.000306	0.000633	0.000326
	(0.000407)	(0.000432)	(0.000376)
Number of Siblings	1.23e-10	0.000	4.10e-10
	(5.37e-10)	(5.69e-10)	(4.96e-10)
Distance to the Airport	4.40e-05	−7.78e-05	−8.55e-05
	(8.14e-05)	(8.64e-05)	(7.52e-05)
No. Hours Internet Access in Last Week	−0.0437*	−0.0163	0.0209
	(0.0209)	(0.0222)	(0.0193)
No. Hours Phone Access in Last Week	−0.0379	0.00224	0.0207
	(0.0299)	(0.0317)	(0.0277)
Income	−4.63e-06	7.55e-07	4.57e-06
	(4.85e-06)	(5.14e-06)	(4.48e-06)
Voted in 2007 Election	−0.0154	0.00595	−0.000970
	(0.0320)	(0.0340)	(0.0296)
Constant	0.887***	0.697***	0.752***
	(0.196)	(0.208)	(0.181)
Observations	654	654	654
R-squared	0.084	0.082	0.068

Note: Analysis conducted using OLS. Column (1) shows determinants of experiencing violence personally; Column (2) of family members experiencing violence; and Column (3) of witnessing violence happening to strangers. *, ** and *** = $p < 0.05$, $p < 0.01$ and $p < 0.001$ respectively.

Table 3
Basic summary statistics of survey data (split by region).

	Kawangware			Kibera			Kisumu			Viwandani		
	(1)	(2)	(3)	(1)	(2)	(3)	(1)	(2)	(3)	(1)	(2)	(3)
VARIABLES	N	mean	sd	N	mean	sd	N	mean	sd	N	mean	sd
Dwelling Type	99	2.286	1.285	313	1.922	1.324	105	2.162	0.622	137	2.107	1.605
Tenure Type	99	1.952	0.436	313	1.952	0.357	105	1.714	0.514	137	1.910	0.386
Tenure Duration	99	53.96	238.7	313	36.85	60.5	105	20.80	14.40	137	17.58	9.917
Literacy	99	1.250	0.436	313	1.274	0.447	105	1.086	0.281	137	1.303	0.462
No. Hours Slept Last Night	99	6.762	2.166	313	6.663	1.917	105	6.695	1.481	137	6.779	1.861
Has Travelled Iternationally	99	1.738	0.442	313	1.711	0.454	105	1.524	0.502	137	1.795	0.405
Size of Household	99	16.39	108.7	313	6.226	26.85	105	4.429	2.499	137	4.893	9.396
Number of Siblings	99	14.92	108.8	313	4.048	3.654	105	4.352	2.469	137	3.074	1.877
Distance to the Airport	99	82.93	121.7	313	84.40	312.1	105	36.34	18.78	137	41.07	29.94
No. Hours Internet Access in Last Week	99	2.095	0.830	313	1.978	0.795	105	2.276	0.849	137	2.049	0.822
No. Hours Phone Access in Last Week	99	2.548	0.547	313	2.437	0.573	105	2.743	0.481	137	2.385	0.636
Income	99	1769	1818	313	1935	3711	105	3399	4426	137	1938	3165
Voted in 2007 Election	99	1.286	0.550	313	1.278	0.559	105	1.105	0.308	137	1.311	0.604

with an ordinal variable that captures the extent of exposure. Third, we conduct FWER multiple hypothesis corrections using Westfall-Young step-down p-values. Finally, we use the experiment that varies the salience of exposure to violence, by asking some individuals to answer the ‘conflict exposure module’ before they completed the behavioral tasks and some after, as both an alternative measure of conflict exposure and as a test of the causality of our findings.

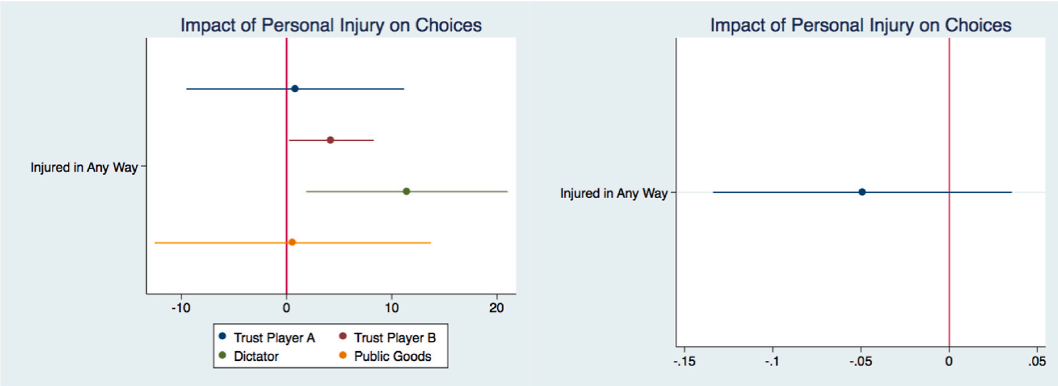

Fig. 2. Impact of personal injury (any exposure versus no exposure at all) on choices in Trust A, Trust B, Dictator and Public Goods Games (Left) and Stag Hunt Game (Right).

Table 4
Impact of differing exposure types (any type of exposure versus No exposure at all) on choices in behavioral games.

	(1)	(2)	(3)	(4)	(5)
VARIABLES	Trust A	Trust B	Dictator	Public Goods	Stag Hunt
Personally Injured	0.836	4.255**	11.44**	0.604	−0.0491
	(5.264)	(2.052)	(4.875)	(6.680)	(0.0432)
Family Injured	5.833	1.227	−4.214	8.216	−0.0103
	(5.177)	(2.018)	(4.794)	(6.561)	(0.0424)
Witnessed Violence	6.253	0.552	0.760	2.566	0.0256
	(4.960)	(1.934)	(4.594)	(6.297)	(0.0407)
Viwandani	−3.346	−2.133	−7.442	−11.08	−0.0526
	(6.245)	(2.435)	(5.784)	(7.945)	(0.0512)
Kawangware	19.42***	4.428	11.68*	17.57*	−0.0517
	(7.170)	(2.795)	(6.640)	(9.121)	(0.0588)
Kisumu	17.55***	5.608**	1.756	18.11**	0.00462
	(6.709)	(2.616)	(6.213)	(8.459)	(0.0550)
Constant	83.91***	38.31***	86.60***	124.2***	0.673***
	(4.700)	(1.832)	(4.353)	(5.976)	(0.0385)
Observations	654	654	654	642	654
R-squared	0.026	0.023	0.019	0.022	0.005

Note: Analysis conducted using OLS. Column (1) shows impacts on choices as Player A role in the Trust Game; Column (2) impacts on choices in the Player B role in the Trust Game; Column (3) choices in the Dictator Game; Column (4) choices in the Public Goods Game; and Column (5) choices in the Stag Hunt Game. All "Injury" variables (Personally Injured; Family Injured; Witnessed violence) are binary variables taking the value of 0 if an individual had no exposure and 1 if an individual had any exposure, regardless of severity. Viwandani, Kawangware and Kisumu are regional fixed effects, taking the value of 1 if an individual is from that place, with Kibera the reference location. Standard errors shown in parentheses. *, ** and *** = $p < 0.05$, $p < 0.01$ and $p < 0.001$ respectively.

3. Results

3.1. Main results

First, we present the impact of each binary form of exposure history on each behavioral choice. We show results in Fig. 2-4[10] and in Table 4. In Fig. 2, we see a positive and significant (5%) effect of personal injury on giving in the dictator game and the Player B role of the trust game. In the dictator game those personally injured give away just over 11 KSH (4.4% of the endowment) more, all other things held constant. All other results are strongly insignificant. These results delineate neatly across the strategic space of the games. In distribution tasks, we show positive and significant effects of personal exposure to violence. In cooperation games, results are highly insignificant. While this challenges the specifics of H_2, it supports the wider notion that impacts should arise in particular behaviors.[11]

In Figs. 3 and 4, these results are not replicated. We see no signs of significant differences as a result of injury to family or witnessing violence. Again, the effects (generally) trend positive but are highly insignificantly different from zero. In combination with the

10 In all coefficient plots, the dot shows the point estimate and the line the 95% confidence interval.

11 This combination of results also lends credence to the nature of causality in our work. Should unobservable prior levels of prosocial behavior lead individuals to place themselves in harm's way, we should see similar results across all behaviors. It is difficult to think of plausible explanations as to why more altruistic individuals would be more likely to be in harm's way but not more cooperative individuals.

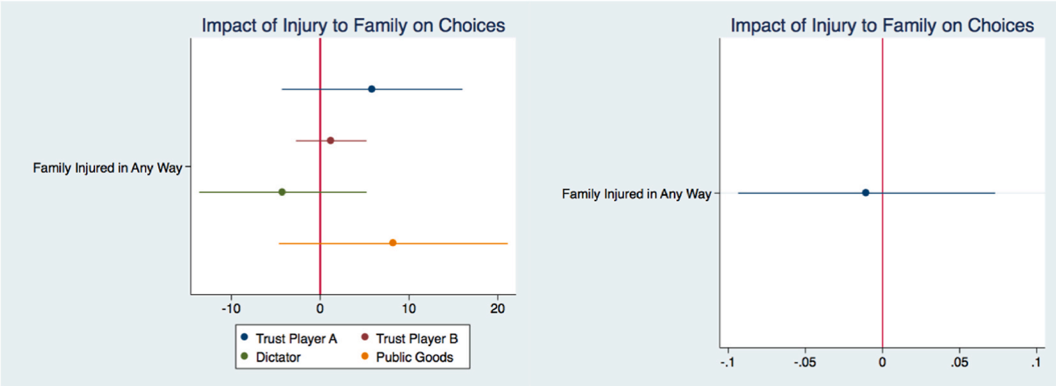

Fig. 3. Impact of family injury (any exposure versus no exposure at all) on choices in Trust A, Trust B, Dictator and Public Goods Games (Left) and Stag Hunt Game (Right).

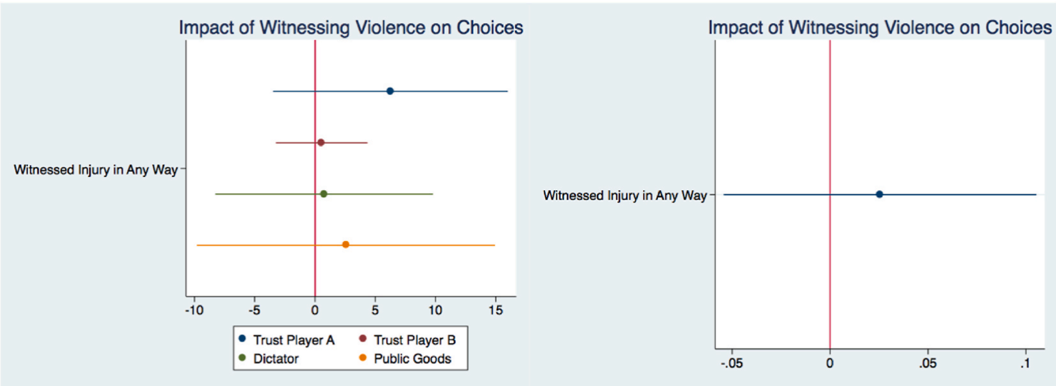

Fig. 4. Impact of witnessing injury (any exposure versus no exposure at all) on choices in Trust A, Trust B, Dictator and Public Goods Games (Left) and Stag Hunt Game (Right).

findings shown in Fig. 2, this lends credence to H_1 – that the application of some common theories predicts variations in impacts depending on how one experiences an episode of violence.

3.2. Additional analyses

3.2.1. Severity of exposure

First, we note that severity of exposure should result in variations in behavior. This test is important in establishing causality. While

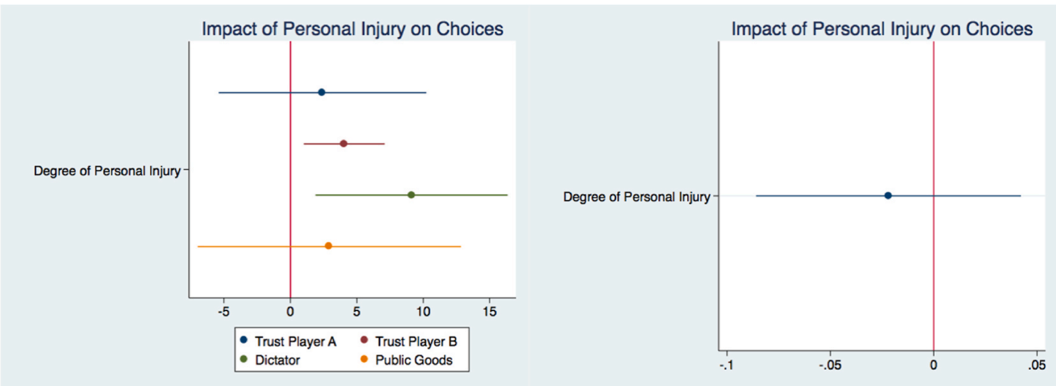

Fig. 5. Impact of degree of personal injury on choices in Trust A, Trust B, Dictator and Public Goods Games (Left) and Stag Hunt Game (Right).

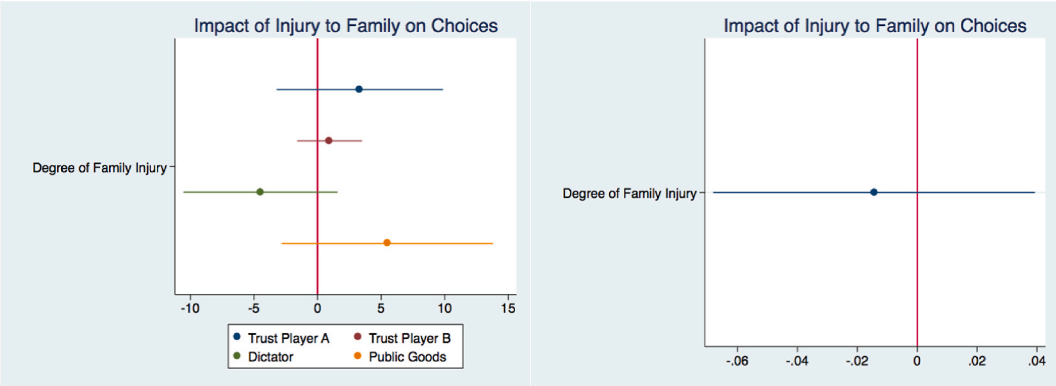

Fig. 6. Impact of degree of family injury on choices in Trust A, Trust B, Dictator and Public Goods Games (Left) and Stag Hunt Game (Right).

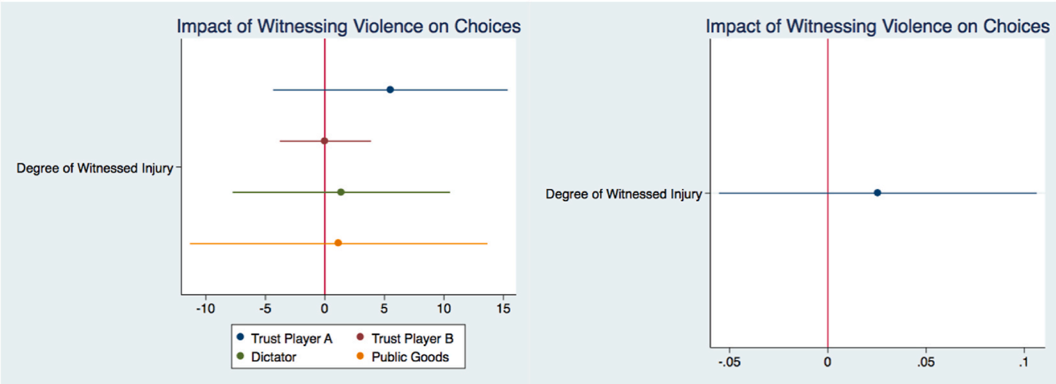

Fig. 7. Impact of degree of violence witnessed on choices in Trust A, Trust B, Dictator and Public Goods Games (Left) and Stag Hunt Game (Right).

Table 5
Impact of severity of differing exposure types on choices in behavioral games.

	(1)	(2)	(3)	(4)	(5)
VARIABLES	Trust A	Trust B	Dictator	Public Goods	Stag Hunt
Personally Injured	2.411	4.052***	9.123**	2.932	−0.0219
	(3.978)	(1.547)	(3.683)	(5.047)	(0.0326)
Family Injured	3.321	0.945	−4.483	5.486	−0.0144
	(3.334)	(1.296)	(3.086)	(4.235)	(0.0273)
Witnessed Violence	5.506	0.0374	1.379	1.160	0.0254
	(5.019)	(1.952)	(4.647)	(6.369)	(0.0412)
Viwandani	−3.087	−2.002	−7.670	−10.65	−0.0506
	(6.239)	(2.426)	(5.776)	(7.933)	(0.0512)
Kawangware	19.46***	4.387	11.40*	17.79*	−0.0488
	(7.157)	(2.783)	(6.626)	(9.102)	(0.0587)
Kisumu	17.43***	5.634**	1.885	18.01**	0.00965
	(6.687)	(2.600)	(6.191)	(8.428)	(0.0549)
Constant	83.98***	38.15***	87.69***	123.7***	0.667***
	(4.439)	(1.726)	(4.110)	(5.642)	(0.0364)
Observations	654	654	654	642	654
R-squared	0.027	0.028	0.020	0.024	0.004

Note: Analysis conducted using OLS. Column (1) shows impacts on choices as Player A role in the Trust Game; Column (2) impacts on choices in the Player B role in the Trust Game; Column (3) choices in the Dictator Game; Column (4) choices in the Public Goods Game; and Column (5) choices in the Stag Hunt Game. All "Injury" variables (Personally Injured; Family Injured; Witnessed violence) are ordinal variables taking the value of 0 if an individual had no exposure, 1 if an individual had minor exposure and 2 if an individual had a major exposure. Viwandani, Kawangware and Kisumu are regional fixed effects, taking the value of 1 if an individual is from that place, with Kibera the reference location. Standard errors shown in parentheses. *, ** and *** = $p < 0.05$, $p < 0.01$ and $p < 0.001$ respectively.

it is possible that unobserved variables jointly determine choices in the games and conflict experience, these processes do not fully explain why some are more severely injured. We generate ordinal measures that differentiate no exposure, minor exposure and serious exposure. Results are shown in Figs. 5–7 and Table 5 and show an identical pattern to our main analyses.

3.2.2. Multiple hypothesis correction

Across multiple games and exposure types, the possibility of statistical errors arising is quite high. We correct our p-values for multiple hypotheses using a FWER correction based on Westfall-Young stepdown adjusted p-values with 1000 bootstraps. We present these adjusted p-values, along with uncorrected p-values, in Tables 6 and 7 for the binary and ordinal exposure histories, respectively. These corrections do not change our main takeaways much, although we note a marginal loss of significance of some findings.

3.2.3. Including the prime and its interactions

As noted, we included an experiment within our analysis that aimed to influence the salience of violence among some individuals. As salience could influence the effects of recalled violence, we augment Equation (2) to include this prime and its interaction with each exposure type. We estimate marginal responses to understand joint impacts of the prime and unprime exposure types. Results are presented in Table 8. The key takeaways, again, are identical to those of our main analyses.

3.3. Experiment on salience of violence

As a final test to refute the potential that our findings are driven by biases in selection into the experience of violence, we focus a final analysis specifically on the experiment conducted within our data collection. The purpose of this experiment was to vary the salience of the episode of violence, by randomly asking some individuals to answer the 'conflict exposure module' before they undertook the behavioral tasks and others to answer it afterwards. We form a slightly different theoretical expectation of this data, in the sense that when such "salience"-based primes have been used in the literature (e.g. Silva and Mace, 2015), associated prosocial behavior tends to decrease. However, should our main hypotheses – that behavioral outcomes vary across exposure histories and behavioral domains – hold, we should still see this delineated within the experiment. To this end, we test the impact of the salience of the episode of violence across each exposure type and the full range of behavioral outcomes. Results are shown in Table 9 with the first regression – focusing on personal injury – in Row1; the second regression – focusing on injury to family – in Row 2; and the effects of witnessing violence in Row 3. As above, p-values are corrected for tests of multiple hypotheses using the Westfall-Young Stepdown approach.

The results shown in Table 9 support, both, the main hypotheses of our work and or theoretical expectation that any effects from priming the salience of violence should be negative. As before, we see significant effects in specific games – in this case, only the dictator game – and that these significant effects arise, only, from personal exposure to violence. These results, the outcome of a randomized experiment, confirm the headline findings of this work – that the nature of exposure to violence and the kind of behavior are important mediators of behavioral responses to violence. Moreso, they also show that effects moving in multiple directions, based on how conflict experience is captured, can be derived from our dataset, further embedding this work in the literature to date.

4. Discussion and conclusions

While the adversities that political violence can induce on victimized populations is well-known, its impact on prosocial behavior is less clear – certainly, in the sense that prosocial behaviors are not universally harmed in the medium- to long-term aftermath of political violence. In a number of situations, prosocial behaviors are observed to improve, although this, too, is not universal. In this

Table 6
Multiple hypotheses corrections for different exposure types (no exposure versus any kind of exposure at all) on choices in behavioral games.

	(1)	(2)	(3)	(4)	(5)
VARIABLES	Trust A	Trust B	Dictator	Public Goods	Stag Hunt
Personally Injured	0.836	4.255	11.44	0.604	−0.0491
	(0.544)	(0.008)***	(0.013)**	(0.561)	(0.503)
	[0.861]	[0.034]**	[0.043]**	[0.861]	[0.861]
Family Injured	5.833	1.227	−4.214	8.216	−0.0103
	(0.319)	(0.466)	(0.146)	(0.195)	(0.599)
	[0.670]	[0.704]	[0.554]	[0.580]	[0.704]
Witnessed Violence	5.506	0.0374	1.379	1.160	0.0254
	(0.273)	(0.985)	(0.767)	(0.856)	(0.537)
	[0.757]	[0.991]	[0.982]	[0.982]	[0.994]
Observations	654	654	654	642	654

Note: Analysis conducted using OLS regressions and p-values corrected for multiple hypotheses using Westfall-Young Stepdown adjusted p-values derived using 1000 bootstraps. Column (1) shows the marginal impact on choices as Player A role in the Trust Game; Column (2) marginal impacts on choices in the Player B role in the Trust Game; Column (3) choices in the Dictator Game; Column (4) choices in the Public Goods Game; and Column (5) choices in the Stag Hunt Game. All "Injury" variables (Personally Injured; Family Injured; Witnessed violence) are binary variables taking the value of 0 if an individual had no exposure and 1 if an individual had any kind of exposure. Uncorrected p-Values shown in (parentheses) and Westfall-Young Stepdown adjusted p-values in [square brackets]. *, ** and *** = $p < 0.05$, $p < 0.01$ and $p < 0.001$ respectively.

Table 7
Multiple hypotheses corrections for different exposure types (severity of exposure) on choices in behavioral games.

	(1)	(2)	(3)	(4)	(5)
VARIABLES	Trust A	Trust B	Dictator	Public Goods	Stag Hunt
Personally Injured	2.411	4.052***	9.123**	2.932	−0.0219
	(0.874)	(0.038)**	(0.019)**	(0.928)	(0.255)
	[0.989]	[0.123]	[0.078]*	[0.989]	[0.542]
Family Injured	3.321	0.945	−4.483	5.486	−0.0144
	(0.259)	(0.543)	(0.379)	(0.210)	(0.809)
	[0.656]	[0.788]	[0.760]	[0.656]	[0.810]
Witnessed Violence	6.253	0.552	0.760	2.566	0.0256
	(0.207)	(0.775)	(0.869)	(0.864)	(0.530)
	[0.652]	[0.962]	[0.962]	[0.962]	[0.946]
Observations	654	654	654	642	654

Note: Analysis conducted using OLS regressions and p-values corrected for multiple hypotheses using Westfall-Young Stepdown adjusted p-values derived using 1000 bootstraps. Column (1) shows the marginal impact on choices as Player A role in the Trust Game; Column (2) marginal impacts on choices in the Player B role in the Trust Game; Column (3) choices in the Dictator Game; Column (4) choices in the Public Goods Game; and Column (5) choices in the Stag Hunt Game. All "Injury" variables (Personally Injured; Family Injured; Witnessed Violence) are binary variables taking the value of 0 if an individual had no exposure and 1 if an individual had any kind of exposure. Uncorrected p-Values shown in (parentheses) and Westfall-Young Stepdown adjusted p-values in [square brackets]. *, ** and *** = $p < 0.05$, $p < 0.01$ and $p < 0.001$ respectively.

Table 8
Impact of different exposure types (no exposure versus any kind of exposure at all) on choices in behavioral games.

	(1)	(2)	(3)	(4)	(5)
VARIABLES	Trust A (dy/dx)	Trust B (dy/dx)	Dictator (dy/dx)	Public Goods (dy/dx)	Stag Hunt (dy/dx)
Personally Injured	1.254	4.370**	11.373**	0.858	−0.047
	(0.812)	(0.034)	(0.020)	(0.898)	(0.276)
Family Injured	5.630	1.237	−3.620	8.125	−0.009
	(0.278)	(0.542)	(0.450)	(0.218)	(0.828)
Witnessed Violence	6.346	0.443	−0.209	2.381	0.025
	(0.203)	(0.820)	(0.964)	(0.707)	(0.549)
Observations	654	654	654	642	654

Note: Analysis shows marginal effects of each injury typology, based on an OLS regression including each exposure kind (Personal Injury; Family Injured; Witnessed violence) and the interaction of each of these exposure kinds with the "prime" that asked some individuals to recall their exposure before completing the experimental tasks. Column (1) shows the impact on choices as Player A role in the Trust Game; Column (2) marginal impacts on choices in the Player B role in the Trust Game; Column (3) choices in the Dictator Game; Column (4) choices in the Public Goods Game; and Column (5) choices in the Stag Hunt Game. All "Injury" variables (Personally Injured; Family Injured; Witnessed violence) are binary variables taking the value of 0 if an individual had no exposure and 1 if an individual had any kind of exposure. P-Values shown in parentheses. *, ** and *** = $p < 0.05$, $p < 0.01$ and $p < 0.001$ respectively.

Table 9
Impact of the salience of violence prime on choices in Trust A, Trust B, Dictator and Public Goods for those who were personally injured (Row 1); who had family members injured (Row 2); and who witnessed violence against strangers (Row 3).

	(1)	(2)	(3)	(4)	(5)
VARIABLES	Trust A	Trust B	Dictator	Public Goods	Stag Hunt
Personally Injured	0.031	−1.872	−17.66	−4.404	−0.121
	(0.997)	(0.920)	(0.040)**	(0.934)	(0.968)
Family Injured	−6.623	−1.153	−11.451	−2.915	−0.020
	(0.717)	(0.947)	(0.185)	(0.947)	(0.947)
Witnessed Violence	3.709	1.292	−14.18	3.676	0.0342
	(0.961)	(0.961)	(0.213)	(0.961)	(0.961)

Note: Analysis conducted using OLS regressions and p-values corrected for multiple hypotheses using Westfall-Young Stepdown adjusted p-values derived using 1000 bootstraps. Column (1) shows the impact on choices as Player A role in the Trust Game; Column (2) marginal impacts on choices in the Player B role in the Trust Game; Column (3) choices in the Dictator Game; Column (4) choices in the Public Goods Game; and Column (5) choices in the Stag Hunt Game. Each row (Personally Injured; Family Injured and Witnessed Violence) present the results of an individual regression on individuals with that exposure type. Westfall-Young Stepdown adjusted p-values in (parentheses). *, ** and *** = $p < 0.05$, $p < 0.01$ and $p < 0.001$ respectively.

article, we posit a simple but substantial concern: A substantive body of work has looked at the relationship between violence and prosocial behavior and has sought to understand the routes through which they are linked, yet results across the literature are, at best, mixed and at worst, quite messy. Despite a lack of clarity, the determinants and correlates of this messiness have not, yet, been considered.

In an effort to understand some of these correlates, we run behavioral experiments with individuals exposed to electoral violence in Kenya. We play a series of games that link to prosocial behavior, generally, but that differ in the nature of the task and outcome: those that are based on cooperation; and those that are based on distribution. We match choices in each of these games to individual exposure histories to violence. Our results show two determinants of heterogeneous within our analysis, which match some of the heterogeneities seen to date in the literature. First, we find effects, only, in distribution scenarios. Second, we find effects only as a consequence of personal injury. Having family who were injured or witnessing violence perpetrated against strangers has no effect on any choices in our analyses. Separate work carried out on this dataset (Barriga et al., 2020) confirms that these outcomes are not the product of parochial drivers in behavior but, rather, reflect a more general increase in prosocial behavior across partner identities.

Our results suggest a very specific pattern of prosocial behavioral responses to violence, which is useful in considering the relevance of the mechanisms that have been posited to link the two. We show that only individuals who are personally injured are more willing to engage in prosocial behaviors. More so, the behaviors they are more willing to engage in are those that are personally costly and not those that increase social welfare. A range of the mechanisms considered to date have predictions that do not satisfy these implications. Investments in social capital, for example, are to the benefit of society as well as the individual, and thus can be viewed as cooperative, rather than distributional, behaviors. Expressive preferences, more generally, tend to focus on behaviors that are "good" for society as a whole. Our results call into question the relevance of these mooted mechanisms, at least in the Kenyan case.

Rather, these results are consistent with broader altruism and prosocial behavior literature. For example, prosocial behavior can be used to win friendship (Olson, 1965) and to signal good intentions (Becker, 1974). Since prosocial behaviors are often used to project public image (Lacetera and Macis, 2010; Bénabou and Tirole, 2006), choices to engage prosocially after violence might signal friendship, peace or good intentions. After violence, we know people are willing to reward good behavior and to punish bad behavior (Sääksvuori et al., 2011; Gneezy and Fessler, 2012; Schiller et al., 2014), suggesting, too, a meaningful response to such signals. Since the experience of conflict is costly (Fearon, 1995), rational individuals should be willing to expend resources to avoid it.

It is, therefore, a plausible, although far from a unique explanation, that individuals who have experienced political violence are willing to use particular prosocial behaviors as a costly means to avoid future violence (Schaub, 2017). While the motivations of this are similar to investments in social capital, the underpinning mechanisms and specific predictions are qualitatively different. Increased generosity, but not increased cooperation, in response to the private experience of violence is consistent with an economic signal, which should be costly and observable, as well as understood by the signal's receiver. This holds for generous or altruistic behavior. The sender accepts a cost by choosing between his income and that of another person. Since the receiver understands the rules of the game, this decision is attributable as the sender choosing the receiver's welfare over his own. In cooperation settings, motivation cannot be so easily attributed as senders' actions could be interpreted as a desire to enrich themselves or to enrich society, as well as the receiver. It is not clear that cooperative actions are costly for the sender, for the same reasons. The potential that "costly" prosocial behavior is used as a "peaceful" signal in post-conflict societies might be worth more direct investigation in future research.

More generally, we do not strictly deviate from the major body of literature to date, which has shown exposure to conflict and prosocial behavior to be positively correlated, although our additional analyses also produce a negative relationship, when we vary the salience, rather than experience, of violence. The refinement of our results is that such outcomes do not hold for all exposure histories or in all behavioral domains. This opens up the potential that how behavior, at the societal level, evolves after conflict could be the product of the sum of individual experiences. How exposure histories aggregate on a national scale might influence how society as a whole emerges from episodes of violence. In cases of intercommunal violence, prosocial attitudes might be more likely to emerge as more individuals privately experience violence than in more idealized-type conflicts that are fought by professional soldiers on front lines distant from civilian populations. Future research, therefore, might like to consider the relationship between conflict structure and post-conflict behaviors in a pooled or more aggregate sense.

We thus conclude that there are indeed grounds to accept that violence can result in prosocial behaviors but that such outcomes are far from certain. Despite the clarity and theoretical foundation of these findings, we note a small number of caveats through which our results should be screened:

First, we sample our responses, only, from men. While there are good grounds to have made this decision, based on the literature, there is no guarantee that these findings can be extended to women. At the same time, we note that Bauer et al. (2016) present evidence that findings do extend to men and women (as well as children), somewhat limiting the impact of this caveat. Second, due to our sampling process, we cannot guarantee that our data is representative and, consequently, that our findings are not sensitive to alternative draws of data from the same populations. Again, however, our results sit clearly in the literature to date, somewhat mitigating the degree of risk.

Third, we lack a "pure" control group, in the sense that all of our observations not only lived through an episode of violence but lived in particularly violent places during it. We are not unique in the literature in this sense. Indeed, we are unaware of any other work that has looked at exposed and non-exposed populations in the purest sense – even individuals who are not harmed live in conflict-affected countries, for example, which brings covariate as well as idiosyncratic shocks. Future research might like to consider the question of the overall population level impact of conflict on prosocial behavior, rather than on the relative outcomes of different groups within the exposed population.

Finally, we rely on what we think of as an advanced form of conditional unconfoundedness to causally identify the main relationships at hand. We note little sign of structural determinants of exposure type or severity and no prior differences in reported prosocial behavior (proxied by voting history before the violence in 2007/8) in our data. We further this by considering the severity or injuries, as well as whether or not an individual was injured or not and by using an experiment that primes on the salience of violence. Our combination of results further supports our causal interpretation. We see no good reasons to believe that some prior prosocial behaviors, like those that govern altruistic behavior, would lead individuals to place themselves in harm's way, while others, like cooperation, would not. Should such biases be present, we would therefore expect to see positive relationships between exposure to violence and prosocial behaviors across the board of behaviors we analyze; and across forms of exposure to violence such as witnessing harm to others and not just being personally injured. That we see effects, only, in specific domains shows not only the interestingness of our analyses but also further supports the causality of the findings in question.

Data availability

Data and codes will be made available upon publication of articles using this dataset.

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Appendix A. Supplementary data

Supplementary data to this article can be found online at https://doi.org/10.1016/j.ejpoleco.2022.102352.

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