

# Civil War and Social Cohesion: Lab-in-the-Field Evidence from Nepal

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*We study effects of wartime violence on social cohesion in the context of Nepal's 10-year civil war. We begin with the observation that violence increased levels of collective action like voting and community organization—a finding consistent with other recent studies of postconflict societies. We use lab-in-the-field techniques to tease apart such effects. Our causal-identification strategy exploits communities' exogenous isolation from the unpredictable path of insurgency combined with matching. We find that violence-affected communities exhibit higher levels of prosocial motivation, measured by altruistic giving, public good contributions, investment in trust-based transactions, and willingness to reciprocate trust-based investments. We find evidence to support two social transformation mechanisms: (1) a purging mechanism by which less social persons disproportionately flee communities plagued by war and (2) a collective coping mechanism by which individuals who have few options to flee band together to cope with threats.*

What are the effects of war-related violence on social cohesion in conflict-affected communities? This question is fundamental in understanding the ways in which the upheavals of war affect the postwar political and economic trajectories of societies. By creating new differences in the collective action potential of communities—that is, by transforming the “social cohesion” landscape—wartime violence may fundamentally alter a country's grassroots politics (Wood 2003, 2008). This makes the political legacy of war much deeper than the terms written into elite-level settlements. Damage to social cohesion undermines communities' collective action capacity and may contribute to conflict and underdevelopment “traps” (Collier et al. 2004; Walter 2004). On the other hand, resilience and even strengthening of social cohesion imply strong potential for recovery and the possibility of new collective action dynamics that fundamentally alter politics

(Bellows and Miguel 2009; Blattman 2009; Blattman and Miguel 2010).

The effects of war on social cohesion are due to a combination of psychological change, changes in individuals' material conditions and the incentives they face, and death and displacement that permanently alter the composition of communities. From a historical perspective, understanding precisely how violence transforms social cohesion is crucial for understanding how war affects a country's political development. From a practical perspective, understanding how violence transforms social cohesion is necessary for anticipating the political consequences of different wartime strategies (e.g., insurgency or counterinsurgency strategies). Changes in the social cohesion landscape are also important features of the social and political context for designing postconflict reconstruction. Indeed, postconflict social and political programming has tended to assume that war fragments

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communities, but recent field studies, including what we present here, challenge this view (Fearon, Humphreys, and Weinstein 2009; Whitt and Wilson 2007).

We combine lab-in-the-field techniques and matching methods to study the effects of violence in postwar Nepal. The lab-in-the-field techniques provide two key benefits. First, they provide incentive-compatible mechanisms through which subjects credibly reveal their degree of prosociality. This helps to overcome difficulties in answering abstract questions and the social-desirability biases that make self-reports problematic. Second, because it is based on individual behaviors in a controlled setting, the lab-in-the-field evidence provides insight into reasons that violence may affect social cohesion. The lab-in-the-field evidence allows us to isolate changes in individuals' social motivations versus other types of effects, such as increased likelihood of being drawn into elite-organized collective action. If one were to focus exclusively on collective behavior such as participation in elections or community organizations, one would not be able to distinguish between such "bottom-up" and "top-down" effects, even though these different-types effects have very different political implications. Both types of evidence should be considered to obtain a complete picture of the nature and real-world significance of war's effects on social cohesion. While this article focuses on the lab-in-the-field results, we motivate our use of the laboratory approach by first presenting contextual evidence on the effects of violence on voting behavior and the functioning of community organizations. The positive effects of violence exposure on these forms of collective behavior beg a more thorough investigation. Our lab-in-the-field evidence provides just that.

We find that members of communities that suffered greater exposure to fatal violence during Nepal's 10-year civil war are significantly *more* prosocial in their relations with each other than were those that experienced lower levels of violence. Our causal-identification strategy couples communities' exogenous geographic isolation from the unpredictable path of war due to Nepal's rugged terrain with matching on a rich set of observables. This rules out many potential sources of confounding in estimating the causal effect of wartime violence. Therefore, the effects that we see on macrolevel outcomes like voting and community organizations may result, at least in part, from fundamental changes in the way individuals relate to each other in violence-stricken communities.<sup>1</sup> The consequences of warfare on society run deep. Furthermore, we

find evidence for two mechanisms through which social transformation occurred: (1) a purging mechanism by which less social persons disproportionately flee communities plagued by war and (2) a collective coping mechanism by which individuals who have few options to flee band together to cope with threats and trauma.

We begin with a discussion of theories of violence and social cohesion as well as an assessment of current evidence. Next, we discuss the context of our study, focusing on the nature of wartime violence in Nepal and its apparent effects on macro outcomes such as voting and community organization. We then describe the methods that we use to isolate effects on social motivation per se. We follow that with the results of our analysis, which show strong positive effects of violence on social cohesion. The next section considers mechanisms linking violence to community-level social cohesion. A conclusion summarizes and draws out implications.

## Violence and Social Cohesion

Community-level social cohesion refers to patterns of cooperation among individuals who live and work in close proximity. This form of social cohesion is often primarily for material welfare and public-goods provision, especially in developing countries such as Nepal.<sup>2</sup> Theoretically, civil war could affect community-level social cohesion in either direction. Civil war could reduce social cohesion within communities in at least two ways. An updating and "security dilemma" story suggests that violence erodes trust: if after each instance of victimization people update their beliefs that others are not trustworthy in general, then levels of trust in a community will decline (Posen 1993; Walter and Snyder 1999). Second, a more direct effect of civil conflict on social cohesion may be that civic associations are destroyed as members are killed or driven off by war and as meeting in groups becomes riskier.

Alternatively, violent conflict may increase community-level social cohesion as the difficulties of war force neighbors to band together, create new "collective coping" arrangements that foster cooperation

<sup>1</sup>Demonstrating such links definitively is very hard, as Bullock, Green, and Ha (2010) explain in their critique of conventional approaches to mediation analysis.

<sup>2</sup>Like Fearon, Humphreys, and Weinstein (2009), we use the term "social cohesion" rather than "social capital" to emphasize community dynamics rather than attributes of individuals in isolation. In the conclusion, we discuss implications for intercommunity and intergroup "bridging" social cohesion/capital (Putnam 2000). To be clear, our study focuses on within-community cohesion, but we hope future studies will allow the comparison of different types of trust and cohesion (see, for instance, discussions in Hardin 2004 and Uslaner 2006).

in order to defend themselves, and cope with the negative consequences of war (Lyons et al. 1998; Pennebaker and Harber 1993). If these patterns of cooperation persist after the war, then communities exposed to violent conflict may actually be more cohesive than those communities that were not exposed directly to the ravages of the war. Some have even argued that humans are predisposed, perhaps physiologically, to heightened cooperation in the presence of violent conflict as a result of evolution: Bowles (2008, 326–27) has argued that violent conflict is “altruism’s midwife.”<sup>3</sup> Bellows and Miguel (2009), Blattman (2009), and Voors et al. (2012) propose that people’s *preferences* may become more prosocial after exposure to conflict. Blattman (2009) proposes further that “post-traumatic growth” may provide the psychological mechanism (Tedeschi and Calhoun 2004).

All of these effects, both positive and negative, may be occurring at once. As such, the net effect of civil war on community-level social cohesion is an empirical question. What does the existing evidence say? Some scholars interpret the evidence on civil-war recurrence as evidence for the first theoretical account. They argue that civil-war recurrence indicates that wartime violence tends mostly to destroy social institutions and economic resources, potentially locking countries into conflict and poverty traps (Collier et al. 2004; Walter 2004). Qualitative field accounts also often emphasize the negative effects of war-time violence on social cohesion. For example, M. Thapa observes the following of violence-affected villages in Nepal’s western hills (an area included in our study):

[V]iolence has polarised communities and strained social cohesion to a breaking point. Living under siege, villagers now hesitate to get involved in the affairs of others, or to help neighbors in need, as they once would have. (2003, 319)

By contrast, recent studies have shown that local-level violence during civil war generates new forms of individual- and community-level political mobilization and other forms of participation in collective action. Blattman (2009) found that the young male abductees in Uganda’s civil war he surveyed exhibit significantly greater (self-reported) political participation than do

young men who were not abducted.<sup>4</sup> Bellows and Miguel (2009) show that citizens in communities more severely affected by the conflict in Sierra Leone manifested greater local collective action capacity according to several survey measures. These effects on participation in collective action are intriguing, but on their own, they do not allow us to distinguish between increases in individuals’ social motivations versus other types of effects. For example, communities or individuals who suffer violence may be left in a more vulnerable position and therefore more likely to be brought into elite-organized collective action. If that is the case, then these seemingly positive effects on collective behavior may in fact be indicative of heightened vulnerability.

A way to move past such observational equivalence is to use controlled, lab-in-the-field methods. A few studies have begun to do so, although taken together, they still provide only an incomplete picture of the effects of wartime violence on social cohesion. Voors et al. (2012) use behavioral-games data from Burundi to show that people in rural communities who were exposed to wartime violence were more altruistic than those who were not. However, they do not study components of social cohesion beyond basic altruism—e.g., they do not study propensity to contribute to public goods or to engage in trust-based exchange. Whitt and Wilson (2007) focus on interethnic relations and find evidence that altruism between groups was surprisingly high following Bosnia’s bloody civil war, suggesting that war is not all that damaging to postwar social cohesion even among members of ethnic groups that were pitted against each other in wartime. Gneezy and Fessler (2011) found that Israeli senior citizens in Tel Aviv were more willing to punish noncooperators and reward cooperators in lab experiments during the 2006 Israeli-Hezbollah war in Lebanon than they were either before or after that war. Taking into account these studies, there appears to be growing evidence that the link between exposure to violent conflict and social motivations is often positive. This does not rule out the possibility that other factors explain participation in collective behavior, but it does sustain the possibility that prosocial motivations play a role.

## Wartime Violence in Nepal

Our study was fielded in postconflict Nepal in 2009. The “People’s War,” as it was called by the Communist Party

<sup>3</sup>Bowles (2007) provides fascinating archeological, genetic, and ethnographic evidence from the Late Pleistocene and Holocene eras for this contention. See Choi and Bowles (2007) for an agent-based model of the emergence of “parochial altruism,” that is, altruism for one’s community members, in early human societies.

<sup>4</sup>He did not find that they were more altruistic according to his “pro-sociality scale,” however.

of Nepal-Maoist, began in February 1996 with a series of raids by the Maoists on police stations in the middle-western part of the country. Just over 13,000 deaths were recorded during 10 years of conflict in this nation of roughly 27 million people (Nepal, Bohara, and Gawande 2011; D. Thapa 2004). According to detailed data gathered by the Nepalese nongovernmental organization Informal Sector Service Centre (1996–2006), the vast majority (86%) of fatalities took place in the Nepalese countryside after 2001, the year in which the Maoists organized a more formidable force under the banner of the People's Liberation Army (PLA), and the Royal Nepalese Army (RNA) mobilized to fight them.<sup>5</sup> Prior to 2001, the state prosecuted the war against the Maoists solely with the Armed Police Force (APF) (International Crisis Group 2005). About two-thirds of the fatalities were attributed to state forces. About 15% of the deaths were police or army casualties. The remainder includes known or accused Maoists and other party workers and civilians. In addition, 100,000 to 200,000 people were displaced as a result of the conflict. State- and Maoist-inflicted abductions, disappearances, civilian targeting, and destruction or confiscation of civilian property were common and well documented (Amnesty International 2002, 2005; BBC News 2010a, 2010b; Gersony 2003; IRIN 2006; Marks 2003; Mehta and Lawoti 2010; M. Thapa 2003). The war ended in November 2006 with the signing of the Comprehensive Peace Agreement between the Maoists and the government. The Maoist movement transformed into a legitimate political party and won 38% of seats in the 2008 constituency elections. Although a great deal of political strife between the Maoists and the other political parties remains, the peace has held.

The dynamics of wartime violence in the Nepalese countryside can be split into two periods: first, from 1996 to 2001, Maoists incrementally expanded control in rural areas, and state forces steadily retreated; then, from late 2002 to 2006, state forces declared a state of emergency and sought to reclaim the countryside from Maoist control. Violence during the first period was low level, almost entirely consisting of targeted killings by Maoists and casualties from light exchanges between Maoist forces and police. The vast majority (86%) of fatalities occurred in the second period. These included state killings of suspected Maoists; civilian deaths, mostly at the hands of state forces; and targeted Maoist killings (Informal Sector Service Centre 1996–2006).

<sup>5</sup>The violence in our sample was mainly perpetrated by state actors. As such, we cannot draw direct comparisons to other research that focuses on Maoist violence—for example, interesting work by Nepal, Bohara, and Gawande (2011) on the effects of horizontal inequality on levels of Maoist-inflicted violence.

Qualitative accounts depict the violence endured by villagers as having been unpredictable and inflicted largely by state or Maoist forces who originated from outside the community.<sup>6</sup> Maoist operatives were assigned to work away from their home areas in order to distance them from “traditional pressure groups . . . formed by kinship or friendship ties” (Lecomte-Tilouine 2012, 399).<sup>7</sup> Their work included destruction of government installations and arrests, beatings, or executions of those suspected of being agents of the state. The apparent goals of the violence were to force state agents to withdraw and isolate communities for political indoctrination (Marks 2003; Thapa, Ogura, and Pettigrew 2012). A wartime memoir by PLA commander Pasang (2008) as well as numerous personal interviews by the authors with PLA and RNA commanders show that the PLA was highly mobile across the country. Attacks on state establishments were designed to maximize shock value, and so unpredictability was of paramount importance. Police, APF, and RNA units were deployed from bases near district headquarters to conduct search-and-destroy missions in the countryside. These missions regularly involved beatings and summary executions of suspected Maoists. Retaliatory killings were common: both Maoists and state forces regularly sought to avenge each death of one's own, regularly doing so by hunting down civilians accused of working with the other side (Amnesty International 2002).

Qualitative accounts characterize state forces as acting indiscriminately—“distant, terrifying, and unpredictable” (Pettigrew 2004, 270), characterized by “aloofness and seemingly callous randomness” (Pettigrew and Adhikari 2012, 409). This was in part due to state forces' apparently poor training, very limited intelligence capacity, a highly fractious political leadership, and absence of mechanisms to hold human rights abusers accountable (Amnesty International 2002, 2005; Dixit 2003; Marks 2003; Mehta and Lawoti 2010). Local politicians used state forces for personal vendettas (Thapa, Ogura, and Pettigrew 2012), further exacerbating the unpredictability of the violence. While some journalistic accounts accuse state forces of targeting specific ethnic groups (*viz.*, Magars) for their purported affinity to the Maoist cause, whether this actually occurred is questionable (Lecomte-Tilouine 2004).

Another cause of the indiscriminateness of state violence appears to be the uncertainty produced by the

<sup>6</sup>We draw from the field accounts of Amnesty International (2002, 2005), Dixit (2003), Gersony (2003), Pasang (2008), Pettigrew (2004), Shah and Pettigrew (2012), D. Thapa (2004, 152–53), and M. Thapa (2003), and military analysis of Mehta and Lawoti (2010) and Marks (2003).

<sup>7</sup>See also Pettigrew (2004, 274).



**TABLE 1** Effects of Violence on Collective Behavior

	(1) Voter Turnout	(2) Community Groups Index
Fatalities per 1,000	0.002*** (0.001)	0.022*** (0.003)
Observations	1149	1228
$R^2$	0.022	0.181
Baseline (no violence)	0.852	0.000

*Note:* Standard errors in parentheses. Weighted least squares with matched-pair block fixed effects. Robust standard errors clustered by VDC. \* $p < 0.10$ , \*\* $p < 0.05$ , \*\*\* $p < 0.01$  (two-sided tests).

Maoists' policy of unilaterally installing and announcing People's Committees in villages, often without any villagers' support.<sup>8</sup> If Maoists arrived to activate such committees, villagers obeyed as "the only logical choice" given Maoist threats (Gersony 2003, 71). However, in many instances the committees existed in Maoist declarations only. Maoists defended this policy as necessary to bring villagers out of "false consciousness." But the effect was to sow confusion for the state forces about the geographic distribution of Maoist support. All locations that hosted People's Committees were at risk of being targeted by state forces, who made little effort to discern whether the People's Committee was an expression of support or something forced upon the village. Locals were often puzzled about the decision to target one village rather than another. This indiscriminate violence is one of the pillars of our identification strategy.

The consequences of such violence on collective behavior in rural Nepal are consistent with what has been found in the studies by Bellows and Miguel (2009) and Blattman (2009) in Sierra Leone and Uganda, respectively: violence tends to be associated with higher levels of political and community-level mobilization. Table 1 shows estimates from the 2009–10 survey of household decision makers that we fielded in 24 matched pairs of violence-affected and violence-unaffected Village Development Committees (VDCs) across 17 of Nepal's districts.<sup>9</sup> (We thus have a total of 48 VDCs in 24 matched pairs across 17 districts.) As described in the methods section below and in the appendix, violence-affected and violence-unaffected VDCs were matched to approximate pairs of communities with identical likelihoods of violence exposure and expected consequences of violence, but for which spillover effects were negligible. We find that voter turnout rates in the 2008 elections increase sig-

nificantly in the amount of wartime violence, measured in terms of wartime fatalities per 1,000 community members. If we coarsen the fatalities measure, we find that voter-turnout rates in communities hardest hit by violence were 7 percentage points higher than in matched violence-unaffected communities (s.e. .02,  $p < .01$ ).<sup>10</sup> The community groups index is an inverse-covariance-weighted average of responses to questions about whether any of 14 types of community organizations were active in the respondents' community.<sup>11</sup> We see that increases in the number of fatalities are associated with more community-level organizational vitality. Less definitive, but nonetheless suggestive, results come from qualitative data that we collected through elite interviews and focus groups in 10 randomly selected matched pairs. These interviews suggested that in seven out of 10 of the violence-affected communities, community-wide forest groups, farmers associations, women's groups, or youth groups were active; such was the case in only four out of the 10 violence-unaffected communities ( $\chi^2_1 = 1.82$ ,  $p = .18$ ). However, in four violence-affected cases with active community organizations, the qualitative interviews made it clear that the community organizations were established through community initiative rather than government agencies or NGOs; in none of the violence-unaffected communities was this the case ( $\chi^2_1 = 5.00$ ,  $p < .05$ ).

These macrolevel results conform to findings from other studies, suggesting the exposure to violence causes increases in various forms of collective behavior. The

<sup>10</sup>The difference is 92% versus 85% in violence-affected versus violence-unaffected communities, respectively.

<sup>11</sup>The types of community organizations include forestry, irrigation, agriculture, women's, religious, sports, cultural, political, labor, academic, health, cooperative, community development, and youth groups. The inverse-covariance-weighted average provides a statistically optimal way to aggregate linearly a set of measures that each measure some component of a common latent variable (Anderson 2008). In this case, the latent variable is community-level organizational vitality.

<sup>8</sup>This policy is described in all of the accounts listed in footnote 6.

<sup>9</sup>This policy is described in all of the accounts listed in footnote 6.

question remains whether such increases in collective behavior are plausibly attributable to the increases in prosocial motivations. The methods that we describe in the next section allow us to address this question.

## Methods

Our explanatory variable of interest is community-level exposure to fatal wartime violence. Our study design matches communities with high levels of fatal wartime violence (above-median levels among communities with any fatalities) to those with no such violence, using data that account for potential confounding based on region, political and military control, timing of war exposure in the area surrounding the community, ethnic and caste composition, level of socioeconomic development, employment, population size, economic inequality, and elevation and ruggedness.<sup>12</sup> Our measure of violence exposure comes from VDC-level fatality figures reported in the annual yearbooks of Informal Sector Service Centre (1996–2006). As expected given overall patterns of violence described above, the vast majority of fatalities in the sample are attributable to state forces entering from outside the communities. Our study thus identifies effects of primarily external, state-inflicted violence. Details are described in the appendix.

Despite the robustness of matching, causal identification is more persuasive when there is an explanation for why similar communities could have experienced different levels of violence (Sekhon 2009). Our discussion above on the nature of the wartime violence suggests how this could have happened. First, optimal insurgency and counterinsurgency strategies require randomization (or *mixing* in the parlance of game theory) on where to send forces to attack and where to defend. This cat-and-mouse dynamic is likely a major reason for the visibly haphazard variation in violence over Nepal's rugged terrain (see Figures 1 and 2 in the supporting information). The qualitative and ethnographic accounts discussed above provide ample anecdotal evidence for this claim. We also might be concerned about spillovers: did exposure to violence in one community also affect neighboring communities? In this case, Nepal's rugged terrain helps to limit such possibilities: communities that resemble each other in many ways are nonetheless sufficiently isolated *from each other* by difficult terrain. Given these factors, matching helps to increase the power of our analysis while also providing

robustness to violations of exogeneity attributable to the factors on which we matched.

The overall survey was conducted in December 2009 and January 2010 in 48 VDCs, matched into pairs, throughout Nepal. VDCs are roughly equivalent to counties. We sampled 12 respondents in each of two randomly chosen wards (equivalent to townships) of these 48 VDCs. We then implemented the lab-in-the-field games with survey respondents in a randomly selected set of 12 of the sampled VDCs: six conflict and six no-conflict VDCs, with two wards per VDC and 12 respondents per ward. We conducted the games in December 2009 through February 2010. Game implementation always followed the survey—anywhere from two days to one month. Survey respondents were not informed of the possibility of participating in the games until after survey enumeration was completed. Overall, a few survey respondents (36 out of 288) failed to appear for the games sessions, and so in some wards, the games were played among fewer than 12 participants. The minimum number of participants was eight. In total, we had 252 lab-in-the-field subjects across the 24 wards.<sup>13</sup>

Our dependent variable is the level of social cohesion among our subjects. Our measurement strategy incorporates suggestions from research by Henrich et al. (2004) and Karlan (2005). We focus on four components of social cohesion that tap into prosocial motivations: (1) *altruism*, (2) a sense of *obligation* to contribute to one's community welfare, in particular to contribute to the community's collective good, (3) *trust* in one's community members, and (4) *trustworthiness* with one's community members. Altruism is sometimes called "other-regarding preferences." An altruistic person is assumed to gain utility from the welfare gains of others.<sup>14</sup> By "obligation" we mean a willingness to contribute to a public good shared by the community even when one can achieve higher personal material rewards by not contributing. By "trust" we mean the belief that one's community members will

<sup>12</sup>Covariate balance is displayed in Table 2 in the supporting information.

<sup>13</sup>A potential concern to any field research in conflict or postconflict settings is that researchers will be taken as affiliated with government bodies. We did apply, prior to both the survey and behavioral games, extensive informed-consent procedures to relieve respondents of such concerns. To get a sense of whether respondents did have anxiety during field research, we examined patterns of nonresponse to sensitive survey questions on people's perceptions about whether the Nepali Army, police, or Maoists are responsible for problems in their community at the time of the interview. As shown in the supporting information, between 95 and 97% of the games participants answered these questions, and there was no evidence of differential response rates over levels of violence.

<sup>14</sup>Or, it could be that giving to others confers individualistic "warm glow" benefits (Andreoni 1990). Either way, the consequence is stronger prosocial motivation.

comply with social norms of reciprocity and fairness even when those community members can gain higher personal material rewards by violating those norms. Trustworthiness is the flip side of trust—it means complying with the norm of reciprocity and fairness even when the trustee can achieve higher material rewards by cheating on that norm.

Questions about these concepts (e.g., “Do you generally find people to be trustworthy?”) sometimes appear in survey questionnaires, but the abstractness of such questions and social-desirability bias raise doubts about how well they measure social cohesion. For this reason, behavioral measures are more appealing. We used behavioral games that have clear incentives and thus permit us to measure these attributes by observing subjects’ behavior in a controlled setting. We implemented adaptations of well-established protocols as used by Karlan (2005), the Foundations of Human Sociality project (Henrich et al. 2004), and other studies in developing countries reviewed in Cardenas and Carpenter (2008). Game instructions were given entirely verbally according to a specific script in Nepali.<sup>15</sup> Illiteracy rates are very high in rural Nepal, and our respondents found the use of paper and pens very challenging. Like Karlan (2005) and Henrich et al. (2004), we had the subjects complete the lottery, dictator, and trust games with supervision by a trained facilitator and a record keeper. Such observation was *not* required for the public-goods game. While we were concerned about Hawthorne effects, having the subjects play under supervision proved to be the only way to make sure that the subjects understood the game they were playing. Total payouts from all four games were aggregated and made in one lump sum at the end of the session. The average payout was about 90 rupees (US\$ 1.30), which corresponds to between 60 and 90% of a day’s wage in the rural areas where we worked.<sup>16</sup>

Persons with greater tolerance for risk may exhibit behavior that on the surface appears more trusting but is in actuality a greater willingness to gamble on the cooperative behavior of the other player.<sup>17</sup> To control for this

potential problem, we measured our subjects’ attitudes toward risk. Subjects were asked to choose one from among five lotteries, each with two possible outcomes. The lotteries were decided by a coin flip performed by the subject. The expected value of all of the lotteries was 40 Nepalese rupees, but the lotteries contained increasing levels of risk. The first lottery contained no risk, with subjects receiving 40 rupees regardless of the result of the coin flip, so the expected payoff had a variance of zero. In the riskiest lottery, subjects would receive zero rupees if they flipped heads and 80 rupees if they flipped tails, for a variance in the payoff of 1,600 rupees. In other words, this game offered a 5-point scale of willingness to gamble for a higher payoff. Risk-averse people should choose lottery 1. Risk-acceptant people should choose one of the higher-numbered lotteries depending on the amount of risk they are willing to accept.<sup>18</sup> Most subjects, some 60%, chose the no-risk or low-risk option (lotteries 1 or 2), but some people did choose higher levels of risk, so we have substantial variation on our measure of willingness to gamble.

We measured subjects’ altruism with a simple dictator game. Subjects were given 40 rupees in eight five-rupee notes. They were asked to decide how much, if any, of the 40 rupees to donate to a local needy family. The subjects were not told the name of the needy family to protect the family’s privacy and also to avoid any differences between subjects in their affinity with the needy family. Each subject was called individually to the games area. The eight five-rupee notes were set side by side on a sheet of paper with a line drawn across the middle. The subjects were instructed to push the amount they wished to donate to the needy family across the line on the paper, and they were told that any remaining amount would be added to the lump sum that they received at the end of the session. The modal category for the amount sent was 10 rupees or one-fourth of the pot. Eighty-six percent of the subjects gave half the pot or less to the needy family, with some 23% giving exactly half the pot.<sup>19</sup>

<sup>15</sup>An English translation of this script is available in our supporting information.

<sup>16</sup>At the end of the session, subjects were given a small thank-you gift in addition to their winnings, in some cases 20 rupees, in other cases soap or cookies worth an equivalent amount. Subjects were told, in the initial invitation, that they would receive a thank-you gift for participating, but they were not told what the gift would be until after game play was completed.

<sup>17</sup>Actually, the evidence for this conjecture is mixed. Schechter (2007) found that the amount sent in the trust game was positively correlated with willingness to take risks among subjects in rural Paraguay, but Ben Nér and Halldorsson (2010) found no such link.

<sup>18</sup>Since all lotteries have the same expected value, risk-neutral people will be indifferent between the five lotteries. Risk neutrality corresponds to a very specific parameterization of the subject’s utility function. Indeed, if the curvature parameter of the subjects’ utility functions is continuously distributed, the probability of a person being *exactly* risk neutral is zero. Therefore, we considered it unlikely that there were any risk-neutral people in our sample, and as such, we were not concerned about this ambiguity for those specific types of risk preferences.

<sup>19</sup>We did, in fact, give the money to a needy family identified after the games sessions in consultation with local community leaders. We chose to use a “local needy family” for the dictator game as we believed this a compelling test of altruism with much external validity. Additionally, this method allowed observations

We used the standard trust-game protocol (Berg, Dickhaut, and McCabe 1995) to measure trust and trustworthiness. The game was conducted in two rounds. In the first round, all subjects were called, one by one, to the private game area. They drew a number from a bag. That number determined whether they were a “sender” or a “receiver,” and senders and receivers were paired according to the number they drew. Both senders and receivers were given an initial endowment of 12 rupees in two-rupee coins. Receivers had no decision to make in the first round. Senders were asked how many coins they wanted to send to their receiver, knowing that we would triple that amount and that in the second round their receiver would decide how much to return to their sender. The six coins were placed side by side on a sheet of paper with a line through the middle. Senders indicated their choice by pushing the number of two-rupee coins they wanted to send to the receiver over a line on a sheet of paper. We then tripled that amount and added the receiver’s endowment of 12 rupees to show the sender exactly how much money the receiver would have in front of her when she made the decision about how much to return. Once all players had been called to the game area, round one ended, and we began round two by calling each player back one by one. Senders had no decision to make in the second round, but they were reminded of the decision that they made in round one. Receivers were shown their pot (triple what the sender had sent plus their initial endowment of 12 rupees) in two-rupee coins placed side by side on the game sheet. Receivers indicated the amount they wished to return to the sender by pushing that number of coins over the line on the sheet of paper. The modal amount sent in the first round was half the pot, six rupees, and about 80% of the subjects sent half the pot or less. The modal category for amount returned is six rupees, the same as the modal amount sent.

The final game that we administered was a public-goods game similar to the one described in Barrett (2005). This game does not require supervision of the subjects to play. Each subject was given two folded cards. One of the cards had an “X” written inside the fold, and the other card was blank inside the fold. Play proceeded in two rounds. In the first round, subjects were asked to turn in one of their cards. For each “X” card that was turned in, every person in the group received four rupees. In the second round, we asked the subjects to turn in their remaining card. If a subject turned in an “X” card in the second round, that subject (and only that subject)

was given an additional 20 rupees on top of the amount determined by the number of “X” cards turned in during the first round. If a subject turned in the blank card in the second round, that subject was given no extra money, only the four rupees per “X” card turned in during the first round. As shown in Table 2, about two-thirds of the subjects contributed to the collective good.

Summary statistics for the games outcomes and covariates are presented in Table 2.<sup>20</sup> The summary statistics reveal several interesting patterns about our subjects, who represent sampled individuals from the population of “household decision makers” in the study sites. Most of our subjects were male, around 70%, which is expected among household decision makers. Literacy rates are similar to the national average for the rural adult population. Values of VDC distances to roads should be interpreted in light of extremely rugged terrain surrounding these communities: the only way to cover 10 kilometers to the nearest road would be by foot through incredibly steep mountain passes, taking perhaps a full day or more.

We estimate the causal effect of exposure to violence using the following specification:

$$Y_{ic} = \beta_0 + \beta_1 \text{Violence}_c + \alpha_{b[c]} + \epsilon_{ic},$$

where  $Y_{ic}$  is the outcome for subject  $i$  in ward  $c$ ,  $\beta_0$  and  $\beta_1$  are coefficients to be estimated,  $\text{Violence}_c$  is an indicator for violence-affected wards,  $\alpha_{b[c]}$  is a matching block fixed effect (FE) for ward  $c$  in block  $b$ , and  $\epsilon_{ic}$  is an individual-level error term. The coefficient  $\beta_1$  measures the effect of violence. We fit the model using weighted least squares, where the weighting accounts for differences between the sample and population distributions over the matching blocks. We use cluster-robust standard errors with clustering at the level of wards to account for community-level correlation in games results.<sup>21</sup>

<sup>20</sup>The number of senders and receivers is unequal (124 and 128, respectively) because on four occasions an odd number of subjects arrived for the games due to attrition. Rather than turn away a sure-to-be-disappointed subject who had traveled on foot over difficult terrain to attend our games session, we randomly matched two receivers to one sender in the trust game in these sessions. In those four cases, receivers received the payoff consistent with their actions, and the relevant senders received the payoff decided by the first receiver with whom they were randomly paired.

<sup>21</sup>Clustering at the level of VDCs does not change the conclusions and actually increases the statistical significance of all findings—see the results reported in the supporting information. Given the relatively small number of clusters, we ensure that our estimates apply appropriate degrees of freedom adjustments.

on all games participants. (If we had created pairs of givers and receivers, we would only obtain altruism measures for half the subjects.) Finally, this provided an opportunity for the project to make a direct contribution to the communities.



TABLE 2 Summary Statistics

Variable Name	Observations	Mean	Std. Dev.	Min.	Max.
Lottery choice	252	2.48	1.38	1	5
Amount sent in Rps. (dictator game)	252	14.722	10.911	0	40
Cooperate (public-goods game)	252	0.663	0.474	0	1
Amount sent in Rps. (trust game)	124	5.129	3.490	0	12
Share returned (trust game)	128	0.229	0.193	0	1
Female <sup>a</sup>	235	0.323	0.469	0	1
Brahmin <sup>a</sup>	235	0.072	0.260	0	1
Chhetri <sup>a</sup>	235	0.421	0.495	0	1
Magar/Rai <sup>a</sup>	235	0.243	0.430	0	1
Literate <sup>a</sup>	235	0.566	0.497	0	1
Age <sup>a</sup>	235	52.187	14.026	21	82
Log VDC population	12	7.426	0.670	6.620	8.613
Distance to nearest road from VDC (km)	12	5.428	5.613	0.417	18.205

<sup>a</sup>Smaller sample size occurred for survey variables because we could not link 17 game subjects to their survey records due to survey-enumerator error.

TABLE 3 Main Results

	(1) Lottery Risk	(2) Dictator	(3) Cooperate	(4) Trust Sent	(5) Trust Return	(6) Soc. Index
Violence	−0.11 (0.24)	2.04 (1.35)	0.16** (0.07)	1.68** (0.63)	0.07* (0.03)	0.57*** (0.13)
Observations	252	252	252	124	128	252
R <sup>2</sup>	0.033	0.075	0.058	0.139	0.124	0.163
Baseline (no violence)	2.53	15.28	0.60	4.82	0.23	0.00

Note: Standard errors in parentheses. Weighted least squares with matched-pair block fixed effects. Robust standard errors clustered by ward. “Soc. Index” is inverse-covariance weighted average of outcomes 2–5. \*p < 0.10, \*\*p < 0.05, \*\*\*p < 0.01 (two-sided tests).

## Results

Table 3 presents the results for each of the games as well as for a “sociality index” (column 6), which is the inverse-covariance weighted average of outcomes for the dictator, public-goods cooperation, and trust games (columns 2–5).<sup>22</sup> For the actions of the returner in the trust game, we use the “share returned,” which is the total amount returned to the sender divided by the amount that the returner received (equal to the amount sent times three).

The results indicate substantial effects of violence on prosocial motivations. The likelihood of cooperating in

the public-goods game is higher by 16 percentage points in violence-affected communities, going from 60% to about 76%. The estimated effects on amounts sent in the dictator and trust games translate into about a 13% increase in the dictator game and a 35% increase in the trust game, although the result for the dictator game is statistically insignificant. The share returned in the trust game increased by about seven percentage points, from a baseline of 23% to about 30%. The sociality index results suggest that violence increased levels of sociality by about six-tenths of a standard deviation, slightly more than a “medium” effect size by conventional definitions (Cohen 1988).<sup>23</sup>

Column 1 of Table 3 shows the relationship between community-violence exposure and risk preferences.

<sup>22</sup>Because subjects were assigned to either a “sender” or “returner” role in the trust game, we have missing data for subjects on one or the other variable. However, because the assignment to these roles was random, this missingness is “completely at random” (Little and Rubin 2002). Therefore, to construct the sociality index for each person, we can simply impute predicted values using the main regression specification without introducing any bias or significant consequences for standard errors. On inverse-covariance weighting, see footnote 11.

<sup>23</sup>Note that these findings show that exposed communities are not just returning to normalcy but achieving a supernormal state of social cohesion. In that way, these results differ from studies by Davis and Weinstein (2002) and Miguel and Roland (2011), who study long-term economic outcomes in postwar settings.

Community-violence exposure has no effect on our subjects' willingness to take risks, which implies no risk-related confound for interpreting the outcomes of the public-goods or trust games. We also checked for the robustness of our findings by including the covariates on which we matched, clustering at the VDC level, and by excluding the one district, Udayapur, where we did not have balance on a community-level ethnic-composition covariate. In all cases, these alternative results are very similar to those reported here (Tables 8–12 in the supporting information).

In interpreting these results, it is crucial to recognize that all interactions during the games sessions were anonymous. No one could know about the actions of others in their community. Trust-game senders and receivers were randomly assigned, and identities were not revealed. Public-goods contributions were tallied in an anonymous fashion. As such, subjects should have had no expectation of reward or punishment by others in the community on the basis of their actions. To the extent that this is true, the observed prosocial behavior must emanate from internal social motivations of the individuals. This is what makes the games so useful in unpacking social cohesion effects: we isolate prosocial motivations, and in this case, the effects of violence on such motivations appear to be quite strong.<sup>24</sup>

## Mechanisms

We have seen that at the macrolevel, violence appears to have caused communities to host more collective behavior, measured in terms of voter turnout and the functioning of community organizations. These macrolevel effects are matched by microlevel effects on prosocial motivations, measured through our suite of behavioral games. As such, wartime violence has transformed the social-cohesion landscape both in terms of behavior and motivations. By what mechanisms could this arise? Before delving deeper in the question of mechanisms, we should make clear that the positive social-cohesion effects were thoroughly unexpected by the authors when the

study was being designed. Indeed, our expectations going into the study were that we would find negative social-cohesion effects, and so the auxiliary data that we collected through our survey were designed to unpack such negative effects. This leaves us in a position with rather limited information to explore mechanisms, although the available data do allow us to perform some suggestive analyses. We present such suggestive analyses in this section.

To get a handle on potential mechanisms, consider the situation that an individual and his or her household would face if members of their community are the target of violence. Fundamental decisions that the household would face include whether to stay or to relocate, and conditional on staying, how to cope with threats. These decisions would be informed by a number of considerations. First would be the level of personal threat that the household felt, which may vary depending on their relationship to the attacking party (which, in the communities we study, are mostly state forces). Second would be the extent to which the household is invested, socially and materially, in their home community. Such investment in the home community would be weighed against the value of assets, in particular human-capital assets, that the household could bring with them to set up a new life were they to relocate. Third would be the overall costs of relocating, which would depend on the extent to which the home community is isolated from other population centers.

Based on these considerations, violence could increase average levels of prosociality in a community through a number of processes. First, violence may induce those who are less socially invested in their home communities to leave, resulting in a selection effect that concentrates prosocial types in the home community. Evidence to support this hypothesis would come, at the most basic level, from indication of selective “purging.” Such purging should result in more subtle correlational patterns as well. For example, we would expect that among those who remain in violence-affected communities, people for whom relocation ought to be relatively easy, for example those with higher human capital, should exhibit *especially high* social investment in the home community; it is only due to such investment in the home community that the option to flee is not taken. Second, in communities where the option to flee is mostly unavailable—for example, in especially remote communities—we might see a different type of adaptation by those who fear being victimized by violence: because the fearful in these communities cannot flee, we would expect them to invest in “collective coping” processes (Lyons et al. 1998; Pennebaker and Harber 1993). Increased social cohesion

<sup>24</sup>One potential concern is the possibility that discussing the civil war during the household survey could have primed behavioral/game participation. However, we do not know of a demonstrated mechanism by which asking sensitive questions may differentially affect people in conflict-affected settings. Additionally, we found respondents to be quite willing to participate and answer sensitive questions, as we describe in the supplementary appendix. Finally, the behavioral games were conducted between several weeks to over a month after the household survey, and as such, we anticipate that any priming effects of participating in the survey are likely to have worn off.

**TABLE 4 Effects on Profile of Remaining Household Decision Makers**

	(1) Avg. Age	(2) Age 25th Pc.	(3) Age Median	(4) Age 75th Pc.	(5) Literate
Violence	4.71 (2.77)	7.00* (3.59)	6.00 (4.10)	3.00 (5.24)	−0.04 (0.10)
Observations	235	235	235	235	235
R <sup>2</sup>	0.056				0.030
Baseline (no violence)	50.63	40.00	50.00	62.00	0.60

*Note:* Standard errors in parentheses. Weighted least squares with matched-pair block fixed effects. Ordinary least squares with block fixed effects. Robust (for 1 and 5) and bootstrapped (for 2–4) standard errors clustered by ward. \* $p < 0.10$ , \*\* $p < 0.05$ , \*\*\* $p < 0.01$  (two-sided tests).

**TABLE 5 Effects of Violence on the Human Capital and Sociality Relationship**

	(1) Soc. Index	(2) Soc. Index	(3) Soc. Index	(4) Soc. Index	(5) Soc. Index	(6) Soc. Index
Violence	0.73*** (0.14)	0.45** (0.20)	0.58*** (0.20)	0.40 (0.26)	0.19 (0.34)	0.17 (0.36)
Age	0.00 (0.01)		0.00 (0.00)	0.00 (0.00)	0.00 (0.00)	0.00 (0.00)
Violence × Age	−0.02 (0.01)		−0.02 (0.01)	−0.02* (0.01)	−0.02* (0.01)	−0.02 (0.01)
Literate		−0.08 (0.26)	−0.07 (0.25)	0.05 (0.21)	0.03 (0.22)	0.01 (0.23)
Violence × Literate		0.41 (0.40)	0.25 (0.37)	0.34 (0.40)	0.34 (0.40)	0.39 (0.41)
Dist. to road (km)					0.04 (0.05)	−0.11 (0.10)
Viol. × Dist. to road						0.07 (0.05)
Observations	235	235	235	235	235	235
R <sup>2</sup>	0.193	0.181	0.196	0.252	0.255	0.258
Baseline (no violence)	0.00	−0.71	−0.71	−0.71	−0.71	−0.71

*Note:* Standard errors in parentheses. Weighted least squares with matched-pair block fixed effects. Robust standard errors clustered by ward. “Soc. Index” is inverse-covariance weighted average of outcomes 2–5. “Age” is centered on its mean (52). Models 4–6 control for caste- and gender-fixed effects. \* $p < 0.10$ , \*\* $p < 0.05$ , \*\*\* $p < 0.01$  (two-sided tests).

may be a by-product of the heightened interaction that this involves.

The two mechanisms are not mutually exclusive, and so we test them both using data available in our survey. To evaluate whether purging occurred, we assess whether violence-affected and violence-unaffected distributions differ in their composition with respect to literacy and age, two proxies for human capital. More literate and younger household decision makers are presumed to have better options to flee. Assuming our matched pairs of communities were similar on these counts at baseline (as we tried to ensure with our matching process), any significant differences today would be indicative of selective purging. We also evaluate the more subtle prediction

that in violence-affected communities, there should be a tighter correlation between prosocial motivations and human-capital attributes such as age and literacy. Finally, to test for collective coping under the constraint that fleeing is not an option, we use the measure of distance to the nearest road as a proxy for remoteness, interacting this with the violence indicator.

Tables 4, 5, and 6 show the results of these tests. As it turns out, we find some support for both the purging and collective-coping mechanisms. The purging hypothesis predicts that households led by younger decision makers would be more likely to flee, as setting up a new life should be easier for those who are younger. Therefore, remaining household decision makers should

**TABLE 6 Interaction with Distance to Road**

	(1) Lottery Risk	(2) Dictator	(3) Cooperate	(4) Trust Sent	(5) Trust Return	(6) Soc. Index
Violence	0.18 (0.17)	−0.73 (1.99)	0.09 (0.07)	1.32* (0.70)	0.02 (0.04)	0.27 (0.17)
Dist. to road (km)	0.10 (0.08)	−1.25 (1.02)	−0.00 (0.04)	−0.03 (0.56)	−0.04 (0.03)	−0.11 (0.07)
Viol. × Dist. to road	−0.07 (0.05)	0.80* (0.40)	0.01 (0.02)	0.05 (0.27)	0.02* (0.01)	0.08** (0.03)
Observations	252	252	252	124	128	252
R <sup>2</sup>	0.040	0.086	0.061	0.140	0.142	0.175
Baseline (no violence)	2.53	15.28	0.60	4.82	0.23	0.00

*Note:* Standard errors in parentheses. Weighted least squares with matched-pair block fixed effects. Robust standard errors clustered by ward. “Soc. Index” is inverse-covariance weighted average of outcomes 2–5. \* $p < 0.10$ , \*\* $p < 0.05$ , \*\*\* $p < 0.01$  (two-sided tests).

tend to be older in violence-affected communities. Table 4 shows that this is the case. The average age in violence-affected communities is about five years higher (column 1, significant at about the .05 level in a one-sided test). Columns 2 through 4 show the results of quantile regressions for the 25th, 50th (median), and 75th percentiles of the age distribution. These regressions show that the difference in the age distributions is due to the lower tail of the age distribution being pushed upward by about seven years in violence-affected communities. Such a pattern would result if the households that were most likely to flee were those with the youngest heads of household. We also see that the proportion of literate household decision makers is slightly lower in violence-affected communities, although the effect is insignificant.

Table 5 tests the more subtle interaction prediction: selection effects due to purging should result in those with higher human capital being more prosocial in violence-affected communities. We regress the sociality index on violence, the human capital indicators, and their interaction. The Age-Violence interaction should have a negative sign, whereas the Literate-Violence interaction should have a positive sign. These are indeed the patterns that we see, although only the age interaction comes close to being significant. (The age interaction is significant at about .05 for a one-sided test across all specifications.) Columns 1 and 2 show the basic relationships for age and literacy, respectively; column 3 estimates them together, columns 4 through 6 control for caste and gender-fixed effects, and columns 5 and 6 incorporate controls for remoteness (measured in terms of distance to road). The expected point estimates hold across all specifications, although the literacy interaction is never close to being statistically significant.

The test for collective coping is presented in Table 6. We predict that the effect of violence should be stronger in communities that are more remote, in which case the Violence-Distance interaction should be positive. This is what we find. The moderating effect of remoteness on the violence-sociality relationship is quite strong, as is seen most clearly in column 6, which uses the sociality index as the dependent variable. As discussed above, the consequences of distance for remoteness are highly exaggerated in Nepal, owing to the rough terrain. Traversing as few as 10 kilometers to reach a road might require a full day or more of hiking up and down rugged mountains. Columns 1 through 5 break out the effects in terms of each of the games. Moderation effects are strongest for the dictator game and share returned in the trust game—both of which are almost pure measures of altruism. We appreciate quite well that this evidence for collective coping is crude. At the same time, it does accord with anecdotal accounts. For example, Pettigrew, in her ethnography of a rural Nepalese town during the war, describes how the war has affected the life of a longtime acquaintance:

Fear has changed residence patterns. Two years ago my friend Gita lived alone, although most nights a relative and her daughter joined her. Now the upstairs of her house is inhabited by a group of young men who are there explicitly to provide a measure of protection, or at the very least a sense of solidarity. (2004, 279)

Thus, statistical and anecdotal evidence provide support for the idea of a collective coping mechanism.<sup>25</sup>

<sup>25</sup> Collective coping and the potential generation of social cohesion can be interpreted as the wartime analogue to Putnam's choral



**TABLE 7 Effects on Posttraumatic Growth Outcomes**

	(1) Trauma Exposure	(2) Apprec. of Life	(3) Hopefulness	(4) Political Confidence
Violence	0.15*** (0.05)	0.02 (0.05)	-0.14 (0.11)	-0.10* (0.06)
Observations	235	232	228	231
$R^2$	0.064	0.121	0.084	0.072
Baseline (no violence)	0.25	1.33	1.83	0.26

*Note:* Standard errors in parentheses. Weighted least squares with matched-pair block fixed effects. Robust standard errors clustered by ward. \* $p < 0.10$ , \*\* $p < 0.05$ , \*\*\* $p < 0.01$  (two-sided tests).

An alternative to the two mechanisms discussed above is the posttraumatic growth mechanism considered by Blattman (2009) in his study of how wartime abduction affected youth in Uganda. Our survey data allowed us to investigate this possibility. Table 7 shows the results of these tests. Column 1 shows the results of what we might call a “manipulation check,” where the dependent variable is a question asking whether the respondent had ever personally been subject to a life-threatening experience. We see that the effect of violence is to raise this by 15 percentage points, as would be expected. The next three columns capture some of the “domains” of growth discussed by Tedeschi and Calhoun (2004) in their original formulation of posttraumatic growth theory. The outcome for column 2 is an index corresponding to the domain of “appreciation of life,” which we created as an index by reverse coding responses to a post-traumatic stress disorder and depression screening battery.<sup>26</sup> The outcome in column 3 is a survey question about whether respondents are hopeful about the future of the peace process in their country, and in column 4 we have a survey question asking about whether people usually feel as if they understand politics. These two outcomes are the best measures that we had available to get at the “personal strength” domain proposed by Tedeschi and Calhoun.<sup>27</sup> The posttraumatic growth hypothesis would predict positive effects for columns 2 through 4. We do not find evidence to support this prediction. Indeed, if anything, we find that violence caused

subjects to have lower political confidence, contrary to what would be expected if there were growth in the domain of personal strength. The difference between these results and those of Blattman (2009) could be due to any number of reasons, including the different populations under study (youth versus household decision makers), different forms of trauma (forcible recruitment versus violence in one’s community), and different cultural contexts.

## Conclusion

We find that community-level exposure to fatal civil-war violence increases community-level social cohesion. This fact is reflected in macrolevel patterns in collective behavior such as voting and vitality of community organizations. To assess what might be driving these macropatterns, we used lab-in-the-field outcome measures that isolate individual-level prosocial motivation. Our research design did all that we thought was possible to rule out sources of confounding by matching on a large set of previolence characteristics and taking advantage of the strategic logic of insurgency and Nepal’s rugged terrain. We found that conflict-affected community members exhibited significantly more prosocial motivation than members of nonaffected communities.

An assessment of potential mechanisms linking violence to community-level social cohesion found evidence in support of both a purging mechanism—whereby less socially motivated individuals are induced to flee, leaving a concentration of socially motivated individuals behind—as well as a collective-coping mechanism—whereby members of communities that have few options to flee band together to cope with threats and trauma, causing social cohesion to increase as a by-product. We found no evidence of posttraumatic growth in this setting.

societies and bowling leagues as social-capital generators (Putnam 2000; Putnam, Leonardi, and Nanetti 1994).

<sup>26</sup>The battery is based on the Post-Traumatic Stress Disorder Screening battery in the World Health Organization Composite International Diagnostic Interview.

<sup>27</sup>Unfortunately, our survey did not have a module that could tap into all five posttraumatic growth domains. Specifically, we did not have questions that tapped into the spirituality, relationships, and new-roles domains.

Our subjects in Nepal were witnesses to victimization most often inflicted by state forces dispatched from bases outside their communities. Ethnographic accounts suggest that Maoist-inflicted violence was also typically instigated by outsiders (Lecomte-Tilouine 2012; Pettigrew 2004). Thus, our findings speak most clearly to the question of how internal, community-level social cohesion is affected by externally inflicted violence. Far from atomizing communities, we find that such violence generates stronger cohesion. Our results do not address the question of how violence might affect relations *between* communities or groups. In the context of our study, it is not clear who the relevant out-group would be, as neither state nor Maoist forces were altogether ethnically or regionally distinct (D. Thapa 2004). The fact that most of the violence was state-inflicted might lead us to expect violence-inflicted areas to take more antigovernment positions and to do so in a more cohesive way. This suggests the possibility of “blowback” effects whereby military pacification campaigns counterproductively inspire more effective resistance (Kalyvas 2006, 6.4). We hope to examine such political consequences in further research.

While no community would willingly suffer the ravages of civil war just to foster social cohesion, our findings offer hopeful insights into how traumatized communities may begin to recover. Given the substantial setback that war undoubtedly brings to communities, the finding that violence-affected communities may actually have a comparative advantage in social cohesion suggests an avenue by which community leaders and relief agencies can begin the work of reconstruction.

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## Supporting Information

Additional supporting information may be found in the online version of this article:

- Identification Strategy and Sampling for the Lab-in-the-Field Component of the Study
- Sampling Design for the Full Study
- Non-Response Issues in the Broader Sample and the Lab-in-the-Field Sample
- Game Scripts
- Constructing the Distance to Road Measure
- Robustness Checks for Main Results