

Can War Foster Cooperation?

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Warfare leaves terrible legacies, from raw physical destruction to shattered lives and families. International development researchers and policymakers sometimes describe war as “development in reverse” (for example, Collier et al. 2003), causing persistent adverse effects on all factors relevant for development: physical, human, and social capital. Yet a long history of scholarship from diverse disciplines offers a different perspective on one of the legacies of war. Historians and anthropologists have noted how, in some instances, war fostered societal transitions from chiefdoms to states and further strengthened existing states (Carneiro 1970; Flannery and Marcus 2003; Tilly 1985; Choi and Bowles 2007;

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Morris 2014; Diamond 1999). Meanwhile, both economists and evolutionary biologists, in examining the long-run processes of institution-building, have also argued that war has spurred the emergence of more complex forms of social organization, potentially by altering people's psychology (Bowles 2008; Turchin 2016).

In this article, we discuss and synthesize a rapidly growing body of research based on a wealth of new data from which a consistent finding has emerged: people exposed to war violence tend to behave more cooperatively after war. We show the range of cases where this holds true and persists, even many years after war. Until recently, a paucity of individual-level data from conflict and post-conflict societies prevented researchers from systematically exploring the legacies of war on social and political behavior. In the last decade, however, interdisciplinary teams of researchers—mainly in economics, anthropology, political science, and psychology—have begun to design research projects specifically to understand how exposure to war violence affects collective action, fairness, cooperation, and other important aspects of social behavior among populations around the globe.

In case after case, people exposed to war violence go on to behave more cooperatively and altruistically, which we will generally call “prosocial” behavior. Table 1, Panel A illustrates the breadth of evidence, referencing studies involving Sierra Leone, Uganda, and Burundi in Africa, as well as the Republic of Georgia, Israel, Nepal, and many other societies. The data come from individual surveys collected in seven countries, plus one paper with comparable data from 35 European countries. This evidence covers both civil and interstate wars, and includes a wide array of wartime violence experiences, ranging from personal exposure in which individuals themselves were targeted or directly witnessed violence, to more indirect exposure in which family members were killed or injured.

The evidence suggests that war affects behavior in a range of situations, real and experimental. People exposed to more war-related violence tend to increase their social participation by joining more local social and civic groups or taking on more leadership roles in their communities. They also take actions intended to benefit others, such as altruistic giving, in experimental laboratory games. Our meta-analysis also suggests the effects of wartime violence are persistent and fairly consistent across cases. Moreover, we see little systematic difference by the type of violence experienced (including crime victimization, as examined by a related body of studies), or across studies with different empirical strategies. The results appear to hold for men and women, as well as children and adults exposed to violence, and are remarkably similar for both the victims and perpetrators of violence. Finally, the impacts of exposure do not diminish with time; indeed, if anything, the opposite seems to be true.

Violence may also affect in-group prosocial behavior most of all: that is, participation with, and altruism towards, members of one's own village or identity group. Too few studies define “out-groups” consistently (or at all), so this in-group bias remains somewhat speculative. Nonetheless, it and some of the other patterns we observe are consistent with a broad literature on human behavior and evolutionary biology emphasizing that parochial altruism is a widespread evolved response to external threats. The increased local cooperation we document might help to explain why some post-conflict countries experience what seem to be almost

miraculous economic and social recoveries. Yet if people become more parochial and less cooperative with out-group members, this behavioral response could also harden social divisions, contribute to conflict cycles, and help explain the well-known pattern that many post-conflict countries soon return to violence.

Understanding the effects of war in all its complexity, including on postwar patterns of individual behavior and institution-building, is of broad importance. Nearly half of all nations in the world have experienced some form of external or internal armed conflict in the past half century (Blattman and Miguel 2010). According to the World Bank, about two billion people live in countries deemed fragile (Burt, Hughes, and Milante 2014). The findings discussed here emphasize that war is not only one of the most consequential forces for economic development and the emergence of state institutions, but also appears to have complex and multifaceted effects on postwar populations, society, and politics.

Case Evidence on the Effects of Exposure to Wartime Violence

To make the discussion more concrete, we begin by highlighting the case of Sierra Leone, a post-conflict society for which there is an unusual wealth of evidence: three studies by three sets of authors, each with different study populations. The Sierra Leone case also illustrates the synergy of diverse measurement and research methods, including survey reports, study of behavior in lab experimental tasks, and observational data.

The Sierra Leone Civil War

A brutal, countrywide civil war afflicted Sierra Leone from 1991 to 2002. The Revolutionary United Front (RUF), a small group of militants who first entered Sierra Leone from Liberia, inspired a violent rebellion which was nominally directed against the corruption and ineffectiveness of the government. The reach and duration of the war were fueled by access to alluvial diamonds and opportunities to loot civilian property. Many communities organized local fighting groups to protect themselves from the violence of the rebels. Neither ethnic nor religious divisions played a central role in this war: both the RUF and the Sierra Leone army were explicitly multi-ethnic. An internationally-brokered peace agreement was signed in 2003 after a large deployment of United Kingdom and United Nations troops. The war killed more than 50,000 civilians and temporarily displaced roughly two million people—nearly half of the country's population. Armed groups mutilated and raped thousands of civilians. Few people escaped some form of assault or other violence. Nonetheless, there was wide variation in the degree of exposure and victimization.

The period since the end of the civil war has seen an almost miraculous recovery. While Sierra Leone remains one of the poorest countries in the world, it has experienced over a decade of peace and has held several rounds of national and local elections, with alternation of political power among the major political parties at the national level. Until the Ebola outbreak during 2014, the local economy had improved in each year since the end of the conflict, often with rapid growth rates and high levels of foreign direct investment (Casey, Glennerster, and Miguel forthcoming).

Table 1
Studies of War Exposure and Cooperation

| <i>Paper</i> | <i>Country</i> | <i>Conflict</i> | <i>Data collection</i> | <i>Sample</i> | <i>Comparable survey measures</i> | <i>Comparable experimental measures</i> | <i>Time since war exposure</i> | <i>Published</i> | <i>Data available</i> |
|--|--|---|-------------------------------------|---|---|--|--|------------------|-----------------------|
| A: Papers eligible for the meta-analysis | | | | | | | | | |
| 1. Annan, Blattman, Mazurana, and Carlson (2011) | Uganda | Lord's Resistance Army (LRA) insurgency (1986–2006) | 2005–2007 | Representative sample of youth, some of whom were conscripted by LRA; N = 613 | Groups, community, trust, voting, interest in politics | – | ~7 years | ✓ | ✓ |
| 2. Bauer, Cassar, Chytilová, and Henrich (2014) | Georgia and Sierra Leone | Georgia: war with Russia over South Ossetia (2008) Sierra Leone: civil war (1991–2002) | Georgia: 2009 Sierra Leone: 2010 | Georgia: children; N = 565 Sierra Leone: adult population; N = 586 | Georgia: groups Sierra Leone: groups, community, trust, voting, interest in politics | Both countries: Allocation tasks (mini-dictator games) | Georgia: 6 months Sierra Leone: 8 years | ✓ | ✓ |
| 3. Bauer, Fiala, and Levely (2014) | Uganda | Lord's Resistance Army insurgency (1986–2006) | 2011 | Young men, some of whom were conscripted by LRA; N = 337 | Groups, community, trust, voting | Trust game | 5 years | ✓ | ✓ |
| 4. Bellows and Miguel (2006, 2009) | Sierra Leone | Civil war (1991–2002) | 2005 and 2007 | Nationally representative sample; N = 10,496 | Groups, community, trust, voting, interest in politics | – | 3–5 years | ✓ | ✓ |
| 5. Blattman (2009) | Uganda | Lord's Resistance Army (LRA) insurgency (1986–2006) | 2005–2006 | Young men, some of whom were conscripted by LRA; N = 741 | Groups, community, voting | – | ~5 years | ✓ | ✓ |
| 6. Cassar, Grosjean, and Whitt (2013) | Tajikistan | Civil war (1992–1997) | 2010 | Adult population; N = 426 | Groups, community, trust, voting | Trust game | 13 years | ✓ | ✓ |
| 7. Cecchi, Leuvel, Voors, and van der Wal (2015) | Sierra Leone | Civil war (1991–2002) | 2010 | Youth male street football players; N = 162 | – | Dictator game | 8 years | ✓ | ✓ |
| 8. De Luca and Verpoorten (2015a) | Uganda | Lord's Resistance Army insurgency (1986–2006) | 2000, 2005, 2012 | Nationally representative sample; N = 4,671 | Groups, trust | – | 12 years | ✓ | ✓ |
| 9. De Luca and Verpoorten (2015b) | Uganda | Lord's Resistance Army insurgency (1986–2006) | 2000, 2005, 2012 | Nationally representative sample; N = 4,671 | Community, voting, interest in politics | – | 12 years | ✓ | ✓ |
| 10. Gilligan, Pasquale, and Samii (2014) | Nepal | Civil war (1996–2006) | 2009–2010 | Household heads; N = 252 | Interest in politics | Dictator game, Public goods game | 3 years | ✓ | ✓ |
| 11. Gneezy and Fessler (2012) | Israel | Israel–Hezbollah war (2006) | 2005–2007 | Senior citizens; N = 50 | – | Ultimatum game, Trust game | 1 year | ✓ | ✓ |
| 12. Grosjean (2014) | 35 countries in Europe, the Caucasus, and Central Asia | WWII (1939–45); Yugoslav wars (1991–95); Kosovo war (1998–99); Tajik civil war (1992–97); Chechen wars (1994–2009); Kyrgyzstan clashes (2010) | 2010 | Nationally representative samples; N = 38,864 | Groups, trust, voting, interest in politics | – | 5 months–65 years | ✓ | ✓ |

(continued on next page)

Table 1
Studies of War Exposure and Cooperation (continued)

| Paper | Country | Conflict | Data collection | Sample | Comparable survey measures | Comparable experimental measures | Time since war exposure | Published | Data available |
|---|---|--|--|--|--|--|-------------------------|-----------|----------------|
| 13. Grossman, Manekin, and Miodownik (2015) | Israel | Israeli–Palestinian conflict (1967+) | 2013 | Former soldiers who enlisted between 1998–2003 and 2004–2009; N = 2,334 | Voting, interest in politics | – | 1–12 years | ✓ | ✓ |
| 14. Rohner, Thoenig, and Zilibotti (2013) | Uganda | Lord's Resistance Army insurgency (1986–2006) | 2000 and 2008 | Nationally representative sample; N = 2,431 | Trust | – | 8 years | ✓ | ✓ |
| 15. Voors et al. (2012) | Burundi | Civil war (1993–2005) | 2009 | Household heads, N = 287 | Groups, community, voting | Allocation tasks (social value orientation experiment) | 4–6 years | ✓ | ✓ |
| 16. Voors and Bulte (2014) | Burundi | Civil war (1993–2005) | 2007 | Adult population; N = 874 | Groups, trust | – | 4 years | ✓ | ✓ |
| B: Papers ineligible for the meta-analysis | | | | | | | | | |
| 17. De Juan and Pierskalla (2016) | Nepal | Civil war (1996–2006) | 2003 | Nationally representative sample; N = 8,822 | Trust in national government | – | 0–7 years | ✓ | |
| 18. Hartman and Morse (2015) | Liberia | Civil war (1989–2003) | 2013 | Adult population; N = 1,600 | Willingness to host refugees | – | 10 years | | |
| 19. Shewfelt (2009) | Indonesia, Bosnia and Herzegovina, United States (Vietnam veterans) | Indonesia: insurgency in Aceh (1976–2005) B&HF: civil war (1992–1995) United States: Vietnam war (1955–1975) | Indonesia: 2007 Bosnia: 2006 United States: 1986 | Indonesia: N = 1,752 Bosnia: nationally representative sample; N = 3,580 United States: male Vietnam theater veterans; N = 1,171 | Indonesia: groups, community, trust, voting Bosnia: groups, voting, interest in politics United States: groups | – | 2–11 years | | |
| C: Papers studying other forms of violence | | | | | | | | | |
| 20. Bateson (2012) | 70 countries | Crime victimization | Americas: 2010 Africa: 2008–2009 Europe: 2000 Asia: 2005–2008 | Latin America: 39,258 United States and Canada: 3,000 Africa: 27,713 Europe: 17,088 Asia: 16,725 | Groups, community, voting, interest in politics | – | | ✓ | ✓ |
| 21. Becchetti, Conzo, and Romeo (2014) | Kenya | Kenyan crisis, post-election violence (2007–2008) | 2010 | Nairobi slum-dwellers; N = 404 | – | Trust game | | ✓ | |
| 22. Hopfenstz and Miquel-Florensa (2014) | Colombia | Colombian conflict (1964+) | 2012 | Coffee farmers; N = 260 | Community | Public goods game | | | |
| 23. Rojo-Mendoza (2014) | Mexico | Crime victimization | 2011 | Nationally representative sample; N = 7,416 | Groups, interest in politics | – | | | |

Note: In the comparable survey measures column, “groups” means “social group participation” and “community” means “community leadership and participation.”

All three studies from Sierra Leone identified the same essential pattern: plausibly exogenous variation in exposure to war-related violence was associated with greater social participation and prosocial behavior. The earliest study in this literature, by Bellows and Miguel (2006, 2009), analyzed patterns of local collective action and individual political engagement using a large-scale nationally representative survey dataset on more than 10,000 Sierra Leone households gathered three to five years after the conflict's end. To measure exposure to war-related violence, they constructed an index from responses to three questions: Were any members of your household killed during the conflict? Were any members injured or maimed during the conflict? Were any members made refugees during the war? Victimization rates were high; for instance, 44 percent of respondents reported a household member being killed during the conflict. They found that people whose households directly experienced war violence displayed much higher levels of civic and political engagement compared to nonvictims: they were more likely to report attending community meetings (by 6.5 percentage points), to vote in elections (by 2.6 percentage points), to join social and political groups, and to participate in school committees and "road brushing," a local infrastructure maintenance activity.

To move past relying on self-reports of behavior, researchers have also carried out incentivized lab-in-field experimental games in Sierra Leone, in order to more directly assess whether war-related violence causes changes in social preferences or in beliefs about others' behavior, albeit in controlled and artificial situations. This experimental evidence complements observational survey evidence, and thus may contribute to a better understanding of competing theories.

Table 1 summarizes the games that were implemented in each study. Different types of experimental games help to distinguish between different factors. In simple allocation tasks, such as a Dictator game or a Social Value Orientation experiment, decisionmakers anonymously allocate rewards between themselves and another person. Because the recipient is passive and the interaction is one-shot and anonymous, beliefs about the reaction of the other player should not in principle affect sharing decisions. Choice situations in which participants not only maximize their own rewards but also take into account the welfare of recipients are taken as measures of social preferences, such as altruism, inequality aversion, or adherence to social norms.

In a second class of games, including the Ultimatum game or Trust game, the recipient is not passive and choices are made sequentially. These tasks are designed to uncover willingness to reciprocate (by rewarding kind acts and punishing unfair behavior) as well as beliefs about cooperative behavior of others. In an Ultimatum game, the first player is given a sum of money to divide with another player. If the second player accepts the division, then both receive the money; but if the second player rejects the division, neither player receives anything. The second player's choices, in particular, rejections of low offers, reveal whether that second player is willing to sacrifice earnings in order to punish unfair behavior, while beliefs about whether others have such fairness motivations should be reflected in the choices of the first player. In a Trust game, the amount given by the first player to the second player is tripled, and then the second player can decide whether to give some of the

money back to the first player. Transfers of the first player reveal trust—that is, beliefs about whether other players will cooperate by returning some of the money—while back transfers made by the second player provide a measure of reciprocity.

Finally, in a Public Goods game, multiple players decide simultaneously (without knowing about the choices of others) whether to contribute to a public good. The private return from contributing is negative, but the total group payoff to contributing is positive because the return to other players combined is substantial. This game thus reveals individual willingness to cooperate or to free ride (that is, hoping that other players will contribute to the public good). The identities of the other players can also vary in these games, in particular by whether players are interacting with those from a group with whom they have some reason to identify, such as an ethnic or social group.¹

Bauer, Cassar, Chytilová, and Henrich (2014) ran various allocation games, sometimes referred to as mini-Dictator games, designed to distinguish selfishness from altruism and inequality aversion, in northwestern Sierra Leone. They experimentally manipulated the identity of an otherwise anonymous recipient to shed light on whether violence increases prosocial behavior only towards people at the local level, or whether the effects on prosocial behavior are more generalized. In the in-group condition, the partner was from the same village as the decision maker, and in the out-group condition the partner was from a “distant village.” Compared to nonvictims, people who were directly exposed to conflict-related violence were less selfish (by 23 percentage points) and more inequality averse (by 25 percentage points) towards in-group members eight years after experiencing war-related violence. Effects were especially large among those exposed to violence during their childhood and adolescence. There were no comparable effects on behavior towards out-group members.

Elsewhere in Sierra Leone, once again eight years post-conflict, Cecchi, Leuveld, Voors, and van der Wal (2015) found similar results among young street soccer players (aged 14–31 years) using both experimental and observational approaches. Players made anonymous choices in the Dictator game, and those who had been exposed to more intense conflict-related violence behaved more altruistically towards their teammates (the in-group) but not towards the out-group (their match opponents). Direct observation of behavior during soccer matches also revealed that the more violence-exposed players were more likely to receive a yellow or red

¹In considering the contribution of these behavioral experiments, an important question is the degree to which links between such measures and the formation of real world institutions and cooperation has been made. Work establishing these links is limited. However, Rustagi, Engel, and Kosfeld (2010) show that communities in Ethiopia with more prosocial individuals, as measured using behavioral games, more effectively form real world cooperatives to monitor forest exploitation, more energetically monitor for free-riders (forest exploiters), and end up cooperating more effectively to manage harvests; these findings hold when the frequency of prosocial individuals is instrumented using the distance from market towns. The results suggest that if these villages were “shocked” (for example, by war) in a way that suddenly increased the frequency of prosocial individuals (as measured by experiments), they might become better at constructing local institutions to address real public goods problems.

(penalty) card during the game, suggesting that a violent conflict not only elevated in-group prosocial behavior but may also have exacerbated out-group antagonism.²

A common feature of this body of research—for Sierra Leone and the other studies discussed below—is that analysis is based on a comparison of individuals who suffered different degrees of war violence. These data do not allow the estimation of impacts on society as a whole since no suitable counterfactual exists.

Other Country Cases: Uganda, Burundi, Georgia, Nepal, and Others

Another much-studied country case is Uganda, with six papers listed in Table 1. Blattman (2009) examines the case of northern Uganda, where for 20 years the rebel group the Lord's Resistance Army (LRA) forcibly recruited tens of thousands of young people. The study attempted to account for confounders and other econometric identification concerns, using rebel raiding patterns as a source of plausibly exogenous variation in armed recruitment. The paper used a prewar sample, tracked survivors, and attempted to account for nonsurvivors, reducing concerns about bias due to selective attrition. An average of five years after temporary conscription into the LRA, the experience led to substantial increases in postwar social participation, in this case, self-reported voting and community leadership (though not social group membership).

Studies from other post-conflict societies in Africa and elsewhere have documented similar patterns. Notably, Voors et al. (2012) implemented a Social Value Orientation experiment (similar to a Dictator game) among adults in rural Burundi to study consequences of the 1993–2003 civil conflict there between the Tutsi-dominated army and Hutu rebels. Nine years after the war, individuals who personally experienced war-related violence, or who lived in attacked communities, behaved more altruistically towards neighbors in the experimental tasks, and were also more likely to report being involved in local community organizations.

Bauer, Cassar, Chytilová, and Henrich (2014) conducted an experimental study in the Republic of Georgia that paralleled their Sierra Leone study. The data were gathered among a sample of children six months after the brief August 2008 war with Russia over South Ossetia. As in Sierra Leone, the authors found evidence of differential treatment towards in-group and out-group members: participants who were more affected by the conflict were less selfish and more inequality averse towards in-group members (their classmates) as compared to their less-affected peers, but there was no such effect on behavior towards out-group members.

In a study of Nepalese society, Gilligan, Pasquale, and Samii (2014) found that members of communities with greater exposure to violence during the 1996–2006 civil war between governmental forces and Maoist revolutionaries exhibited greater levels of cooperation when interacting with each other: three years post-conflict, they were more trustworthy in a Trust game, more willing to contribute to the

²While not directly comparable due to a lack of data on in-group cooperation, Miguel, Saiegh, and Satyanath (2011) show that professional soccer players (in the major European leagues) who lived in conflict settings as children are also more prone to committing violent card fouls against the opposing team during matches.

common pot in the Public Goods game, and they reported being more active in community organizations.

In Israel, meanwhile, results from Ultimatum and Trust games indicate that living in a society with an active ongoing conflict (the Israel–Hezbollah conflict of 2006) temporarily increased the willingness of senior citizens to punish noncooperators and to reward cooperation (Gneezy and Fessler 2012). An aspect of this study is that it relied on a comparison of choices made before, during, and after the conflict and thus does not account for any time effects that occurred contemporaneously with the conflict.

In a study in Tajikistan, more than a decade after its 1992–1997 civil war, Cassar, Grosjean, and Whitt (2013) explored the effects of war-related violence on trust and cooperation. The war in Tajikistan has been described as a power struggle pitting former communists against a highly fractionalized group of challengers with diverse ideologies (including Islamist groups, ethnic nationalists, and prodemocratic reformers). During this civil war, a complex network of rivalries emerged within local communities during the fighting, often resulting in neighbors fighting neighbors (intragroup conflict). This contrasts with the above-mentioned studies, in which violence was typically perpetrated by people from outside of the affected communities (intergroup conflicts). In experimental games, Cassar, Grosjean, and Whitt (2013) matched subjects with another (anonymous) individual from the same village, and thus with some probability with someone from an antagonistic group. It turns out that the exposure to violence during the civil war was associated with a decrease in trust (measured by the first mover transfers in the Trust game). Interestingly, these negative effects were quite heterogeneous and appear to have depended on the nature of infighting within local communities: effects were particularly negative in regions where opposing groups were residentially intermixed and where local allegiances were thus split, indicating that exposure to violence reduced cooperative behavior when people thought they may interact with members of an opposing group in the conflict. Yet the authors also found evidence of elevated participation in local groups and associations among the war exposed, as in other studies. In the case of local group participation, individuals presumably had some ability to choose with whom they would interact (in contrast to the games, where matching was random), and so this result is also consistent with war exposure raising levels of prosocial behavior towards in-group members, although alternative interpretations remain possible.

The broad pattern of war exposure stimulating greater cooperation also holds in large-scale national surveys across multiple countries. Grosjean (2014) linked comparable nationally representative surveys from the Life in Transition Survey project, which gathered data from 35 countries in central and eastern Europe, the Baltic states, southeastern Europe, the former Soviet Union, and Mongolia in 2010. Nearly 40,000 individuals answered questions about their own and their parents' and grandparents' war exposure, with the relevant recall period covering World War II (1939–1945), as well as the civil wars in the former Yugoslavia (in the 1990s), the Tajik civil war (1992–1997), Chechen wars (1994–2009), and the Kyrgyzstan clashes in 2010. The incidence of World War II exposure was very high: the average proportion of respondents who reported that they or their parents/grandparents were injured

or killed was nearly 30 percent overall. Grosjean then focused on within-country variation in exposure to war violence. The results show a positive link between past experiences related to violent conflict and contemporary participation in community groups, collective action, and membership in political parties—although there was also a negative effect on trust in central government institutions.³

Disentangling Correlation and Causation

An obvious econometric concern is the possibility that the correlation between war exposure and cooperation is driven by some omitted variable that has a confounding effect, rather than reflecting a causal impact. For instance, more cooperative people might be more likely to participate in collective action, including civil defense forces or armed organizations that represent their groups during wartime, and thus more likely to live in a family that experiences some form of direct war victimization. Or perhaps attackers systematically target people who are likely to be more cooperative in nature, such as leading families or wealthy and influential citizens. If true, statistical tests would overstate the effect of war victimization on later civic participation and social capital. Attrition poses another potential challenge for causal identification if the least prosocial or cooperative people are also more likely to die, migrate, or be displaced and not return home.

Given the impossibility of randomized experiments involving targeted violence, studies in this area have taken various analytical steps to mitigate some of the most worrisome confounders. For example, Bellows and Miguel (2009) use three strategies in their study of Sierra Leone. First, they control for local fixed effects, typically at the village level, thus removing potential regional and local omitted variables, and show that within-village variation in violence exposure helps to explain patterns of within-village cooperation. In some settings, the qualitative evidence suggests violence is relatively indiscriminate in nature within a village, which is supported by statistical tests documenting the weak relationship between observable prewar characteristics and the likelihood of falling victim to violence. Second, the researchers attempt to control for local confounders with an extensive set of prewar characteristics, such as wealth or whether victimized households were more central to local politics. González and Miguel (2015) expand on this issue, discuss limitations of the original Bellows and Miguel (2009) analysis, and present alternative ways of accounting for the possible selection into war violence exposure. Third, they estimate effects among subsamples for which victimization was likely to

³Some evidence suggests that the effects of experiencing war-related violence may be more persistent if experienced during childhood and adolescence, in line with a broader literature on critical periods in the formation of preferences and noncognitive skills (Heckman 2006; Almås Cappelen, Sørensen, and Tungodden 2010; Bauer, Chytilová, and Pertold-Gebicka 2014; Kosse, Deckers, Schildberg-Hörisch, and Falk 2014). In Sierra Leone, Bauer, Cassar, Chytilová, and Henrich (2014) find the strongest effects on social preferences among those who were children or adolescents during the civil war. Similarly in Uganda, Bauer, Fiala, and Lively (2014) show that effects are driven mainly by those who soldiered during childhood or early adolescence.

be less systematic: for example, for individuals who were children too young to have been prewar community leaders, or for individuals living in areas where fighters were unlikely to have detailed knowledge of the local area, in which case indiscriminate violence seems more likely.

These three strategies describe nearly every study in our sample. All make some form of a conditional unconfoundedness assumption, and control (where such data exist) for possible confounders. Every war is different, of course, and so there is no universal set of confounders. But each paper makes a plausible case that the remaining variation in violence is largely idiosyncratic. Despite these efforts, none of these empirical strategies can fully eliminate concerns about bias from selection and omitted variables. As we show in the meta-analysis, the results are nonetheless relatively consistent across different studies and approaches to causal identification, arguably generating more confidence that the estimated relationships are causal.

Meta-analysis

The existence of so many new papers tackling the same core question with similar data permits us to formalize some of the cross-paper comparisons with a formal reanalysis.

We identified 23 published and unpublished papers that estimate the effects of violence on social behavior, and report them in Table 1. Of these, 19 focus on war violence (as opposed to violence in the form of crime or during elections) and we focus our analysis on these war-related papers here. Of these, 16 studies meet two additional criteria for our reanalysis: the dependent variable was some measure of social participation, cooperation, or prosociality; and the individual data were available online or from the authors.⁴ We perform a meta-analysis of these 16 studies using the original data, calculating the average effect of war violence on cooperation as a weighted mean across studies. The online appendix available with this paper at <http://e-jep.org> summarizes details of the formal literature search, inclusion and exclusion criteria, and discusses the statistical methods and results in greater detail.

Outcome Measures

Outcome measures vary across studies, and not all outcomes are gathered in every paper. To simplify comparisons, we employ the data from each study to construct a standardized index of outcomes that has a mean of zero and unit

⁴We excluded one paper for which data are unavailable, and excluded two papers that examine behaviors that are not comparable to other studies (such as trust in the national government, or willingness to host refugees). Panel B in Table 1 provides information on these three studies. In addition, we identified four related studies focusing on other types of exposure to violence (such as crime, electoral violence, or displacement) in Panel C. We explored the robustness of our results to including some of these additional studies in the meta-analysis, and find qualitatively similar patterns. The results are available in the online appendix.

standard deviation. The outcome variables generally fall into six categories, as follows (and we summarize them for each study in Table 1):

1) *Social group participation*. This variable captures participation in local social clubs, sports teams, or community organizations. Some studies report the number of groups in which an individual participates, and we standardize the summed measure. If a study uses a binary indicator for group participation and no data is available for the number of groups, we standardize the binary measure.

2) *Community leadership and participation*. This variable includes indicators for community leadership and engagement, such as participating in local meetings, volunteering for community work, and/or being a community leader or mobilizer. We sum the available indicators for each study and standardize.

3) *Trust*. For each study, we sum the available trust variables (such as “How much do you trust members of your village?”) and standardize the sum. Since trust in in-group and out-group members might differ, we also create separate variables for these subgroups. We define in-group members as people from the same family, village, class, and ethnic group. Out-group members are classified as individuals from other ethnic groups or parts of the country.

4) *Prosocial behavior in experimental games*. Measures of prosocial behavior vary by study (see Table 1), ranging from altruistic and inequality-averse behavior in allocation tasks (such as the Dictator game), trust and reciprocal behavior in a Trust game, punishment of unfair offers in an Ultimatum game, and contributions in a Public Goods game. As the scale of each outcome measure varies by game and study, we standardize each outcome, where higher (positive) values correspond to more prosocial behavior. We also distinguish between prosocial behavior toward in-group and out-group members for studies that manipulated the identity of the experimental counterpart accordingly.

5) *Voting*. This variable measures voting in local and national elections. We sum the number of elections in which participants were registered to vote, planned to vote, or voted, and standardize the summed measure.

6) *Knowledge of and interest in politics*. This measure combines binary indicators for familiarity with political figures or events and more general interest in a country’s politics. For each study, we sum these indicators and standardize the summed measure.

To enhance comparability, as well as address the multiple comparison problem, we also create a summary index of all cooperation measures. In particular, for each study, we generate a mean effect across all available outcomes (following the approach of Kling and Liebman 2004; Kling, Liebman, and Katz 2007), where the indices are calculated from the standardized outcome measures of each study.

Statistical Approach

We replicate each study’s original research design, taking the study’s identification strategy, measure of violence exposure, control variables, and observation weights at face value.⁵ Each study has a different empirical strategy for identifying

⁵There is one small exception to this statement: namely, if a paper uses a continuous measure of violence, we convert it to an indicator for comparability with other studies and ease of interpretation. In the

the impact of war violence exposure, and as noted above, most papers assume conditional unconfoundedness—namely, that after adjusting for any observed variables (including location fixed effects in many cases) that would help to determine violence, the remaining exposure to violence can be treated as random.

Violence is rarely truly random, of course, and not all the plausible determinants of violence are observed. Thus, the plausibility of the econometric identification assumptions vary from paper to paper, and these causal claims must be taken with some caution. To analyze this issue more systematically, we code studies by their analytical approach, and document the details in the online Appendix. For example, some studies possess prewar data on victims, some have a long list of “substantive” control variables that go beyond basic demographics to control for the specific confounders (such as wealth or status) that arguably could drive victimization risk.

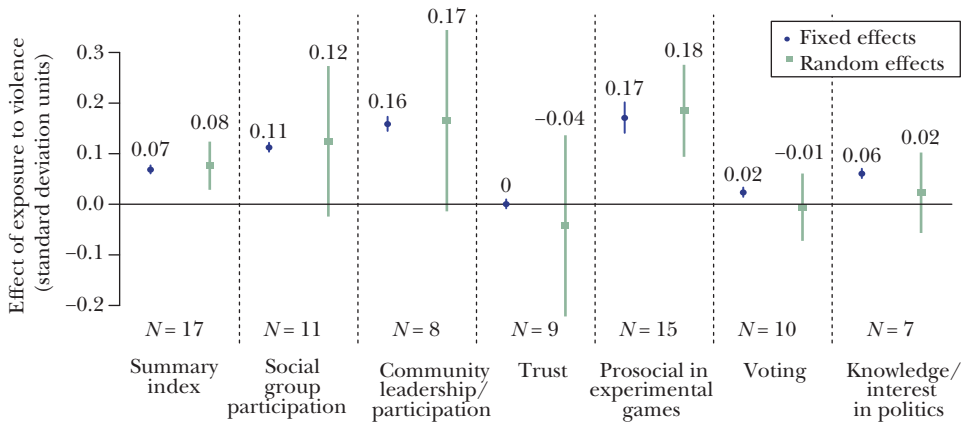
First, however, we estimate overall effects of violence on prosocial behavior. We use both fixed effects and random effects models for this meta-analysis, though note that this terminology has a somewhat different meaning in a meta-analysis than it would when referring to the use of fixed or random effects in a regression model in a single study. In a meta-analysis, a fixed effect refers to whether the effects of the independent variable are indicative of a single stable underlying parameter, while a random effect allows the effect of the variable to differ across contexts in possibly idiosyncratic ways. To put it another way, a fixed effect meta-analysis model is based on the assumption that there is a common effect across all the studies, and thus effectively assumes that studies are drawn from the same population, with larger sample studies thus receiving much more weight in the analysis. In contrast, random effects models allow the true effect magnitude to vary across studies, perhaps because the nature of war violence effects is context-specific. In this case, the studies included in the meta-analysis are simply thought of as a sample from the broader distribution of effects, and smaller sample studies receive relatively more weight than they do in the fixed effects meta-analysis.

In this meta-analysis, the random effects model is arguably preferable on conceptual grounds, since the nature and effects of war violence are likely to be heterogeneous across contexts, but we also report the results of fixed effects approaches, as is common in the related meta-analysis literature, in order to assess robustness to statistical modeling assumptions.⁶ Below we also explicitly model the heterogeneity in effect estimates as a function of observed study factors (for example, duration since war exposure), in order to better characterize the nature of context-dependence, something random effects meta-analysis alone is unable to shed light on.

appendix, we also consider alternative independent variables: standardized continuous measures; indicators of the respondent’s direct or personal exposure to violence; and indicators of indirect exposure to violence (for example, through the household or community’s exposure; these include, for example, having household members killed or injured, or being in a community that was targeted by violence). Results, reported in Appendix Table A17, are qualitatively similar using alternative approaches.

⁶The online Appendix available with this paper also considers a third approach, following Stanley and Jarrell (1989), to include studies without published data. To do so, we use *t*-statistics as a standardized measure of effect size. As can be seen in Table A18, we find qualitatively similar results.

Figure 1

Meta-Analysis Results, War Exposure, and Cooperation

Notes: The figure plots the meta-analysis results reported in Table 2. The effect of exposure to violence on each outcome is estimated using fixed-effects (circles) and random-effects (squares) meta-analysis models. Results are reported in standard deviation units. The vertical lines denote 95 percent confidence intervals. N denotes the number of studies/games included in the meta-analysis for each outcome.

Results

Figure 1 displays the average effect of war violence on the standardized indexes, as well as on the overall summary index of all cooperative and prosocial behaviors. There is some variation in the number of studies that capture particular aspects of cooperative behavior, as indicated in the figure, with $N = 17$ studies contributing to the summary index. We present both the fixed- and random-effects average treatment effects with 95 percent confidence intervals. Table 2 reports the corresponding coefficients, standard errors, and p values.⁷

Overall, exposure to war violence is associated with a positive and statistically significant increase in the summary index, with a coefficient of 0.07–0.08 standard deviation units and statistical significance for both the fixed effects (p value < 0.01) and random effects (p value < 0.01) approaches. We interpret this as a rejection of the null of no effect, and substantial evidence of positive effects, albeit with only moderate magnitude.

When considering different types of outcomes, the standard errors in the random effects models are much larger than in the fixed effects case, which is not uncommon in a meta-analysis. Precision is increasing in both the number of subjects per study as well as the number of studies, and so the effects are least precise where we have a small number of studies (as in the case of trust). Taken together, there is substantial evidence of an increase in several dimensions of cooperation and

⁷In the online Appendix available with this paper, Figures A4 to A25 present the study-by-study estimates that make up the meta-analysis, for each outcome. The count for the summary index is 17 (and not 16, the total number of analyzed studies) because the Bauer, Cassar, Chytilová, and Henrich (2014) paper has data from two countries, as we thus consider them as two estimates here.

Table 2

Meta-analysis Results: Estimated Population Effects of Exposure to Violence across Studies

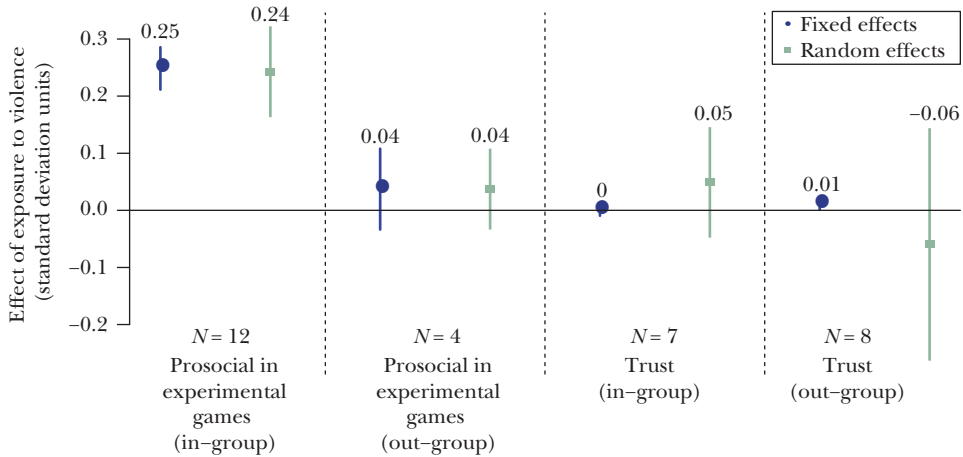
| <i>Outcome (Standardized)</i> | <i>Estimate</i> | <i>Fixed effects</i> (1) | <i>Random effects</i> (2) |
|---|-----------------|-----------------------------|------------------------------|
| Summary index (mean effects) | Coefficient | 0.07*** | 0.08*** |
| | Standard error | 0.00 | 0.02 |
| | <i>p</i> -value | < 0.01 | < 0.01 |
| Social groups participation | Coefficient | 0.11*** | 0.12 |
| | S.E. | 0.00 | 0.08 |
| | <i>p</i> -value | < 0.01 | 0.10 |
| Community leadership/ participation | Coefficient | 0.16*** | 0.17* |
| | Standard error | 0.01 | 0.09 |
| | <i>p</i> -value | < 0.01 | 0.07 |
| Trust | Coefficient | 0.00 | -0.04 |
| | Standard error | 0.00 | 0.09 |
| | <i>p</i> -value | 0.87 | 0.64 |
| Prosocial behavior in experimental games | Coefficient | 0.17*** | 0.18*** |
| | Standard error | 0.02 | 0.05 |
| | <i>p</i> -value | < 0.01 | < 0.01 |
| Voting | Coefficient | 0.02*** | -0.01 |
| | Standard error | 0.00 | 0.03 |
| | <i>p</i> -value | < 0.01 | 0.86 |
| Knowledge/interest in politics | Coefficient | 0.06*** | 0.02 |
| | Standard error | 0.00 | 0.04 |
| | <i>p</i> -value | < 0.01 | 0.57 |

Notes: Meta-analysis results for each outcome are reported in the rows. Column (1) reports results from a fixed-effects model; column (2) reports results from a random-effects model. In a meta-analysis, a fixed effect refers to whether the effects of the independent variable are indicative of a single stable underlying parameter, while a random effect allows the effect of the variable to differ across contexts in possibly idiosyncratic ways. The coefficient represents the estimated population effects of exposure to violence across studies, measured in standard deviation units. This analysis excludes exposure to crime violence. ***, **, and * indicate statistical significance at the 1 percent, 5 percent, and 10 percent levels, respectively.

prosocial behavior with exposure to war violence. The fixed effect estimates are positive and statistically significant for participation in social groups, community leadership and participation, prosocial behavior in experimental games, voting, and knowledge of politics (all with *p* value < 0.01). However, the effect of exposure to war violence on trust is close to zero. The random effect estimates are positive and significant for prosocial behavior in experimental games, and marginally significant for community leadership and participation in social groups, while effects are not distinguishable from zero for the other categories.

In Figure 2, we examine behavior towards in-groups versus out-groups, focusing on the papers and outcomes with appropriate data. For experimental game measures of prosocial behavior, there are positive and significant impacts of war exposure on behavior towards in-group members in both the fixed effect and random effect models, with substantial gains of 0.24 to 0.25 standard deviation units and statistically significant findings (*p* value < 0.01). In contrast, effects are

Figure 2

Meta-analysis Results, In-Group versus Out-Group Effects

Note: The figure plots the meta-analysis results, broken down by behavior towards in-group and out-group. The effect of exposure to violence on each outcome is estimated using fixed-effects (circles) and random-effects (squares) meta-analysis models. Results are reported in standard deviation units. The vertical lines denote 95 percent confidence intervals. N denotes the number of studies/games included in the meta-analysis for each outcome. A meta-regression test for the difference in behavior towards in-group and out-group shows that for games, the difference is significant under both fixed-effects and random-effects model assumptions. For trust, we do not find a significant difference in attitudes towards in-group and out-group members.

smaller in magnitude (at 0.04 standard deviation units) for behavior towards out-group members and not statistically significant in either model. While there is no indication of negative effects towards the out-group, there is significantly less prosocial behavior towards them than towards the in-group. For the stated trust measures, there are no statistically significant effects overall or towards in-group or out-group individuals separately, nor do we find a significant difference between effects on in-group and out-group members, although it is worth recalling that there are relatively few studies with the detailed trust questions needed to undertake this analysis.

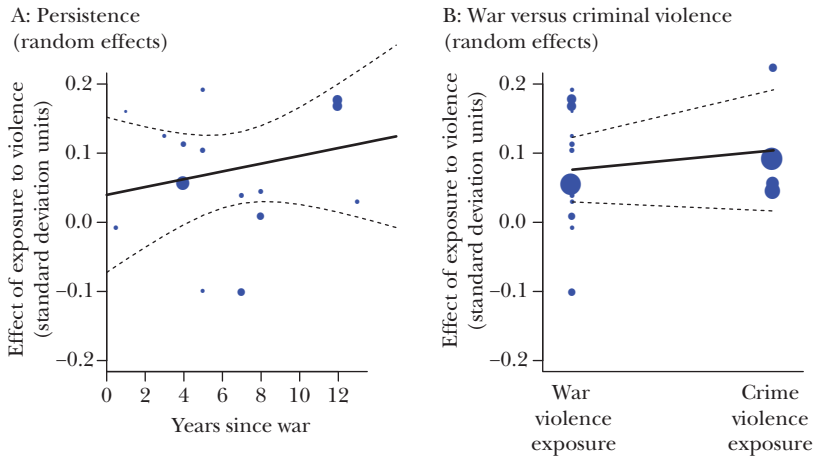
Patterns across Studies

It is informative to examine how circumstances, settings, or study characteristics correlate with the estimated effects of violence on prosocial behavior, although standard errors are relatively wide given the $N = 17$ estimates in hand.

First, we see no evidence that the effects of war violence on prosocial behavior decline over time. We regressed the estimated effect from each study on the length of time between the end of the conflict and the study measures (Table 1 reports the time since war exposure for each study). Figure 3A illustrates the results in a meta-analytic scatterplot; the figure shows the observed effects estimated for individual studies (measured as a standardized index of all cooperation outcomes) plotted against the length of time (in years) between the end of the conflict and the timing

Figure 3

The Effect on Cooperation of War Violence Exposure over Time, and of War versus Crime-Related Violence



Notes: Figure 3A presents the meta-analytic scatterplot of the observed effects estimated for individual studies, where the dependent variable is an index of all cooperation outcomes, plotted against the length of time between the end of the conflict and the timing of each study. Figure 3B plots the observed effects against an indicator of war/crime violence exposure. The point sizes are proportional to the inverse of the standard errors, which means that studies with larger samples tend to have visually larger points. The predicted average effects are included (with corresponding 95 percent confidence intervals), calculated from the random effects meta-analysis model. Grosjean (2014) is dropped from analysis in panel A because of high variability in the years-since-war across the 35 countries studied.

of each study. The resulting regression line has a small positive slope of 0.01 that is not statistically significant in the random effects model (although the fixed effects estimate, reported in the online Appendix, is significant).

Second, in Figure 3B, we compare the war violence studies to data from a study that examines exposure to criminal violence and prosocial behavior in multiple countries (Bateson 2012), and obtain similar average effects. Indeed, the estimated effects from crime studies are, if anything, somewhat larger: the difference in average effect size is 0.03, although it is again not significant in the random effects model. Of course, the difference between war violence and criminal violence is often hard to distinguish, especially if crime involves victims and perpetrators arrayed across a salient social cleavage or carried out by gangs, so some crime incidents could also have an organized intergroup dimension; the data do not allow us to say. But the evidence at least suggests that the “war” aspect may not be at the core of whatever is causing this phenomenon.

Third, the estimated effects are fairly consistent across the various empirical strategies used in the emerging literature. As discussed above, we coded variables that capture different aspects of the research designs, including the use of prewar data, substantive controls, community fixed effects, instrumental variables, and sensitivity analyses. The results, reported in the online Appendix (Figure A3), show that the empirical strategy does not significantly predict variation in the magnitude of the

effects across studies in a random effects model, although some study-level covariates are significant in the fixed effects model. For instance, we find that estimates from studies that control for prewar individual covariates are of larger magnitude, and estimates from studies that employ sensitivity analyses are somewhat smaller in magnitude (and these patterns are significant in the fixed effects meta-analysis), although we have not identified a definitive explanation for these differences.

Fourth, we examine whether the way in which violence exposure is measured—on the personal and household level, as opposed to the community, municipality, or district level—might explain the variation in the magnitudes of the effects. We find that studies using measures of personal exposure have smaller coefficients, on average, than studies using more aggregated measures of exposure. We also categorize each study based on whether those exposed to violence were civilians, as opposed to combatants, and find that exposure to violence as a civilian is associated with larger effects.

Theoretical Explanations

The research to date has done a far better job of establishing the effect of war violence on later cooperation than of explaining it. Most papers propose at least one economic, evolutionary, or psychological theory consistent with the observed patterns, but few are able to directly test alternative theoretical predictions of specific models, and the existing pattern of results does not strongly favor any single theoretical perspective. Here, we try to organize the various explanations into a somewhat more coherent conceptual framework.

Changes in Constraints, Economic Payoffs, and Beliefs

Interestingly, almost none of the studies in Table 1 proposes an explanation rooted in the logic of neoclassical economics—that is, an explanation in which social participation or prosocial behavior becomes the optimal choice after war due to the effects of violence on people's economic incentives, constraints, and beliefs. Even so, it is possible that violence affects behavior in this way.

Several economic channels may be relevant. First, greater cooperation may arise from the greater value of social insurance. War frequently destroys household assets, and may make victims of violence more dependent on local informal systems of risk-sharing and insurance, especially among kin and neighbors, thus increasing the return to investments in social capital. Moreover, during wartime, investments in various types of physical and human capital may have been too risky, too constrained, or too expensive relative to investments in social capital. Those most victimized (or most at risk of violence) would thus have an incentive to make larger social capital investments, which could be reflected after war in group memberships, community leadership, and other forms local participation. Second, cooperative behavior could emerge from motives of personal safety and protection. During and after war, property rights and personal security would likely be endangered, and investments in local social capital could be a valuable form of self-protection—for example, in the

case of mutual assistance patrols of the neighborhood or village against intruders). It is also possible that the rapid economic recovery many postwar societies experience—such as Sierra Leone after its civil war, or many of the European cases studied in Grosjean (2014) after World War II—could produce the effects we document, if improving economic circumstances tend to generate more social cooperation.

War-related experience may also induce changes in people's beliefs that make prosocial behavior more (or less) persistent. If a sufficiently large number of community members experience the war "shock" at the same time, the entire community could be driven to a more prosocial equilibrium. In this situation, war-affected individuals would appear particularly prosocial soon after the war, but in the long run they would not be distinguishable from the rest of their community because all community members would converge to the new equilibrium. Alternatively, assuming that only a subset of a community experienced the shock of war at the same time, then perhaps the community as a whole does not shift to a new equilibrium. Instead, the prosocial behavior of war-affected individuals might decline over time as their beliefs converge back to the prevailing reality in their communities.

Changes in Parochial Norms and Preferences

Social scientists commonly seek to explain variation in individual social and political activity by pointing to variation in altruism, ethical norms, intrinsic motives to serve the public good, and other "social preferences." Some researchers have suggested that exposure to war-related violence may shape these underlying preferences.

In particular, evolutionary theories suggest that changes due to war violence might lead to favoring one's own group rather than social and political action in general. More specifically, evolutionary researchers from several disciplines have argued that our species' long history of intergroup competition may have favored adaptive psychological responses that promote the success of an individual's group relative to other groups—especially relative to antagonistic out-groups (Alexander 1987; Boyd, Gintis, Bowles, and Richardson 2003; Darwin 1871 [1981]; Henrich 2004). This idea has spurred two theoretical variants, one rooted in purely genetic evolution, and a second that considers the interaction between cultural and genetic evolution.

In the purely genetic version, intergroup competition directly favors prosociality toward in-group members and the derogation of those in competing groups (Bowles 2006; Choi and Bowles 2007; Haidt 2012; Wilson 2012). The prediction from this approach is that intergroup competition—and especially war, an extreme form of such competition—will increase individuals' prosocial behavior toward in-group members. These effects are expected to shift people's social preferences—their intrinsic motivations—to make them more parochially prosocial.

In the culture-gene coevolutionary variant, intergroup competition favors cultural practices in the form of social norms or institutions that promote success in intergroup competition (Henrich and Boyd 2001; Richerson and Boyd 2001). Meanwhile, operating within groups, natural selection favors psychological reactions that motivate stronger adherence to these local social norms, institutional

practices, and cultural beliefs in favor of culturally defined in-groups. This psychological response to intergroup competition is favored because cultural evolution has long selected cooperative combinations of norms, institutions, and beliefs—so greater norm adherence, including a greater willingness to punish norm-violators, should promote competitive success.

To the degree that local norms prescribe cooperative behavior, individuals more exposed to intergroup competition—including war—should reveal greater prosociality. Since norms are eventually internalized as motivations (or preferences), this approach predicts a shift in preferences similar to that noted above for the purely genetic version. However, unlike in the genetic version, this war exposure could also increase adherence to other norms: for example, if local social norms derogate homosexuality, favor attendance at religious rituals, or promote belief in a particular god, then more war-exposed individuals also ought to be more inclined to derogate homosexuality, attend rituals, and believe in the relevant deity (Henrich 2016).

To study changes in parochial norms and preferences, it is essential to assess what the relevant in- and out-groups are. For example, the experience of a civil war that pits one ethnic group against another might strengthen coethnic prosociality, while corroding the between-ethnic group social capital that could be necessary for later nation-building. Conversely, the experience of an external aggressor attacking a population that already possesses a national identity might bond that entire population even more tightly together and potentially enhance the opportunities for constructing effective national-level institutions in the postwar period. In both cases, and more speculatively, war experience would harden people's parochial prosociality, but the downstream consequences for social stability might depend on how the in-group is interpreted, and what role the relevant out-group plays in social and political life going forward.

Changes in General Preferences and Other Psychological Explanations

A final set of theories and articles propose that preferences for participation and prosociality shift more generally, rather than for or against a particular group. For example, there is substantial evidence that war violence is linked to symptoms of depression and distress, which include a general malaise and lack of desire to engage with people, avoidance of places or people that remind one of the traumatic event, difficulty in maintaining close relationships, an inability to experience positive emotions, negative feelings about oneself or others, and hopelessness about the future (Ehlers and Clark 2000; Galovski and Lyons 2004). Most victims of wartime violence do tend to recover from these symptoms with time, but an important minority continues to experience moderate to severe symptoms for many years, or even the rest of their lives. When people speak of the harmful effects of war on social and political activity, they often have this kind of lasting psychological damage in mind. What is striking is that, in spite of the well-documented effects of violence on distress and depression for some individuals, the emerging empirical evidence reveals an increase in average cooperation and community participation.

Along the same lines of generalized preference change, other psychologists have documented the opposite reaction to violence, a phenomenon they have

labeled “post-traumatic growth.” Working with the survivors of serious accidents, rape, or other near-death experiences, psychologists have noted that some people respond to trauma by reflecting on and reevaluating their lives, especially in terms of what they regard as important and valuable, such as family and relationships; this research is based largely on case studies. For instance, some victims report a greater valuing of life, more meaningful relationships with others, greater personal hardiness, a realization of new possibilities, and increased spirituality (Tedeschi, Park, and Calhoun 1998; Tedeschi and Calhoun 2004). After war violence, it is possible to imagine victims changing their priorities in life and placing renewed value on relationships with family and community, and even changing other-regarding preferences. Such changes need not be parochial in nature; the existing literature in this area is silent on this point.

Yet another perspective on preference change comes from the political science literature on rebellion. Some ethnographers studying who joins rebel movements (and why) have argued that the experience of injustice, particularly war-related violence, increases individual preferences for collective action. Wood (2003), studying insurgents in El Salvador, noted that people tended to join or support the rebel movement in response to government violence against them or their family members. She argues that material considerations (such as destruction of property or aspirations of land distribution) played little role in who joined. Rather, Wood argues that the injustice of being the subject of violence instilled a “pleasure in agency”—an increase in the intrinsic value in collective action and associational life.

Political scientists use the intrinsic pleasure of participation or expression to explain a variety of behaviors, perhaps most importantly to explain why people expend time and energy to vote, and these intrinsic motives are referred to as “expressive preferences” (for example, Brennan and Lomasky 1997). As with the economists’ closely related concept of social preferences, it is not clear what drives these expressive preferences, or how they respond to experience or investment. Some ethnographers have argued that injustices instill a desire for revenge and a pleasure in punitive action (for example, Petersen 2001). Wood’s (2003) work in El Salvador has powerful parallels to psychological narratives of post-traumatic growth. On the other hand, since the participation Wood observes is inherently parochial, it is possible that these expressive preferences are also sometimes parochial and could have similar evolutionary origins.

What Does the Evidence Suggest?

Each of the above theories is intuitive and plausible, but empirical support is, so far, relatively limited. Nonetheless, the patterns in the emerging literature do weigh against certain interpretations and lend some support to others. Our reading is that the evidence favors the idea that war violence influences individual social preferences or adherence to existing social norms, and there is suggestive evidence that these changes may be parochial in nature.

For instance, several patterns suggest skepticism towards neoclassical economic explanations. First, the evidence from anonymous behavioral games seems to suggest that something beyond a straightforward calculated response to costs and benefits

is occurring. Second, some studies document effects even among young children, and children are more likely to be influenced by prevailing norms and social preferences than by economic cost–benefit considerations or constraints. Third, the war violence effects we document endure long after the conflicts end and even when postwar prosperity and security have improved relative to the prewar (or immediate postwar) situation. Finally, if it were simply a matter of postwar household economic circumstances driving cooperation, one might expect that improving living standards driven by external assistance programs would have a similar effect on local cooperative behavior, but there is little evidence of such a relationship. For example, in a randomized controlled trial in postwar Sierra Leone, Casey, Glennerster, and Miguel (2012) show that large amounts of aid increased local incomes and market activity but did not translate into improvement in a wide range of measures of village meeting participation, social capital, and cooperation.

Nor do we see much evidence consistent with the view that a change in beliefs about the behavior of others is key. Such a view would have two empirical implications: first, that behavioral differences between war-affected people and others are driven by possibly ephemeral differences in information and beliefs, and second, there may not be any enduring long-run differences between the war-affected individuals and the rest of the community (although there may be persistent differences between entire communities subjected to war and those that were not, if a new local equilibrium emerges). Yet neither of these is borne out in the data. War-exposed individuals do not expect others to be more cooperative in survey questions on trust, they behave more prosocially even in games in which beliefs about the behavior of others should not matter, and the behavioral differences between more- and less-war-exposed members of the same community are not ephemeral: they appear to last for many years after conflict ends.

There are at least three reasons, meanwhile, to suggest that war violence may lead to changing social preferences. First, several studies document behavioral changes in experiments that were specifically designed to identify social preferences or adherence to social norms, while controlling for other motivations. Second, the body of qualitative studies and case evidence from the political analysis of conflict, described above, documents self-reported changes in preferences following war victimization. Third, several studies document a change in in-group prosociality, but not out-group prosociality—a form of social preference change predicted by the theory.

Ultimately, there is still insufficient evidence to conclude decisively in favor of one theory over another, but the generation of such evidence is a clear direction for future research.

Conclusion

In less than a decade, nearly 20 observational studies have emerged on the same basic question in different settings, 16 of which are sufficiently similar and have publicly available data such that they can be jointly reanalyzed. This in itself is a striking accomplishment: not only did a few provocative early papers promote a

flurry of replications and extensions around the world, but in nearly every case the data have been made freely available online or shared with us directly by the authors, even for unpublished papers. This replication and openness, and the synthesis it permits here, generate some important and perhaps surprising conclusions about violence, psychology, and the formation of social capital, conclusions that differ in some cases from the arguments in the individual papers themselves.

Most of the papers in this emerging literature agree on one central matter: that the data strongly reject the common view that communities and people exposed to war violence will inevitably be deprived of social capital, collective action, and trust. Across the 16 studies from economics, anthropology, political science, and psychology, the average effect on a summary index of cooperation is positive and statistically significant, if moderate in magnitude.

Looking across many studies, however, systematic patterns emerge which were not readily apparent in any single article. For instance, despite early indications that political behavior might also be as positively affected as prosociality (Blattman 2009), this increase in political engagement is not borne out in several more recent studies (for example, Voors et al. 2012; Cassar, Grosjean, and Whitt 2013; Bauer, Fiala, and Lively 2014). Another example comes from the lab experiments, which more often than not have been showing that the prosociality that emerges is focused on in-group interactions but not on behavior towards out-groups. This evidence for parochial altruism, while preliminary, matters because war might enhance in-group cooperation and facilitate post-conflict reconstruction while simultaneously raising the risk of future social divisions and renewed intergroup conflict.

The most important next step will be for researchers to focus on establishing the reach and generality of this parochial altruism finding. Does it withstand scrutiny, and can we decisively rule out generalized changes in prosocial preferences, or more standard economic arguments? This necessitates a sharper focus on behaviors towards out-group members that belong to the antagonistic group in the war, which is not the case in most existing studies.

Another important direction is to examine other forms of physical insecurity, including crime, state repression, natural disaster, life-threatening accidents, and domestic abuse. In particular, the distinction between wartime violence and urban crime may not be large in certain cases, especially where widespread organized crime takes on characteristics of civil conflict, such as the cases of Mexican or Colombian drug trafficking organizations. Early evidence does indeed suggest that our findings on violence and cooperation could generalize to a wider range of situations. The meta-analysis finds that those who have experienced crime-related violence are also more likely to display cooperative behavior, just like war victims. There are parallels in related literatures, including findings that victims of crime are more likely to participate in community and political meetings, be interested in politics, and engage in group leadership (Bateson 2012). Other emerging evidence exploring the effects of post-election violence (Becchetti, Conzo, and Romeo 2014), and earthquake and tsunami damage (Caló-Blanco et al. 2015; Cassar, Healy, and Von Kessler 2011; Rao et al. 2011) also mimics the main finding of this paper, namely that survival threats tend to enhance local cooperation. We expect that work in these areas will yield new

insights about what psychological, economic, and social mechanisms could lead those who experience violence to shift to more cooperative behavior.

The core empirical finding we identify—that exposure to wartime conflict fosters cooperative behavior—resonates with the experience of rapid postwar political, social, and economic recovery in many war-torn societies, as well as their tendency to implement egalitarian social policies, including progressive taxation and gender equality reforms (Tripp 2015; Scheve and Stasavage 2010, 2012). While the human costs of war are horrific, there may at least be some reason for optimism once the violence ends.

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