

War increases religiosity

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Does the experience of war increase people's religiosity? Much evidence supports the idea that particular religious beliefs and ritual forms can galvanize social solidarity and motivate in-group cooperation, thus facilitating a wide range of cooperative behaviours including—but not limited to—peaceful resistance and collective aggression. However, little work has focused on whether violent conflict, in turn, might fuel greater religious participation. Here, we analyse survey data from 1,709 individuals in three post-conflict societies—Uganda, Sierra Leone and Tajikistan. The nature of these conflicts allows us to infer, and statistically verify, that individuals were quasirandomly afflicted with different intensities of war experience—thus potentially providing a natural experiment. We then show that those with greater exposure to these wars were more likely to participate in Christian or Muslim religious groups and rituals, even several years after the conflict. The results are robust to a wide range of control variables and statistical checks and hold even when we compare only individuals from the same communities, ethnic groups and religions.

What is the relationship between religion and war? Most of the research on this question has focused on the pathway going from religious beliefs and rituals to the kind of solidarity and cooperation required for organized, collective action, including conflict^{1–9}. Religion's direct role in organized conflict is not well understood; some evidence suggests that religious commitment can contribute to prejudice and aggression^{10–14} while other evidence suggests that various aspects of religiosity can contribute to cooperation and actually attenuate prejudice or even make individuals more amenable to intergroup cooperation^{15–17}. Much less attention has been paid¹⁸ to the pathway going in the reverse direction: can the experience of war foster greater ritual participation and religious engagement? Here, we focus on this pathway using survey data from three war-torn regions—Sierra Leone, Uganda and Tajikistan—and test whether people who have experienced more war-related violence participate more in religious groups and ritual events.

Why would war increase religiosity? Here, we consider two interrelated sets of hypotheses derived from cultural evolutionary theory^{19–21}. First, both theory and evidence suggest that external threats cause people to adhere more tightly to social norms, including their religious beliefs and practices. Recent cultural evolutionary modelling, for example, reveals that potent external threats—including intergroup conflict but also earthquakes, droughts and so on—favour the cultural evolution of both strict norm adherence and harsher punishments for violators because of the central role norms play in in-group cooperation, public goods and coordination²². Placed within a broader culture–gene coevolutionary framework²³, such cultural

evolutionary processes would be likely to favour genes that increased people's facultative or developmental responsiveness to external threats, increasing their sociality, norm adherence and willingness to punish norm violators (potentially signalling norm compliance)²³. These ideas are broadly supported by descriptive evidence gathered from war combatants²⁴, correlational studies^{25,26}, laboratory-experimental evidence^{27–34} and natural experiments^{31,35–37}. Some work even suggests that the long-term psychological effects of group threats may be strongest during middle childhood, when many prosocial norms are internalized^{36,38}. Under this *war–sociality* hypothesis, we expect that the experience of violent conflict will increase people's engagement with religious groups and rituals. Here, any connection between war and religion would merely reflect a more generalized increase in sociality and norm compliance rather than representing something special about religion per se.

But, is there anything special about religion besides creating social groups and associations? This brings us to the second set of hypotheses. Religions may have culturally evolved to specifically exploit the psychological states created by uncertainty and existential threats as a means to more effectively disseminate themselves. Existing evidence suggests that both rituals and beliefs may help people cope with such difficult psychological states. For rituals, much evidence suggests that people may be attracted to rituals or ritualized practices as a means of relieving anxiety or stress and that performing religious rituals may help to mitigate the impacts of traumatic experiences on well-being^{5,39–43}. Similarly, the prosociality induced by commitments to gods, divine protection and beliefs about life after death may help individuals operate in the face of mortal threats, suffering and existential uncertainty^{3,44–46}. Such beliefs may also create particularly supportive communities or associations that draw in those seeking social connections, charity or mutual aid (as per the above hypothesis)^{47,48}. Under this *war–religion* hypothesis, we expect war experiences to have particularly potent effects on measures of religious engagement and ritual participation.

We explore these hypotheses by first establishing if the *war–sociality* hypothesis applies to religious groups and ritual participation. This extends previous work which has already established that war increases sociality and group participation^{35–37,49}. Then, using our detailed surveys, we aim to test the *war–religion* hypothesis by isolating the unique effect of war on religion over and above that found for non-religious associations, clubs and organizations. Though we cannot explore which specific elements of rituals, beliefs and communities make religions special, our evidence supports both hypotheses.

The diversity in locations and nature of the conflicts allows us to assess the robustness of our findings. Moreover, because the data

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sets were collected with different time lags after the termination of each of the conflicts, we can study the extent to which the effects of war experience are enduring. In all of our cases, the war exposure resulting from brutal civil conflicts endured for years. In Uganda, beginning around 1987, the Lord's Resistance Army (LRA), led by the self-proclaimed prophet and spirit-medium Joseph Kony, abducted people, including children, and compelled them to fight against government forces, terrorize civilians and loot property⁵⁰. In Sierra Leone⁵¹, from 1991 to 2002, the coordination between the rebel group—the Revolutionary United Front—and the Sierra Leone Army wreaked havoc on civilians who, in turn, responded by organizing themselves into Civil Defence Forces (militia), which then also often engaged in violent abuse⁵². In Tajikistan, in the wake of the Soviet collapse, a declaration of independence led to a disastrous civil war. Local factions, including former communists, Islamist groups, ethnic nationalists and prodemocratic reformers, fought ferociously for national dominance over the course of five years until a peace agreement in 1997⁵³.

Table 1 summarizes information about our samples at each site. In total, our data span 1,709 individuals from 71 villages in three countries. In Uganda, just five years after a peace was initiated with Kony, a sample of individuals aged 18–55 was recruited from 33 villages⁵⁴. Virtually the entire sample was nominally Christian, composed of a mix of Catholics, Anglicans, Evangelicals and Pentecostals. In Sierra Leone, about eight years post-conflict, 21 villages were selected and surveyed based on preliminary data indicating the existence of substantial variation in war exposure within each community⁵⁶. This sample contains both Christians (36%) and Muslims (63%). In Tajikistan, roughly thirteen years after the conflict's tenuous end, participants were recruited from 17 villages scattered across the country using a multistage sampling approach⁵³. Virtually all participants were Muslims (97%), most of them Sunnis (87%). See the Supplementary Methods for further details about sampling at each site.

Across all three sites, those more exposed to war were more likely to be members of religious groups and attend rituals. Focusing on membership, Fig. 1 shows that individuals who were more exposed to violence during the war were—years later—more likely to be members of a religious group. The More affected individuals in Fig. 1 are those for whom the value of our normalized war exposure index exceeds zero, and the Less affected individuals are those

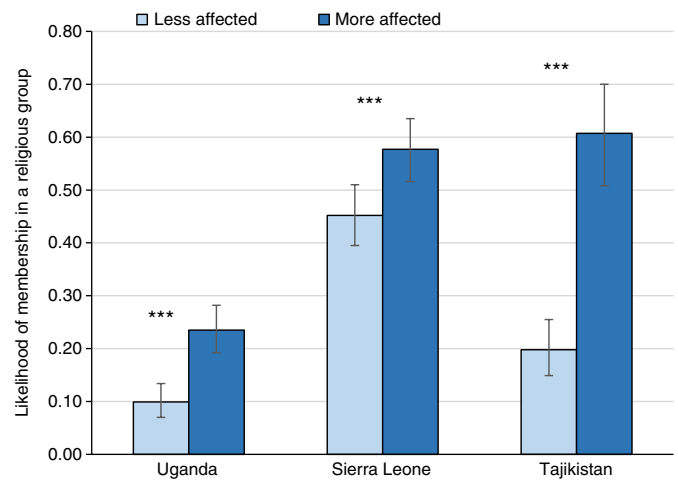


Fig. 1 | Individuals more exposed to war are more likely to be members of religious groups years after the end of local conflicts. The error bars provide 95% exact confidence intervals. 'More affected' individuals are those for whom the value of the normalized war exposure index exceeds zero. 'Less affected' individuals are those with negative values on our war exposure index. The index uses all of the available information for each person at each site by summing the dichotomous answers to all questions on war violence (experienced, witnessed or perpetrated against participants' families) and loss of property. There are three, five and twelve such questions in Tajikistan ($N=339$, $n=232$ Less affected, $n=107$ More affected), Sierra Leone ($N=584$, $n=303$ Less affected, $n=281$ More affected) and Uganda ($N=713$, $n=355$ Less affected, $n=358$ More affected), respectively. ***Significant differences at the 1% level (chi-square test).

with negative values on our war exposure index. With respect to the Less affected group, the likelihood of membership in a religious group increases by 12, 14 and 41 percentage points for the more war affected group in Sierra Leone, Uganda and Tajikistan, respectively (all $P < 0.01$, see Supplementary Table 4).

To explore this more deeply, we estimated a linear probability model by regressing membership in a religious group on our war exposure index for each country separately. The regression coefficients for war exposure are statistically significant and large in magnitude, ranging between 6 and 19 percentile points. The effects are robust for controls for observable characteristics, village fixed effects (absorbing all the between-village variation) and alternative estimators (Table 2, OLS regressions, panel A, columns 1–3; Supplementary Table 5). Note that for two reasons, we estimated a linear probability for our binary outcome using OLS instead of probit or logit regression models. First, for convenience, the coefficients generated by this estimation can be read directly as changes in probability of the outcome variable. Second, the probit estimator drops variables that perfectly predict success or failure in the dependent variable along with their associated observations. Since we control for village fixed effects, the probit model drops observations from villages in which either nobody or everybody is a member of a religious group/attends mosque or church. For these reasons, we present the linear probability model (OLS) here and verify these results in the Supplemental Material with probit models (Supplementary Table 5).

When using our other two war exposure measures, we arrive at similar conclusions (Table 2, panels B and C, columns 1–3): having a household member killed (panel B), injured or abducted (panel C) predicts an increased likelihood of religious group membership in all three countries and for all measures, except for Sierra Leone in panel B, where the effect of having a household member killed, while still positive and of similar magnitude, is less precisely estimated.

Table 1 | Site descriptive information

Country	Sierra Leone	Uganda	Tajikistan
	(1)	(2)	(3)
Conflict	Civil war (1991–2002)	Lord's Resistance Army insurgency (1986–2006)	Civil war (1991–2002)
Year of data collection	2010	2011	2010
Sample size	584	713	412
Female participants	71%	29%	71%
Age (mean and range)	41 (18–84)	34 (18–55)	40 (17–77)
Number of villages	21	33	17
Religion	Muslim 63%, Christian 36%	Christian 99%	Muslim 97%
Member of a religious group	51%	17%	33%
Church/mosque attendance	N/A	76%	15%

Table 2 | Religious engagement and ritual participation increases with war exposure

Dependent variable	Member of a religious group			Church attendance	Mosque attendance
	Sierra Leone	Uganda	Tajikistan	Uganda	Tajikistan
Country	(1)	(2)	(3)	(4)	(5)
Panel A					
War exposure index	0.06*** (0.02)	0.06*** (0.02)	0.19*** (0.03)	0.07*** (0.02)	0.03 (0.02)
Observable characteristics	yes	yes	yes	yes	yes
Village fixed effects	yes	yes	yes	yes	yes
Observations	575	708	330	693	373
Panel B					
Household member killed during the conflict	0.05 (0.04)	0.08** (0.03)	0.45*** (0.07)	0.09** (0.04)	0.10* (0.05)
Observable characteristics	yes	yes	yes	yes	yes
Village fixed effects	yes	yes	yes	yes	yes
Observations	576	712	331	697	374
Panel C					
Household member injured (in Sierra Leone and Tajikistan) / abducted (in Uganda)	0.12*** (0.04)	0.08** (0.03)	0.43*** (0.07)	0.13*** (0.04)	0.10* (0.05)
Observable characteristics	yes	yes	yes	yes	yes
Village fixed effects	yes	yes	yes	yes	yes
Observations	577	712	331	697	374

Notes: OLS, standard errors in parentheses. ***Significance at the 1% level, ** at the 5% level and * at the 10% level. The Supplementary Methods provides definitions of the dependent variables and war exposure measures, as well as the full list of control variables for each country. The war exposure index in panel A has been standardized to a mean of zero and unit standard deviation, so the magnitudes are comparable across sites. Controls include variables related to age, sex, education, siblings, ethnicity and religious tradition. Uganda measures of exposure in panels B and C include killing or abduction of a friend, in addition to household members as in the other two countries.

Complementing our measure of religious membership, we also examined the effects of war experience on attending churches or mosques, a measure of religiosity available in both Uganda and Tajikistan. We find similar patterns (Table 2, columns 4–5): war-exposed individuals were more likely to report greater church attendance (Uganda) or mosque attendance in the week before the interview (Tajikistan). A unit increase in war exposure on any of our indices predicts an increase of between 7 and 13 percentile points in reports of ritual attendance except in Tajikistan in panel A, where the estimated effect drops to 3 percentile points ($P=0.13$).

Two supplementary analyses indicate that these results are robust for the particular questions included in our war exposure indices. First, in the Ugandan data we can break down the overall index into three distinct components—subindices of war-related violence: (1) received, (2) witnessed and (3) against family members. We find that each of these components positively predicts both of our religiosity measures (Supplementary Table 6) with very similar effect sizes ranging from 4 to 7 percentile points. Second, we use each of the available questions on war exposure in all three countries as an explanatory variable in a separate regression (Supplementary Tables 7–9). We find that the positive relationship between responses to questions on war exposure and religiosity is very systematic: 32 of the 35 estimated coefficients are positive (26 are significant statistically) while only 3 are negative and not significant statistically. Two of the three negative effects involve property damage as does one of the non-significant positive effects.

The observed effects of war on religiosity are similar for both Christians and Muslims. In fact, our findings among Ugandan Christians are strikingly parallel to those observed among Tajik Muslims. In Sierra Leone, where the sample contains 36% Christians and 63% Muslims, we separately estimated our main

regressions for each sub-sample. The results are qualitatively similar (Supplementary Table 16), although the coefficients are not always accurately estimated due to the reduced sample sizes.

Overall, in this first section of the results, we have provided evidence establishing a link between war and both religious engagement and ritual participation.

The findings above provide support for the war–sociality hypothesis³⁵, but is there evidence for the war–religion hypothesis? To examine this, we performed four analyses. First, we reran the regressions from Table 2 (columns 1–3), but now controlling for the total number of non-religious group memberships (Table 3). Comparing the coefficients in Tables 2 and 3 reveals that holding constant the impact of joining other non-religious groups has little impact on the relationship between war and religiosity. This result suggests that those exposed to war are still more likely to join religious groups even if they've already joined other non-religious groups.

To confirm the centrality of religious groups, we classified individuals according to their reported group participation into four types (four binary variables): (1) being a member of a religious group only, (2) being a member of a religious group as well as other groups, (3) being a member of non-religious groups only and (4) not being a member of any group. The results displayed in Supplementary Table 12 show that individuals with more war exposure are more likely to be members of a religious group, which may be only religious memberships as in Tajikistan or in combination with other groups as in Uganda and Sierra Leone. By contrast, those with greater war exposure are not more likely to be members of non-religious groups only or in no groups at all. In fact, war exposure often predicts that individuals will be less likely to be in only non-religious groups or no groups at all.

Table 3 | War exposure and membership in a religious group, now controlling for the number of other group memberships

Dependent variable	Member of a religious group		
	Sierra Leone	Uganda	Tajikistan
Country	(1)	(2)	(3)
Panel A			
War exposure index	0.05** (0.02)	0.06*** (0.01)	0.16*** (0.03)
Number of other group memberships	0.08*** (0.02)	0.05*** (0.01)	0.07** (0.03)
Observable characteristics	yes	yes	yes
Village fixed effects	yes	yes	yes
Observations	575	708	283
Panel B			
Household member killed	0.05 (0.04)	0.07** (0.03)	0.41*** (0.07)
Number of other group memberships	0.09*** (0.02)	0.05*** (0.01)	0.07* (0.03)
Observable characteristics	yes	yes	yes
Village fixed effects	yes	yes	yes
Observations	576	712	284
Panel C			
Household member injured (in Sierra Leone and Tajikistan) / abducted (in Uganda)	0.11*** (0.04)	0.07** (0.03)	0.40*** (0.08)
Number of other group memberships	0.08*** (0.02)	0.05*** (0.01)	0.06* (0.03)
Observable characteristics	yes	yes	yes
Village fixed effects	yes	yes	yes
Observations	577	712	284

Notes: OLS, standard errors in parentheses. ***Significance at the 1% level, ** at the 5% level and * at the 10% level.

Third, we separately regressed memberships in each of the prevalent types of groups in each country, from social clubs and savings groups to village committees and parent–teacher associations, on our three primary war exposure measures (Supplementary Tables 13–15). Compared with non-religious groups, the results reveal that war exposure most consistently increases membership in specifically religious groups, not sports teams, peace clubs, neighbourhood associations or other non-religious groups.

Finally, using the more detailed Ugandan data, we examined the relationship between our war exposure variables and whether individuals reported that a religious group was their closest or most valued group membership (Supplementary Table 11). Indeed, a standard deviation increase in war exposure predicts an increase of 3 percentile points in a person's chances of picking a religious group as their closest or most valued.

Taken together, these analyses support the hypothesis that war has special effects on religious engagement and ritual participation beyond its impact on more general sociality and social group memberships. Religious groups are indeed special kinds of groups.

Our hypotheses propose that war exposure creates psychological effects that increase people's inclinations towards social engagement and religious participation. The most important concern that might jeopardize the effects we've estimated from our natural experiment arises from the potential for endogenous selection—for example, something associated with people's religiosity led them to experience greater war exposure. To address this, in the main analysis, we controlled for a long list of observable characteristics—characteristics that could have led to greater war exposure. Nevertheless, although these control variables provide a substantial amount of information about individuals and households, the possibility of some omitted variable bias remains.

A series of additional analyses supports the interpretation that the estimated relationship between war and religiosity is causal (see further discussion on inferring causality in the Supplementary Discussion). We first gauge how much the importance of unobservable variables would need to be, relative to observable factors, to explain away the entire effect of war exposure on religiosity. In particular, we computed Altonji ratios⁵⁵, which compare how much the coefficient on the variables of interest (our war exposure measures) declines when we add additional variables to the model. Overall, the results indicate that it is unlikely that omitted variable bias could account for the full effect (Supplementary Table 17).

To further address this endogenous selection issue, we used an alternative strategy that involves estimating the effects for sub-samples in which there are fewer reasons to worry about the selective targeting of violence. In all three countries, we estimated the effects among individuals who were too young to have been community leaders before the war and thus less likely to be singled out for targeted violence. The effects hold for the sub-sample of younger individuals (less than 19 years old at the start of the war), providing further support that selective targeting is unlikely to be driving the effect (Supplementary Table 18). Finally, to account for possible selection due to migration, we estimate the effects among sub-samples of individuals who lived in the same village before and after the conflict. The estimated effects are very similar for the sub-samples of non-migrants as for the whole sample (Supplementary Table 19).

Taken together—the careful selection of natural experiments, the statistical checks to verify quasirandom assignment to war exposure, the use of fixed effects to compare only individuals within the same community, the inclusion of extensive observable characteristics and the above checks on endogenous selection—these analyses substantially mitigate most of the concerns associated with interpreting correlational analyses as causal. However, despite these efforts, we cannot fully eliminate the concern about bias from selection and omitted variables, and future research should focus on addressing this issue by gathering data on religiosity both pre- and post-conflict.

We find that people who have experienced more war-related violence participate more in religious groups and ritual events, using data from three diverse post-conflict societies. These effects on religiosity persist even 5, 8 and 13 years post-conflict and hold for both Christians and Muslims. Our results have implications for understanding the relationship between war, religion and the evolution of complex societies^{3,45,56} as well as for designing policies aimed at stifling religiously motivated or rationalized violence⁶. In particular, our findings take an important step towards establishing a causal pathway between war and religiosity. In the light of existing evidence linking certain religious beliefs and rituals to more intensive forms of cooperation in larger groups^{1–9}, the existence of this war–religion pathway closes a potential feedback loop in which war fuels greater religiosity and then religiosity fosters stronger forms of parochial cooperation that may catalyse ongoing cycles of violent conflict as well as forms of non-violent resistance.

To measure religiosity, we relied primarily on self-reports of (1) religious membership and (2) ritual attendance as well as on the importance of religious memberships. These measures do not tell us anything directly about people's religious beliefs or supernatural commitments, so future work should deploy an extensive array of measures of religious belief, personal devotion, such as prayer frequency, and communal ritual participation. Nevertheless, a growing body of work suggests that ritual participation, especially during childhood, is associated with deeper religious faith and a belief in God later in life^{57–61}. This implies that even if all conflict exposure does is increase people's attendance at rituals, it can still have long-term impacts on religious beliefs because of how rituals have culturally evolved to effectively instil beliefs and supernatural commitments^{62,63}. Notably, our results reveal that war impacts children's religious engagement and ritual attendance at least as much as adults', and war-exposed adults who more frequently attend religious events and rituals may bring their own children. Thus, there's reason to suspect that war's effect on religiosity will extend to religious beliefs, particularly in the long run.

Recent studies have shown that exposure to conflict intensifies local collective action, sharing in behavioural experiments and participation in local groups^{35,36}. Extending this previous work on the war–sociality hypothesis, our findings show that war impacts not only parochial sociality and norm adherence but also religious engagement and ritual participation. This is important, because although a combination of elevated religiosity, stronger group orientation and greater adherence to cooperative norms may facilitate social cohesion and post-conflict reconstruction at a local level, it may, at the same time, increase the risk of future group conflicts given their persistence. In the Tajik case, exposure to conflict has not only increased participation in local community meetings, associations and, especially, religious groups, but it has also eroded support for market liberalization and democratic reform^{53,64}. In Uganda, conflict reduced interethnic trust and trade⁶⁵. Simply by increasing ritual attendance, war may fuel intergroup hostility. Previous research in six countries suggests that ritual attendance, but not personal prayer, is associated with out-group hostility and support for suicide attacks⁶⁶. To empirically reconcile this with the evidence that religion promotes less prejudice^{15–17}, we would need to attend to the factors that maintain violence and rule out the possibility that religious mediators are actually present beyond their facilitation of parochial solidarity.

For the war–religion hypothesis, our findings are consistent with a host of proximate psychological mechanisms that can explain why war exposure causes people to become more religious. War may create lasting trauma, intensify uncertainty, and highlight thoughts of death. Previous research suggests that religious beliefs and practices can, in turn, help individuals to cope with such conditions. For instance, the 'ritual uncertainty hypothesis' argues that individuals will be attracted to engaging in arbitrary, stereotyped ritualistic behaviour as a way to cope with conditions of duress and unpredictability, particularly when stakes are high. In addition to religion, this use of ritualized behaviour to exert some control on individuals' contexts has also been found in a variety of domains including—but not limited to—athletics, academic examinations and gambling^{40,67–72}. Similarly, those suffering from war-induced trauma may be attracted to religious groups insofar as religious rituals, coupled with the prosocial benefits and comforting supernatural beliefs, may provide a context to counter the apparent lack of control that stems from warfare. Similarly, terror management theory postulates that religious beliefs and practices may mitigate the existential anxiety associated with fear of death, low self-esteem, and a threatened worldview^{46,73,74}. People who are more religious were found to be more resilient to thinking about death⁷⁴. Religious attendance was shown to increase in the aftermath of September 11th⁷⁵, and psychological experiments suggest that

awareness of mortality increases belief in religious notions, even when those notions involve culturally unfamiliar supernatural agents⁷⁶. All these mechanisms—conditions of trauma, mortality salience, uncertainty and the need for social support—may help explain the psychological attraction towards certain religious beliefs and practices induced by war.

Further, an interesting direction for future research is to explore potential moderators of the war–religiosity effects, based on personality differences. For example, some laboratory experiments suggest that mortality salience affects attitudes to immortality, but only among individuals with secure social attachments⁷⁷. Estimating whether similar moderating effects exist in the context of an actual violent conflict would require researchers to collect prewar measures of personality and security of social relations, which were not gathered in the surveys we analysed.

Now, it is conceivable that the relationships between war and religion observed across our three sites arise in part from the actions of religious organizations—both Christian and Muslim—to specifically target individuals who have experienced war-related trauma. Remember, however, that we are comparing individuals in the same communities, so the effects cannot be due to targeting war-torn areas in general. Nevertheless, even if this were the case, the question remains as to why religions are so effective, especially over the long term, compared with political, economic and social organizations who are also vying for memberships and active participation. Something about both Christian and Muslim communities at all three sites makes them particularly effective at attracting and keeping those most afflicted by war in the wake of violent conflict.

Consistent with these patterns, the effects of war on religiosity seem to be enduring, as our measures capture religiosity five (Uganda), eight (Sierra Leone) and thirteen (Tajikistan) years post-conflict. These findings are further informed by two other recent studies. In Sierra Leone, based on data collected only 3–5 years after the civil war, exposure to war-related violence was positively related to both religious group membership and ritual attendance (church/mosque), though the effect on group membership was not significant at conventional levels⁵¹. Meanwhile, in Northern Uganda, data gathered shortly before the end of the conflict with the LRA reveals no systematic link between violence exposure and measures of religiosity⁷⁸.

Taken together, these patterns suggest that the effects of war on elevated religiosity are enduring, but may only emerge gradually and strengthen over time after the conflict. One possibility is that once the fighting ends, people are motivated to seek out social groups in general, but only gradually through trial and error do they increasingly sort themselves preferentially into religious groups. Notably, in Tajikistan where the time since the end of the conflict was the longest, we found not only the strongest effects of war exposure on religion, but that the effects of war were much larger than on any of the non-religious groups (Supplementary Table 15). Future research should examine this using longitudinal methods.

In conclusion, our results suggest that the experience of war-related violence increases religious engagement and ritual participation. The potential existence of these relationships has important theoretical, political and social implications. Theoretically, the results support the idea that in addition to a host of other factors, both religion and warfare have played an important role in the development of larger-scale social organizations and the expansion of human communities⁴⁵. If intergroup conflict increases religious commitment, and at least some kinds of religious beliefs and rituals extend parochial prosociality while galvanizing social solidarity in ways that foster success in intergroup competition, then the ingredients exist for a feedback loop that will drive the cultural evolution of religions while scaling

up human societies⁵⁶. At a policy level, this research points in the opposite direction to those approaches emphasizing material costs and rational choice^{79–81}. Instead, it suggests that combatting religiously motivated, rationalized or justified violence with war will further solidify people's commitment to religious groups, and thus further catalyse the parochial solidarity and defence of sacred values that fuels so many conflicts^{2,9,79}. Thus, even if claims that religion generally promotes violence are true^{82–84} and the evidence showing the exact opposite is deeply flawed^{15–17}, our results caution against inciting ongoing cycles of violence with aggressive action.

Methods

To explore these hypotheses, we located three potential natural experiments in post-conflict societies, where individuals were, by chance, exposed to differing intensities or 'dosages' of war. We took advantage of microlevel data sets collected in these societies and used in recently published papers that did not explore religion^{36,53,54}. At each of these carefully selected sites, previous qualitative assessments and quantitative analyses^{51,53,54} have made a strong case that individuals—after controlling for a set of potential confounds—were effectively quasirandomly assigned to varying intensity levels of war victimization. To further verify this, Supplementary Tables 1–3 show that an extensive list of prewar characteristics, including age, sex, family composition and socio-economic status, mostly do not predict affliction by war. The few correlates that do show some significance within sites are: number of sisters in Sierra Leone, female and age in Uganda, and education and residence in Tajikistan. To account for this possible, albeit small, endogenous selection into victimization, we hold constant that same list of prewar individual and household characteristics in the main analysis where we regress religiosity on war experience. Furthermore, by including various fixed effects for people's community of residence, we eliminate any variation in war exposure across villages, ethnicities and religions, so we only need variation in war-affliction to be random within narrow localities—within villages, ethnic groups and religions. To the degree that these conditions hold, these conflicts provide natural experiments that allow us to go beyond correlation and reveal a plausible causal effect of experiencing war—as assessed by these surveys—on our measures of religious engagement and ritual participation.

Each study was designed to elicit locally salient forms of victimization. As explanatory or 'treatment' variables, our primary analyses used three measures of war exposure. For the first, we constructed an overall index of war exposure—our war exposure index—that uses all of the available information for each individual by summing the dichotomous answers to all available questions on war-violence (experienced, witnessed or perpetrated) and loss of property. There are three such questions in Tajikistan, five in Sierra Leone, and twelve in Uganda (see Supplementary Methods). The index was standardized to have a mean of zero and unit standard deviation. For the second measure, we created a variable directly comparable across sites by classifying individuals into two categories of victimization based on a survey question that asked whether any household members (in Sierra Leone and Tajikistan) or family members or friends (in Uganda) were killed during the conflict. For the third measure, we created a variable, semicomparable across sites, that classified respondents based on a question that asked whether any household members were injured during the conflict (in Sierra Leone and Tajikistan) or whether a family member or friend had disappeared (in Uganda). Finally, we complemented these three primary measures by conducting analyses using every site-specific question on war exposure as a separate explanatory variable.

For our primary outcome measure, we studied two different variables. Our first measure of religiosity—available in all three data sets—is membership in a religious group, each is simply coded as a one if a respondent reported being a member of a religious group and zero if not. Our second measure of religiosity is ritual attendance, at churches or mosques, which is available only in the Ugandan and Tajikistan data sets. In Uganda, participants were asked whether they attended church often, and in Tajikistan, whether they had gone to the mosque the previous week. Code for analysis is available in the Supplementary software at <https://github.com/bgpurzycki/Religion-and-Violence>.

This study uses only previously published and publically available data. The procedures for data collection in Sierra Leone and Tajikistan were approved by the Institutional Review Board (IRB) for the Protection of Human Subjects at the University of San Francisco. The data collection in Uganda was approved by the Director of the Institute of Economic Studies at the Faculty of Social Sciences, Charles University in Prague, as the institutions that Bauer (Fiala and Levely) were affiliated with during the data collection do not have IRBs. In all three studies, participation was voluntary and subjects could leave at any time.

Reporting Summary. Further information on research design is available in the Nature Research Reporting Summary linked to this article.

Code availability

All code files for a complete reproduction of the analyses herein are available at: <https://github.com/bgpurzycki/Religion-and-Violence>.

Data availability

All data and analytical scripts are available at: <https://github.com/bgpurzycki/Religion-and-Violence>.

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Author contributions

M.B., J.C. and J.H. conceived the study and initiated manuscript preparation. M.B., A.C. and J.C. collected data (M.B. acknowledges N. Fiala and I. Levely as collaborators in the Uganda project, A.C. acknowledges P. Grosjean and S. Whitt as collaborators in the Tajik project). M.B., J.C. and B.P. conducted analysis. M.B., A.C., J.C., J.H. and B.P. contributed to preparation of the manuscript, J.H. and B.P. had a lead role.

Competing interests

The authors declare no competing interests.

Additional information

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Study description	Quantitative survey data
Research sample	1,709 individuals from 71 villages from three post-conflict societies—Uganda, Sierra Leone and Tajikistan. In Uganda, just five years after a peace was initiated with Kony, a sample of individuals aged 18-55 was recruited from 33 villages. Virtually the entire sample was nominally Christian, composed of a mix of Catholics, Anglicans, Evangelicals and Pentecostals. In Sierra Leone, about eight years post-conflict, 21 villages were selected and surveyed based on preliminary data indicating the existence of substantial variation in war exposure within each community. This sample contains both Christians (36%) and Muslims (63%). In Tajikistan, roughly thirteen years after the conflict's tenuous end, participants were recruited from 17 villages scattered across the country using a multi-stage sampling approach. Virtually all participants were Muslims (97%), most of them Sunnis (87%). See Table 1 in the main paper and the Supplementary Methods for further in depth details about sampling at each site.
Sampling strategy	<p>The Sierra Leone data collection took place in the Bombali district, which is located in the Centre-North of Sierra Leone. The data was collected in 2010 in 21 villages selected based on existing evidence indicated substantial variation in war exposure (Bellows & Miguel, 2009). In each village, the data collection was organized in cooperation with a local school. Parents or guardians of students from randomly selected classes were interviewed by trained enumerators during survey meetings at local schools. The sample are adults across a diverse age range (18-84). In the analysis, we use 584 observations from the original dataset (n = 586) for which measures of both war exposure and religiosity are available.</p> <p>The Uganda data collection took place in rural areas of Gulu and Kitgum districts in Northern Uganda in 2011. The data contains a representative sample of individuals aged 35-55 (n = 373) and a random sample of males aged 18-34 (n = 343), the range most likely to include LRA ex-soldiers/abductees. The sampling started with a list of communities known to be affected by LRA abduction. A random sub-set of 33 villages was selected out of 52 villages in which at least 20 ex-abductees were living. In each village, 40 households were randomly selected from a village roster of all households and a member of each household was invited to participate in a short pre-survey. Using the information from the pre-survey, a list of individuals and their characteristics was compiled. In each village, on average 15 individuals aged 35-55 were randomly selected to participate in a detailed survey. The younger participants were randomly sampled from the pool of men aged 18-34 and former soldiers were oversampled. The participants were interviewed in private by trained enumerators during survey meetings that typically took place in local schools. In the analysis, we pool both datasets and use 713 observations from the original dataset (n = 721) for which measures of both war exposure and religiosity are available.</p> <p>The Tajikistan data collection took place in Dushanbe, Khatlon, Gharm and Pamir regions in Tajikistan in 2010. The subjects were selected using a multi-stage sampling method in 17 villages. In Dushanbe, Pamir and Gharm, the selection of villages was made at random with probability of selection proportional to population size. Villages in Gharm were chosen at random within the sub-stratum of the Rasht Valley. Sampling was based on the latest available census data of Tajikistan. On arriving at the sampling point, each enumerator was randomly assigned a starting point within the town or village from which she followed the standard "random route" technique, starting with 5th numbered apartment building or house selecting every 5th entrance. Individual respondents (one per household) were chosen using a random selection key (a 12-face die) where every adult member of the household had an equal probability of being selected. In each village, all recruitment of subjects and data collection was conducted on the same day using a team of trained enumerators. Most of the subjects were interviewed privately in their home by a local enumerator. In cases where the home environment was not sufficiently private or accommodating, subjects were interviewed outdoors or at another private location. In the analysis, we use 412 observations from the original dataset (n = 426) for which measures of both war exposure and religiosity are available.</p>
Data collection	The instruments used to collect the data were paper and pencil. In addition to the subjects and the experimenters, local enumerators were present during the sessions.
Timing	The data were collected May-September 2010 in Sierra Leone and Tajikistan and 2011 in Uganda.
Data exclusions	<p>We didn't exclude any of the collected data, but, given that we allowed the subjects not to answer questions if they felt uncomfortable doing so, our datasets, depending on the econometric model specifications, ended up with a few observations less at each site because data on war exposure and religiosity were not available (subject refused to answer). In particular:</p> <p>For Sierra Leone we had in the original dataset 586 observations, but in the analysis we use 584 observations because for 2 subjects we didn't have both measures of war exposure and religiosity. For Uganda, we used 713 observations from the original dataset (n = 721) as for 8 subjects we didn't have both measures of war exposure and religiosity. For Tajikistan, in the analysis we use 412 observations from the original dataset (n = 426) as for 14 subjects we didn't have both measures of war exposure and religiosity.</p>
Non-participation	2 subjects in Sierra Leone, 8 subjects in Uganda, 14 subjects in Tajikistan didn't answer either one or more of the questions about their personal experience with war victimization exposure or about religiosity. To protect our subjects, each survey question contained, as answer, the possibility of not answering (if preferred).

Randomization

Our main explanatory variable is war exposure. Given its nature, it was not randomly assigned to subjects by the experimenter but we tried to assess to the best of our capabilities to what extent it could be treated as the result of a natural experiment. Taken together—the careful selection of natural experiments, the statistical checks to verify quasi-random assignment to war exposures, the use of fixed effects to compare only individuals within the same community, the inclusion of extensive observable characteristics and the checks on endogenous selection—these analyses substantially mitigate most of the concerns associated with interpreting correlational analyses as causal. However, despite these efforts, we cannot fully eliminate the concern about bias from selection and omitted variables, a job we leave for future research.

Reporting for specific materials, systems and methods

Materials & experimental systems

n/a	Involvement in the study
<input checked="" type="checkbox"/>	<input type="checkbox"/> Unique biological materials
<input checked="" type="checkbox"/>	<input type="checkbox"/> Antibodies
<input checked="" type="checkbox"/>	<input type="checkbox"/> Eukaryotic cell lines
<input checked="" type="checkbox"/>	<input type="checkbox"/> Palaeontology
<input checked="" type="checkbox"/>	<input type="checkbox"/> Animals and other organisms
<input type="checkbox"/>	<input checked="" type="checkbox"/> Human research participants

Methods

n/a	Involvement in the study
<input checked="" type="checkbox"/>	<input type="checkbox"/> ChIP-seq
<input checked="" type="checkbox"/>	<input type="checkbox"/> Flow cytometry
<input checked="" type="checkbox"/>	<input type="checkbox"/> MRI-based neuroimaging

Human research participants

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Population characteristics

See above (Research Sample). In addition, with regards to gender and age: Sierra Leone sample was 71% female, mean age 41 (18-84 range); Uganda was 29% female, mean age 34 (18-55 range); Tajikistan was 71% female, mean age 40 (17-77 range).

Recruitment

See above (Sampling Strategy).