Can War Foster Cooperation?

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Abstract: In the past decade, nearly 20 studies have found a strong, persistent pattern in surveys and behavioral experiments from over 40 countries: individual exposure to war violence tends to increase social cooperation at the local level, including community participation and prosocial behavior. Thus while war has many negative legacies for individuals and societies, it appears to leave a positive legacy in terms of local cooperation and civic engagement. We discuss, synthesize and reanalyze the emerging body of evidence, and weigh alternative explanations. There is some indication that war violence especially enhances in-group or "parochial" norms and preferences, a finding that, if true, suggests that the rising social cohesion we document need not promote broader peace.

Warfare leaves terrible legacies, from raw physical destruction to shattered lives and families. Many international development researchers and policymakers describe war as "development in reverse" (Collier et al. 2003), having 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 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; 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 2015).

In this article, we discuss and synthesize a rapidly growing body of research from which a new stylized fact 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. We can do so because of a wealth of new data. 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 aspect 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, what we will generally call "prosocial" behavior. Table 1 illustrates the breadth of

evidence, with references to 16 studies involving Sierra Leone, Uganda and Burundi in Africa, to the Republic of Georgia, Israel, Nepal, and many other societies. The data come from individual surveys collected in 10 countries, plus one paper with comparable data from 35 European countries.[1] This evidence covers both civil and interstate wars; and includes a wide array of wartime violence experiences, ranging from personal exposure, where individuals themselves were targeted or directly witnessed violence, to more indirect exposure, where 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 in experimental laboratory games, such as altruistic giving.

Our meta-analysis also suggests the effects of violence are persistent and fairly consistent across cases. Moreover, we see little systematic difference by the type of violence experienced (including in a related body of studies that estimate impacts of crime victimization), 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.

[1] These figures refer to the studies listed in Panels A and B of Table 1 only, meaning that they are studies that employ war violence as an explanatory variable. Considering only studies that meet our inclusion criteria (described below), we focus on data from seven different countries in 15 studies, plus the 35-country European study.

Violence may also affect in-group prosocial behavior – namely, participation with, and altruism towards, members of one's own village or identity group – most of all. 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 is 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 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 post-war 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 or with high homicides rates (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 post-war populations, society and politics.

Case Evidence on the Effects of Exposure to Wartime Violence

To make the discussion more concrete, we highlight 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, 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 the country remains one of the poorest 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 very rapid growth rates and high levels of foreign direct investment (Casey, Glennerster, and Miguel 2015).

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?", and "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 non-victims: 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 only 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 lab experimental evidence complements observational survey evidence by helping to map out changes to economic primitives, 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, decision-makers 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 recipients' welfare 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. Specifically, 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 she 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, i.e., beliefs about whether other players will cooperate, while back transfers made by the second player provide a measure of reciprocity.

Finally, 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 increases since the return to other players is substantial. This game thus reveals individual willingness to cooperate or to free ride (i.e., 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, or not.[2]

Bauer et al. (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 non-victims, 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 as compared to non-victims, 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 et al. (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

[2] In 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, in Ethiopia, Rustagi, Engel, and Kosfeld (2010) show that communities 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' (e.g., 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.

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 foul card during the game, suggesting that a violent conflict not only elevated in-group prosocial behavior but may also have exacerbated out-group antagonism.[3]

Notice that 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 pre-war sample, tracks survivors, and attempts to account for non-survivors, reducing concerns about bias due to selective attrition. An average of five years after temporary conscription into the

[3] While 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.

LRA, the experience led to substantial increases in post-war 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 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.

Beyond Africa, Bauer et al. (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 an in-group versus out-group gap: 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.

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 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 conducted before, during and after the Israel-Hezbollah conflict of 2006 indicated that living in a society with an active ongoing conflict temporarily increased the willingness of senior citizens to punish non-cooperators and to reward cooperation (Gneezy and Fessler 2012). An aspect of this study is that it relied on a pre-post research design 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 (e.g., Islamist groups, ethnic nationalists, and pro-democratic reformers). What makes this civil war interesting is the complex network of rivalries that emerged within local communities during the fighting, often resulting in neighbors fighting neighbors (intra-group conflict). This contrasts with the above mentioned studies, in which violence was typically perpetrated by people from outside of the affected communities (inter-group 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 inter-mixed 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), 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, south-eastern Europe, the former Soviet Union, and Mongolia in 2010. Nearly forty thousand individuals answered questions on 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 Kyrgyzstan clashes in 2010. The incidence of WWII exposure was very high: the average proportion of respondents who reported that they or their parents/grandparents were injured or killed was nearly 30% overall. Grosjean then focused on within-country variation in exposure to war violence. The results show a positive link between past violent conflict experiences and contemporary participation in community groups, and collective action and membership in political parties, while there is also a negative effect on trust in central government institutions.[4]

[4] Some 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 non-cognitive skills (Heckman 2006; Almås et al. 2010; Bauer, Chytilová, and Pertold-Gebicka 2014; Kosse et al. 2014). Specifically, in Sierra Leone Bauer et al. (2014) find the strongest effects on social preferences among those who were children or adolescents during the civil war, and similarly in Uganda Bauer, Fiala, and Levely (2014) show that effects are driven mainly by those who soldiered during childhood or early adolescence.

Disentangling Correlation and Causation

The 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 could be more likely to participate in collective action, including civil defense forces or armed organizations that represent their groups during wartime, and thus are 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 impact 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 pre-war characteristics and the likelihood of falling victim to violence.

Second, the researchers attempt to control for local confounders with an extensive set of pre-war 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 sub-samples for which victimization was likely to be less systematic: for example, for individuals too young (e.g., children) 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.[5] 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 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.

[5] We 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.

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 (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.[6] Each study has a different empirical strategy for identifying 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 so 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 pre-war 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.

[6] There is one small exception to this, 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 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 (e.g., 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.

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 to refer to the use of fixed or random effects in a regression model in a single study. In 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 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.[7] Below we also explicitly model the heterogeneity in effect estimates as a function of observed study factors (e.g.,

[7] The 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.

duration since war exposure), in order to better characterize the nature of context-dependence, something that random effects meta-analysis alone is unable to shed light on.

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.[8]

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 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

[8] In 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 et al (2014) paper has data from two countries, as we thus consider them as two estimates here.

trust). Taken together, there is substantial evidence of an increase in several dimensions of cooperation and prosocial behavior. 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 social group participation and community leadership, while effects are not distinguishable from zero for the other categories.

In Figure 2, we examine in-group versus out-group behavior, 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 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 not 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 declines 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 (see Table 1 for details on this variable). Figure 3, Panel A 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 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 is significant).

Second, we compare the war violence studies to the handful of studies that examine exposure to criminal violence and prosocial behavior in Panel B of Figure 3, 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 make, 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 inter-group dimension; the data do not allow us to say. But the evidence at least suggests that the "war" aspect of violence victimization is not necessarily at the root of the 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 papers' research designs, including the use of pre-war 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 pre-war 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 civilians were exposed to violence, 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 seem likely to be low, 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 post-war societies experience—such as Sierra Leone after its civil war, or many of the European cases studied in Grosjean

(2014) after WWII—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 (see Alexander 1987; Boyd et al. 2003; Darwin 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 co-evolutionary 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 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 2015).

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 co-ethnic pro-sociality, 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 already possessing 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 post-war period. In both cases, and more speculatively, war experience would harden people's parochial pro-sociality, 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 reminds them 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 (Calhoun et al. 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 where 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 post-war prosperity and security have improved relative to the pre-war (or immediate post-war) situation. Finally, if it were simply a matter of post-war 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 post-war 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. This perspective has 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 pro-so-

cially 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 cases, meanwhile, to be made for war violence changing social preferences. One is that 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. The second piece of evidence is the body of qualitative studies and case evidence from the political analysis of conflict, described above, that documents self-reported changes in preferences following war victimization. The third is the fact that several studies document a change in in-group, 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 generating such evidence provides 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, to be jointly re-analyzed. 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 do 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 (e.g., Voors et al. 2012; Cassar, Grosjean, and Whitt 2013; Bauer, Fiala, and Levely 2014). Another example comes from the lab experiments, which more often than not have shown that the pro-sociality 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 intragroup 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 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 post-war 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 also be at least some reason for optimism once the violence ends.

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Table 1: Studies of war exposure and cooperation

	Paper	Country	Conflict	Data collection	Sample	Comparable survey measures	Comparable experimental measures	Time since war exposure	Published	Data available
A. Papers eligible for the meta-analysis										
1.	Annan et al. (2011)	Uganda	Lord's Resistance Army (LRA) insurgency (1986-2006)	2005-7	Representative sample of youth, some of whom were conscripted by LRA; N=613	Groups, community, trust, voting, interest in politics	--	~7 years	✓	✓
2.	Bauer et al. (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)	2005-6	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 et al. (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)	2009-10	Household heads; N=252	Interest in politics	Dictator game, Trust game, Public goods game	3 years	✓	✓
11.	Gneezy and Fessler (2012)	Israel	Israel-Hezbollah war (2006)	2005-7	Senior citizens; N=50	--	Ultimatum game, Trust game	1 year	✓	✓

12.	Grosjean (2014)	35 countries in Europe, the Caucasus and Central Asia	WWII (1939-1945); Yugoslav wars (1991-5); Kosovo war (1998-9); Tajik civil war (1992-7); Chechen wars (1994-2009); Kyrgyzstan clashes (2010)	2010	Nationally representative samples; N=38,864	Groups, trust, voting, interest in politics	--	5 months – 65 years	✓	✓
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 (2014)	Nepal	Civil war (1996-2006)	2003	Nationally representative sample; 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 Hercegovina, USA (Vietnam veterans)	Indonesia: insurgency in Aceh (1976-2005) B&H: civil war (1992-1995) USA: Vietnam war (1955-1975)	Indonesia: 2007 Bosnia: 2006 USA: 1986	Indonesia: N=1,752 Bosnia: nationally representative sample; N=3,580 USA: male Vietnam theater veterans; N=1,171	Indonesia: Groups, community, trust, voting Bosnia: Groups, voting, interest in politics USA: Groups	--	2-11 years		
C. Papers studying other forms of violence										
20.	Bateson (2012)	70 countries	Crime victimization	Americas: 2010 Africa: 2008-9 Europe: 2000 Asia: 2005-8	Latin America: 39,238 U.S. and Canada: 3,000 Africa: 27,713 Europe: 17,088 Asia: 16,725	Groups, community, voting, interest in politics	--		✓	✓
21.	Becchetti, Conzo, and	Kenya	Kenyan crisis, post-election violence (2007-8)	2010	Nairobi slum-dwellers; N=404	--	Trust game		✓	

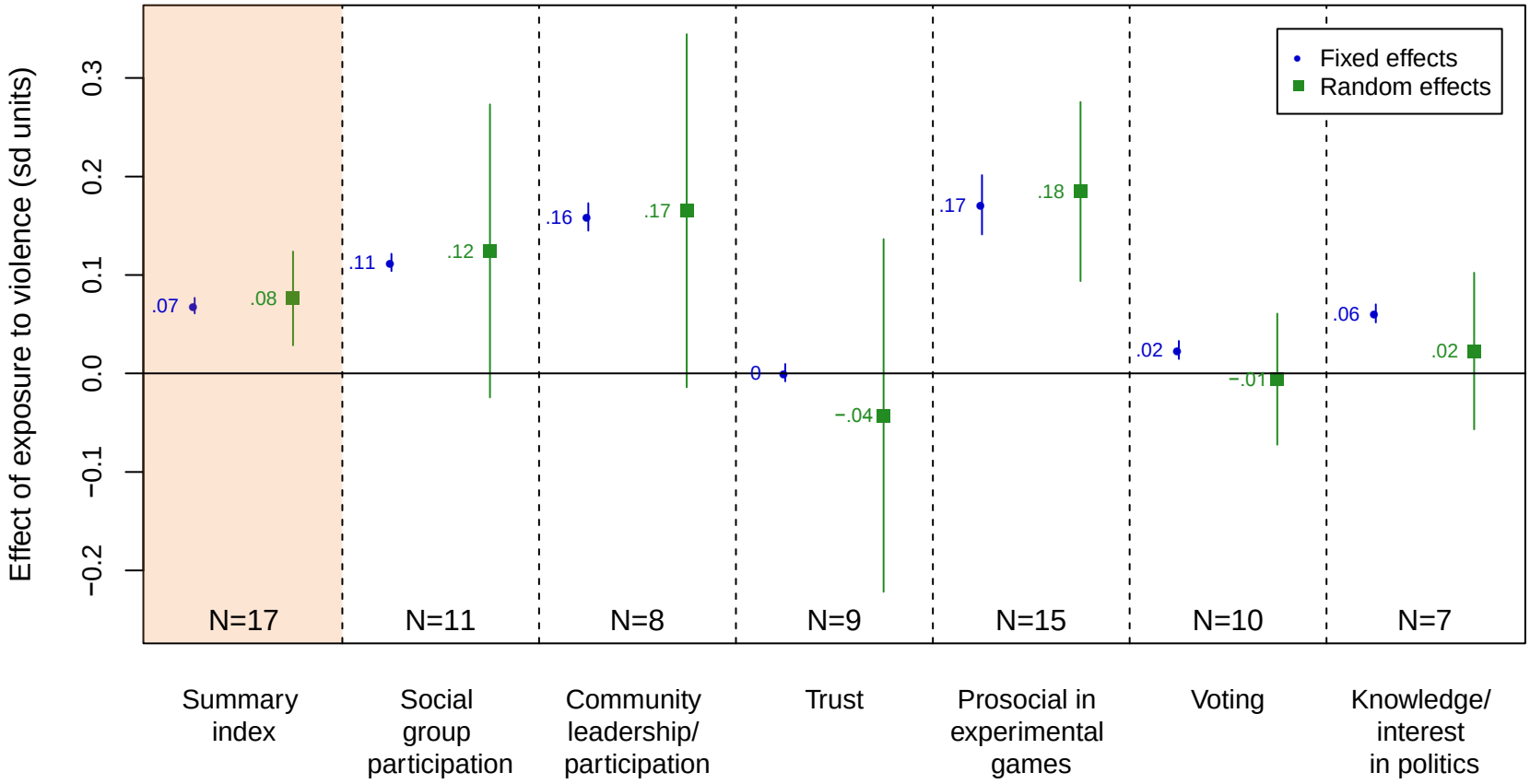
	Romeo (2014)									
22.	Hopfensitz and Miquel-Florensa (2014)	Colombia	Colombian conflict (1964+)	2012	Coffee farmers; N=260	Community	Public goods game			
23.	Rojo-Mendoza (2014)	Mexico	Crime victimization	2011	Nationally repre-sentative sample; N=7,416	Groups, interest in politics	--			

Table 2: Meta-analysis results

Outcome *(Standardized)*	Estimate	(1) Fixed Effects	(2) Random Effects
Summary index (mean effects)	Coef.	0.07***	0.08***
	Std. Err	0.00	0.02
	P-val	0.00	0.00
Social groups participation	Coef.	0.11***	0.12
	Std. Err	0.00	0.08
	P-val	0.00	0.10
Community leadership/participation	Coef.	0.16***	0.17*
	Std. Err	0.01	0.09
	P-val	0.00	0.07
Trust	Coef.	0.00	-0.04
	Std. Err	0.00	0.09
	P-val	0.87	0.64
Prosocial behavior in experimental games	Coef.	0.17***	0.18***
	Std. Err	0.02	0.05
	P-val	0.00	0.00
Voting	Coef.	0.02***	-0.01
	Std. Err	0.00	0.03
	P-val	0.00	0.86
Knowledge/interest in politics	Coef.	0.06***	0.02
	Std. Err	0.00	0.04
	P-val	0.00	0.57

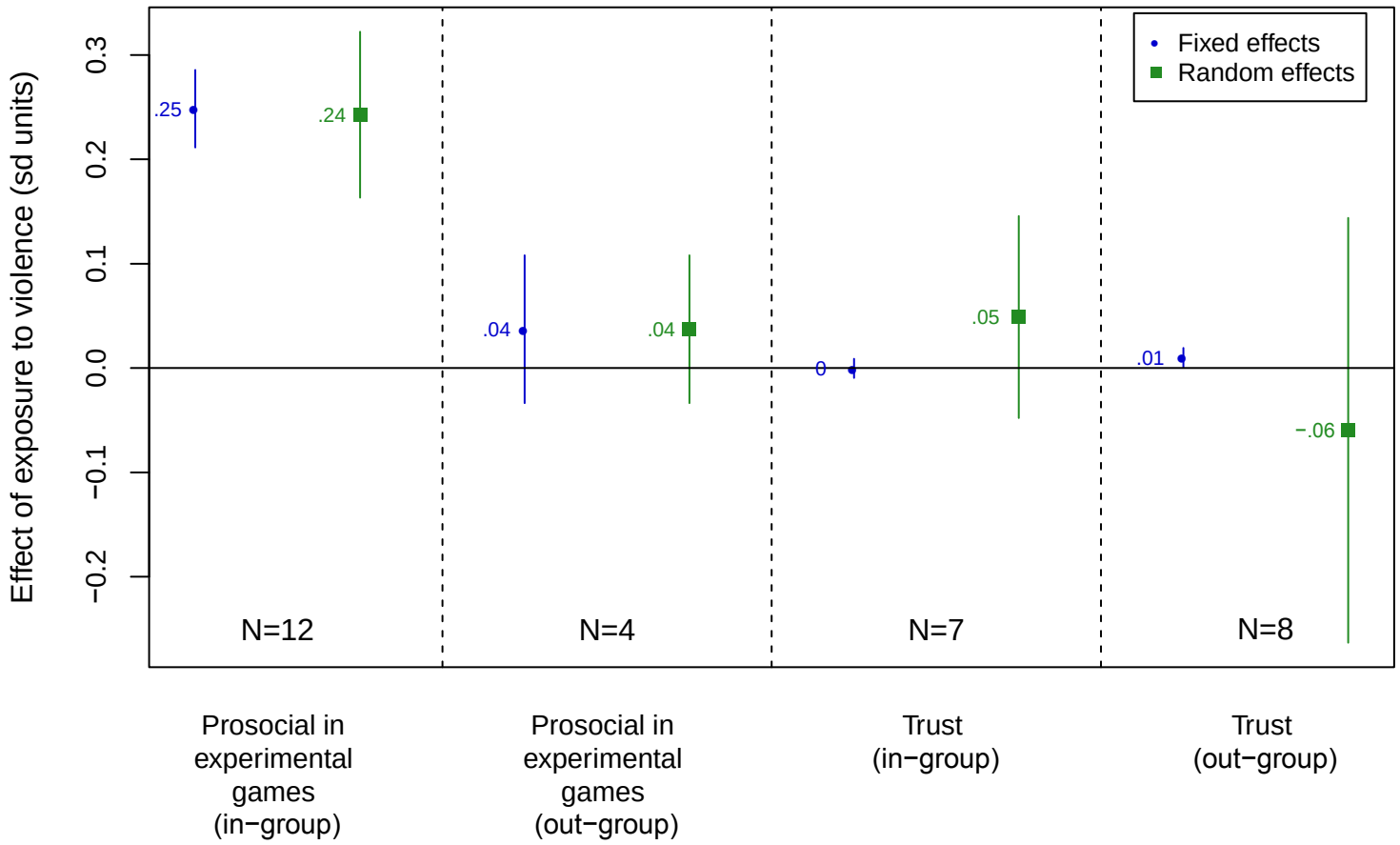
Note: * p<0.10, ** p<0.05,*** p<0.01. Meta-analysis results for each outcome reported in the rows. Column (1) reports results from a fixed-effects model; Column (2) reports results from a random-effects model. The coefficient represents the estimated population effects of exposure to violence across studies, measured in standard deviation units. This analysis excludes exposure to crime violence.

Figure 1: Meta-analysis results, war exposure and cooperation



Note: 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 (blue) and random-effects (green) meta-analysis models. Results are reported in standard deviation units. The vertical lines denote 95% confidence intervals. N denotes the number of studies/games included in the meta-analysis for each outcome.

Figure 2: Meta-analysis results, in-group versus out-group effects



Note: The figure plots the meta-analysis results, broken down by in-group and out-group. The effect of exposure to violence on each outcome is estimated using fixed-effects (blue) and random-effects (green) meta-analysis models. Results are reported in standard deviation units. The vertical lines denote 95% confidence intervals. N denotes the number of studies/games included in the meta-analysis for each outcome. A meta-regression test for the difference between 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 between in-group and out-group members.

Supplementary Appendix:

Can War Foster Cooperation?

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Contents

A Literature search

We sought all papers that: (i) have one or more outcomes that relate to social and political participation, cooperation, or trust; (ii) use a measure of exposure to violence as opposed to other experiences, such as displacement or crime victimization. We include published and unpublished papers.

A.1 Inclusion and exclusion criteria

We exclude two papers from our analysis that do not include one of the main dependent variables of interest (e.g. they look at trust in government not in fellow citizens), and four papers that do not use war violence as an independent variable (but rather crime, electoral violence, or displacement). Perhaps more importantly, we exclude one paper that does use the dependent and independent variables of interest but for which the micro data are not available. A major reason is that the papers in our analysis vary widely in the measurement and scale of the dependent and independent variables, and in order to make the meta-analysis meaningful, we need access to the raw data of each paper to create standardized measures.

In an alternative approach, we use information on t-statistics reported in the papers (Stanley and Jarrell, 1989). This approach does not require raw replication data. For this analysis we include all papers that have independent variables related to wartime violence exposure and have comparable outcome measures on cooperation. There are two drawbacks to this method. First, a t-statistic combines information on sample size as well as magnitude; hence, a large t-statistic may not necessarily reflect a large effect size. Second, using reported coefficients does not allow investigating relevant dependent variables that the paper does not report. Nonetheless, we consider the results of this alternate approach below.

B Methods

We use the original data to construct standardized coefficients, as well as to estimate effects for additional outcomes not reported in the original papers but for which data are available. We replicate the studies' original research designs. This is important, since each study has a different empirical strategy for identifying the impact of violence exposure. Research on meta-analysis of multivariate regression slopes emphasizes the importance of having each study capture the true effect of the independent variable, which depends on model specification (Becker and Wu, 2007). Thus, in replicating the models of each study, we assume that the authors of the papers in our sample have made the best efforts to identify the effect of violence on prosocial behavior.

In this approach, we first regress each standardized outcome variable on a binary measure of violence exposure. We use survey weights and/or control variables as specified in the replication file for each study. After calculating the effect of exposure to violence for each study, we create a matrix for each outcome in which each row represents a study. We preserve the regression coefficient,

standard error, and the number of observations.

In addition, since all of the papers in our study use multivariate regressions, we follow the recommendations of existing literature on meta-analysis of multivariate regression coefficients (Becker and Wu, 2007; Patall and Cooper, 2008), and also examine our results using standardized ("beta") slopes. In order to preserve similarity in the estimations across studies, we use ordinary least squares to estimate the impact of exposure to violence for each study. Hence, for studies using probit or logit estimations we changed the model to an OLS.

In order to overcome the multiple comparisons problem, we also generate a summary index of all outcome measures. For each study, we generate a mean effects index (Kling and Liebman, 2004), calculated from the standardized outcome measures of each study.

B.1 Meta-analysis models

We estimate the results using fixed effects and random effects meta-analysis models. For each prosocial outcome, we have k studies reporting estimates for the effect of violence exposure. Meta analysis models assume that each estimate corresponds to a true latent effect size, measured with some error:

$$y_i = \theta_i + \varepsilon_i \tag{1}$$

In the equation above, y_i represents the estimate for study i, θ_i is the (unknown) true effect, and ε_i is a sampling error, assumed to be distributed normally with mean 0 and variance v_i. The sampling variance is calculated as:

$$v_i = \frac{\left(1 - y_i^2\right)^2}{N_i - 1} \tag{2}$$

Where y_i is the estimate for study i, and N_i is the sample size of the ith study.

B.1.1 Fixed effects

The fixed-effects model makes a conditional inference only for the k studies in our sample. It use weighted least squares to estimate the true average effect:

$$\bar{\theta}_w = \frac{\sum_{i=1}^{k} w_i \theta_i}{\sum_{i=1}^{k} w_i} \tag{3}$$

In the equation above, $\bar{\theta}_w$ represents the weighted average of the true effects (θ_i), where the weight is inverse-proportional to the sampling error: $w_i = \frac{1}{v_i}$. In other words, the model gives more weight to studies with smaller sampling variance.

B.1.2 Random effects

The random effects model allows the true effect to vary between studies, and treats this heterogeneity as random. If θ_i represents true effect for study i, then the model assumes that:

$$\theta_i = \mu + u_i \tag{4}$$

where μ is the true *average* effect, and u_i is a normally distributed error with mean 0 and variance τ^2. As such, the random effects model estimates the average population effect by taking into account an additional source of variation between studies:

$$y_i = \theta_i + u_i + \varepsilon_i \tag{5}$$

Similar to the fixed effects model, the random effects model gives more weight to studies with more observations. However, the random effects model weighs studies a bit differently, by drawing on both within-study and between-study variation. It should be noted that fixed effects and random effects models would yield similar results if the variance of u_i is equal to zero, which means that the true effect is homogeneous across studies (Viechtbauer et al., 2010).

C Data

C.1 Independent variable

Using the raw data from each paper, we construct three sets of measures of violence exposure:

1. A measure of the paper's violence exposure indicator;
2. Indicators of the respondent's direct or personal exposure to violence; and
3. Indicators of direct or indirect exposure to violence, 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.

Table A1 reports summary statistics of these various measures of violence exposure.

C.2 Dependent variables

We construct six standardized outcome variables for the studies in our sample. These outcomes include social groups participation, community leadership/participation, trust, prosocial behavior in experimental games, voting, and knowledge/interest in politics. Tables A2-A7 provide details on the construction of these outcome measures for each study in our sample.

Table A1: Summary statistics of alternate coding of violence exposure

Author	Country	Violence type	Mean	Min	Max	N
Annan et al. (2011)	Uganda	Paper's indicator	0.37	0	1	619
		Community exposure	0.39	0	1	619
		Personal exposure	0.28	0	1	619
Bauer et al. (2014)	Georgia	Paper's indicator	0.24	0	1	565
		Community exposure	0.07	0	1	518
		Personal exposure	0.28	0	1	549
Bauer et al. (2014)	Sierra Leone	Paper's indicator	0.22	0	1	585
		Community exposure	0.32	0	1	584
		Personal exposure	0.35	0	1	586
Bauer, Fiala and Levely (2014)	Uganda	Paper's indicator	0.55	0	1	337
		Community exposure	0.79	0	1	337
		Personal exposure	0.58	0	1	337
Bellows and Miguel (2009)	Sierra Leone	Paper's indicator	0.39	0	1	10496
		Community exposure	0.39	0	1	10496
Blattman (2009)	Uganda	Paper's indicator	0.62	0	1	741
		Community exposure	0.42	0	1	739
		Personal exposure	0.49	0	1	738
Cassar, Grosjean and Whitt (2013)	Tajikistan	Paper's indicator	0.16	0	1	420
		Community exposure	0.19	0	1	420
		Personal exposure	0.16	0	1	420
Cecchi, Leuveld and Voors (2016)	Sierra Leone	Paper's indicator	0.90	0	1	324
		Personal exposure	0.90	0	1	324
De Luca and Verpoorten (2015*a*)	Uganda	Paper's indicator	0.29	0	1	4607
		Community exposure	0.29	0	1	4607
De Luca and Verpoorten (2015*b*)	Uganda	Paper's indicator	0.29	0	1	4607
		Community exposure	0.29	0	1	4607
Gilligan, Pasquale and Samii (2014)	Nepal	Paper's indicator	0.47	0	1	252
		Community exposure	0.47	0	1	252
Gneezy and Fessler (2012)	Israel	Paper's indicator	0.40	0	1	50
		Community exposure	0.40	0	1	50
Grosjean (2014)	European countries	Paper's indicator	0.28	0	1	35674
		Community exposure	0.28	0	1	35674
Grossman, Manekin and Miodownik (2015)	Israel	Paper's indicator	0.42	0	1	2334
		Personal exposure	0.17	0	1	2200
Rohner, Thoenig and Zilibotti (2013)	Uganda	Paper's indicator	0.67	0	1	2431
		Community exposure	0.54	0	1	2431
Voors et al. (2012)	Burundi	Paper's indicator	0.71	0	1	286
		Community exposure	0.71	0	1	286
Voors and Bulte (2014)	Burundi	Paper's indicator	0.28	0	1	872
		Community exposure	0.08	0	1	872

Table A2: Social groups participation

Paper	Country	Mean	SD	Min	Max	N
Annan et al. (2011)	Uganda	1.06	1.3	0	7	619
Bauer et al. (2014)	Georgia	0.49	0.5	0	1	422
Bauer et al. (2014)	Sierra Leone	3.15	1.56	0	8	586
Bauer, Fiala and Levely (2014)	Uganda	1.26	1.14	0	5	337
Bellows and Miguel (2009)	Sierra Leone	2.35	1.78	0	7	6686
Blattman (2009)	Uganda	0.75	1.01	0	6	741
Cassar, Grosjean and Whitt (2013)	Tajikistan	0.64	0.97	0	5	296
De Luca and Verpoorten (2015*a*)	Uganda	0.64	0.96	0	3	4640
Grosjean (2014)	European countries	0.6	1.2	0	8	38860
Voors et al. (2012)	Burundi	0.2	0.4	0	1	285
Voors and Bulte (2014)	Burundi	0.18	0.39	0	1	854

Table A3: Community leadership/participation

Paper	Country	Participation in community meetings	Helping/ volunteering in the community	Holding community leadership position	Mean	SD	Min	Max	N
Annan et al. (2011)	Uganda		✓	✓	0.06	0.23	0	1	619
Bauer et al. (2014)	Sierra Leone	✓	✓		0.97	0.17	0	1	572
Bauer et al. (2014)	Uganda		✓	✓	0.11	0.31	0	1	337
Bellows and Miguel (2009)	Sierra Leone	✓		✓	0.7	0.46	0	1	10496
Blattman (2009)	Uganda		✓	✓	0.09	0.29	0	1	741
Cassar, Grosjean and Whitt (2013)	Tajikistan	✓	✓		0.45	0.5	0	1	396
De Luca and Verpoorten (2015*b*)	Uganda	✓			0.76	0.42	0	1	4619
Voors et al. (2012)	Burundi		✓		0.2	0.4	0	1	283

Table A4: Trust

Paper	Country	Trust variables	In-group	Out-group	Mean[1]	SD	N
Annan et al. (2011)	Uganda	Considers as brothers and sister (a) neighbors,(b) members of one's tribe, (c) people from northern ethnic groups, (d) people from southern and central ethnic groups	(a) neighbors, (b) members of one's tribe	(c) people from northern ethnic groups, (d) people from southern and central ethnic groups	-0.01	1.00	617
Bauer et al. (2014)	Sierra Leone	Trust people from (a) family, (b) neighborhood, (c) friends, (d) another religion, (e) another ethnicity, (f) people in general	(a) family, (b) neighborhood, (c) friends	(d) another religion, (e) another ethnicity, (f) people in general	0.00	1.00	585
Bauer, Fiala and Levely (2014)	Uganda	Trust people (a) in the village, (b) in the sub-county	(a) village	(b) sub-county	-0.00	1.00	335
Bellows and Miguel (2009)	Sierra Leone	Trust (a) people from the community, (b) outsiders	(a) people from the community	(b) outsiders	0.00	1.00	9605
Cassar, Grosjean and Whitt (2013)	Tajikistan	Trust people from (a) family, (b) neighborhood, (c) other religion, (d) other nationality	(a) family, (b) neighborhood	(c) other religion, (d) other nationality	-0.00	1.00	421
De Luca and Verpoorten (2015*a*)	Uganda	Trust in people in general		People in general	0.00	1.00	4595
Grosjean (2014)	European countries	Trust people from (a) family, (b) neighborhood, (c) friends, (d) another religion, (e) another nationality	(a) family, (b) neighborhood, (c) friends	(d) another religion, (e) another nationality	0.00	1.00	33800
Rohner, Thoenig and Zilibotti (2013)	Uganda	Trust in (a) relatives, (b) people in general	(a) relatives	(b) people in general	-0.00	1.00	2423
Voors and Bulte (2014)	Burundi	Trust (a) people from community, (b) people in general	(a) people from community	(b) people in general	0.00	1.00	860

Note: [1]The mean is calculated from a standardized measure of all trust variables for each study.

Table A5: Prosocial behavior in experimental games

Paper	Country	Game	In-group	Out-group	N
Bauer et al. (2014)	Georgia	Sharing, Envy	Classmates	Subjects from a distant school	565
Bauer et al. (2014)	Sierra Leone	Sharing, Envy	Same village	Distant village	581
Bauer, Fiala and Levely (2014)	Uganda	Trust (returned)	Nearby village		337
Cassar, Grosjean and Whitt (2013)	Tajikistan	Trust		Distant village	426
Cecchi, Leuveld and Voors (2016)	Sierra Leone	Dictator	Soccer teammates		324
Gilligan, Pasquale and Samii (2014)	Nepal	Public goods, dictator, trust	Same village		252
Gneezy and Fessler (2012)	Israel	Trust, Ultimatum	Same senior housing facility		50
Voors et al. (2012)	Burundi	Social Value Orientation	Same community		285

Note: Some studies have multiple games. Descriptive statistics for each game are not shown.

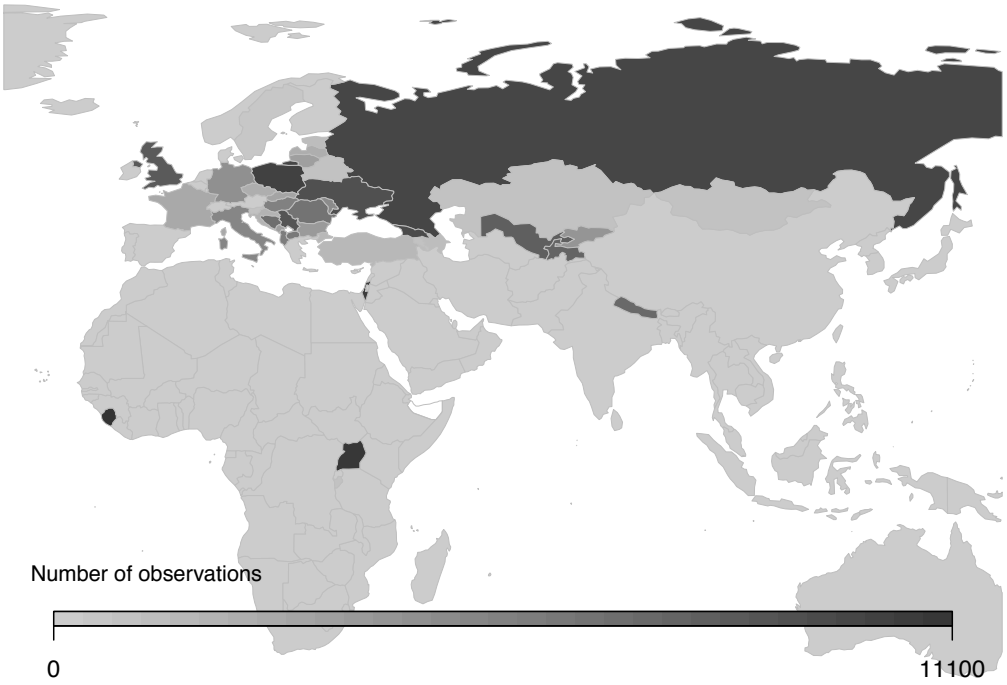
Table A6: Voting

Author	Country	Elections	Mean	SD	Min	Max	N
Annan et al. (2011)	Uganda	Voted in the 2006 Presidential elections; voted in the 2005 referendum	1.12	0.91	0.00	2.00	534
Bauer et al. (2014)	Sierra Leone	Voted in the last presidential general election; voted in the last local government election	1.90	0.38	0.00	2.00	585
Bauer, Fiala and Levely (2014)	Uganda	Voted in the recent election (2011)	0.85	0.35	0.00	1.00	299
Bellows and Miguel (2009)	Sierra Leone	Registered to vote for the presidential and general elections of 2002; registered to vote for the local government elections of 2004; planning on voting in the upcoming presidential election	0.94	0.23	0.00	1.00	10494
Blattman (2009)	Uganda	Voted in the 2001 presidential elections; voted in the 2005 referendum	0.99	0.83	0.00	2.00	473
Cassar, Grosjean and Whitt (2013)	Tajikistan	Voted in the past parliamentary elections; voted in the past presidential elections, voted in the past local elections	2.29	1.03	0.00	3.00	416
De Luca and Verpoorten (2015*b*)	Uganda	Voted in the 1996, 2006, and 2011 presidential elections	0.81	0.39	0.00	1.00	4642
Grosjean (2014)	European countries	Voted in the most recent local-level elections; voted in the most recent parliamentary elections; voted in the most recent presidential elections	2.02	1.23	0.00	3.00	29813
Grossman, Manekin and Miodownik (2015)	Israel	Voted in the 2013 elections	0.95	0.23	0.00	1.00	2334
Voors et al. (2012)	Burundi	Voted in the last general elections; voted in the last municipal elections; voted in the last referendum	2.84	0.54	0.00	3.00	285

Table A7: Knowledge/interest in politics

Author	Country	Variables	Mean	SD	Min	Max	N
Annan et al. (2011)	Uganda	Knows the name of her current LC3; knows the name of her current LC5	0.99	0.76	0.00	2.00	616
Bauer et al. (2014)	Sierra Leone	Is able to name the Local Councilor from ward; is able to name the Paramount Chief for chiefdom	1.37	0.72	0.00	2.00	586
Bellows and Miguel (2009)	Sierra Leone	Is able to name the Paramount Chief for chiefdom; is able to name the Local Councilor who represents him/her in the council; knows the date of the next presidential election	1.58	0.94	0.00	3.00	5193
De Luca and Verpoorten (2015*b*)	Uganda	Frequency of discussing politics with friends, family, or neighbors	1.03	0.71	0.00	2.00	4628
Gilligan, Pasquale and Samii (2014)	Nepal	Most of the time understands what politicians are doing	0.23	0.42	0.00	1.00	231
Grosjean (2014)	European countries	Member of a political party	0.06	0.23	0.00	1.00	38447
Grossman, Manekin and Miodownik (2015)	Israel	Interested in politics; member of a political party; member of a group that advocates social and political issues	0.71	0.80	0.00	3.00	2315

Figure A1: Exposure to wartime violence across the world



Note: The map reports the countries included in the analysis (excluding crime violence). The shading corresponds to the number of observations, such that darker colors represent larger samples of individuals.

Table A8: Additional studies not included in the meta-analysis

Paper	Country	Published	Data available	Comparable measures	N	Reason for exclusion
Beber, Roessler and Scacco (2014)	Sudan			Community, interest in politics	1,380	Data collected but paper not yet written.
Blattman & Hartman	Liberia		✓	Groups, community, trust, voting, interest/knowledge of politics	9,388	Data collected but paper not yet written.
Gilligan, Pasquale and Samii (2014)	Nepal			Trust, voting, interest in politics	1,228	Data collected but paper not yet written.

D Heterogeneity in the Effect Size Across Studies

This section analyzes how various study-level covariates moderate the effects of exposure to violence across studies. First, we analyze whether effects vary with studies' empirical strategy. Table A9 reports the identification strategy of each study, broken into several categories. Tables A10 through A13 provide more details on the method of each paper. It can be seen, for example, that almost all studies use multivariate regressions; about half control for local fixed effects; and about a third add "substantive" controls that might drive victimization. Table A14 shows the correlation between study-level empirical strategy variables.

In Table A15 we report results from a meta analysis including these study-level moderators, where the dependent variable is an index of all cooperation outcomes, using both fixed effects and random effects specifications. It can be seen that the addition of substantive controls is not significantly associated with the magnitude of the coefficient across studies. The inclusion of community fixed effects correlates with smaller coefficients, on average, but the relationship is not statistically significant at conventional levels. Studies that control for pre-war covariates tend to report larger effects, and this relationship is statistically significant in the fixed effects model. Further, the use of various sensitivity analyses is significantly associated with smaller effect sizes in the fixed effects estimation. Finally, effect size is negatively correlated with the use of instrumental variables, but the relationship is not statistically significant.

Figure A3 shows a meta analytic scatterplot of the observed effects estimated for individual studies against a continuous scale ranging from 0 to 1, capturing the strength of the causal identification. The scale is constructed from the average of the variables: *Substantive controls*, *FE design*, *Pre-war data* and *Sensitivity*. In the scale, 0 indicates little attempts to measure causal relationships and 1 indicates the use of more tools to identify a causal effect. Overall, it can be seen that studies' empirical strategy does not account for much of the variation in the effects across studies.

Second, we analyze whether the heterogeneity in the effect of violence exposure can be explained by other study-level covariates. In Table A16, we examine the moderating effect of the level of violence exposure captured in each study (personal/household level or community/district level); the length of time between the end of the conflict and the timing of each study; the type of victims (civilians or combatants); and the type of violence (war or crime). We also examine regional variation in the results. It can be seen that the effect decreases in studies measuring violence exposure on the individual level, as opposed to more aggregate levels. In addition, the effect is larger for studies that measure pro-social behaviors later in time. We also find that the effect is larger for studies in which civilians were exposed to violence, as opposed to combatants, and for studies that use crime as the measure of violence exposure.

Table A9: Empirical Strategy

Paper	Country	Reg. with controls	Reg. with controls and community FE	Substantive controls	IV	Sensitivity analysis	Pre-war data
Annan et al. (2011)	Uganda	✓	✓	✓		✓	✓
Bauer et al. (2014)	Georgia	✓	✓			✓	
Bauer et al. (2014)	Sierra Leone	✓	✓				
Bauer et al. (2014)	Uganda	✓	✓			✓	✓
Bellows and Miguel (2006, 2009)	Sierra Leone	✓	✓	✓		✓	✓
Blattman (2009)	Uganda	✓	✓	✓		✓	✓
Cassar et al. (2013)	Tajikistan	✓	✓			✓	✓
Cecchi et al. (2015)	Sierra Leone	✓				✓	
De Luca and Verpoorten (2015a)	Uganda	✓			✓	✓	✓
De Luca and Verpoorten (2015b)	Uganda	✓			✓	✓	✓
Gilligan et al. (2014)	Nepal	✓					✓
Gneezy and Fessler (2012)	Israel						
Grosjean (2014)	European countries	✓	✓	✓			
Grossman et al. (2015)	Israel	✓			✓	✓	
Rohner et al. (2013)	Uganda	✓			✓	✓	✓
Voors et al. (2012)	Burundi	✓	✓	✓	✓	✓	✓
Voors and Bulte (2014)	Burundi	✓	✓	✓	✓	✓	✓

Table A10: Empirical Strategy

Paper	Country	Method	Author's self-described identification method	Controls	Pre-war data?	Substantive controls?	Type of violence exposure	Unit of analysis of DV	Unit of analysis of IV	Selection/ survival/ attrition bias	Author's control for bias?
Annan et al. (2011)	Uganda	OLS with controls, sub-county fixed effects.	A regression of the outcome on an abduction indicator, controlling for pre-abduction covariates and sub-county fixed effects. Regressions are weighted by the inverse of an estimate of the propensity scores of attrition and abduction. Abduction was plausibly as-if random.	age, parents' education, parents' died before the war, HH size before the war, HH covariates	Yes, retrospective	Yes	Personal, on family	individual	individual	Yes	The study's estimates are weighted by inverse sampling probabilities and inverse attrition probabilities.
Bauer et al. (2014)	Georgia	OLS with controls	A regression of the outcome on a violence exposure indicator, controlling for demographic variables. Violence exposure is plausibly exogenous because Russian soldiers could not have selected victims since fighting involved aerial, artillery, and tank fire bombings, was short, and soldiers did not have prior knowledge of the local population.	age, gender, brother, sister	No	No	Personal, on family	individual	individual	Yes	To control for measurement bias in children's reporting of victimization, the study correlates victimization measures reported by children to those reported by their teachers. To account for selection bias, the study analyzes rural and urban subsamples, a subsample of children who come from areas not known to fighters, and a model with region fixed effects, finding similar results.
Bauer et al. (2014)	Sierra Leone	OLS with controls	A regression of the outcome on a violence exposure indicator, controlling for demographic variables. The authors preselected villages for the study based on evidence from Bellows & Miguel (2009) indicating substantial variation in war exposure.	age, gender, brothers, sisters, education, religion, ethnicity	No	No	Personal, on family	individual	individual	Yes	The study notes that households of community leaders may have been selectively targeted; In an analysis of the characteristics predicting violence exposure, it finds few significant variables.
Bauer, Fiala, and Levely (2014)	Uganda	OLS with controls	A regression of the outcome on an indicator of abduction to the LRA, controlling for individual characteristics. Abduction was plausibly as-if random.	age, family size, parents' education, parents alive before the war, income, HH size, married, literacy, education, wealth, experimental counterpart is male	Yes, retrospective	Yes	Personal, on family	individual	individual	Yes	The study shows that the the results hold when including village fixed effects, as well as when dividing the data into subsamples. The study also conducts a sensitivity analysis to examine selective survival, and finds some evidence that non-cooperative individuals were less likely to survive.
Bellows and Miguel (2006, 2009)	Sierra Leone	OLS with controls, enumeration area fixed effects.	A regression of the outcome variable on a violence exposure variable, controlling for individual characteristics, with enumeration area fixed effects. The attacks on villages were indiscriminate.	age, gender, education, traditional leader; prewar:* HH head education , HH head traditional leader, HH head community leader	Yes, retrospective	Yes	On family	individual	individual	Yes	The study controls for a set of post-war and pre-war characteristics that can predict postwar political and socioeconomic outcomes. To minimize selection, it analyzes subsamples that were less likely to be targeted, such as individuals who were too young to be prewar community leaders. To rule out attrition due to migration, the study analyzes a subsample of individuals in the same chiefdom before and after the war, and finds similar results.

Table A11: Empirical Strategy (Cont.)

Paper	Country	Method	Author's self-described identification method	Controls	Pre-war data?	Substantive controls?	Type of violence exposure	Unit of analysis of DV	Unit of analysis of IV	Selection/ survival/ attrition bias	Author's control for bias?
Blattman (2009)	Uganda	OLS with controls, sub-county fixed effects	A regression of the outcome on an abduction indicator, controlling for pre-abduction covariates and sub-county fixed effects. Regressions are weighted by the inverse of an estimate of the propensity scores of attrition and abduction. Abduction was plausibly as-if random.	age, parents' education, parents' died before the war, HH size before the war, HH covariates	Yes, retrospective	Yes	Personal, on family	individual	individual	Yes	The study's estimates are weighted by inverse sampling probabilities and inverse attrition probabilities.
Cassar, Grosjean, and Whitt (2013)	Tajikistan	OLS with controls, village fixed effects	A regression on the outcome on a violence exposure variable, controlling for individual and household characteristics, and village fixed effects.	age, gender, HH member in communist party, education, ethnicity region lived during the conflict	Partial, retrospective	Yes	Personal, on family	individual	individual	Yes	To account for selection, the study analyzes a subsample of individuals who were too young to be systematically targeted, as well as individuals who never migrated out of their villages.
Cecchi et al. (2015)	Sierra Leone	OLS with controls	A regression of the outcome on a violence exposure variable, controlling for individual characteristics. The key identifying assumption is that exposure to violence was exogenous with respect to individual characteristics.	age, education, meals per day, religion, ethnicity, played whole game, self-declared football skills, scored, left footed, won football game	No	No	Personal, on family	individual	individual	Yes	The study restricts its conclusions to individuals in one locality with varying degrees of violence exposure, and acknowledges that its conclusions may not generalize, because of selective migration.
De Luca and Verpoorten (2015a)	Uganda	Diff-in-diff with district fixed effects	A difference-in-differences estimation, where the treatment is the logged number of violent events in a district taking place between the implementation of the 2000 (pre-conflict) and 2005 (ongoing conflict), and the 2012 (end of the conflict) Afrobarometer survey rounds. The model compares changes in social capital between survey rounds across districts with low levels of violence and districts with high levels of violence.	age, gender, urban/rural, ethnicity, education, district fixed effects, and district-level characteristics interacted with the post-conflict year, including violence perpetrated by other groups during the time span considered and ethnic fractionalization	Yes, prospective	Yes	District-level violent events	individual	District	Yes	The study uses Afrobarometer survey data, which was not collected in highly insecure areas during the conflict; highly insecure enumeration areas were replaced by more safe areas within the same district. The study also examines the impact of selective migration across survey rounds on the results and finds little evidence that the results are driven by migration.
De Luca and Verpoorten (2015b)	Uganda	Diff-in-diff with district fixed effects	A difference-in-differences estimation, where the treatment is the logged number of violent events in a district taking place between the implementation of the 2000 (pre-conflict) and 2005 (ongoing conflict), and the 2008 and 2012 (end of the conflict) Afrobarometer survey rounds. The model compares changes in social capital between survey rounds across districts with low levels of violence and districts with high levels of violence.	age, gender, urban/rural, ethnicity, education, district fixed effects, and district-level characteristics interacted with the post-conflict year, including violence perpetrated by other groups during the time span considered and ethnic fractionalization	Yes, prospective	Yes	District-level violent events	individual	District	Yes	The study uses Afrobarometer survey data, which was not collected in highly insecure areas during the conflict; highly insecure enumeration areas were replaced by more safe areas within the same district. It also examines the validity of the difference-in-differences model assumption and finds that districts affected by violence had similar trends in electoral turnout as districts not affected by violence. In addition, it examines the impact of selective migration and finds that it does not drive the results.

Table A12: Empirical Strategy (Cont.)

Paper	Country	Method	Author's self-described identification method	Controls	Pre-war data?	Substantive controls?	Type of violence exposure	Unit of analysis of DV	Unit of analysis of IV	Selection/ survival/ attrition bias	Author's control for bias?
Gilligan, Pasquale, and Samii (2014)	Nepal	WLS with matching and matched-pair block fixed effects.	A matching of communities with high levels of violence and those with no violence. A regression of the outcome on a violence exposure indicator on the ward level, with a matching block fixed effect. The model is fitted using weighted least squares, where the weighting accounts for differences between the sample and population distributions over the matching blocks. Standard errors are clustered at the ward level.	matching covariates at the VDC level (in 2001): Maoist control, an indicator for the first hosted armed confrontations up to 2001; an indicator of which ethnic group was the plurality in the VDC; elevation; population, unemployment, illiteracy, school absenteeism,	Yes, prospective	Yes	Village-level measures of fatalities	individual	Village Development Committee (VDC), similar to county	Yes	The study points to the unpredictable fighting pattern during the conflict, a function of both the nature of the insurgency and the country's rough terrain, as a source of exogeneity. As a result, very similar communities were arbitrarily exposed to different levels of violence.
Gneezy and Fessler (2012)	Israel	OLS	Difference in outcome means between wartime and peacetime games.	No controls	No	No	Timing: war vs. peace	individual	Time periods of violence (national)	Yes?	The study does not provide information on mitigating biases.
Grosjean (2014)	European countries	OLS with controls, country fixed effects and PSU (village, suburb) fixed effects.	A regression of the outcome on a violence exposure indicator, controlling for type of conflict (international conflict won, lost, or a civil conflict), individual and household characteristics, country and PSU fixed effects.	age, religion, employment, education, HH size, Communist party membership, ethnicity, PSU fixed effects, country fixed effects	No	No	Personal, on family	individual	individual	Yes	The study acknowledges that victimization may have not been random, but points to the fact that victimization during WWII was operating on respondents' parents and grandparents, mitigating selection concerns with respect to respondents. To account for selective migration, the study restricts the analysis to a subsample of individuals who never moved.
Grossman, Manekin, and Miodownik (2015)	Israel	IV	A a two-stage least square (2SLS) instrumental variable (IV) regression with no controls. The IV model estimates the effect of combat exposure on the outcome variable using a health profile score as a binary instrument that takes the value of 1 for combat-eligible soldiers, and 0 otherwise.	No controls	No	No	Personal	individual	individual	Yes	The study argues that its instrument for combat experience, soldiers' health ranking, is valid because it does not correlate with household income, which may be related to political attitudes; it also argues that sorting into a health raking is unlikely because of the comprehensive nature of the medical examination.

Table A13: Empirical Strategy (Cont.)

Paper	Country	Method	Author's self-described identification method	Controls	Pre-war data?	Substantive controls?	Type of violence exposure	Unit of analysis of DV	Unit of analysis of IV	Selection/ survival/ attrition bias	Author's control for bias?
Rohner, Thoenig, and Zilibotti (2013)	Uganda	OLS with controls, IV	A regression of the outcome on a violence exposure variable, controlling for pre-conflict trust level, county/ district-level, ethnic-group level, and individual-level characteristics.	Pre-war (district-level) trust; ethnic-group specific controls; individual level controls: age, education, employment, gender, rural/urban, religion, ownership of TV/radio; district level population, urbanization rate, demographic structure, share of manufacture and subsistence farming, net migration, fertility, number of micro-enterprises, and unemployment; County-level ethnic fractionalization, avg. satellite nightlight intensity	Yes, prospective	Yes	District-level violent events	individual	District/ county	Yes	The study argues that concerns of reverse causality are mitigated by the fact that the outcome is measured three years after the end of the conflict. As a robustness, it instruments violence exposure with distance to Sudan. To account for selective migration, the the study notes that by the time of the survey, most displaced individuals returned to their homes; in addition, most migration took place within counties and not across counties.
Voors et al. (2012)	Burundi	OLS with controls, stratum fixed effects	A regression of the outcome on a violence exposure variable, controlling for individual and village-level characteristics, and stratum fixed effects.	individual level: literacy, age, gender; village level: land holdings per capita, language, Gini coefficient, distance to market, conflict over land, ethnic homogeneity, socioeconomic homogeneity, population density, total expenditure per capita, 1998 HH controls, stratum fixed effects	No	Yes	Comm. level share of war related deaths	individual	village/ individual	Yes	The study points to the indiscriminate nature of the conflict to argue that violence exposure was arbitrary, which is supported by a comparison of communities targeted and not targeted by violence. An examination of migration patterns shows that selection due to migration is unlikely to drive the results.
Voors and Bulte (2014)	Burundi	OLS with controls and community fixed effects, IV	A regression of the outcome on a violence exposure variable, controlling for individual and village level characteristics, and community fixed effects.	individual level: age, gender, education, wealth, pre-war ownership of livestock or farmed cash crops. Village level: distance to an agricultural market, a Gini variable measuring land inequality	Yes, retrospective	Yes	Personal, on family	individual	individual	Yes	The study examines the likelihood that the results are driven by attrition or selection bias using instrumental variables and pre-war data, and find little evidence for bias due to attrition or selection.

Table A14: Correlation matrix

	Substantive controls	FE design	Pre-war data	Sensitivity	IV
Substantive controls	1.00	0.62	0.12	0.12	-0.03
FE design	0.62	1.00	0.03	0.10	-0.38
Pre-war data	0.12	0.03	1.00	0.38	0.12
Sensitivity	0.12	0.10	0.38	1.00	0.41
IV	-0.03	-0.38	0.12	0.41	1.00

Table A15: Sources of Heterogeneity in Meta Analysis Results: Empirical Strategy

	Dependent variable: index of cooperation outcomes					
	Fixed effects with moderators			Random effects with moderators (Mixed effects)		
	Coefficient	Std. Err.	p-value	Coefficient	Std. Err.	p-value
Intercept	0.14***	0.02	0.00	0.13*	0.07	0.06
Substantive controls	-0.03	0.02	0.21	0.00	0.07	0.97
FE design	-0.06	0.03	0.10	-0.10	0.08	0.19
Pre-war data	0.16***	0.02	0.00	0.08	0.05	0.17
Sensitivity	-0.14***	0.02	0.00	0.00	0.07	0.99
Instrumental variables	-0.02	0.02	0.31	-0.10	0.07	0.13
	Number of studies: 17					
	Total number of subjects: 60,989					

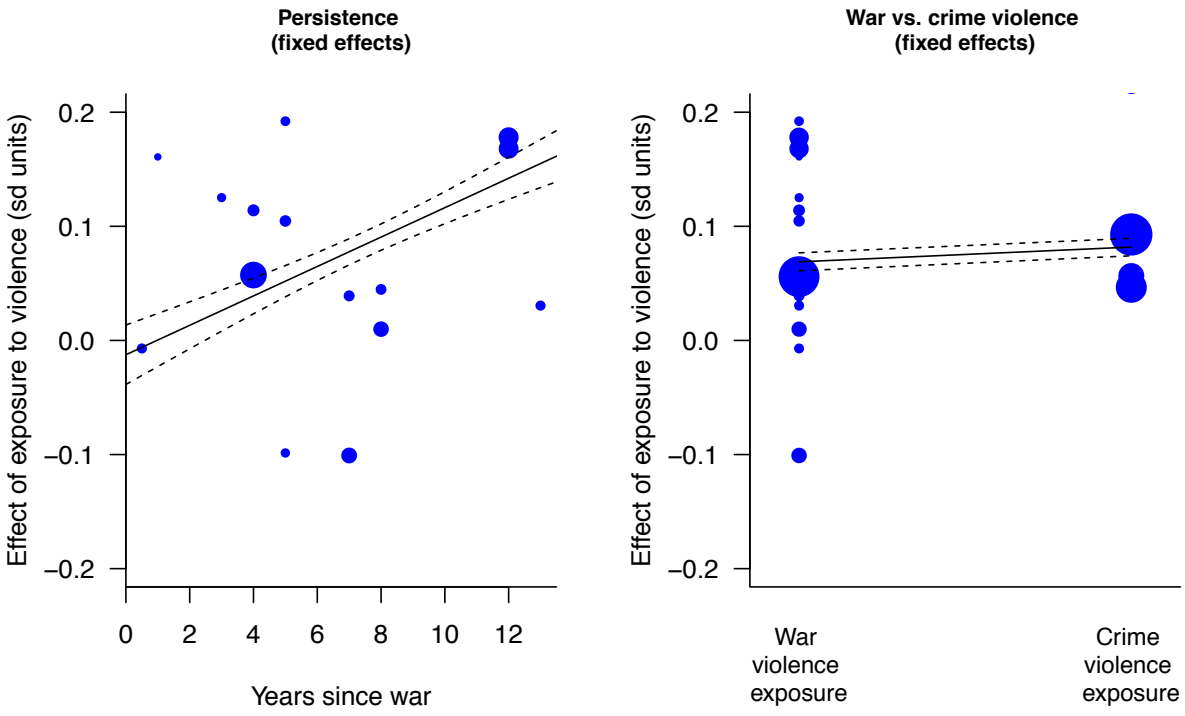
Note: *p<0.10, ** p<0.05, *** p<0.01.
The table reports meta analysis results when including study-level covariates in the models. *Substantive controls* is an indicator for studies that control for confounders that associate with risk of violence exposure; *FE design* is an indicator for studies that use community-fixed effects, comparing neighbors within the same community; *Pre-war data* indicates studies that control for pre-war covariates; *Sensitivity* is an indicator for studies that conduct various robustness tests to strengthen main results; and *Instrumental variables* indicates studies that use instrumental variables to for causal identification.
† This analysis excludes crime data.

Table A16: Sources of Heterogeneity in Meta Analysis Results: Other study-level covariates

	Dependent variable: index of cooperation outcomes					
	Fixed effects with moderators			Random effects with moderators (Mixed effects)		
	Coefficient	Std. Err.	p-value	Coefficient	Std. Err.	p-value
	Personal exposure[1] *(K=17, N=60,989)*					
Intercept	0.14***	0.01	0.00	0.12**	0.05	0.01
Personal exposure	-0.09***	0.01	0.00	-0.07	0.05	0.22
	Years since war[1,2] *(K=16, N=28,873)*					
Intercept	-0.01	0.01	0.35	0.04	0.06	0.49
Years since war	0.01***	0.00	0.00	0.01	0.01	0.45
	Violence exposure as a civilian[1] *(K=17, N=60,989)*					
Intercept	-0.02	0.02	0.29	0.05	0.05	0.31
Civilian	0.09***	0.02	0.00	0.03	0.06	0.56
	Crime vs. war violence exposure (K=21, N=125,416)					
Intercept	0.07***	0.00	0.00	0.08***	0.02	0.00
Crime	0.01**	0.01	0.02	0.03	0.05	0.58
	Region[1] *(K=17, N=60,989)*					
Intercept (Africa)	0.10***	0.01	0.00	0.10***	0.03	0.00
Asia	-0.16***	0.02	0.00	-0.08	0.06	0.19
Europe	-0.05***	0.01	0.00	-0.07	0.07	0.32

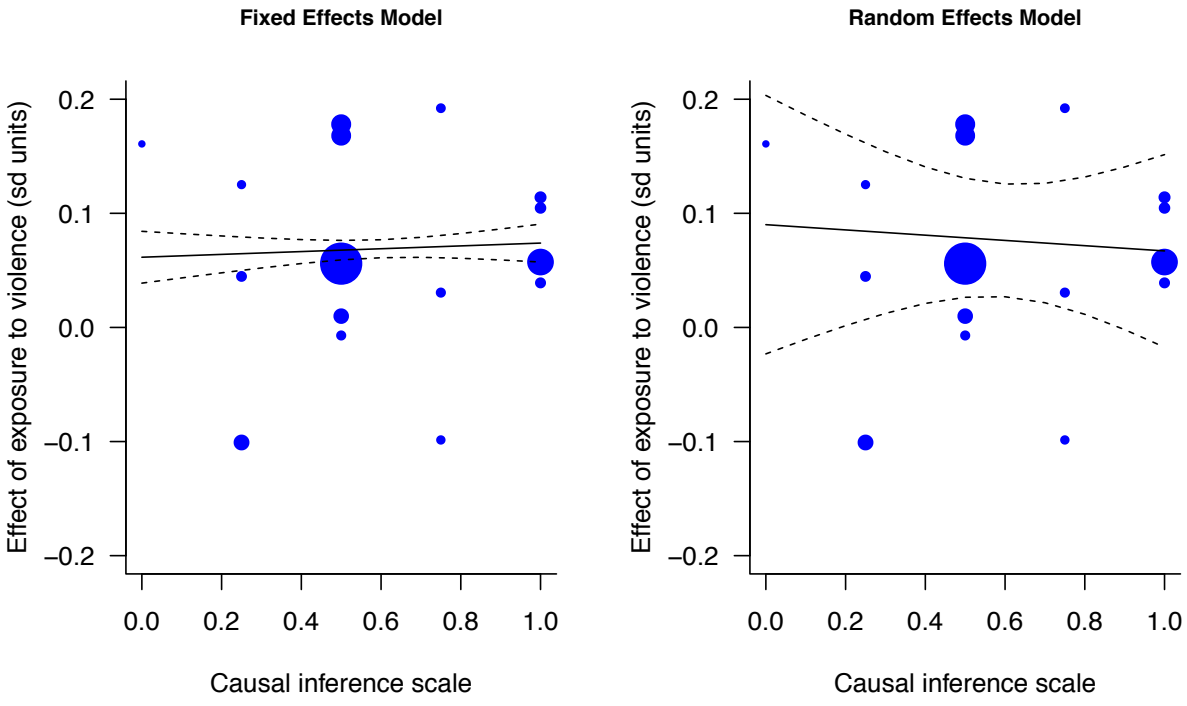
Note: *p<0.10, ** p<0.05, *** p<0.01.
The table reports meta analysis results when including study-level covariates in the models. Each panel represents a separate regression. *Personal exposure* indicates studies for which exposure to violence is on the personal/household level, as opposed to community/district level; *Years since war* measures the length of time between the end of the conflict and the timing of each study; *Civilian* is an indicator for studies in which civilians were exposed to violence, as opposed to combatants; *Crime* indicates studies for which violence exposure is crime; Finally, *Africa*, *Asia*, and *Europe* are indicators for studies' location.
[1] This analysis excludes crime data.
[2] Grosjean (2014) is dropped from the analysis because of high variability in years since war variable.

Figure A2: The effect of violence exposure over time and crime violence



Note: The left panel shows 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. The right panel 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 have larger points. The predicted average effects are added to the plot (with corresponding 95% confidence interval bounds, calculated from a fixed effects model). The Grosjean (2014) study is dropped from the analysis of the left panel because of high variability in years since war variable.

Figure A3: The effect of violence exposure as a function of causal inference design



Note: This is a meta analytic scatterplot, showing the observed effects estimated for individual studies, where the dependent variable is an index of all cooperation outcomes, plotted against a causal inference scale. The scale ranges from 0 to 1, where 0 indicates little attempts to measure causal relationships and 1 indicates studies using more tools to causally identify the effect of exposure to violence. The scale is constructed from variables capturing studies' use of community fixed effects, pre-war data, substantive controls, and sensitivity analysis. The point sizes are proportional to the inverse of the standard errors, which means that studies with larger samples have larger points. The predicted average effect is added to the plot (with corresponding 95% confidence interval bounds).

E Additional Results

E.1 Study-by-study meta-analysis results

Figures A4 - A25 report study-by-study meta-analysis results for fixed-effects and random-effects models. The results are reported in forest plots, in which each line represents an estimate for one study, and the size of the square for each study reflects the its weight in the meta-analysis. Studies with more observations receive a higher weight. The forest plots also report 95% confidence intervals for the meta-analysis model, derived from the studies' sampling variances. The average effect of exposure to violence across studies is plotted as a diamond at the bottom of the plots. The coefficients in the plot are derived from studies' regressions where the independent variable is a binary indicator of violence exposure, and the various outcome variables are standardized.

E.2 Meta-analysis results with alternative independent variables

As the effects of exposure to violence might be different for different types of violence, we also analyze the results using alternative independent variables: standardized continuous measures; standardized measures of the respondent's direct or personal exposure to violence (e.g., being beaten or injured); and of direct and indirect exposure to violence (e.g., through the household or community's exposure).

Table A17 reports the results. The top panel reports coefficients from a meta-analysis using standardized measures of violence exposure; the middle panel reports the results using standardized measures of exposure to violence at the community level; and the bottom panel reports the results using standardized measures of personal exposure. Overall, we find similar results in all these analyses, where social group participation and community leadership/participation robustly hold across specifications. In some estimations of personal exposure we find negative coefficients on some of the outcomes, but these results should be taken with caution because of the small number of studies measuring personal, direct exposure to violence.

E.3 Meta Regression Analysis of reported t-statistics

As a robustness test, we employ a Meta Regression Analysis (MRA) of reported t-statistics (Stanley and Jarrell, 1989). Our main results are limited to studies for which we have raw replication data. In order to examine results from additional studies, we extracted the t-statistics of results reported in papers by dividing reported coefficients by their standard errors. We prefer t-statistics to regression coefficients to measure effect sizes, because coefficients in our sample are not comparable as a result of heterogeneity across studies in the measurement of the dependent and independent variables. As Stanley and Jarrell (1989) recommend, a t-statistic can be used as a standardized measure of the coefficient of interest.

We estimate an ordinary least squares model in which the dependent variable is the t-statistic reported in each paper, and the independent variable is a weight calculated as the inverse of each

paper's standard error. In the meta regression, we control for the number of observations in each study. In addition, for the behavioral games outcome, which employs several game measures from the same context, we add a control for the country of each study.

Results are reported in Table A18. It can be seen that the coefficients on social group participation and prosoical behavior in experimental games are positive and significant, corroborating our main results. The coefficients for community participation and interest in politics also positive but are not significant at conventional levels. This is partly because of the small number of studies reporting such results for these outcomes ($N = 4$). Finally, we do not find statistically significant coefficients for trust or voting, similar to our analysis of the raw data.

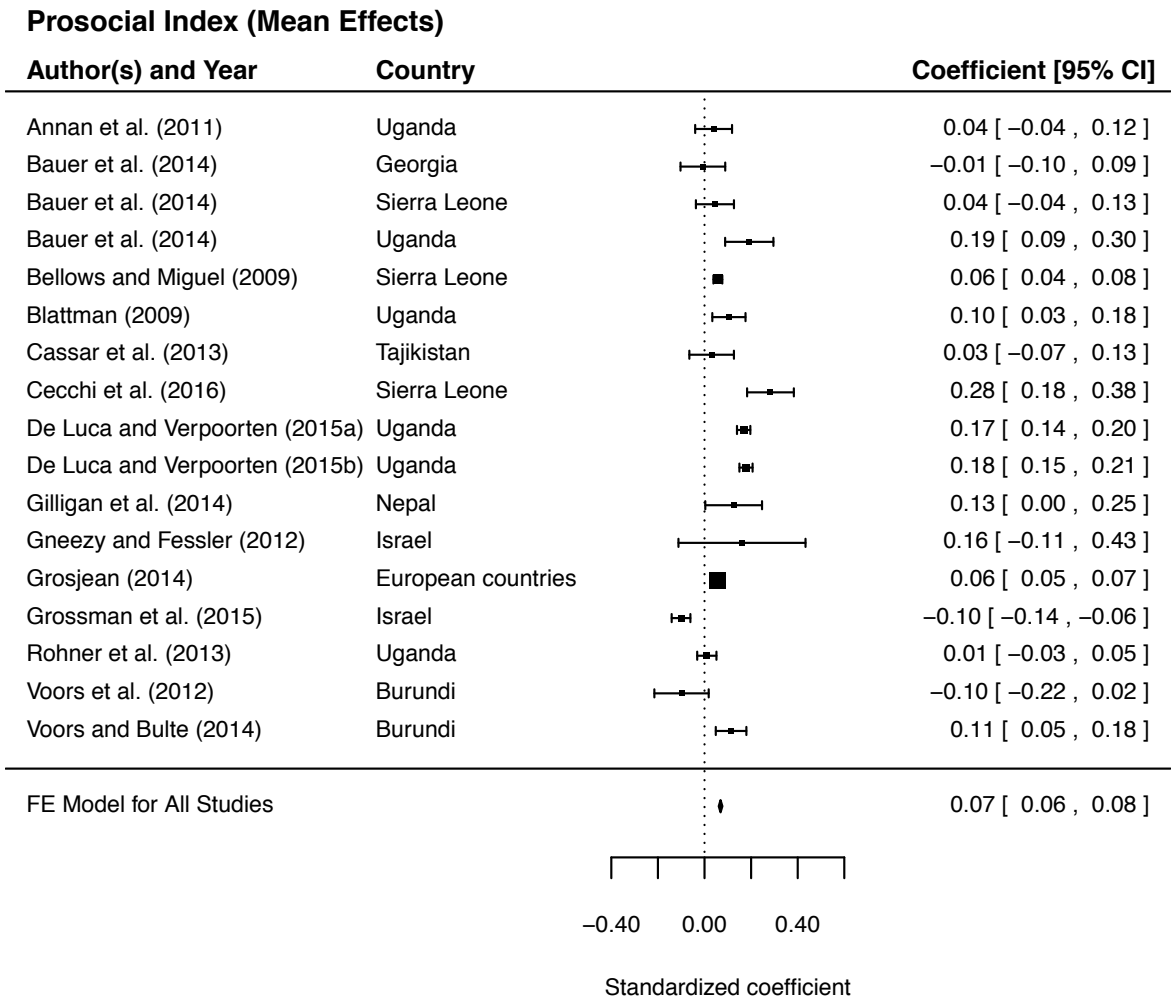
E.4 Meta-analysis results including crime

We also estimated the results by including additional data on exposure to crime violence across the globe (Bateson, 2012). We estimated the same models reported in the main paper. Table A19 reports the results. Overall, we find that violence exposure is associated with a statistically significant increase in the prosociality summary index. The fixed-effects coefficient is 0.08 standard deviation units (s.e. 0.00, P-value$<$0.01), and the random-effects coefficient is 0.08 (s.e. 0.02, P-value$<$0.01).

Looking at the results for different types of outcomes, we find, in both fixed-effects and random effects models, positive and statistically significant coefficients for participation in social groups, community leadership and participation, prosocial behavior in experimental games, and knowledge of politics. We do not find positive and significant effects for voting and trust.

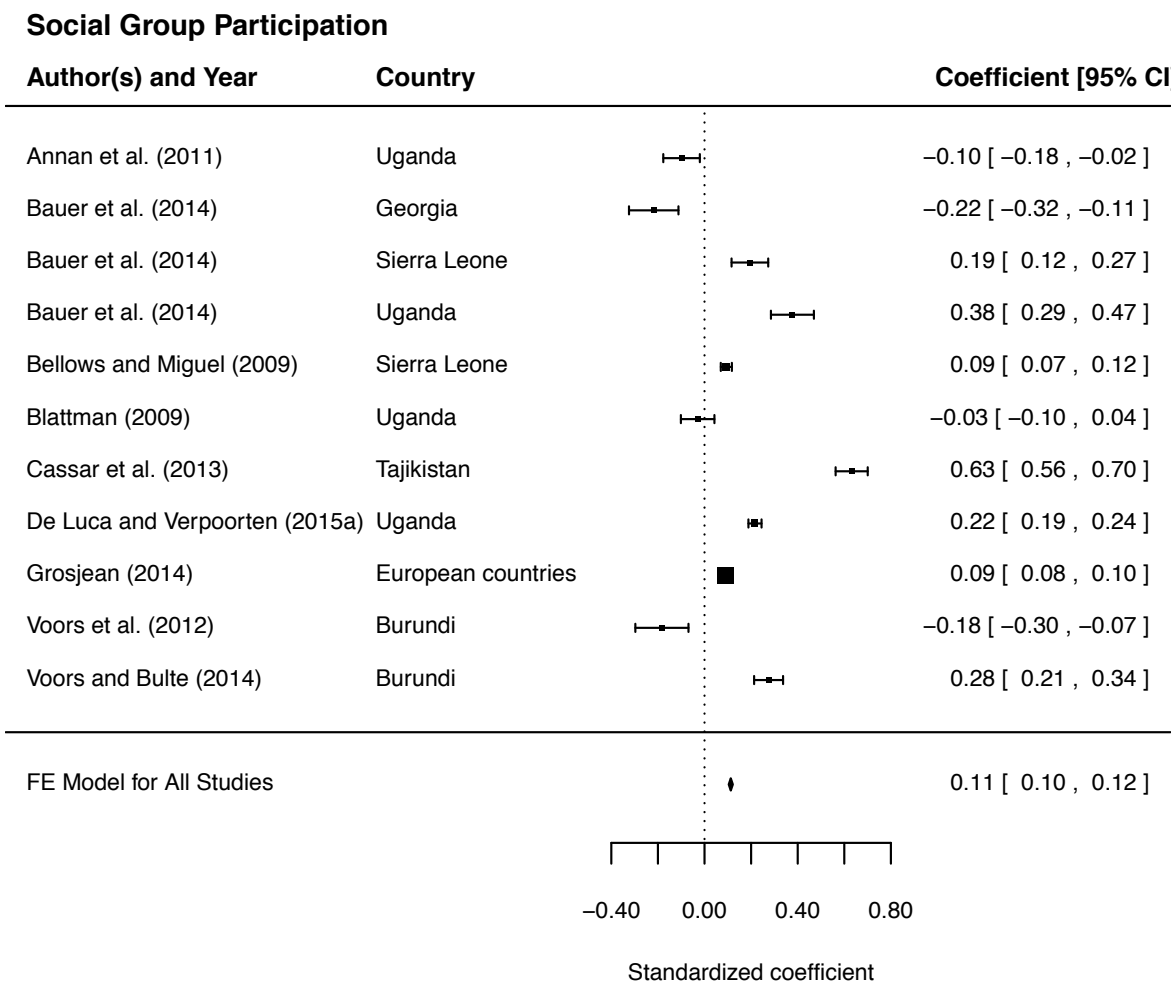
E.5 Main results in forest plots: Fixed effects models

Figure A4



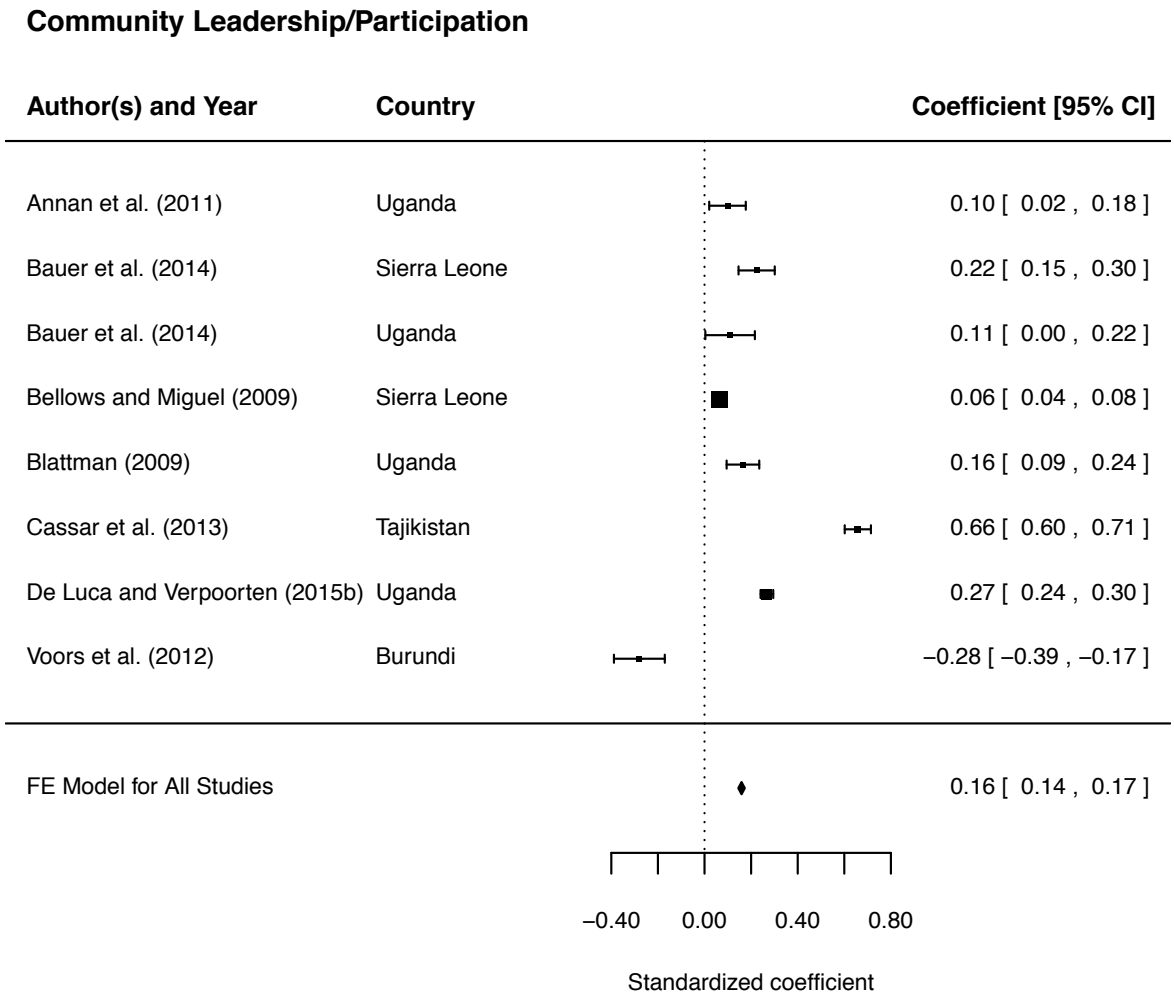
Note: The figure shows a forest plot of fixed-effects meta-analysis results for the summary index (mean effects), calculated in standard deviation units. Each square represents an estimate for one study, where square sizes are proportional to the weights used in the meta-analysis. Studies with more observations receive a higher weight. The figure plots 95% confidence intervals for the meta-analysis model, derived from the studies' sampling variances. The average effect of exposure to violence across studies is plotted as a diamond at the bottom of the figure.

Figure A5



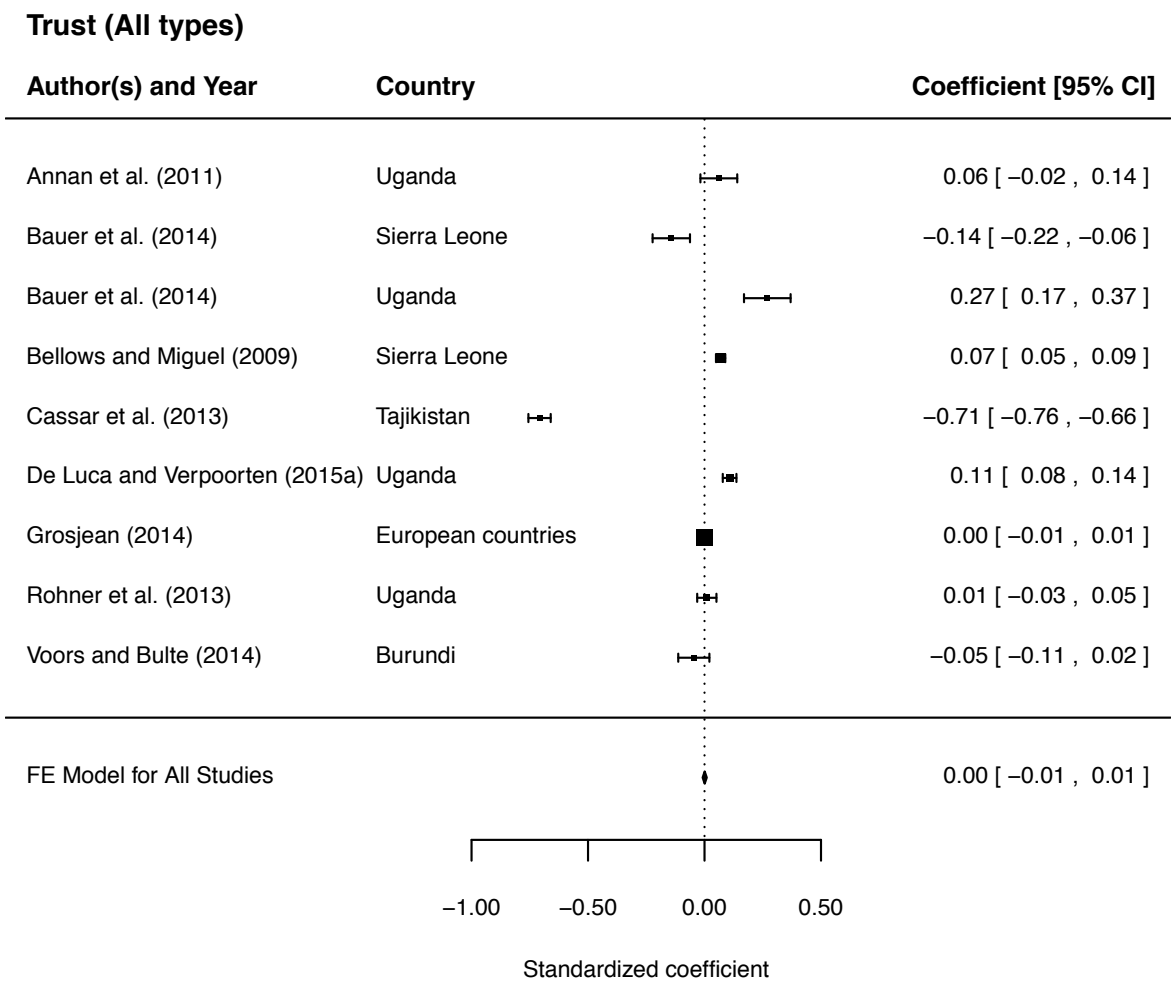
Note: The figure shows a forest plot of fixed-effects meta-analysis results for social groups participation, calculated in standard deviation units. Each square represents an estimate for one study, where square sizes are proportional to the weights used in the meta-analysis. Studies with more observations receive a higher weight. The figure plots 95% confidence intervals for the meta-analysis model, derived from the studies' sampling variances. The average effect of exposure to violence across studies is plotted as a diamond at the bottom of the figure.

Figure A6



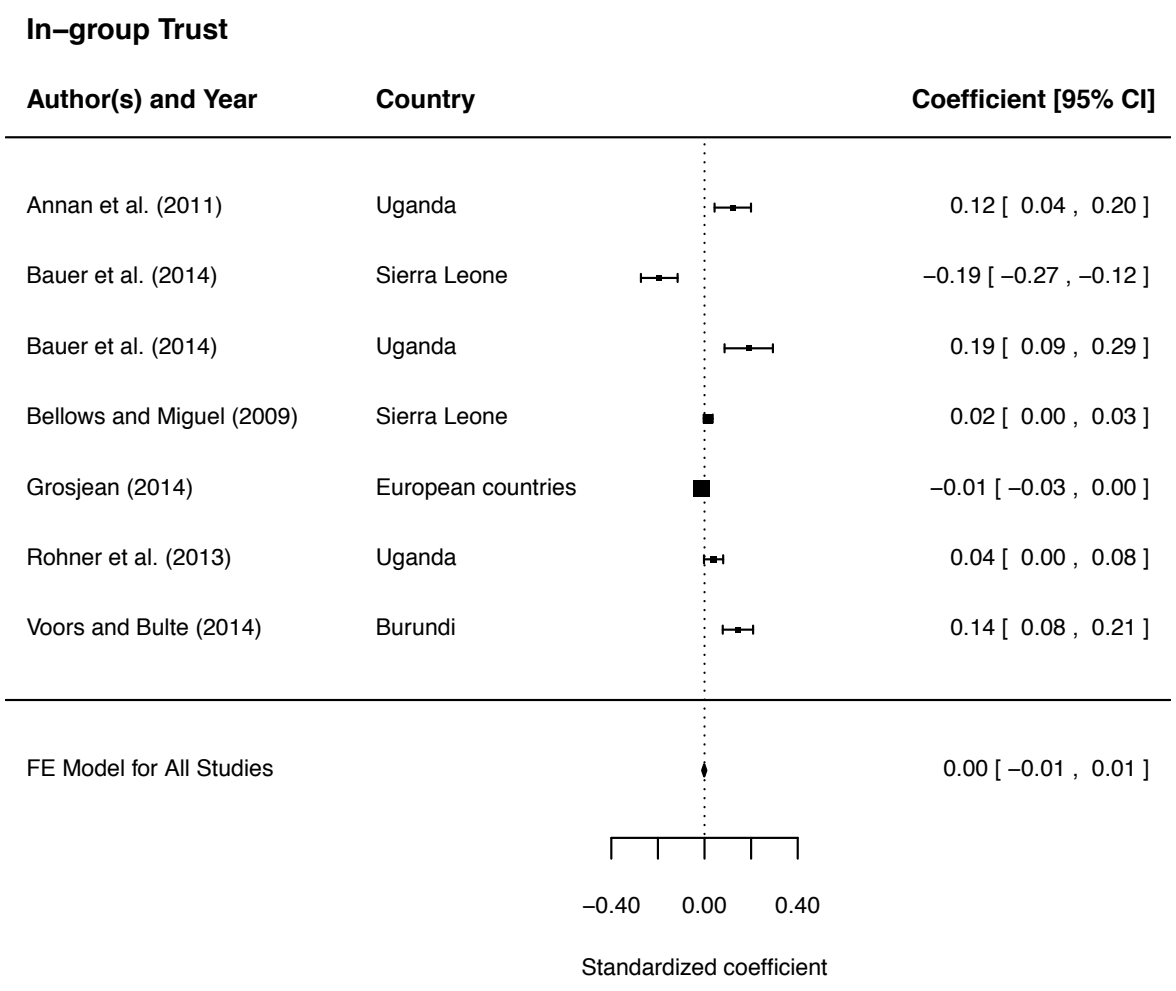
Note: The figure shows a forest plot of fixed-effects meta-analysis results for community leadership/participation, calculated in standard deviation units. Each square represents an estimate for one study, where square sizes are proportional to the weights used in the meta-analysis. Studies with more observations receive a higher weight. The figure plots 95% confidence intervals for the meta-analysis model, derived from the studies' sampling variances. The average effect of exposure to violence across studies is plotted as a diamond at the bottom of the figure.

Figure A7



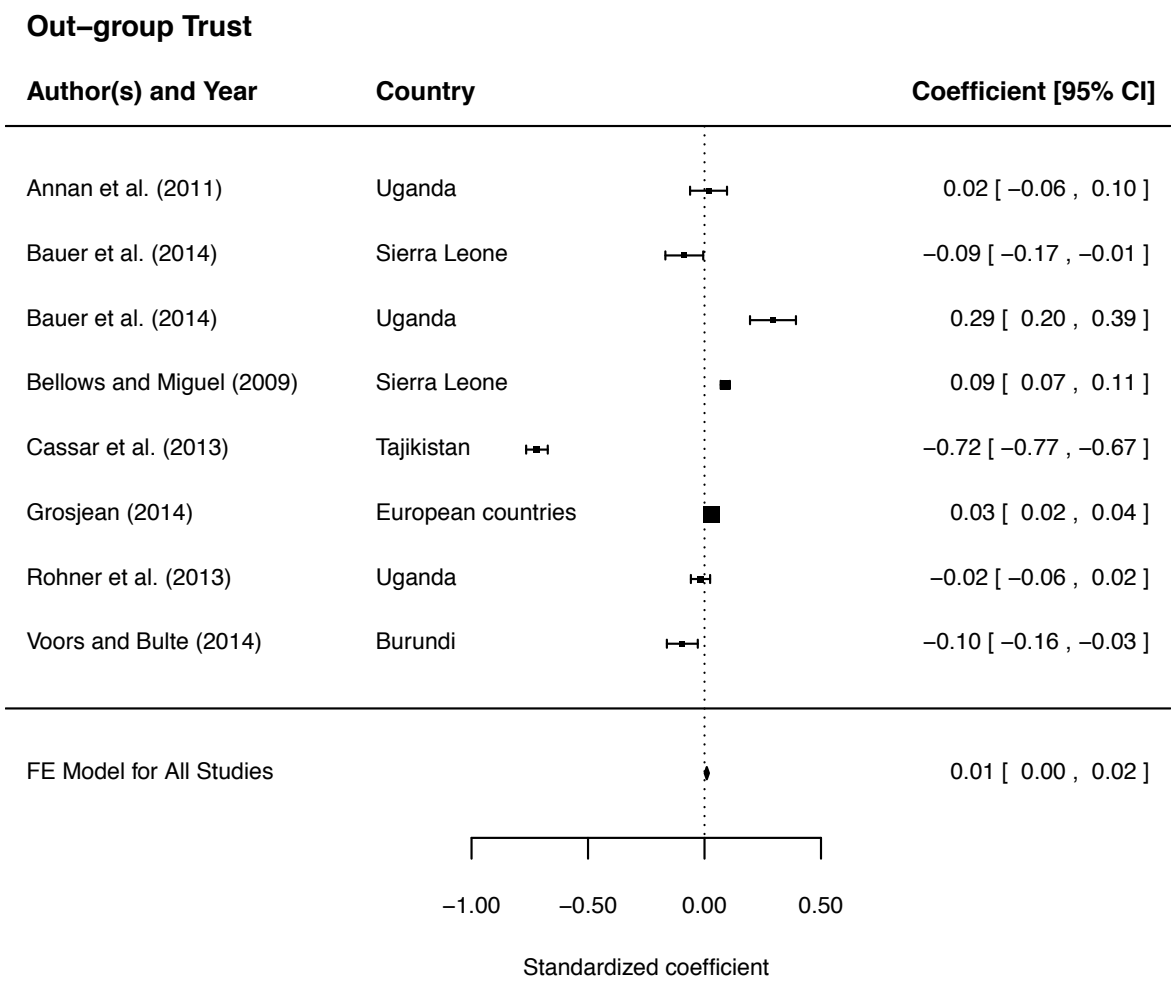
Note: The figure shows a forest plot of fixed-effects meta-analysis results for trust, calculated in standard deviation units. Each square represents an estimate for one study, where square sizes are proportional to the weights used in the meta-analysis. Studies with more observations receive a higher weight. The figure plots 95% confidence intervals for the meta-analysis model, derived from the studies' sampling variances. The average effect of exposure to violence across studies is plotted as a diamond at the bottom of the figure.

Figure A8



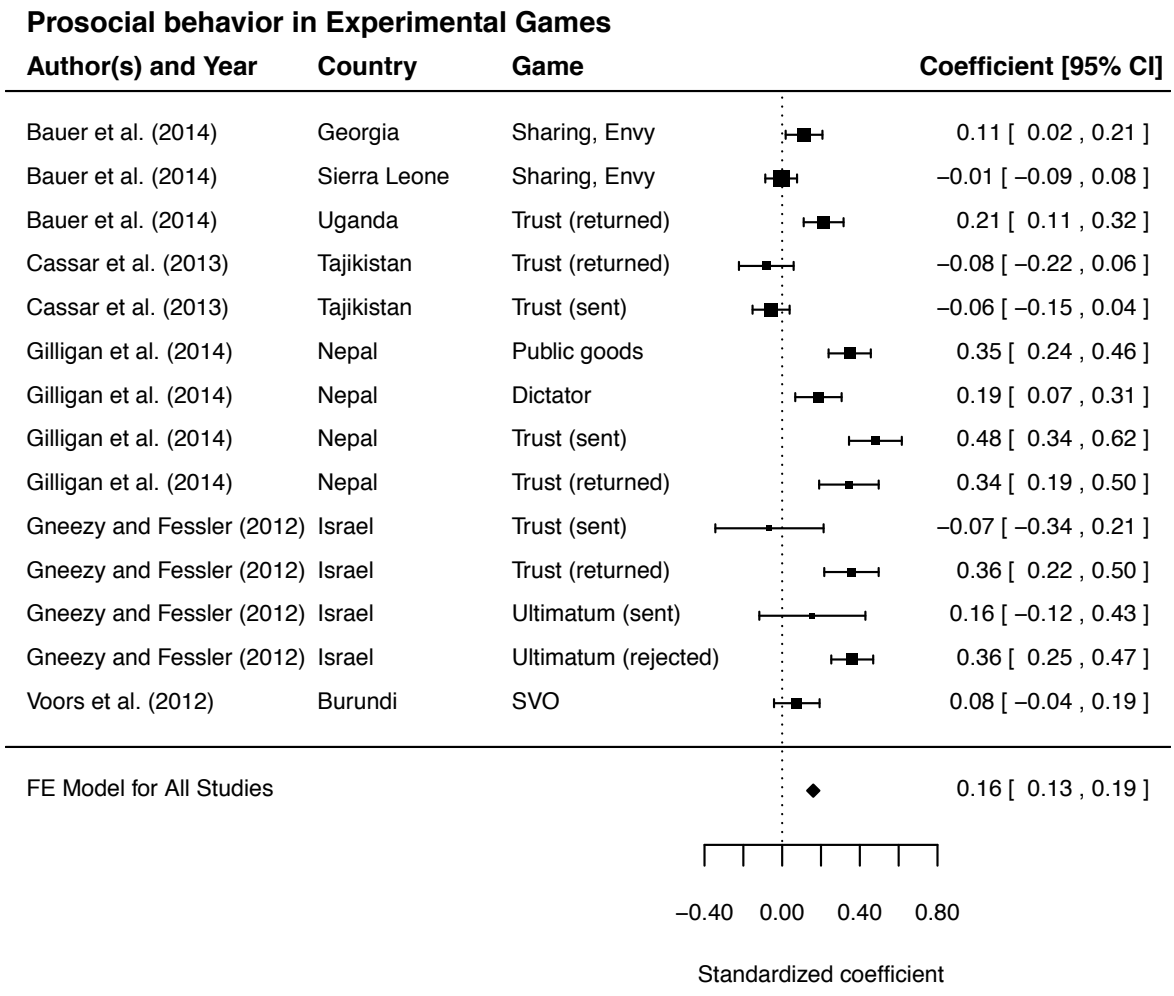
Note: The figure shows a forest plot of fixed-effects meta-analysis results for trust in in-group members, calculated in standard deviation units. Each square represents an estimate for one study, where square sizes are proportional to the weights used in the meta-analysis. Studies with more observations receive a higher weight. The figure plots 95% confidence intervals for the meta-analysis model, derived from the studies' sampling variances. The average effect of exposure to violence across studies is plotted as a diamond at the bottom of the figure.

Figure A9



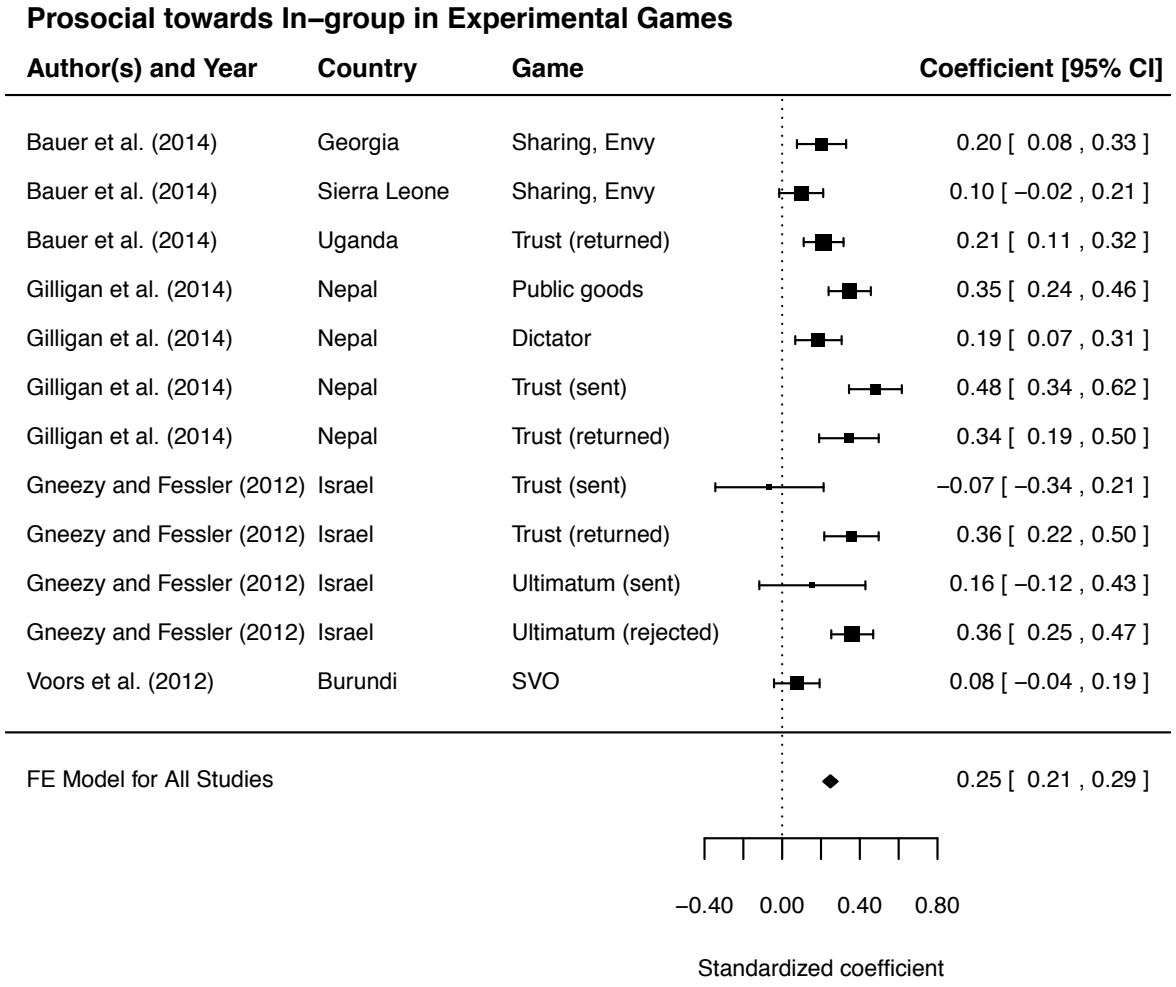
Note: The figure shows a forest plot of fixed-effects meta-analysis results for trust in out-group members, calculated in standard deviation units. Each square represents an estimate for one study, where square sizes are proportional to the weights used in the meta-analysis. Studies with more observations receive a higher weight. The figure plots 95% confidence intervals for the meta-analysis model, derived from the studies' sampling variances. The average effect of exposure to violence across studies is plotted as a diamond at the bottom of the figure.

Figure A10



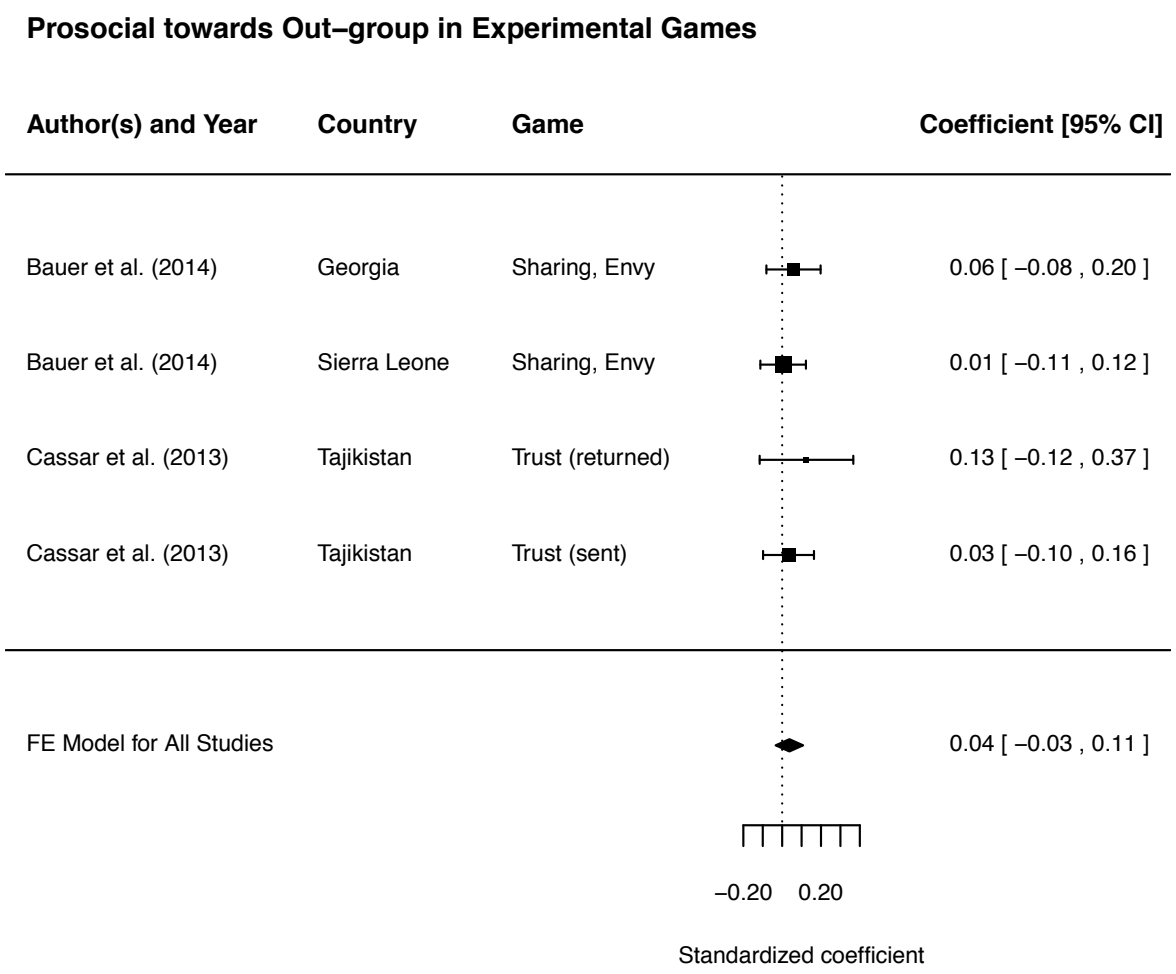
Note: The figure shows a forest plot of fixed-effects meta-analysis results for prosocial behavior in experimental games, calculated in standard deviation units. Each square represents an estimate for one study, where square sizes are proportional to the weights used in the meta-analysis. Studies with more observations receive a higher weight. The figure plots 95% confidence intervals for the meta-analysis model, derived from the studies' sampling variances. The average effect of exposure to violence across studies is plotted as a diamond at the bottom of the figure.

Figure A11



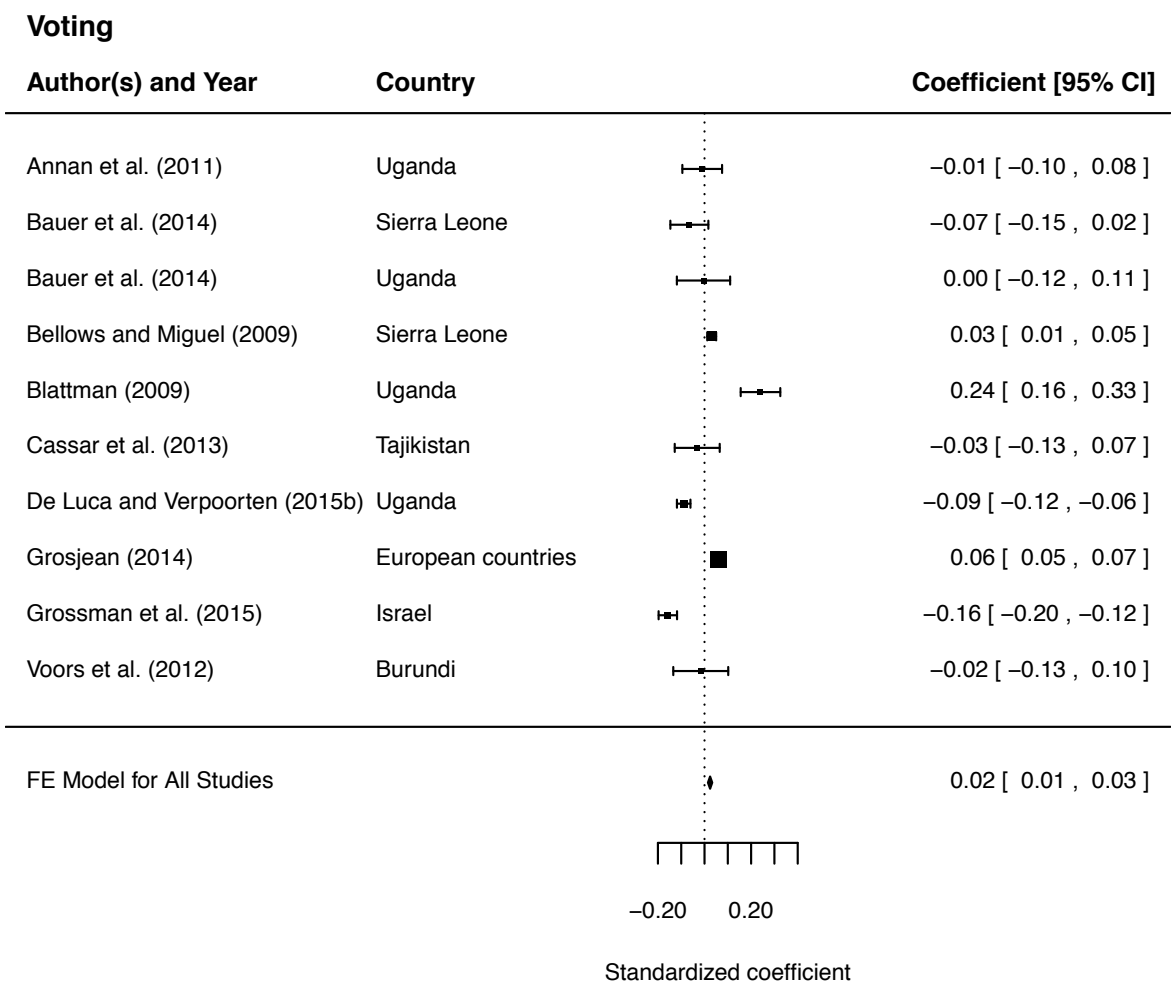
Note: The figure shows a forest plot of fixed-effects meta-analysis results for prosocial behavior in experimental games towards in-group members, calculated in standard deviation units. Each square represents an estimate for one study, where square sizes are proportional to the weights used in the meta-analysis. Studies with more observations receive a higher weight. The figure plots 95% confidence intervals for the meta-analysis model, derived from the studies' sampling variances. The average effect of exposure to violence across studies is plotted as a diamond at the bottom of the figure.

Figure A12



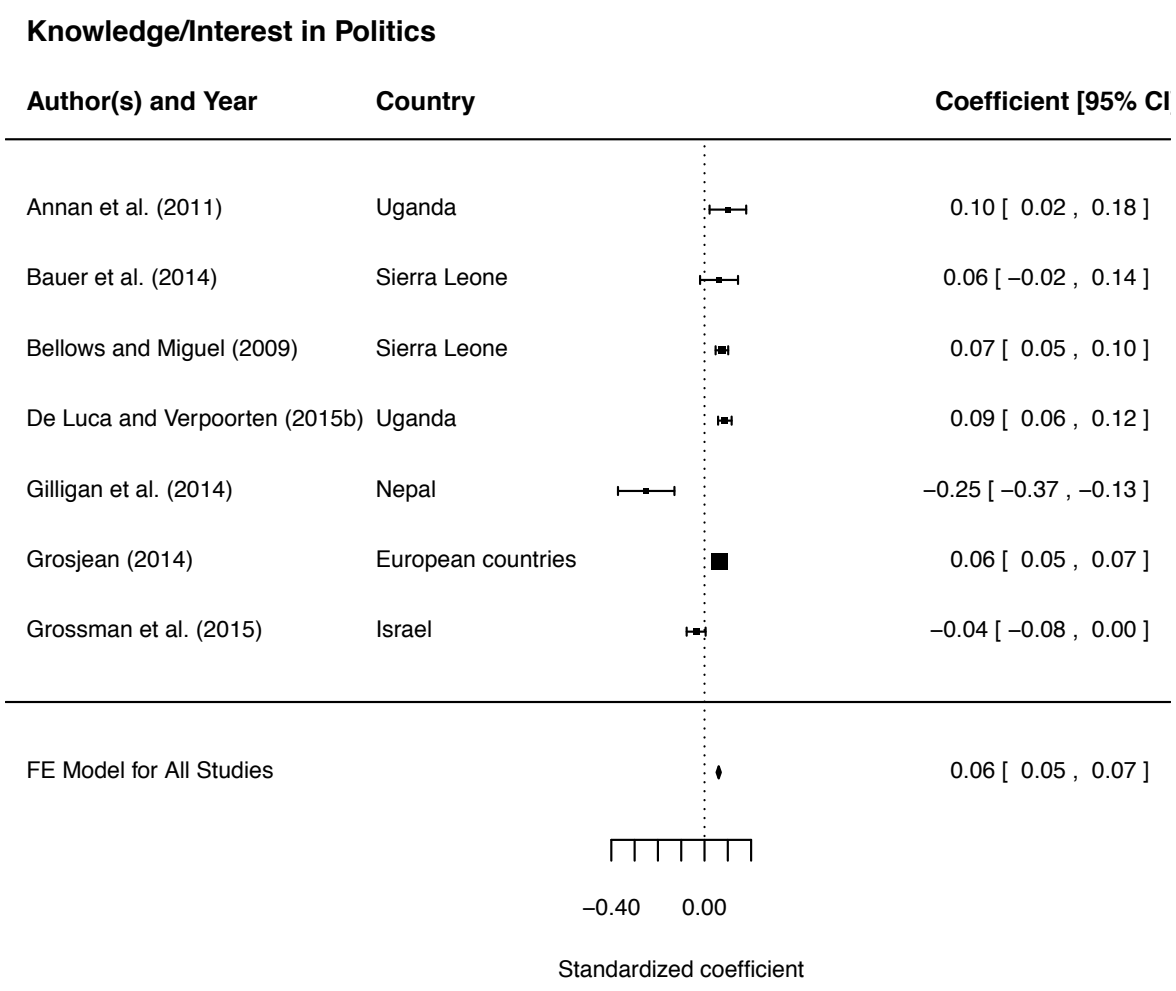
Note: The figure shows a forest plot of fixed-effects meta-analysis results for prosocial behavior in experimental games towards out-group members, calculated in standard deviation units. Each square represents an estimate for one study, where square sizes are proportional to the weights used in the meta-analysis. Studies with more observations receive a higher weight. The figure plots 95% confidence intervals for the meta-analysis model, derived from the studies' sampling variances. The average effect of exposure to violence across studies is plotted as a diamond at the bottom of the figure.

Figure A13



Note: The figure shows a forest plot of fixed-effects meta-analysis results for voting, calculated in standard deviation units. Each square represents an estimate for one study, where square sizes are proportional to the weights used in the meta-analysis. Studies with more observations receive a higher weight. The figure plots 95% confidence intervals for the meta-analysis model, derived from the studies' sampling variances. The average effect of exposure to violence across studies is plotted as a diamond at the bottom of the figure.

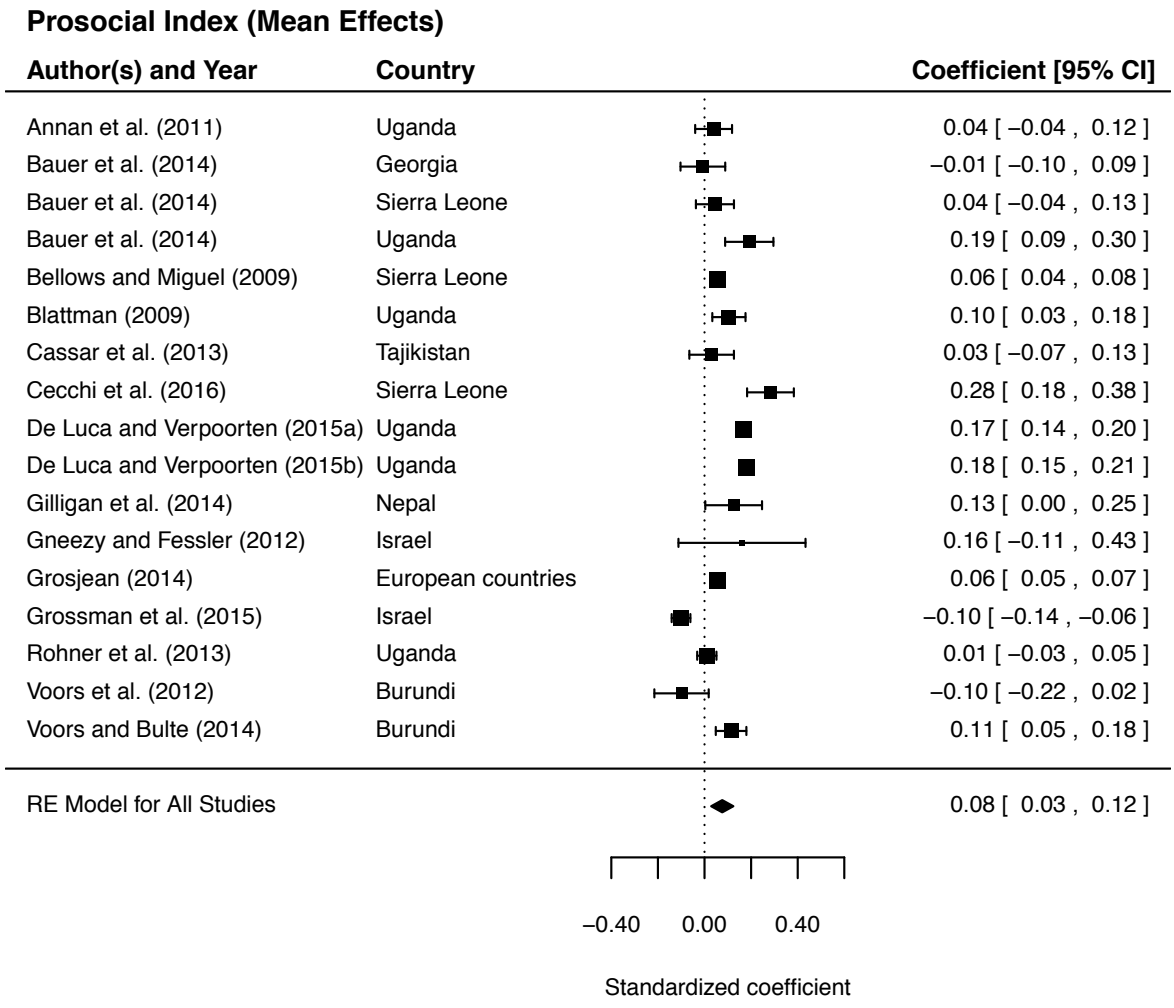
Figure A14



Note: The figure shows a forest plot of fixed-effects meta-analysis results for knowledge/interest in politics, calculated in standard deviation units. Each square represents an estimate for one study, where square sizes are proportional to the weights used in the meta-analysis. Studies with more observations receive a higher weight. The figure plots 95% confidence intervals for the meta-analysis model, derived from the studies' sampling variances. The average effect of exposure to violence across studies is plotted as a diamond at the bottom of the figure.

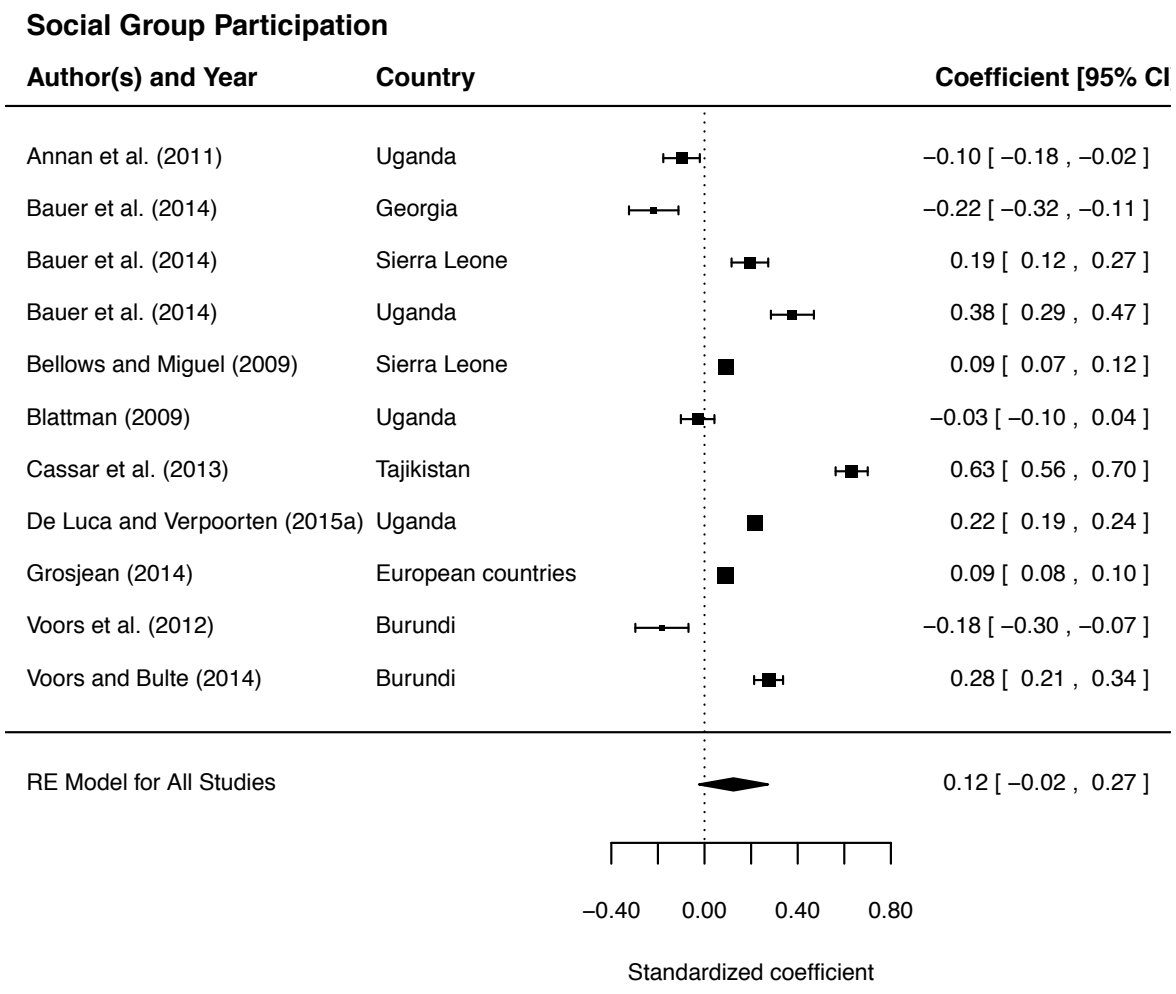
E.6 Main results in forest plots: Random effects models

Figure A15



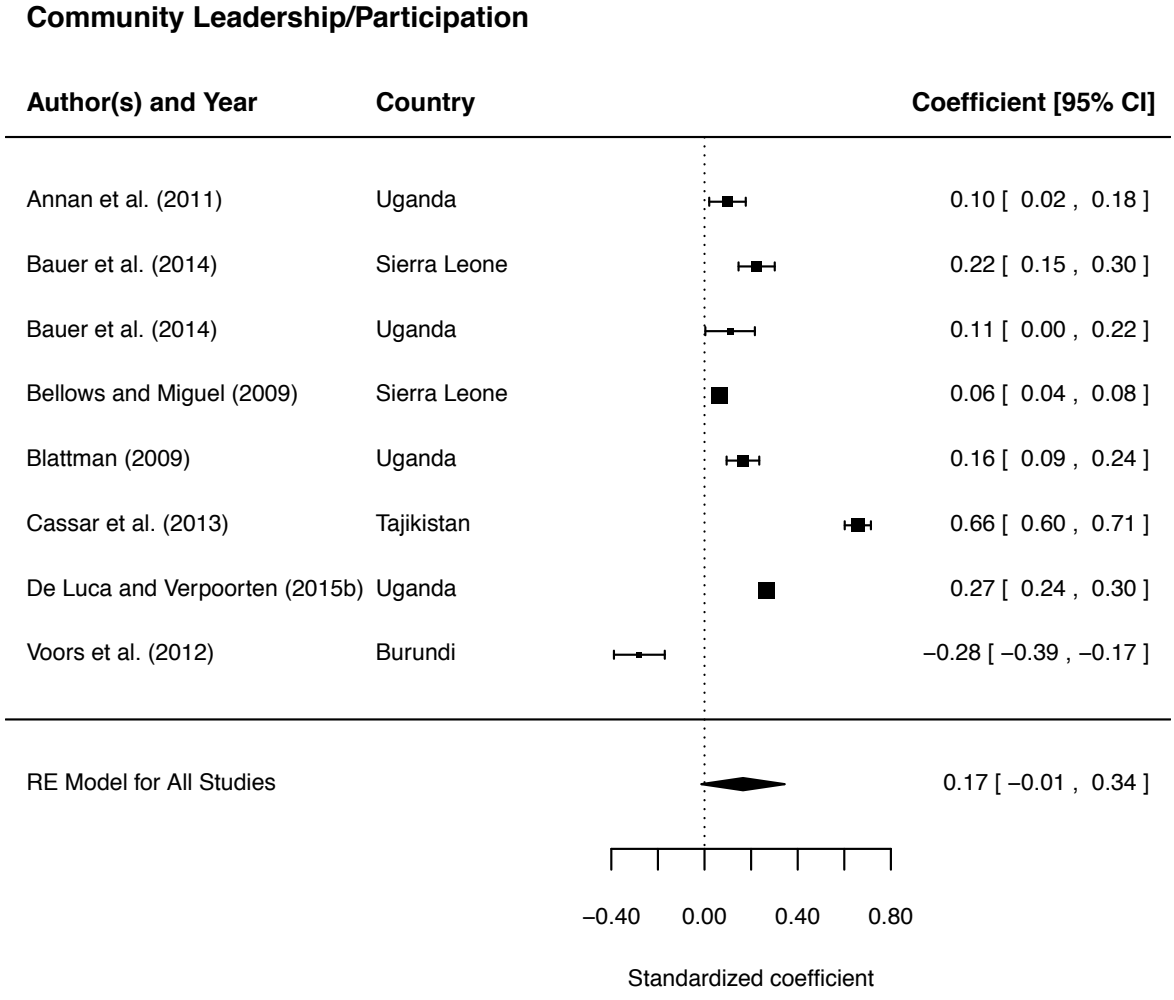
Note: The figure shows a forest plot of random-effects meta-analysis results for the summary index (mean effects), calculated in standard deviation units. Each square represents an estimate for one study, where square sizes are proportional to the weights used in the meta-analysis. Studies with more observations receive a higher weight. The figure plots 95% confidence intervals for the meta-analysis model, derived from the studies' sampling variances. The average effect of exposure to violence across studies is plotted as a diamond at the bottom of the figure.

Figure A16



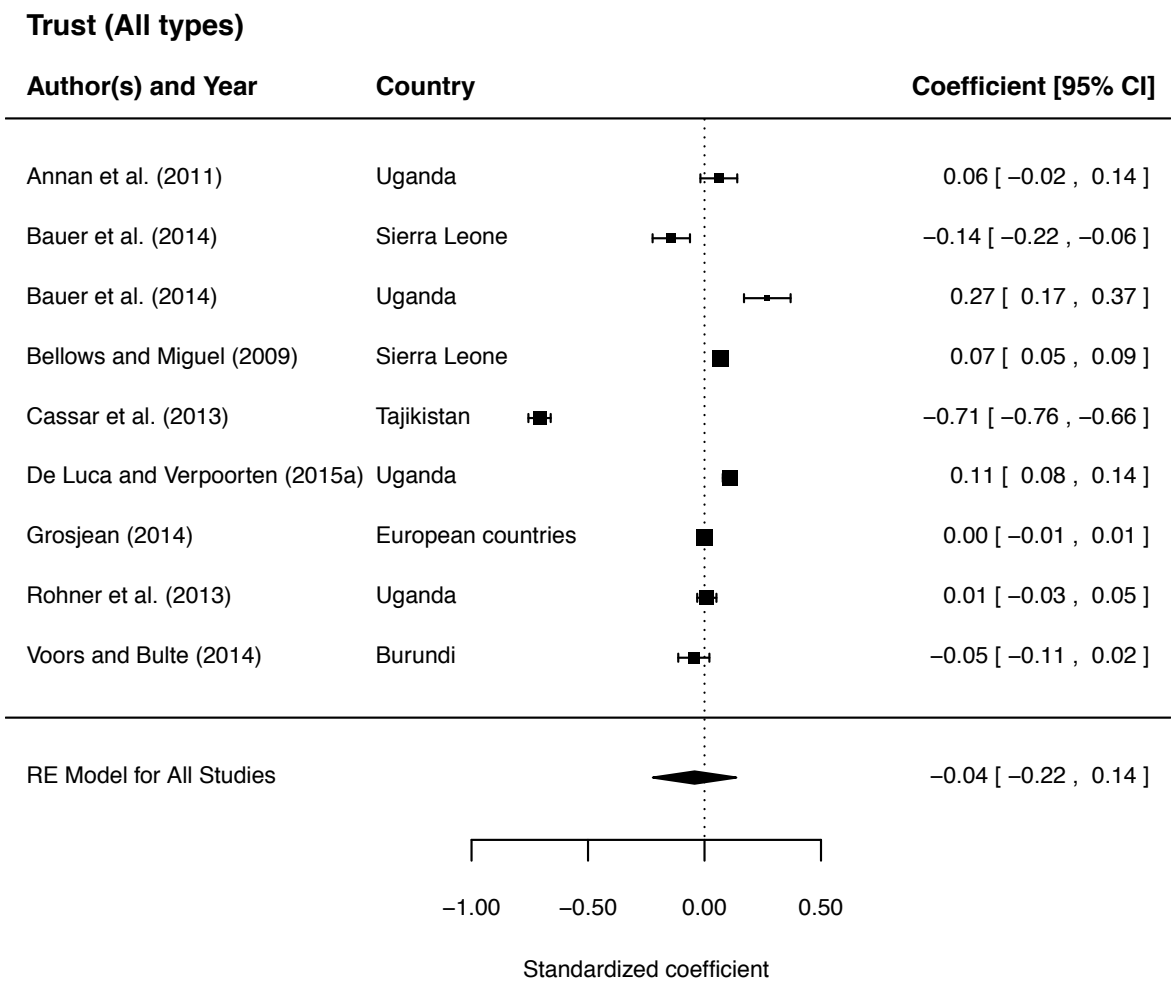
Note: The figure shows a forest plot of random-effects meta-analysis results for social groups participation, calculated in standard deviation units. Each square represents an estimate for one study, where square sizes are proportional to the weights used in the meta-analysis. Studies with more observations receive a higher weight. The figure plots 95% confidence intervals for the meta-analysis model, derived from the studies' sampling variances. The average effect of exposure to violence across studies is plotted as a diamond at the bottom of the figure.

Figure A17



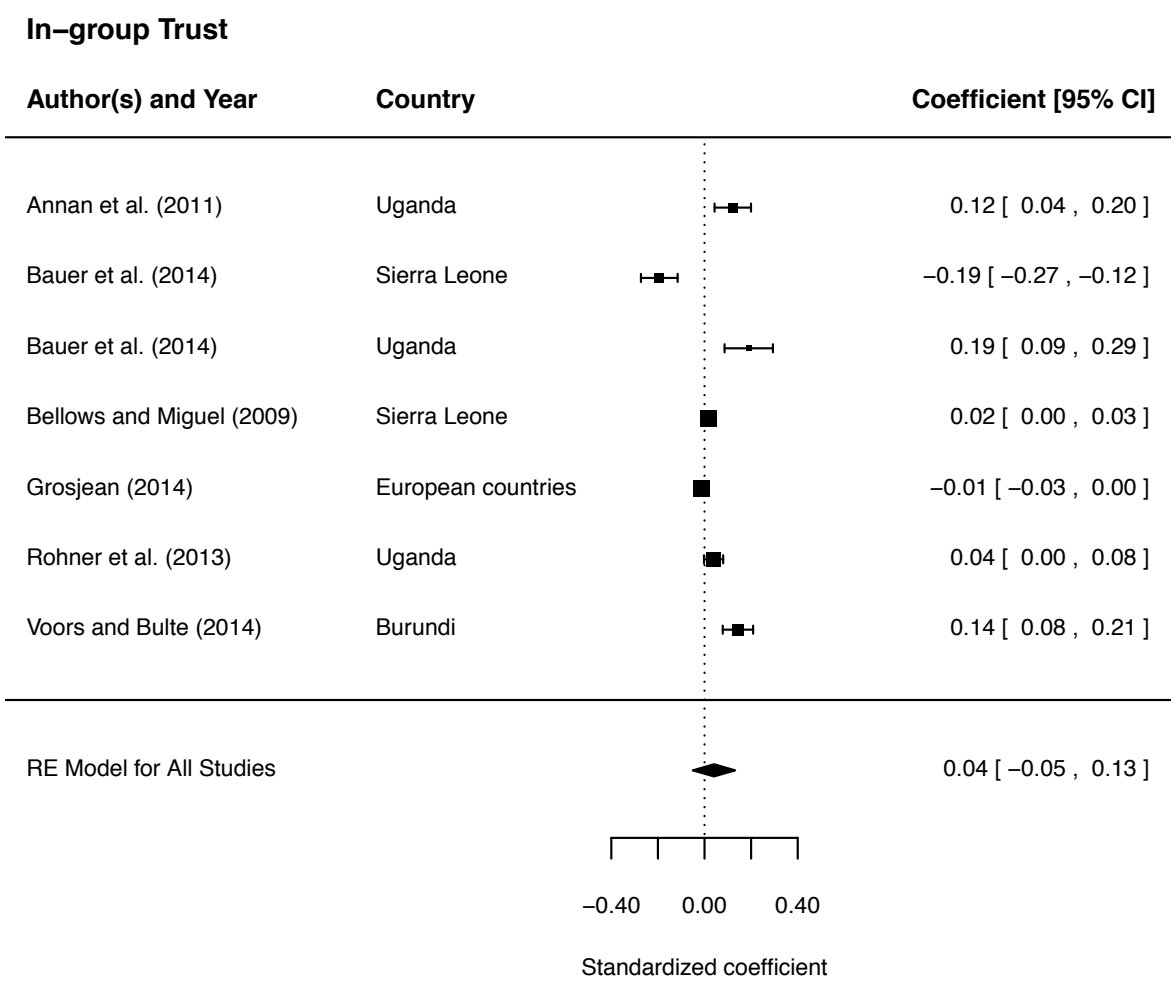
Note: The figure shows a forest plot of random-effects meta-analysis results for community leadership/participation, calculated in standard deviation units. Each square represents an estimate for one study, where square sizes are proportional to the weights used in the meta-analysis. Studies with more observations receive a higher weight. The figure plots 95% confidence intervals for the meta-analysis model, derived from the studies' sampling variances. The average effect of exposure to violence across studies is plotted as a diamond at the bottom of the figure.

Figure A18



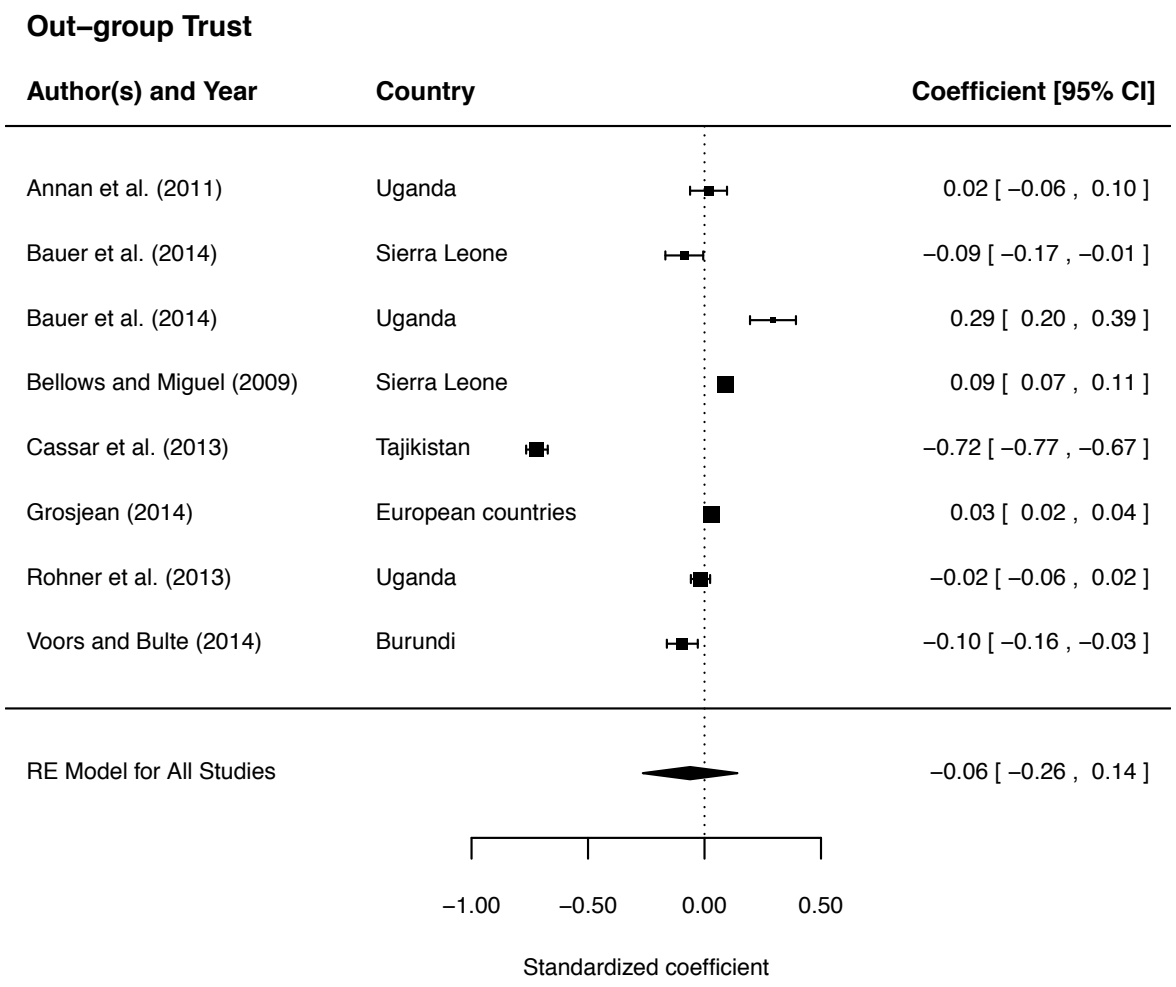
Note: The figure shows a forest plot of random-effects meta-analysis results for trust, calculated in standard deviation units. Each square represents an estimate for one study, where square sizes are proportional to the weights used in the meta-analysis. Studies with more observations receive a higher weight. The figure plots 95% confidence intervals for the meta-analysis model, derived from the studies' sampling variances. The average effect of exposure to violence across studies is plotted as a diamond at the bottom of the figure.

Figure A19



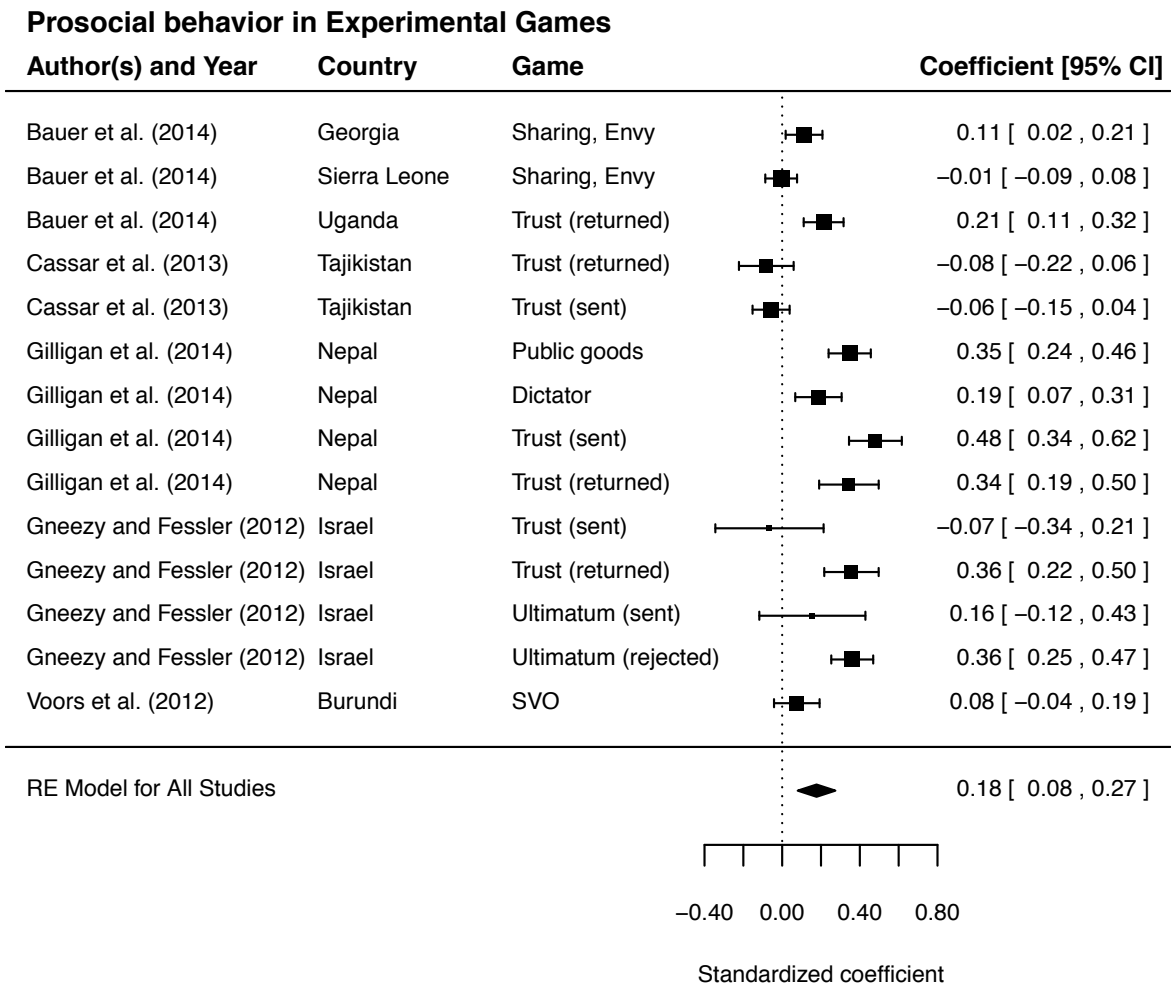
Note: The figure shows a forest plot of random-effects meta-analysis results for trust in in-group members, calculated in standard deviation units. Each square represents an estimate for one study, where square sizes are proportional to the weights used in the meta-analysis. Studies with more observations receive a higher weight. The figure plots 95% confidence intervals for the meta-analysis model, derived from the studies' sampling variances. The average effect of exposure to violence across studies is plotted as a diamond at the bottom of the figure.

Figure A20



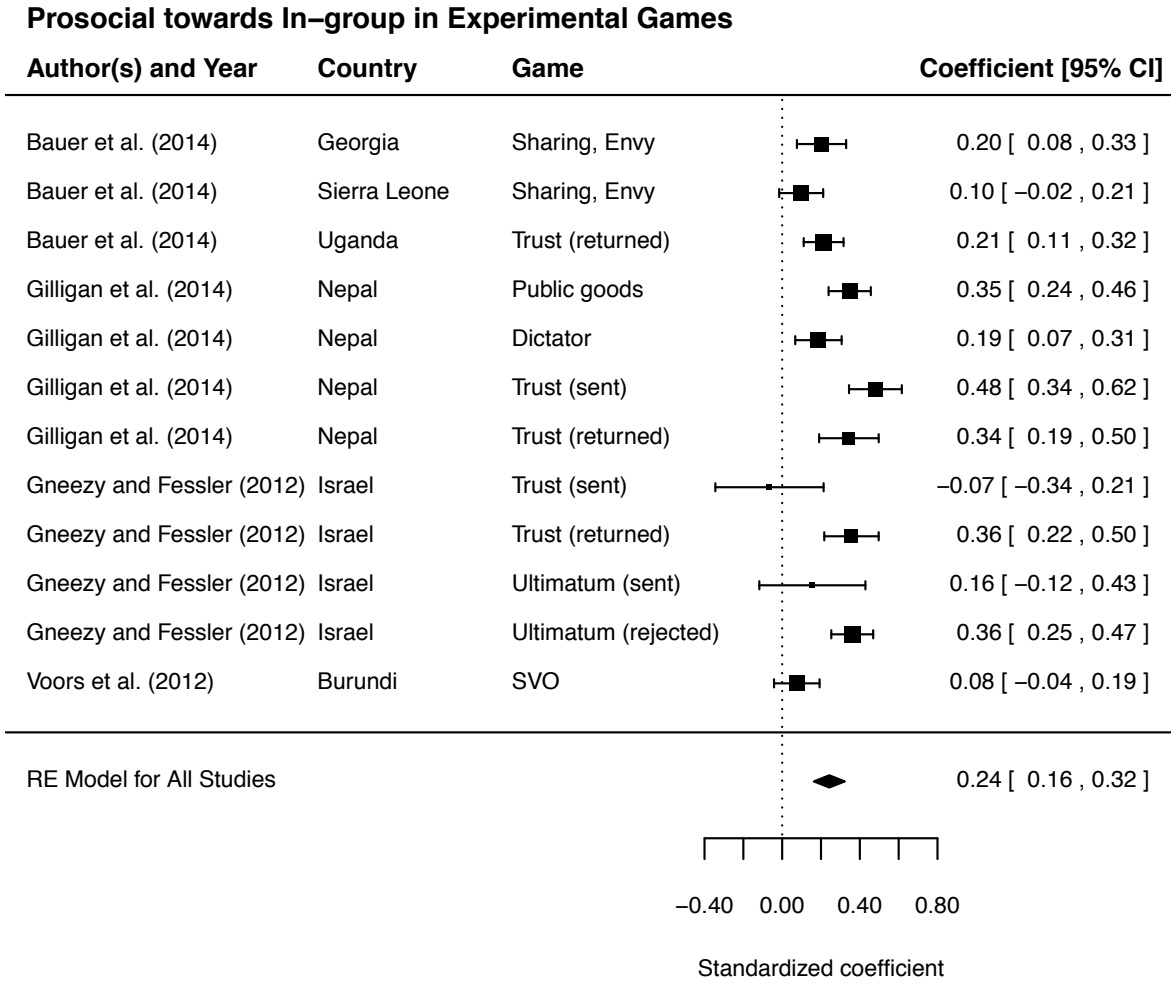
Note: The figure shows a forest plot of random-effects meta-analysis results for trust in out-group members, calculated in standard deviation units. Each square represents an estimate for one study, where square sizes are proportional to the weights used in the meta-analysis. Studies with more observations receive a higher weight. The figure plots 95% confidence intervals for the meta-analysis model, derived from the studies' sampling variances. The average effect of exposure to violence across studies is plotted as a diamond at the bottom of the figure.

Figure A21



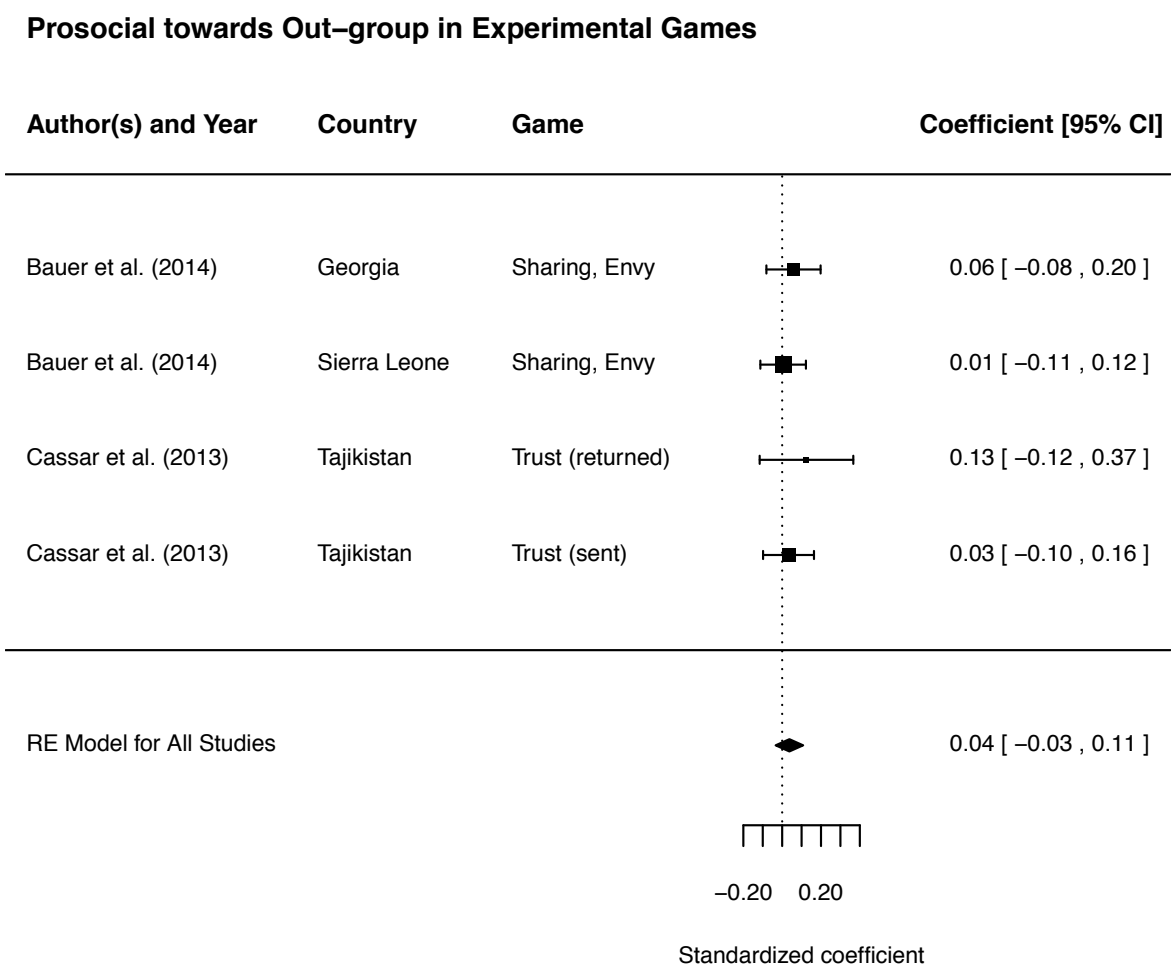
Note: The figure shows a forest plot of random-effects meta-analysis results for prosocial behavior in experimental games, calculated in standard deviation units. Each square represents an estimate for one study, where square sizes are proportional to the weights used in the meta-analysis. Studies with more observations receive a higher weight. The figure plots 95% confidence intervals for the meta-analysis model, derived from the studies' sampling variances. The average effect of exposure to violence across studies is plotted as a diamond at the bottom of the figure.

Figure A22



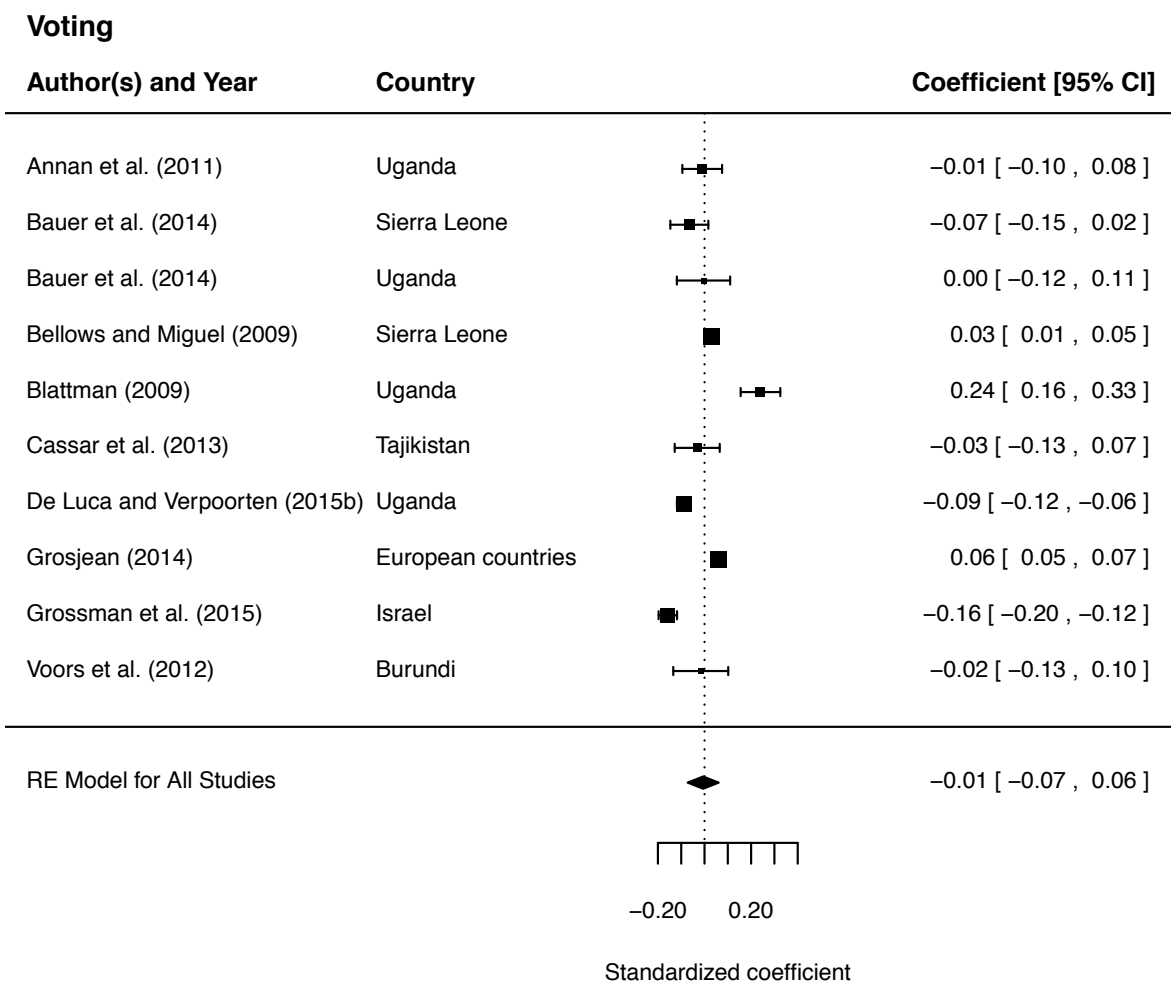
Note: The figure shows a forest plot of random-effects meta-analysis results for prosocial behavior in experimental games towards in-group members, calculated in standard deviation units. Each square represents an estimate for one study, where square sizes are proportional to the weights used in the meta-analysis. Studies with more observations receive a higher weight. The figure plots 95% confidence intervals for the meta-analysis model, derived from the studies' sampling variances. The average effect of exposure to violence across studies is plotted as a diamond at the bottom of the figure.

Figure A23



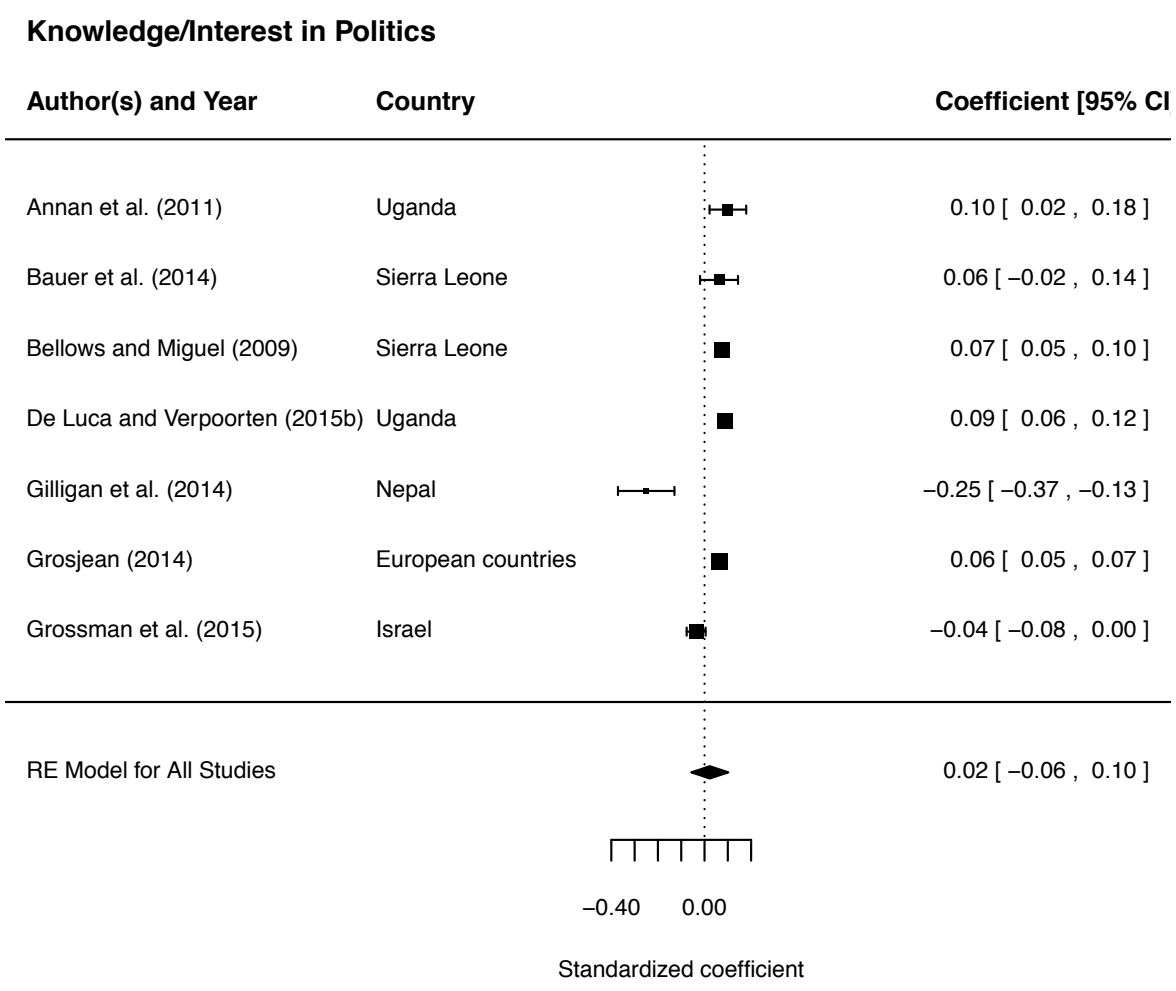
Note: The figure shows a forest plot of random-effects meta-analysis results for prosocial behavior in experimental games towards out-group members, calculated in standard deviation units. Each square represents an estimate for one study, where square sizes are proportional to the weights used in the meta-analysis. Studies with more observations receive a higher weight. The figure plots 95% confidence intervals for the meta-analysis model, derived from the studies' sampling variances. The average effect of exposure to violence across studies is plotted as a diamond at the bottom of the figure.

Figure A24



Note: The figure shows a forest plot of random-effects meta-analysis results for voting, calculated in standard deviation units. Each square represents an estimate for one study, where square sizes are proportional to the weights used in the meta-analysis. Studies with more observations receive a higher weight. The figure plots 95% confidence intervals for the meta-analysis model, derived from the studies' sampling variances. The average effect of exposure to violence across studies is plotted as a diamond at the bottom of the figure.

Figure A25



Note: The figure shows a forest plot of random-effects meta-analysis results for knowledge/interest in politics, calculated in standard deviation units. Each square represents an estimate for one study, where square sizes are proportional to the weights used in the meta-analysis. Studies with more observations receive a higher weight. The figure plots 95% confidence intervals for the meta-analysis model, derived from the studies' sampling variances. The average effect of exposure to violence across studies is plotted as a diamond at the bottom of the figure.

Table A17: Additional measures of exposure to violence

Outcome *(Standardized)*	Estimate	(1) Fixed Effects	(2) Random Effects	N
A. All violence exposure (standardized)				
Summary index (mean effects)	Coef.	0.03***	0.02	17
	Std. Err	0.00	0.02	
	P-val	0.00	0.32	
Social groups participation	Coef.	0.05***	0.04	11
	Std. Err	0.00	0.03	
	P-val	0.00	0.20	
Community leadership/participation	Coef.	0.08***	0.09***	8
	Std. Err	0.01	0.02	
	P-val	0.00	0.00	
Trust	Coef.	-0.00	-0.02	9
	Std. Err	0.00	0.04	
	P-val	0.48	0.66	
Prosocial behavior in experimental games	Coef.	0.09***	0.10***	15
	Std. Err	0.02	0.03	
	P-val	0.00	0.00	
Voting	Coef.	0.01**	-0.01	10
	Std. Err	0.00	0.02	
	P-val	0.01	0.60	
Knowledge/interest in politics	Coef.	0.03***	0.02	7
	Std. Err	0.00	0.02	
	P-val	0.00	0.46	
B. Community violence exposure (standardized)				
Summary index (mean effects)	Coef.	0.03***	0.02	15
	Std. Err	0.00	0.02	
	P-val	0.00	0.25	
Social groups participation	Coef.	0.05***	0.06**	11
	Std. Err	0.00	0.03	
	P-val	0.00	0.02	
Community leadership/participation	Coef.	0.08***	0.11***	8
	Std. Err	0.01	0.03	
	P-val	0.00	0.00	
Trust	Coef.	-0.00	-0.05	9
	Std. Err	0.00	0.03	
	P-val	0.42	0.16	
Prosocial behavior in experimental games	Coef.	0.06***	0.07***	14
	Std. Err	0.02	0.03	
	P-val	0.00	0.00	
Voting	Coef.	0.02***	0.01	9
	Std. Err	0.00	0.01	
	P-val	0.00	0.57	
Knowledge/interest in politics	Coef.	0.04***	0.04**	6
	Std. Err	0.00	0.02	
	P-val	0.00	0.01	
C. Personal violence exposure (standardized)				
Summary index (mean effects)	Coef.	-0.02*	0.01	8
	Std. Err	0.01	0.03	
	P-val	0.08	0.70	
Social groups participation	Coef.	0.05***	0.07	6
	Std. Err	0.02	0.06	
	P-val	0.00	0.29	
Community leadership/participation	Coef.	0.11***	0.11**	5
	Std. Err	0.02	0.04	
	P-val	0.00	0.01	
Trust	Coef.	-0.09***	-0.08	4
	Std. Err	0.02	0.09	
	P-val	0.00	0.40	
Prosocial behavior in experimental games	Coef.	0.03	0.03	6
	Std. Err	0.02	0.02	
	P-val	0.22	0.22	
Voting	Coef.	-0.09***	-0.04	6
	Std. Err	0.01	0.04	
	P-val	0.00	0.37	
Knowledge/interest in politics	Coef.	-0.04**	-0.04**	3
	Std. Err	0.02	0.02	
	P-val	0.01	0.01	

* p<0.10, ** p<0.05,*** p<0.01.

Note: The table reports meta-analysis results for each outcome reported in the rows. Column (1) reports results from a fixed-effects model; Column (2) reports results from a random-effects model. The coefficient represents the estimated population effects of exposure to violence across studies, measured in standard deviation units.

Table A18: Meta regression analysis of reported t-statistics

	Estimate	**Std. Err.**	**P-value**	**N**
Social groups participation	0.02**	0.01	0.01	7
Community leadership/participation	0.03	0.01	0.17	4
Trust	0.01	0.02	0.47	5
Prosocial behavior in experimental games	0.25***	0.03	0.00	16
Voting	0.00	0.00	0.53	5
knowledge/interest in politics	0.04	0.02	0.18	4

* p<0.10, ** p<0.05,*** p<0.01.

Note: The Table reports meta regression analysis (Stanley and Jarrell, 1989) results of reported t-values, for each outcome reported in the rows. The coefficient represents the estimated population effects of exposure to violence across studies, adjusted for the dispersion of the data underlying each study. N reflects the number of studies/games analyzed for each outcome.

Table A19: Including exposure to crime violence

Outcome *(Standardized)*	**Estimate**	**(1) Fixed Effects**	**(2) Random Effects**
Summary index (mean effects)	Coef.	0.08***	0.08***
	Std. Err	0.00	0.02
	P-val	0.00	0.00
Social groups participation	Coef.	0.11***	0.13**
	Std. Err	0.00	0.06
	P-val	0.00	0.03
Community leadership/participation	Coef.	0.17***	0.19**
	Std. Err	0.00	0.07
	P-val	0.00	0.01
Trust (all)	Coef.	-0.01**	-0.04
	Std. Err	0.00	0.08
	P-val	0.01	0.60
Trust (in-group)	Coef.	-0.01***	0.02
	Std. Err	0.00	0.04
	P-val	0.00	0.50
Trust (out-group)	Coef.	0.00	-0.06
	Std. Err	0.00	0.09
	P-val	0.89	0.53
Prosocial behavior in experimental games (all)	Coef.	0.17***	0.18***
	Std. Err	0.02	0.05
	P-val	0.00	0.00
Prosocial behavior in experimental games (in-group)	Coef.	0.25***	0.24***
	Std. Err	0.02	0.04
	P-val	0.00	0.00
Prosocial behavior in experimental games (out-group)	Coef.	0.04	0.04
	Std. Err	0.04	0.04
	P-val	0.30	0.30
Voting	Coef.	0.00	-0.00
	Std. Err	0.00	0.03
	P-val	0.50	0.99
Knowledge/interest in politics	Coef.	0.08***	0.07**
	Std. Err	0.00	0.03
	P-val	0.00	0.03

* p<0.10, ** p<0.05,*** p<0.01.

Note: The Table reports meta-analysis results for each outcome reported in the rows. Column (1) reports results from a fixed-effects model; Column (2) reports results from a random-effects model. The coefficient represents the estimated population effects of exposure to violence across studies, measured in standard deviation units. This analysis includes exposure to crime violence.

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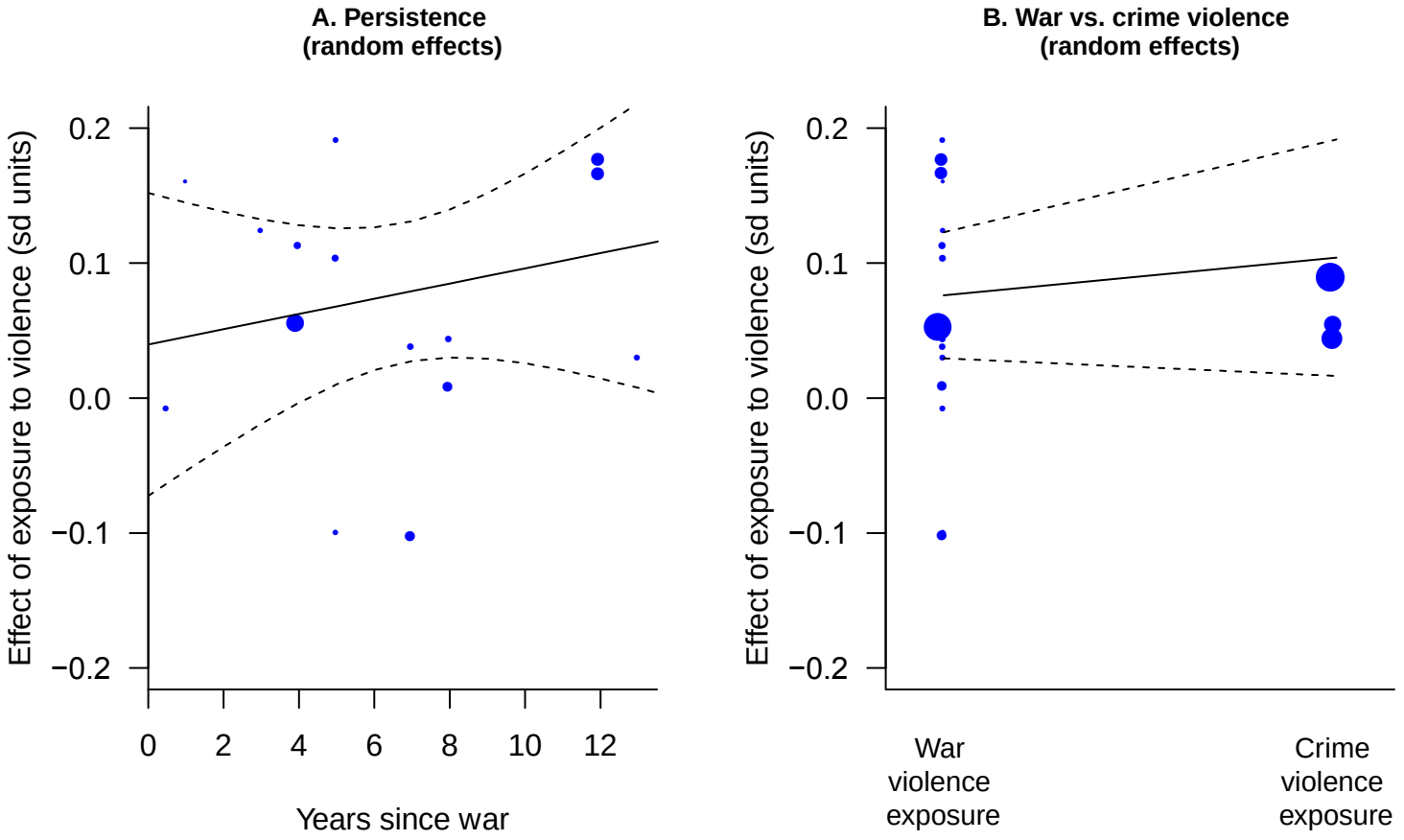
Figure 3: The effect of war violence exposure over time (Panel A) and versus crime-related violenc (Panel B)



Note: Panel A 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. Panel B 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% confidence intervals), calculated from the random effects meta-analysis model. Grosjean (2014) is dropped from analysis in panel A because of high variability in years since war variable across countries.