The social and political consequences of wartime sexual violence

New evidence from list experiments in three conflict-affected populations

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Abstract: Wartime sexual violence is widespread across conflict zones and thought to leave a disastrous legacy for survivors, communities, and nations. Yet, systematic studies on i) the prevalence and ii) the social and political consequences of wartime sexual violence are fraught with severe data limitations. Based on individual-level survey evidence from three conflict-affected populations in Democratic Republic of Congo, Liberia, and Sri Lanka, we make two contributions. First, we demonstrate the potential of list experiments for overcoming under-reporting bias and estimating population-based prevalence rates of sexual violence. Second, we estimate the effect of sexual violence on key outcomes of social and political development: civic participation, interethnic relations, and political trust. Across all three populations, exposure to wartime sexual violence increases civic participation. While interethnic relations remain largely unaffected, the impact on political trust varies across contexts. This cumulative evidence suggests that survivors are more resilient to wartime sexual violence than acknowledged in prior research and policy interventions.

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Conflict-related sexual violence is an issue of pressing importance. This grave human rights abuse is as destructive as any bomb or bullet. [...] It *destroys families and communities and tears the social fabric of nations*.

Former UN Secretary-General Ban Ki-Moon (UN 2014, emphasis added)

Conflict-related sexual violence *remains underreported* owing to a lack of trust in the justice system, a fear of reprisals, pressure from family members, and stigma [...].

Current UN Secretary-General António Guterres (UN 2021, emphasis added)

1   Introduction

Wartime sexual violence is a widespread human rights violation in conflict zones around the world. A recent report by the Peace Research Institute Oslo documents that out of 53 ongoing armed conflicts in 2019, more than 20 involved sexual violence perpetrated by armed actors, putting millions of girls and women but also boys and men at risk. Since 2000, the share of conflicts with sexual violence has at times reached 90 per cent of all yearly conflicts (Nagel et al. 2021). This magnitude explains why campaigns and policies against sexual violence are central to international institutions, foreign policy, and human rights activists.

Broadly defined, wartime sexual violence refers to rape and sexual abuse by armies, rebel groups, and militias in the context of wars and armed conflicts (e.g. Wood 2015a). While scholarly interest in wartime sexual violence has significantly increased from fewer than five publications in 2001 to more than 100 in 2019 (Nordås and Cohen 2021) and now spans across several disciplines including political science, psychology, sociology, and public health, severe data limitations persist. As a result both i) the prevalence of wartime sexual violence among conflict-affected populations and ii) its social and political consequences remain poorly understood.[1]

We address these challenges by drawing on three surveys with more than 10,000 respondents from three conflict-affected populations: Democratic Republic of Congo (DRC), Liberia, and Sri Lanka. First, we estimate population-based prevalence rates of wartime sexual violence, taking into account that responses to sensitive survey questions on sexual violence are likely to be under-reported due to stigmatization. To that end, we measure sexual violence in two distinct ways: i) a conventional direct question which is prone to non-disclosure, and ii) an indirect, unobtrusive technique known as list experiment which grants respondents anonymity when answering sensitive questions. Second, we assess the mid-to long-term social and political consequences of wartime sexual violence, in particular how sexual violence victimization affects three central concepts in the social sciences and of particular relevance to social and political development: i) civic engagement, ii) interethnic relations, and iii) trust in political institutions.

Our findings provide substantively new insights and promote the use of our conceptual and methodological framework for future research in conflict-affected populations. First, in terms of prevalence rates, we find that the direct questions on wartime sexual violence suggest population-level prevalence rates of 1 per cent in Sri Lanka, 5 per cent in Liberia, and 6 per cent in DRC. However, the list experiment results in prevalence rates twice to ten times higher. This suggests that this technique grants sufficient anonymity to address social desirability bias which we interpret as compelling evidence that the list experiment outperforms naive direct questions in terms of measurement validity.

Second, we use the list experiment and the naive direct question as predictors to assess the effect of wartime sexual violence on key social and political outcomes at the individual level. Using the list

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[1] In this article we use the terms wartime sexual violence, conflict-related sexual violence, and sexual violence interchangeably.

experiment measure of sexual violence, we come to the following conclusions. Across all three populations, we find that sexual violence victimization significantly increases civic engagement.[2] We do not find any effect of sexual violence on interethnic relations, and the impact on political trust varies considerably across the three populations. The bias-prone conventional direct question produces substantively different results that we discuss in more detail in the empirical section.

Despite the horrific nature of wartime sexual violence and the adverse impact on psychological well-being, our findings provide an alternative perspective to conventional wisdom which has been suggesting that survivors of wartime sexual violence are socially excluded and marginalized (Albutt et al. 2017; Johnson et al. 2010; Kelly et al. 2012). What we find reflects, despite stigmatization, a more optimistic outlook that taps into recent debates on women's mobilization (Berry 2018; Kreft 2019), social resilience (Koos 2018), and network effects (González and Traunmüller 2020) as a response to sexual violence in conflict. Our findings thereby suggest that social resilience and political mobilization may occur alongside adverse psychological effects.

Our findings contribute to two influential literature strands. First and primarily, we expand the comparative literature on sexual violence in civil wars (Cohen 2013; Koos 2018; Kreft 2019; Leiby 2009; Traunmüller et al. 2019; Wood 2006, 2009) by providing rare micro-level evidence on the social and political consequences, an area that has received only scant attention due to a lack of data (for exceptions, see González and Traunmüller 2020; Koos 2018[3]). Furthermore, our findings complement the qualitative literature, which has provided detailed accounts of survivors' struggles but is neither designed nor intended to uncover systematic and representative relational patterns of sexual violence and its social and political impacts (e.g., Kelly et al. 2012; Sideris 2003; Skjelsbaek 2006; Yohani 2014).

Second, our article extends and adds important nuance to the growing literature on the legacy of violence in civil wars (Bauer et al. 2016; Blattman 2009; De Juan and Pierskalla 2016; Gates and Justesen 2020; Hager et al. 2019; Koos 2017b; Krakowski 2020; Schaub 2017). We show that taking into account sexual violence as a distinct form of violence while controlling for other types of violence is of crucial importance (Gutiérrez-Sanín and Wood 2017). In fact, across our models, it is often only sexual violence that significantly affects our outcomes, while other experiences of war-related violence and trauma remain insignificant. This raises new questions regarding the underlying mechanisms and processes that underpin the relationship between exposure to violence and social and political attitudes and behaviour.

Finally, our article has important methodological implications for policy makers and practitioners. The list experiment is a relatively easy-to-implement survey measure to overcome social desirability bias in public health surveys (e.g., Demographic Health Surveys). It can be easily adapted and used to improve the study of other sensitive questions such as intimate partner violence, early marriages, female genital mutilation, and discriminatory attitudes. By including list experiments in public health and demographic surveys, policies and humanitarian interventions can be targeted more effectively to vulnerable populations.

1.1 Civil war violence affects individual attitudes and preferences

The fact that exposure to violence in civil wars impacts the mid- and the long-term preferences and behaviours of individuals, families, and communities is well documented. Studies have shown that people's direct or indirect exposure to violence increases social cohesion and civic engagement towards

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[2] While we do not claim this effect to be causal, we use pre-exposure measures of social inclusion to reduce the risk of reverse causality and perform sensitivity analyses to quantify and discuss the size a potential confounder would need to have to overturn the results.

[3] González and Traunmüller (2020) is available as a working paper at the time of submission.

their in-group (Bauer et al. 2016; Koos 2018; Krakowski 2020; Schaub 2017) but aggravates intergroup relations,[4] at least in the short term (Cecchi et al. 2016). Over time and through contact, perceptions of out-groups have been shown to improve (Mironova and Whitt 2016; Mousa 2020; Scacco and Warren 2018), but distrust and negative sentiments towards political institutions often remain (De Juan and Pierskalla 2016; Gates and Justesen 2020; Koos 2017b). Several mechanisms have been proposed to explain these observed relationships, including post-traumatic growth (Blattman 2009), social compensation (Koos 2018), and distrust in the state due to its specific targeting of communities (De Juan and Pierskalla 2016).

This literature has provided new insights on how violence affects individuals. However, one critical limitation is that this literature focuses largely on experiences of lethal violence but does not distinguish between different—in particular non-lethal—forms of violence against civilians. This matters, as Gutiérrez-Sanín and Wood (2017) argue that a focus on lethal violence fails to uncover the heterogeneity by which people are affected by violence, and arguable how it affects social and political outcomes. One particularly common and horrific form of violence is wartime rape. For instance, Nordås (2011) finds that mass rape often occurs in the absence of mass killings in civil wars in Africa. Similarly, Koos (2018) documents that the prevalence of killings and sexual violence in the civil war in Sierra Leone differs distinctly across communities and over time. Hence, it is plausible to assume that non-lethal violence such as wartime rape has largely not been accounted for in prior quantitative research. As we will discuss, wartime sexual violence is assumed to have different, largely adverse consequences for survivors and communities, including stigmatization and social marginalization.

1.2   What do we know about the consequences of wartime sexual violence?

Psychological studies have documented that both female and male sexual violence survivors experience elevated levels of post-traumatic stress disorder, anxiety, and depression (Amowitz et al. 2002; Dumke et al. 2021; Johnson et al. 2010; Peterman et al. 2011). According to qualitative studies, this also holds for husbands and men in a community, because they experience a sense of failure in their hetero-normative role as protectors (Kelly et al. 2012; Woldetsadik 2018). Moreover, several qualitative studies find that fear of sexually transmitted diseases, children born of rape, and broken norms of sexual fidelity increase stigmatization (Koos and Lindsey 2021) and hinder the acceptance and integration of sexual violence survivors (e.g., Albutt et al. 2017; Kelly et al. 2011; Mukamana and Brysiewicz 2008; Sideris 2003).[5] These perceptions and social norms also hinder post-conflict reconciliation at the local community level and signal that 'life together is finished' (Hayden 2000: 32).

While the relationship between sexual violence and mental health is based on population-based data, it is important to note that most studies on survivors' social integration and marginalization are based on small convenience samples from hospitals and psychosocial support programmes. The possibility of sample selection bias by excluding survivors who received support from their families and communities and did not seek institutional support may therefore bias the findings towards claims of survivors' exclusion and marginalization (Cohen and Hoover Green 2012; Human Security Report Project 2012; Koos 2017a).

Another set of studies uses country-level regressions to assess the political effects of wartime sexual violence, in particular based on the Sexual Violence in Armed Conflict Dataset (SVAC) (Cohen and Nordås 2014). SVAC is based on human rights reports and codes the use of sexual violence for a defined list of armed actors into four distinct categories (none, isolated, numerous, massive). Using SVAC,

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[4] An exception is a study by Hager et al. (2019) which finds reduced levels of prosocial behaviour towards in-group members in Kyrgyzstan.

[5] For an overview of this literature, see Koos (2017a).

Kreft (2019) has found that *countries* affected by sexual violence have a higher likelihood of having an internationally supported women's organization in the country, which she interprets as women's collective mobilization against sexual violence. Other studies argue that in countries in which armed groups perpetrate sexual violence, mediation processes with rebels are more likely (Nagel 2019). While SVAC data are an invaluable source to analyze armed groups' repertoires of violence, and how these change over time, it is not designed to understand the consequences for individual survivors and their communities.

1.3 Micro-level theory and evidence on wartime sexual violence

While sexual violence has been shown to increase stigmatization under specific conditions (Koos and Lindsey 2021), general research in psychology emphasizes notions of agency, resilience, and mobilization among people who experience stigmatization. Despite the impact of sexual violence on social stigmatization, sexual violence may therefore simultaneously affect social and political action through a number psychological and social response mechanisms. For instance, building on the psychological concept of *post-traumatic growth* (Calhoun et al. 2010), experiences of violence may transform what people value in their lives, such as social relationships or religion, and result in more civic and political engagement (see also Blattman 2009). Similarly, *social resilience* as a response to overcoming stigmatization has been investigated extensively in social psychological research, and the evidence suggests increased civic and political engagement to be a response mechanism for group acceptance (Bonanno et al. 2011; Molden and Maner 2013; Shih 2004). Moreover, increased civic engagement can lead to knock-on effects when people increase their *social network* both within and beyond their community (Berry 2018; Wood 2015b). As a result, survivors' associations can expand social networks, form new group identities to collectively fight against stigmatization, and can actively promote local peace building and conflict resolution (Berry 2018; Masika Bihamba 2017; Koos 2018; Utas 2009).

Only two recent quantitative studies have assessed the impact of sexual violence using population-based data. Drawing on household-level data from post-conflict Sierra Leone, Koos (2018) documents that sexual violence survivors and their families have engaged in significantly higher levels of prosocial behaviour in their communities to avert stigmatization and social marginalization. In a working paper, González and Traunmüller (2020) show that individual experiences of sexual assault during the Sri Lankan civil war resulted in higher levels of political participation.

While existing population-based studies address important shortcomings, in particular systematic sample selection bias in small-N studies and aggregation bias in cross-country analyses, three limitations remain. First, micro-level studies largely rely on direct survey questions to measure whether people have experienced sexual violence during war (Amowitz et al. 2002; Johnson et al. 2010; Koos 2018; Østby et al. 2019; Peterman et al. 2011; an exception is González and Traunmüller 2020). However, questions on sensitive behaviour or experiences like sexual violence are typically subject to non-disclosure and may therefore lead to biased estimates (Traunmüller et al. 2019). To remedy this limitation, we adapt list experiments to the study of wartime sexual violence, an unobtrusive measure that reduces the risk of social desirability bias by protecting respondents' anonymity. We describe the logic of list experiments and their application to our focus of study in the methods section.

Second, the limited quantitative micro-level evidence on the social and political impact focuses on single country cases. What we do not know is how context-specific or generalizable these findings are. To that end, we present evidence based on original survey data from three post-conflict contexts in one coherent conceptual and empirical framework. This allows us to asses common and diverging patterns of the social and political impact of wartime sexual violence across a diverse set of cases.

Third, existing quantitative studies are limited to estimating the effect of wartime sexual violence on single outcome categories. In this article we analyze a wider set of key outcomes that matter for post-

conflict reconstruction, reconciliation, and nation-building including i) civic participation, ii) interethnic relations, and iii) people's trust in political institutions. All these outcomes reflect central social and political attitudes of citizens. According to the established literature on wartime sexual violence, we expect negative effects of sexual violence due to stigmatization, social exclusion, interethnic conflict, and the absence of the state in providing security.

2   Methods

2.1   The surveys and the study context

Population-based data on wartime sexual violence are scarce.[6] Therefore, our empirical design builds on original survey experiments from a diverse set of conflict-affected populations: DRC, Liberia, and Sri Lanka.[7] All three country cases are well known for sexual violence against civilians during their respective conflict periods. Where these cases differ is with regard to region (West Africa, Central Africa, South Asia), differences in conflict termination, conflict dimensions, and the identity of perpetrators (state vs. non-state actors).

We implemented the DRC survey as part of a research project in 2017 and sought to explicitly examine the social and political consequences of wartime sexual violence. The DRC survey builds on a representative sample of 1,000 respondents in 100 villages in South Kivu in eastern DRC, [8] a hotspot of violence including sexual violence during the past 20 years. We collected the Liberia survey data in the context of a baseline survey of a randomized controlled trial (RCT) for a post-war reconstruction programme in south-east Liberia, specifically the counties of Grand Geddeh, River Gee, and Maryland, together with Welthungerhilfe, a German non-governmental organization (NGO). The data from 7,500 respondents in 121 villages in south-east Liberia were collected in 2019, 16 years after the end of the civil war.[9] We collected the Sri Lanka survey as part of a research project in 2016 together with the University of Colombo,[10] just seven years after the civil war had ended, and included a random sample of 1,800 respondents from all 25 districts in Sri Lanka, including the Northern and Eastern province at the centre of the conflict. In total our analyses rely on more than 10,000 individual observations.

While all three cases share the history of a recent civil war with widespread violence against civilians including sexual violence, the time since the end of the war varies. Liberia's civil war ended in 2001, and the country had since then enjoyed a relatively peaceful transition with some episodes of violence near the border to Côte d'Ivoire. Sri Lanka's civil war ended in 2009, and the country has embarked on a relatively successful transition to stability and development. However, given that the government defeated the Liberation Tigers of Tamil Eelam (LTTE), the main rebel group, many war crimes against

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[6] Exceptions include psychological studies reviewed above (Amowitz et al. 2002; Johnson et al. 2010; Koos 2018; Peterman et al. 2011), but with the exception of Koos (2018), none of these studies assess the social and political consequences of wartime sexual violence. Moreover, none of these studies deal with non-disclosure of sexual violence due to stigmatization.

[7] The sampling protocol is available in Section E in the appendix.

[8] The data were collected by Research Initiative for Social Development, a survey organization in Bukavu, under the supervision of one of the authors. Other articles using these data but different key variables include De Juan and Koos (2019) and Koos and Lindsey (2021).

[9] The data used here have been collected prior to the programme intervention. We gratefully acknowledge the permission by Anselm Hager (Humboldt University Berlin), the co-principal investigator (Co-PI) of the RCT, for using these data. The Liberia data have not been used in other publications.

[10] We gratefully acknowledge the contribution of Markus Freitag (University of Bern) in the data collection and funding of the survey. A subset of this dataset has been used in González and Traunmüller (2020), Kijewski and Rapp (n.d.), Rapp et al. (2019), and Traunmüller et al. (2019).

Tamils remain unaddressed. The eastern DRC best qualifies as a contemporary conflict context. Rival non-state militias and local armed groups continue to fight over resources and land, involving violence against civilians and displacement.

2.2   Ethics

Empirical research on the prevalence of violence, sexual violence in particular, and its social and political implications in conflict-affected populations is not only important for basic research but also to provide evidence for policy makers and organizations who provide services and support for survivors and communities. Survey data collection with individual respondents requires particular safeguards to protect participants' well-being, interests, and rights. During the training of local survey enumerators, local and international practitioners and scholars with expertise in local gender politics and traditions have provided important input to contextualize and adapt the survey questions. During the survey implementation, as far as possible, female enumerators were interviewing female respondents and vice-versa to establish rapport. Moreover, the informed consent meant that enumerators informed respondents both in the beginning of the survey about the topics addressed (e.g. experiences of violence) and before questions on experiences of violence, asking respondents whether they would like to continue or abort. The research plan and survey design has been submitted for ethical assessment and given no objection by the institutional review boards.[11]

2.3   Measuring wartime sexual violence in surveys: direct questions and list experiments

Asking individuals whether they themselves or household members have been sexually assaulted is likely to result in under-reporting, because they may feel ashamed, stigmatized or fear consequences. We employ two techniques to measure sexual violence victimization in our surveys. The first is a direct naive question on victimization which we assume not every respondent will disclose to by responding 'yes'.[12]

Table 1 shows the question wording for the direct question in the three surveys. The differences in wording for these three questions arise from the fact that the surveys needed to be adapted to the local context. For instance, in DRC the presence of gender-related programmes made questions on sexual violence a comparatively insensitive topic (Quillard 2016), while in Liberia our local partners advised to frame the question less explicit since everyone knew that 'sexual violence during the civil war' meant rape by combatants. This was similar in Sri Lanka, where our partners proposed that sexual assault during the civil war was a synonym for rape or sexual torture by the army.

The questions differ in two main dimensions: (i) potential survivors and (ii) specification of sexual violence. In the DRC, respondents were specifically asked about whether they themselves or household members have been raped by armed groups. In Liberia, the question referred only to the respondent (not household members) and did not explicitly define sexual violence as rape by armed groups. Similarly in Sri Lanka, respondents were asked whether they themselves (not household members) were sexually assaulted. While the questions differ, they also reflect the importance of adapting such sensitive questions to local contexts. Broadly speaking, our impression has been that that in Liberia and Sri Lanka, questions regarding the civil war were more sensitive than in DRC where violent local conflicts have been ongoing for more than two decades.

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[11] University of Konstanz (8 Sept. 2015, ref. 14/2015), University of Liberia (20 Oct. 2020, ref. 20-10-236).

[12] Note, however, that when large-scale health and household surveys ask questions on sexual violence, this is the way they are typically asked (e.g., Amowitz et al. 2002; Johnson et al. 2010; Koos 2018; Peterman et al. 2011).

Table 1: Direct questions

DRC	Liberia	Sri Lanka
Have you or anyone else in your household ever been raped by armed groups since 2002, that is physically forced to have sexual intercourse?	Were you a victim of sexual violence during the civil war?	During the period of war, from 1983 to 2009, which of the following things did you personally directly experience, see or witness with your own eyes and ears, directed at you, your family, or community? You becoming sexually assaulted.

Source: authors' elaboration based on surveys mentioned in Section 2.1.

To address potential under-reporting bias to these direct questions, all three surveys included list experiments. List experiments are unobtrusive methods to indirectly measure sensitive attitudes, behaviour, or experiences by granting respondents anonymity (Blair and Imai 2012; Glynn 2013; Traunmüller et al. 2019). The reason why list experiments can elicit truthful answers relies on the fact that respondents are not asked about each experience individually, but only the sum of them. During the survey, each respondent was randomly assigned either to a control condition or a treatment condition. The respondent was then read the list of experiences[13] in Table 2. After all three (control group) or four (treatment group) experiences have been read, the respondent only reported the number of items experienced, not which ones.[14]

As with the direct questions, the list experiments—in particular the treatment items—reflect the differences discussed above in terms of (i) potential survivors and (ii) the specification of sexual violence. Importantly, the direct questions and the list experiment are consistent and aim to measure the same concept within each survey.

Table 2: List experiment

	DRC	Liberia	Sri Lanka
Control	(1) I moved away from my original place of birth. (2) I have lost a family member in an armed group attack. (3) I have experienced looting or theft of my house or property.	(1) I had to flee because of fighting. (2) I have lost family members. (3) I killed someone to protect myself.	(1) I won money in a lottery or competition. (2) I was involved in an accident. (3) I received help from a stranger.
Treatment	(4) I or a member of my household has been raped by an armed group.	(4) I was a victim of sexual violence.	(4) I was personally sexually assaulted.

Source: authors' elaboration based on surveys mentioned in Section 2.1.

To estimate the prevalence of the sensitive experience (4), one can then simply compare the difference in the average number of reported items between the treatment and control group. The strength of the list experiment is that it provides instant anonymity because respondents do not need to report sexual violence exposure directly.

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[13] For each survey the list of experiences was developed with our local survey partners and experienced practitioners working in women's empowerment programmes.

[14] Note that enumerators were instructed to turn away from the respondents during the list experiment.

2.4   Outcome variables

Our theoretical interest is to asses to what extent wartime sexual violence victimization alters social and political attitudes and behaviour. We focus on three measures that reflect important dimensions of social and political preferences in the social science literature in general and for post-conflict recovery in particular.

Civic participation

The first measure focuses on the immediate social reference system beyond the family: the community. We measure people's civic participation through the following questions. The DRC survey asked: 'Many people are active in different kinds of groups of people with whom they share similar interests. The next few question will be about your involvement or membership in such groups.' There were a total of eight items indicating whether the respondent or a family member is a member of different local associations ranging from farmers' associations, education committees to local NGOs. We use a binary indicator of being a member in at least one organization (65.5 per cent in the sample). The survey in Liberia only included the following proxy variable for civic participation: 'Is your community able to mobilize resources for minor repair work by collecting user fees?'.[15] This indicator is a simple binary variable with 77.2 per cent saying 'yes'. In the Sri Lanka survey, the question read: 'Now I am going to read of a list of voluntary organizations. For each organization, could you tell me whether you are an active member, a passive member, or not a member of that type of organization?'. Respondents chose from among ten different organizations, ranging from those involved in charity and social welfare work to sport and outdoor activities. Some 35 per cent are a member of at least one organization. Since the question wording differs across the surveys, some caution is in order regarding direct comparison across surveys. Nevertheless, all measures tap into the similar theoretical construct. This note also applies to the next two outcome categories.

Interethnic relations

The second outcome measure goes beyond the own community and measures people's preferences towards interethnic relations. In DRC, respondents were asked 'Let's talk about the relation to people outside your community, in particular to people from other ethnic backgrounds. Other ethnic groups may include groups with whom your ethnic group has been in conflict with. Please tell me whether you agree or not agree with the following statements.' They were then presented eight different statements, such as 'I am comfortable living in the same community with members of other ethnic groups.' We used simple averages across the eight statements resulting in a scale ranging from 1 to 4 (with a mean of 3.3 and a standard deviation of 0.6). The Liberia survey included a generalized trust item ('Generally speaking, would you say that most people can be trusted or that you need to be very careful in dealing with people?') which has been shown to tap into outgroup trust, including trust in interethnic outgroups (e.g. Uslaner 2002). The measure is binary with 27.5 per cent indicating that members of ethnic outgroups can be trusted. In Sri Lanka, respondents were asked to indicate on a seven-point scale: 'How much you trust people from various groups in Sri Lanka.' We constructed a measure of interethnic relations from their average trust toward the following ethnic groups: Sinhalese, Sri Lankan Tamil, Indian Tamil, and Muslim Moor (excluding respondents' own ethnic group from the calculation). This measures runs from 1 to 7 with a mean of 3.3 and a standard deviation of 1.5.

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[15] This specific question reflects a community's collective action potential and thus taps into the construct of interest. We also tested for alternative outcomes on individuals' propensity to donate to the community: 'Imagine the following situation: You won 10,000 LRD [Liberian dollars] in a lottery. Considering your current situation, how much of it would you donate to a charitable organization?' and 'How do you assess your willingness to share with others without expecting anything in return when it comes to donating?'. In both instances, we found positive associations with experience of wartime sexual violence.

Political trust

The third outcome looks at the nation as a whole and measures people's trust in political institutions. The DRC survey asked respondents 'The next questions are about the performance of [the following] persons or organizations in improving the living conditions in your village. How would you describe the job they are doing?'. To measure political trust, we rely on the item 'National politicians'. The answer scale ranged from 1 'very bad' to 4 'very good' (with a mean of 1.7 and a standard deviation of 0.8). To measure political trust for the Liberia sample, we averaged the agreement with the following two items: 'The local government is more present these days than it used to be.' and 'The local government provides more services than it used to.' The measure runs from 1 to 5 with a mean of 3.1 and a standard deviation of 1.1. In the Sri Lanka survey, respondents were asked 'On a score of 1-7 how much you personally trust each of the institutions I read out. 1 means you do not trust an institution at all, and 7 means you have complete trust.' We average the four answers to trust in the president, the parliament, politicians, and political parties. The resulting political trust measure ranges from 1 to 7 with a mean of 2.9 and a standard deviation of 1.3.

2.5   Statistical analysis

The key methodological challenge is to relate the unobtrusively measured and indirectly inferred experience of sexual violence from the list experiment to the individual outcome variables, while controlling for potential confounding factors. To this end, we rely on statistical techniques that incorporate the answers to list experiments in regression analyses (Blair and Imai 2012). Specifically, we include the predicted (latent) experience of sexual violence as an explanatory variable in regression models for civic participation, interethnic relations, and political trust (Imai et al. 2015). While we of course cannot observe the individual experience of sexual violence, the list experiment allows us to identify the *joint distribution* of this experience and the control items. This possibility is then used in a multivariate modeling strategy that simultaneously models the response to the sensitive list item, the control list items, and the outcome of interest. We estimate these joint models using the expectation–maximization algorithm implemented in the R package `list` (Blair et al. 2016).

In addition, we analyze the prevalence and effects of the direct question item with linear and logistic models using the same specification as in the list experiment models.

3   Results

3.1   The prevalence of wartime sexual violence and non-disclosure

We start our analysis by estimating the prevalence of wartime sexual violence. Figure 1 presents the estimates derived from our three list experiments.[16] To illustrate the benefits of this approach, we compare the estimates of the *list experiments* to the *direct items*. This also allows us to quantify the degree of sensitivity (*difference*) attached to asking respondents about their experiences with sexual violence.

According to the list experiments, 12 (90%CI [1:23]) per cent of the respondents from DRC, 14 (90%CI [11:17]) per cent from Liberia, and 13 (90%CI [8:18]) per cent from Sri Lanka are survivors of wartime sexual violence. This suggests that roughly similar population shares in these contexts have experienced sexual violence, despite the differences in question wording. Given the delicate nature of this experience, it is not surprising to see that this unobtrusive measure yields a higher share of survivors than the direct question item. Only 6 (90% CI [5.1:7.7]) per cent in DRC, 5 (90% CI [4.9:5.7]) per cent in Liberia, and 1

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[16] Appendix Section A shows evidence against design effects.

(90%CI [0.9:1.9]) per cent in Sri Lanka openly report their experience of sexual violence in an interview. This means that list experiments elicit a prevalence rate of wartime sexual violence that is between two and ten times higher than direct questioning. We find significant sensitivity bias in Liberia (-9, 90%CI [-12:-6]) and Sri Lanka (-12, 90%CI [-7:-17]) but not in DRC, which could be due to the smaller sample size, less stigmatization due to a high prevalence of sexual violence, and support programmes (Quillard 2016), as well as the household-level measure.[17]

Figure 1: Different estimates of wartime sexual violence prevalence with 90% confidence intervals: list experiments, direct survey questions, and their difference (i.e. degree of sensitivity)
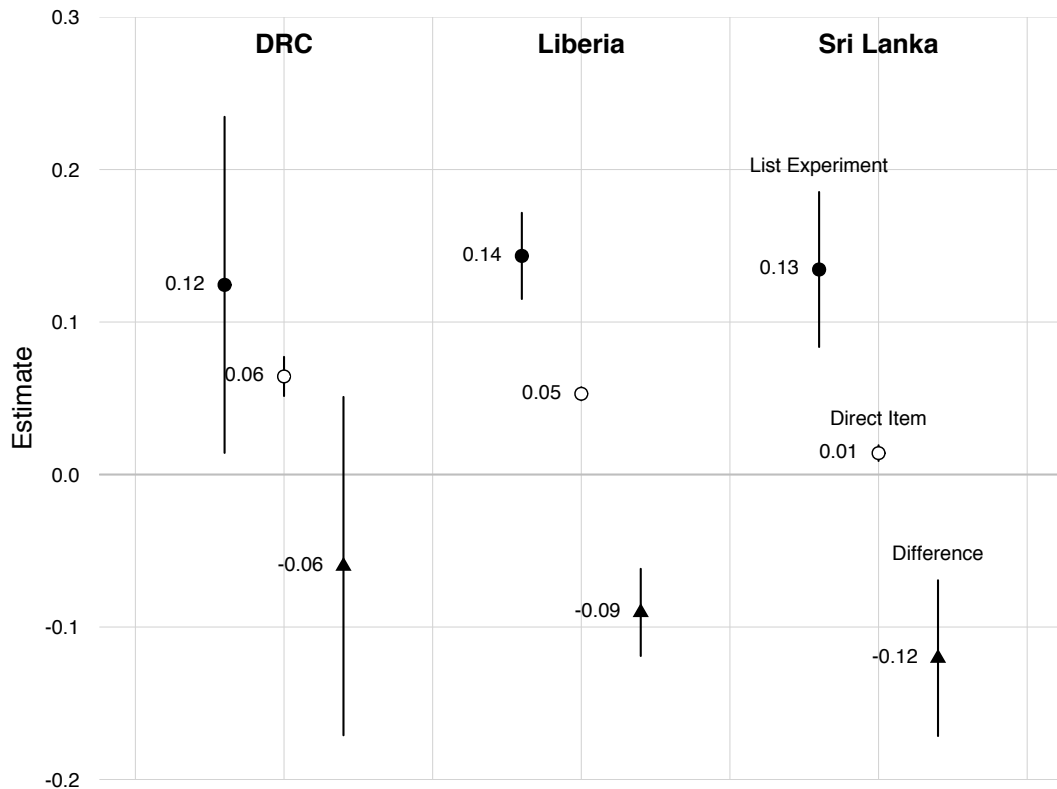
Note: DRC N=1000, Liberia N=7493, Sri Lanka N=1800.
Source: authors' calculations based on data from surveys described in Section 2.

The fact that the list experiment at least doubles the prevalence rates of wartime sexual violence suggests that it provides respondents with more anonymity than the direct question. This strengthens our confidence that the list experiment outperforms the direct question in terms of measurement validity, albeit we do not have any evidence to know how close it approximates the true prevalence in the population.[18]

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[17] In Appendix Section B we provide evidence on socio-demographic risk factors that are associated with sexual violence.

[18] This difference in magnitude between the list experiment and the direct question also matches with Cullen's (2020) comparison of direct and list experiments to study intimate partner violence.

3.2 The effect of wartime sexual violence on social and political outcomes

We now turn to our second aim: assessing the impact of wartime sexual violence on the three primary outcome variables—civic participation, interethnic relations, and trust in political institutions. Figure 2 presents differences in predicted probabilities of *civic participation* from logistic regression models using sexual violence as a predictor and while controlling for other war-related traumatic experiences (e.g., killings, injuries), socio-demographics, as well as regional fixed effects. In addition, to preclude reverse causality where those who are civically active are also more likely to be targeted for sexual violence, we also control for pre-exposure civic engagement.[19] For each post-conflict country we report two effects: i) the effect of the indirect list experiment and ii) the effect of the standard direct measure of sexual violence. Full regression tables are available in Appendix Section C.

Although wartime sexual violence is often associated with a destruction of the social fabric of communities, we find no such evidence. Instead, the results for the list experiments seem to suggest a mobilizing effect. When looking at the effect of sexual violence elicited from list experiments, we find positive effects on civic participation in all three countries: DRC +.18, 90%CI [.03, .30], Liberia +.17 [.02, .23], and Sri Lanka .+18 [.02, .33]. The causal interpretation of this effect rests on the assumption of no unobserved confounding. Using sensitivity analyses, we find that it is unlikely that an unobserved confounder threatens our inference (see Section D in the appendix). Importantly, using direct question items these effects would have been either missed (as is the case for DRC and Liberia) or even assigned the wrong sign leading to wrong conclusions (as for Sri Lanka).

Figure 2: The effect of wartime sexual violence on civic participation in three post-conflict contexts
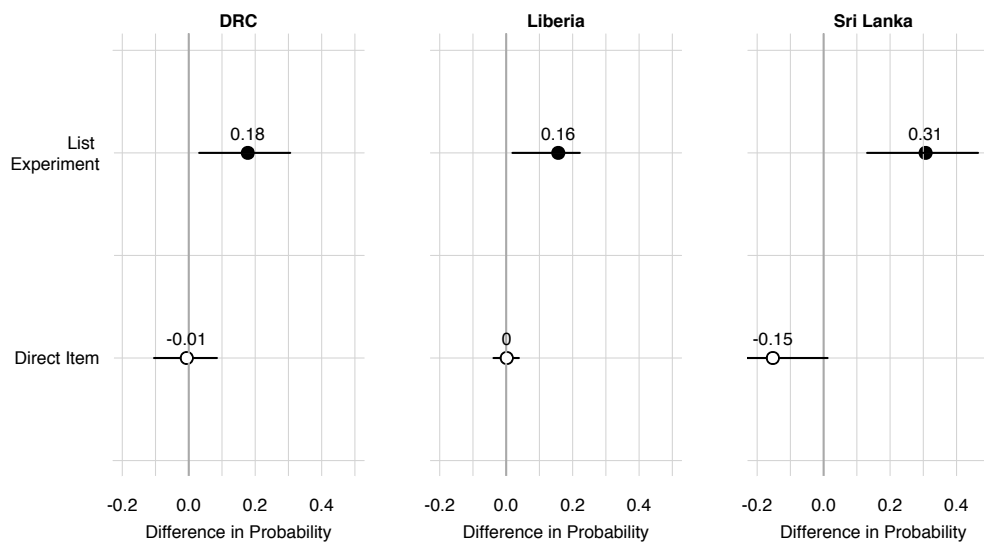
Note: averaged differences in predicted probability along with 90% intervals. Effects are adjusted for other wartime trauma, pre-exposure civic participation (DRC and Sri Lanka only), socio-demographics, and region fixed effects. Full results can be found in the appendix.
Source: authors' calculations based on data from surveys described in Section 2.

If wartime sexual violence does not decrease civic participation and community involvement, does it negatively impact *interethnic relations*? The results in Figure 3 again suggest otherwise. We find no effect of the list experiment on perceived interethnic relations. Again, the direct items produces diverging results. It is negatively related to interethnic trust only in DRC (with a difference of -.26, 90%CI [-.46, -.06] standard deviations), but positively related in Liberia (+.07, 90%CI [.03, .11] in predicted probability) and Sri Lanka (+.41 90%CI [.07, .77] standard deviations). But these estimates from the direct

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[19] Measures for pre-exposure civic engagement are only available for DRC and Sri Lanka, but not for Liberia.

question should not be trusted as none of the equations using the list experiment show any statistically significant effect of wartime sexual violence on interethnic relations.

Figure 3: The effect of wartime sexual violence on interethnic relations in three post-conflict contexts
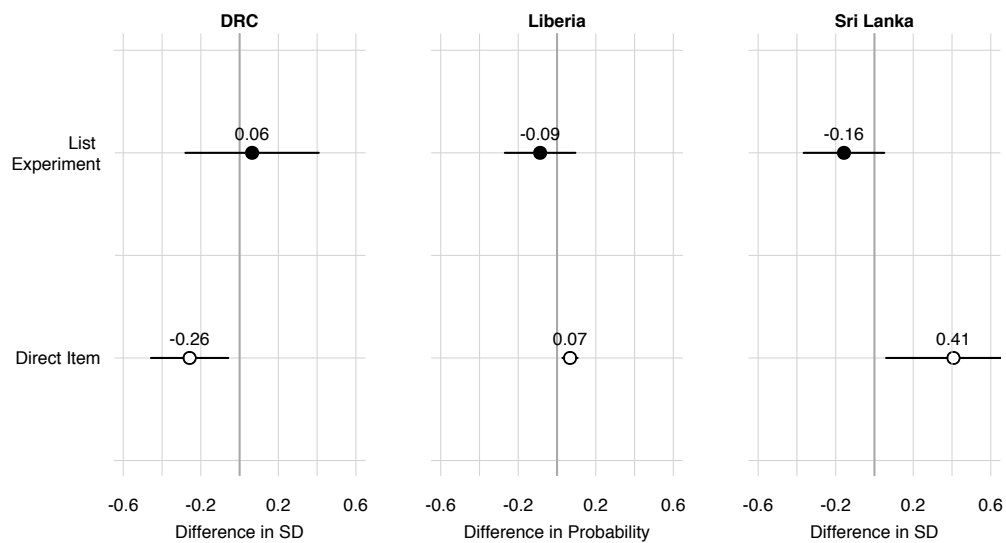
Note: averaged differences in standard deviations/predicted probability along with 90% intervals. The outcome in model for Liberia is binary. Effects are adjusted for other wartime trauma, socio-demographics, and region fixed effects. Full results can be found in the appendix.

Source: authors' calculations based on data from surveys described in Section 2.

Finally, Figure 4 shows that the effects of wartime sexual violence on *political trust* are by no means uniformly negative. While the direct measurement is not significantly related to political trust in any of the three post-conflict countries, we do find a negative effect of experiencing sexual violence on political trust in DRC when relying on the list experiment (-.96, 90% CI [-1.22, -.71] standard deviations). However, that experience of sexual violence does not automatically lower political support is apparent in the Sri Lankan sample. Here the list experiment measure is significantly related to a higher level of political trust for both, at least in the full sample (+.27 90%CI[.01, .53] standard deviations).

Figure 4: The effect of wartime sexual violence on political trust in three post-conflict contexts

How can we make sense of the commonalities and differences of these results across the three different outcome variables and contexts? First, the results on civic participation in the social community are robust across all three countries and are in line with prior studies on wartime sexual violence and social outcomes (Koos 2018) as well as the larger violence–prosocial behaviour literature (e.g. Bauer et al. 2016). One important finding in that regard is that, in our models, it is only the sexual violence list experiment measure that is significant, not the other measures for experiences of violence.

Second, the null effect of sexual violence on interethnic relations is also robust across the three contexts. While in all three civil wars ethnic (DRC, Liberia) and religious identity (Sri Lanka) played a substantive role, sexual violence in itself seemingly did not perpetuate interethnic relations.

Third, with regard to trust in political institutions, the results differ significantly. We believe—and this is certainly more speculative—that conflict characteristics factor in into these results. The negative results in the DRC suggest strong disappointment and perceptions of injustice in political institutions who were unable to protect survivors and families, or were even perpetrators themselves. In Liberia, the time since the conflict as well as transitional justice may have facilitated reconciliation with the state. The positive result for Sri Lanka is somewhat surprising, especially since the government won the war and the army was at the same time the main perpetrator of sexual violence. The variation in results on political institutions could also be due to different identities of the main perpetrators, specifically in the case of Sri Lanka.

4 Discussion

Sexual violence against civilians is widespread in conflict zones worldwide. But so far we have little population-based data of its prevalence among conflict-affected populations. Drawing on original population-based surveys in DRC, Liberia, and Sri Lanka, this article finds that using list experiments in the study of wartime sexual violence outperforms naive direct questions in terms of validity. The population-based prevalence rates of wartime sexual violence vary between 12 and 14 per cent accord-

ing to the list experiment, twice to ten times as much compared to a naive direct questions. While these prevalence rates are alarmingly high, it also suggests that list experiments are an effective way to provide respondents with anonymity and get closer to *true* prevalence levels. Our application shows that list experiments are a useful and straightforward technique that should be used in public health survey (e.g. demographic health surveys) among conflict-affected populations to get a more precise empirical understanding of the extent of sensitive experiences that are normally under-reported in administrative data, hospital statistics, and conventional surveys.

Beyond estimating prevalence rates, our analyses show that list experiments can be effectively used as independent variables to assess the consequences of wartime sexual violence on a comprehensive set of social and political outcomes. The analyses show that sexual violence victimization is consistently associated with higher levels of civic participation. This somewhat optimistic finding indicates that survivors and their families take agency and mobilize locally as a response to wartime sexual violence. This finding adds an important new insight to previous studies that have largely focused on pathological mental and social effects. Sensitivity analyses suggest this relationship to be robust and unlikely to be affected by confounding bias (see Section D of the appendix). Moreover, we do not find any negative effects of sexual violence on people's perceptions of interethnic relations. The absence of intergroup animosities due to wartime sexual violence can be interpreted as an important condition for societal reconciliation in post-conflict contexts. While the results for civic participation and intergroup relations are consistent across all three diverse cases, our analyses also show that political institutional trust varies extensively across cases, reflecting—most likely—differences in the identity of perpetrators and the outcomes of the wars and trajectories in the post-conflict period.

This brings us to the implications of this article. First and primarily, our article has direct implications for future research on wartime sexual violence, specifically by taking into account active social psychological responses of survivors and communities and a research design that is suited to uncover local variations in exposure and outcomes. It thus provides a new approach situated between macro-level cross-country correlations and small-N narrative-based research. While we have proposed several mechanisms that could underpin our results—e.g., post-traumatic growth, social resilience, collective threat aversion, norm shifts—future research will benefit from building on the proposed theoretical framework, empirical design, and findings to further investigate these mechanisms underpinning these results in more detail.

Second, our article adds important supporting evidence to the research programme on the legacy of violence in civil wars. This growing literature suggests that individuals and communities show considerable resilience to exposure to violence and provides a hopeful prospect for post-conflict societies. But wartime sexual violence is a highly distinct and psychologically pernicious form of violence. According to conventional knowledge it therefore poses a particularly hard test for the resilience hypothesis. Our finding on the mobilizing effect of experiencing wartime sexual violence therefore contributes a fresh and significant piece of corroborating evidence to the broad literature on the legacy of violent conflict.

Third and more generally, we employed a coherent conceptual framework and empirical design for three diverse conflict-affected populations. This provides a more comprehensive understanding of responses to wartime sexual violence across country cases and sets of key social and political outcomes. Investigating several outcomes across diverse cases is an important feature on its own, and we hope that our article inspires scholars to build on, adapt, and expand on the introduced framework and research design. Beyond a systematic advancement of knowledge in political science, psychology, sociology, public health, and elsewhere, this will also increase the evidence base for international organizations, social activists, advocacy groups, and humanitarian actors.

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Appendix

A Test of no design effect

In addition to randomization, the analysis of list experiments rests on two additional assumptions (Blair and Imai 2012). First, we have to assume that our participants respond truthfully to the sensitive item. Unfortunately this 'no liars' assumption cannot be directly tested. Second, we have to assume that the presence of the sensitive item does not affect the answers to the remaining control items. A test of this 'no design effect' assumption fails to reject the null for all three list experiments and thus supports the assumption (see Table A1). The Bonferroni-corrected p-value for Democratic Republic of Congo (DRC) list experiment is .8, for the Liberia and Sri Lanka experiments it is 1.

Table A1: Test of no design effect for three list experiments

	Est.	SE
DRC		
$p_i(Y_i(0) = 0, Z_i = 1)$	0.01	0.03
$p_i(Y_i(0) = 1, Z_i = 1)$	-0.01	0.03
$p_i(Y_i(0) = 2, Z_i = 1)$	0.06	0.02
$p_i(Y_i(0) = 3, Z_i = 1)$	0.06	0.01
$p_i(Y_i(0) = 0, Z_i = 0)$	0.20	0.02
$p_i(Y_i(0) = 1, Z_i = 0)$	0.30	0.03
$p_i(Y_i(0) = 2, Z_i = 0)$	0.30	0.03
$p_i(Y_i(0) = 3, Z_i = 0)$	0.08	0.02
Bonferroni corrected p-value	0.80	
Liberia		
$p_i(Y_i(0) = 0, Z_i = 1)$	0.02	0.01
$p_i(Y_i(0) = 1, Z_i = 1)$	0.06	0.01
$p_i(Y_i(0) = 2, Z_i = 1)$	0.05	0.01
$p_i(Y_i(0) = 3, Z_i = 1)$	0.01	0.00
$p_i(Y_i(0) = 0, Z_i = 0)$	0.07	0.00
$p_i(Y_i(0) = 1, Z_i = 0)$	0.22	0.01
$p_i(Y_i(0) = 2, Z_i = 0)$	0.52	0.01
$p_i(Y_i(0) = 3, Z_i = 0)$	0.04	0.00
Bonferroni corrected p-value	1.00	
Sri Lanka		
$p_i(Y_i(0) = 0, Z_i = 1)$	0.08	0.02
$p_i(Y_i(0) = 1, Z_i = 1)$	0.03	0.01
$p_i(Y_i(0) = 2, Z_i = 1)$	0.01	0.01
$p_i(Y_i(0) = 3, Z_i = 1)$	0.01	0.00
$p_i(Y_i(0) = 0, Z_i = 0)$	0.66	0.02
$p_i(Y_i(0) = 1, Z_i = 0)$	0.17	0.02
$p_i(Y_i(0) = 2, Z_i = 0)$	0.03	0.01
$p_i(Y_i(0) = 3, Z_i = 0)$	0.00	0.00
Bonferroni corrected p-value	1.00	

Source: authors' calculations based on data from surveys described in Section 2.

B   Risk factors of wartime sexual violence and under-reporting

Which social groups are most at risk of experiencing sexual violence? Tables A2, A3, and A4 present the results of multivariate regression models that jointly regress a) the *sensitive outcome* of the list experiment and b) the *misreporting* against key socio-demographic variables (see Eady 2017 for methodological details; the equation for control items is not shown). We find that women report sexual violence victimization more often in DRC (1.18, SE: ±0.40), but not in Liberia and Sri Lanka, where both men and women report sexual violence at similar rates. In all three contexts, the risk of sexual violence is also evenly distributed across age groups and education. However, poorer respondents in terms of income are disproportionately more affected in all three post-conflict countries. The coefficients for income are -1.60(SE: ±.51) in DRC, -0.64(SE: ±.28) in Liberia, and -1.16 (SE: ±.60) in Sri Lanka. Household size is unrelated to the risk of sexual violence victimization.

Reporting an experience of sexual violence may come with different costs for different groups, which would be reflected in differences in under-reporting. However, as the misreporting equations reveal, under-reporting is a behaviour common across all socio-demographic groups. We only find significant differences in Liberia, where the higher educated and economically more well off are less likely to under-report an experience of sexual violence (coefficient of -0.95, SE: ±0.48 for education and -3.98, SE: ±1.85 for income).

Table A2: a) Sensitive item outcome and b) misreporting equations of a multivariate regression model of wartime sexual violence for DRC

	Est.	(SE)
a) Sexual violence		
Intercept	-2.17	(0.32)
Female	1.18	(0.40)
Age	-0.04	(0.33)
Education	0.82	(0.44)
Income	-1.60	(0.51)
HH size	0.41	(0.30)
b) Misreporting		
Intercept	0.03	(0.56)
Female	0.90	(0.61)
Age	0.45	(0.51)
Education	0.70	(0.63)
Income	-1.26	(0.83)
HH size	0.08	(0.44)

Note: logits and standard errors in parentheses. Control item equation not shown.

Source: authors' calculations based on data described in Section 2.

Table A3: a) Sensitive item outcome and b) misreporting equations of a multivariate regression model of wartime sexual violence for Liberia

	Est.	(SE)
a) Sexual violence		
Intercept	-2.59	(0.11)
Female	0.15	(0.18)
Age	0.23	(0.18)
Education	-0.15	(0.16)
Income	-0.64	(0.28)
HH size	-0.01	(0.13)
b) Misreporting		
Intercept	-1.18	(0.39)
Female	0.40	(0.44)
Age	-0.09	(0.50)
Education	-0.95	(0.48)
Income	-3.98	(1.85)
HH size	-0.19	(0.35)

Note: logits and standard errors in parentheses. Control item equation not shown.

Source: authors' calculations based on data described in Section 2.

Table A4: a) Sensitive item outcome and b) misreporting equations of a multivariate regression model of wartime sexual violence for Sri Lanka

	Est.	(SE)
a) Sexual violence		
Intercept	-2.42	(0.54)
Female	0.04	(0.50)
Age	-0.24	(0.82)
Education	0.31	(1.17)
Income	-1.16	(0.60)
HH size	0.60	(0.66)
b) Misreporting		
Intercept	3.34	(0.97)
Female	-1.15	(1.08)
Age	0.09	(1.24)
Education	-0.21	(1.44)
Income	-1.17	(1.69)
HH size	1.79	(1.11)

Note: logits and standard errors in parentheses. Control item equation not shown.

Source: authors' calculations based on data described in Section 2.

C   Full regression results

Table A5: Regression models of civic participation for DRC

	List experiment		Direct item	
	Est.	(SE)	Est.	(SE)
Wartime sexual violence	1.11	(0.56)	-0.03	(0.32)
Pre-exposure participation	1.19	(0.18)	0.99	(0.12))
Other wartime trauma	0.14	(0.31)	0.31	(0.21)
Female	0.17	(0.22)	0.28	(0.17)
Age	0.33	(0.21)	0.28	(0.17)
Education	0.89	(0.24)	0.87	(0.20)
Income	0.28	(0.18)	0.18	(0.17)
HH size	0.64	(0.20)	0.57	(0.16)
Intercept	-2.36	(0.46)	-1.80	(0.30)
Region FEs	yes		yes	

Note: *List experiment*: logistic outcome equation from a multivariate sensitive item predictor model. *Direct item*: logistic regression model.

Source: authors' calculations based on data described in Section 2.

Table A6: Regression models of civic participation for Liberia

	List experiment		Direct item	
	Est.	(SE)	Est.	(SE)
Sexual violence	1.75	(1.00)	0.01	(0.14)
Other wartime trauma	0.01	(0.01)	0.02	(0.01)
Female	-0.15	(0.07)	-0.18	(0.07)
Age	-0.03	(0.07)	0.00	(0.06)
Education	0.16	(0.07)	0.13	(0.07)
Income	0.73	(0.11)	0.71	(0.17)
HH size	0.06	(0.06)	0.03	(0.06)
Intercept	1.38	(0.06)	1.40	(0.06)
Region FEs	yes		yes	

Note: *List experiment*: logistic outcome equation from a multivariate sensitive item predictor model. *Direct item*: logistic regression model.

Source: authors' calculations based on data described in Section 2.

Table A7: Regression models of civic participation for Sri Lanka

	List experiment		Direct item	
	Est.	(SE)	Est.	(SE)
Wartime sexual violence	1.50	(0.53)	-1.05	(0.67)
Pre-exposure participation	0.42	(0.46)	1.02	(0.41)
Other wartime trauma	0.00	(0.29)	-0.08	(0.28)
Other wartime trauma 2	0.26	(0.39)	-0.21	(0.40)
Female	-0.20	(0.18)	-0.20	(0.17)
Age	0.05	(0.22)	0.07	(0.20)
Education	0.80	(0.19)	0.53	(0.16)
Income	-0.15	(0.26)	-0.19	(0.22)
HH size	0.22	(0.18)	0.20	(0.17)
Intercept	-0.84	(0.37)	-0.34	(0.32)
Region FEs	yes		yes	

Note: *List experiment*: logistic outcome equation from a multivariate sensitive item predictor model. *Direct item*: logistic regression model.

Source: authors' calculations based on data described in Section 2.

Table A8: Regression models of interethnic relations for DRC

	List experiment		Direct item	
	Est.	(SE)	Est.	(SE)
Wartime sexual violence	0.06	(0.21)	-0.26	(0.12)
Other wartime trauma	0.05	(0.10)	0.10	(0.08)
Female	-0.53	(0.09)	-0.51	(0.07)
Age	0.11	(0.06)	0.10	(0.06)
Education	0.24	(0.08)	0.24	(0.07)
Income	0.21	(0.07)	0.21	(0.06)
HH size	0.15	(0.07)	0.15	(0.06)
Intercept	0.21	(0.10)	0.21	(0.09)
Region FEs	yes		yes	

Note: *List experiment*: linear outcome equation from a multivariate sensitive item predictor model. *Direct item*: ordinary least squares (OLS) model.

Source: authors' calculations based on data described in Section 2.

Table A9: Regression models of interethnic relations for Liberia

	List experiment		Direct item	
	Est.	(SE)	Est.	(SE)
Wartime sexual violence	-0.09	(0.11)	0.07	(0.02)
Other wartime trauma	0.00	(0.00)	0.00	(0.00)
Female	0.03	(0.01)	0.03	(0.01)
Age	-0.01	(0.01)	-0.01	(0.01)
Education	-0.02	(0.01)	-0.02	(0.01)
Income	0.03	(0.01)	0.03	(0.01)
HH size	0.01	(0.01)	0.02	(0.01)
Intercept	0.29	(0.03)	0.29	(0.01)
Region FEs	yes		yes	

Note: *List experiment*: linear probability outcome equation from a multivariate sensitive item predictor model. *Direct item*: linear probability model.

Source: authors' calculations based on data described in Section 2.

Table A10: Regression models of interethnic relations for Sri Lanka

	List experiment		Direct item	
	Est.	(SE)	Est.	(SE)
Wartime sexual violence	0.12	(0.16)	0.41	(0.21)
Other wartime trauma	-0.05	(0.10)	0.01	(0.10)
Other wartime trauma 2	0.27	(0.11)	0.34	(0.12)
Female	-0.12	(0.05)	-0.13	(0.05)
Age	0.14	(0.06)	0.15	(0.06)
Education	0.20	(0.06)	0.20	(0.06)
Income	-0.07	(0.07)	-0.09	(0.06)
HH size	0.02	(0.06)	0.01	(0.06)
Intercept	0.09	(0.09)	0.10	(0.09)
Region FEs	yes		yes	

Note: *List experiment*: linear outcome equation from a multivariate sensitive item predictor model. *Direct item*: OLS model.

Source: authors' calculations based on data described in Section 2.

Table A11: Regression models of political trust for DRC

	List experiment		Direct item	
	Est.	(SE)	Est.	(SE)
Wartime sexual violence	-0.96	(0.15)	-0.08	(0.13)
Other wartime trauma	0.20	(0.10)	0.10	(0.09)
Female	0.74	(0.11)	0.31	(0.07)
Age	-0.03	(0.08)	0.00	(0.07)
Education	-0.10	(0.08)	-0.13	(0.08)
Income	0.09	(0.08)	0.21	(0.07)
HH size	-0.04	(0.07)	-0.05	(0.06)
Intercept	0.04	(0.10)	-0.11	0.10
Region FEs	yes		yes	

Note: *List experiment*: linear outcome equation from a multivariate sensitive item predictor model. *Direct item*: OLS model.

Source: authors' calculations based on data described in Section 2.

Table A12: Regression models of political trust for Liberia

	List experiment		Direct item	
	Est.	(SE)	Est.	(SE)
Wartime sexual violence	-0.17	(0.15)	0.07	(0.05)
Other wartime trauma	-0.00	(0.00)	-0.00	(0.00)
Female	-0.38	(0.03)	-0.37	(0.03)
Age	0.04	(0.02)	0.05	(0.02)
Education	0.06	(0.03)	0.07	(0.03)
Income	-0.01	(0.03)	-0.01	(0.02)
HH size	-0.03	(0.02)	-0.03	(0.02)
Intercept	0.11	(0.02)	0.09	(0.02)
Region FEs	yes		yes	

Note: *List experiment*: linear outcome equation from a multivariate sensitive item predictor model. *Direct item*: OLS model.

Source: authors' calculations based on data described in Section 2.

Table A13: Regression models of political trust for Sri Lanka

	List experiment		Direct item	
	Est.	(SE)	Est.	(SE)
Wartime sexual violence	0.27	(0.16)	-0.18	(0.21)
Other wartime trauma	-0.23	(0.10)	-0.10	(0.10)
Other wartime trauma 2	0.39	(0.12)	0.39	(0.12)
Female	-0.06	(0.06)	-0.04	(0.05)
Age	0.11	(0.07)	0.13	(0.06)
Education	0.11	(0.06)	0.12	(0.06)
Income	0.07	(0.07)	0.05	(0.06)
HH size	-0.01	(0.06)	-0.03	(0.06)
Intercept	-0.26	(0.05)	0.04	(0.09)
Region FEs	yes		yes	

Note: *List experiment*: linear outcome equation from a multivariate sensitive item predictor model. *Direct item*: OLS model.

Source: authors' calculations based on data described in Section 2.

D Sensitivity to unobserved confounding

Our causal interpretation of the effect of wartime sexual violence on civic participation rests on the assumption of no unobserved confounding. Given the observational nature of our data and the often purposeful use of sexual violence, this assumption is almost certainly wrong. However, this does not necessarily invalidate our inference. In this section, we present a sensitivity analysis to assess the robustness of our results to its violation.[20]

An omitted confounder that could jeopardize our causal claim must be related to both wartime sexual violence and civic participation. According to the omitted variable bias formula, the size of the bias is the product of *impact × imbalance*: the strength of the relation between confounder and outcome times the difference in the confounder between those who did and those who did not experience wartime sexual violence. Following Cinelli and Hazlett (2020), we can re-express the two quantities in this product in more intuitive terms as hypothetical *partial R^2s*.[21]

Figure A1 visualizes at which hypothetical levels of the partial R^2s the causal effect would be exactly 0, thus threatening our inference. For the Sri Lanka result, for instance, our causal effect would already equal zero if an unobserved confounder were somewhat predictive of experiencing sexual violence (partial $R^2 \approx .05$) while at the same time also explaining 15 per cent of the variation in political participation not already accounted for by the controls (partial $R^2 \approx .15$).

To assess whether such an unobserved confounder is indeed realistic, we can compare it to actually observed covariates that we know are related to experiences of wartime sexual violence and/or civic participation. For Sri Lanka, an omitted factor that would threaten our inference would have to be *forty times* as powerful as pre-exposure participation in explaining victimization and civic participation. For Liberia a critical unobserved confounder would have to be *thirty times* stronger than education.

We find it very hard to believe that such an explanatory factor exists. Instead, it is safe to assume that the relationship between the personal experience of sexual violence and civic participation is indeed causal. For DRC, the confounder would still have to be four times as predictive of wartime sexual violence and civic participation as the observed pre-exposure participation variable. While not impossible, we find it unlikely that such a variable exists.

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[20] For a similar approach, see Bellows and Miguel (2009) and Blattman (2009).

[21] For R^2s to be meaningful, we re-estimated the logistic regression models as linear probability models.

Figure A1: Sensitivity to unobserved confounding
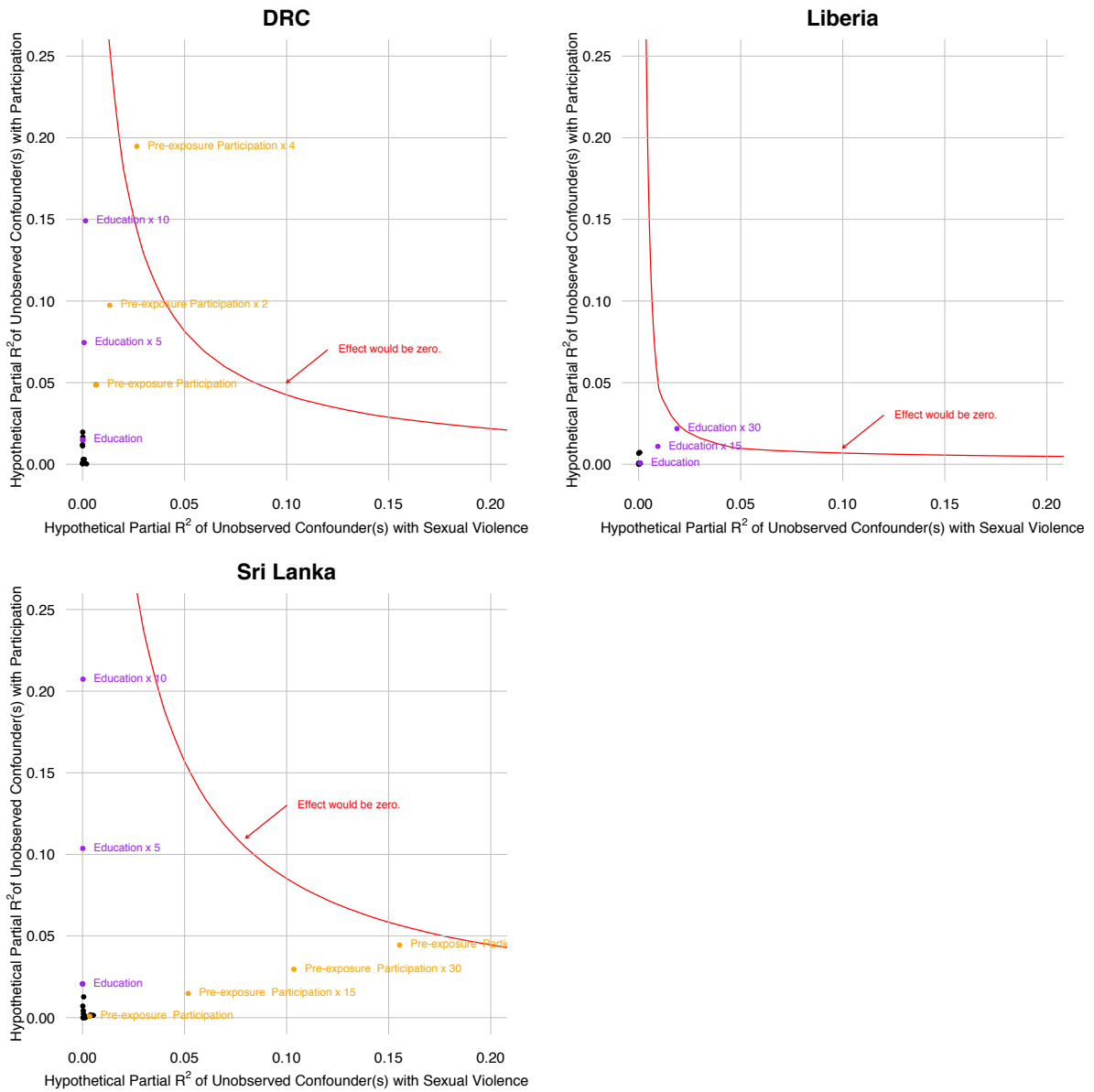
Source: authors' calculations based on data described in Section 2.

E   Sampling & survey protocols

Democratic Republic of Congo

We targeted a sample of 1,000 respondents in South Kivu. Our sampling protocol starts with the premise that ten respondents had to be randomly sampled per village. To do so, 100 villages needed to be randomly sampled. Furthermore, we agreed with the survey organization that three villages were randomly sampled per groupement (in one groupement, four villages were sampled), the administrative unit above the village. We therefore applied to protocol outlined below to select 33 groupements in South Kivu.

Stage 1: Territoire level.   The territoire is the administrative unit below the province level. We sampled the 33 groupements in six out of eight existing territoires in South Kivu. We excluded the territoires Island of Idjwi and Shabunda. Idjwi, being an island in Lake Kivu, hardly had any exposure to the war compared to the other territoires. Shabunda is the most-western territoire and only accessible by plane which would have increased the cost for the survey tremendously. Finally, our sample is representative of the following territoires: Kabare, Kalehe, Fizi, Mwenga, Uvira, and Walungu which represents an estimated population of 4.4 million people. We used estimated population figures from the Ministry of Interior at the territoire level to determine the number of groupements to be sampled. The number of groupements to be sampled per territoire ranged from three, meaning nine villages (Mwenga) to nine, meaning 18 villages (Kalehe).

Stage 2: Chefferie level.   Each territoire consists of several chefferies (mean: 2.5) for which estimated population data exist as well. Some chefferies had to be excluded for security reasons: Nindja in Kabare, Itombwe in Mwenga, and Lulenge in Fizi. In addition, we excluded Wamuzimu in Mwenga which is a larger urban area and as such does not suit the purpose of the study which is interested in rural settings. We then distributed the calculated number of groupements per territoire to the relative population size of the remaining chefferies in each territoire (e.g. [33 target groupements x population in territoire] / population in province).

Stage 3: Groupement level.   In the next step, we selected the actual groupements within chefferies. Chefferies have between eight and 30 groupements. Since we did not have any population figures for groupements, we randomly selected the assigned number of groupements per chefferie. In addition, we sampled three contingency groupements to be taken should the original groupements need to be replaced because of security reasons.

Stage 4: Village level.   Within each selected groupement, three villages were randomly selected. Groupements have between seven and 145 villages. We randomly selected three villages per groupement. Again, we selected three contingency villages.

Stage 5: Household level.   The household sampling was done by the enumerators. Once the enumerators arrived in the village they had to meet the chairman and traditional chief to introduce the survey and to establish a household roster. Enumerators listed all households of village on paper and then applied a simple random sampling strategy. Enumerators were instructed to list internally displaced people as well as maisons de femmes.

Stage 6: Respondent level.   Within each household, respondents were supposed to interview the head of household or the spouse. Female enumerators interviewed women (usually the spouse of the head of household) and male enumerators interviewed men. If none of them was at home, interviewers listed all persons above 18 years into the survey software, and the software drew a random person of that list to be interviewed.

Liberia

The Liberia survey we draw on in this article was part of a baseline survey in the context of a randomized controlled trial of the Recovery and Rehabilitation Program in three counties (the first-level administrative unit) of south-east Liberia: Grand Geddeh, River Gee, and Maryland. The survey has been implemented in 121 pre-selected communities which represent the major communities in a relatively sparsely populated part of Liberia. The three counties hold a total population of 328,985[22] according to the latest available census from 2008.[23]

The goal of this protocol is to ensure that, in the context of the baseline survey, a representative sample of the target population is included in both treatment and control communities.

General requirements. Female enumerators should always interview female respondents, and vice versa. This makes sure that both women and men feel more comfortable to discuss sensitive gender-related questions. If recruitment of female enumerators is restricted, men must be explicitly made aware of gender-sensitive questions (e.g., domestic violence, gender-related attitudes). The sample size to be collected per community has been provided in the form of an spreadsheet.

We used the Expanded Program for Immunization (EPI) sampling method, also referred to as 'random walk' method (Milligan et al. 2004). The enumerator teams have experience with this sampling method, and research has shown that, when sampling frames are unavailable, the EPI method yields reasonable random samples. Once a team of enumerators arrives in a village, the following steps need to be followed in order to collect data from a random sample:

Stage 1: Village sample size. A critical component of the survey is a random sample. The aim of a random sample is to ensure that the sample reflects the village population as good as possible with regard to its properties. We have calculated the sample size per village based on the population figures provided by Welthungerhilfe. The village sample sizes vary with village size. We sampled 4 per cent of the village population with a minimum of ten households. The sample sizes are provided in a separate spreadsheet (available on request).

Stage 2: Locating central square and selecting directions. Enumerators go to the central square (community meeting place). There enumerators throw and spin a pen so that it lands on the ground (it should spin). The direction of the pen determines which direction the female enumerator should walk. The male enumerator takes the opposite direction.

In larger communities, several central places (e.g., school, police station, market) should be chosen as starting points to have the sample reflect the diversity of the population more accurately than if only sticking to one starting point. We suggest that, for every 20-30 respondents, one starting point should be chosen. This means for instance that, for a sample size of 90, between three and four starting points are sufficient.

Stage 3: Household level. Once the direction is known, as a starting point the enumerator who interviews women selects the third house on the right side of the street (or path), whereas the enumerator who interviews men selects the third house on the left side of the street. After the interview has been conducted, the second-next house will be chosen on the same side as the first one. After every two houses, enumerators switch to the other side of the road or path.

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[22] Grand Gedeh: 125,258, River Gee: 66,789, Maryland: 136,938.

[23] See: https://www.emansion.gov.lr/doc/Population_by_County.pdf.

Stage 4: Respondent level. Once the household has been selected, the male or female head of household should be interviewed. We recommend focusing only on the heads of household since almost all aspects of RRP-V require reliable factual information which in many instances are only reliably known by decision-making household members. We further recommend (as discussed above) to assign an enumerator to interview only one sex (either men or women). This will allow us to have a balanced sample between men and women (50/50). Thereby, each enumerator will interview only male or female heads of households, depending on her/his assignment.

Sri Lanka

This section contains information about the survey that was carried out in Sri Lanka in the first half of 2016, seven years after the armed conflict between the Sri Lankan state and the Liberation Tigers of Tamil Eelam (LTTE) had ended.

Sampling. The survey took place across all 25 districts of Sri Lanka, including the Northern and Eastern Provinces at the centre of the conflict. The selection of respondents followed a multistage stratified random sample procedure. In each district, three lower-level administrative units known as the Grama Niladhari (GN) divisions were randomly selected. From these, 24 households were randomly sampled based on the updated voter registry of the Election Commissioner Department of Sri Lanka. Finally, within the household the member with the most recent birthday and at least 18 years old was interviewed. If the relevant respondent was not present on that particular day, another one was selected to return to the same household and complete the interview. If the member of a selected household refused to participate in the study, it was replaced by a new one using the same sampling procedure. Generally, non-response was not an issue. Tamils were oversampled to guarantee reliable estimates for this important ethnic minority group.

Questionnaire construction. The standardized questionnaire was constructed in close cooperation with local senior researchers who provided us with the necessary social, cultural, and political background and advised us in adapting question wording to the specificities of the Sri Lankan context. Survey questions were originally formulated in English and then translated into Sinhala and Tamil with re-translations as a quality check. To be able to implement the experimental logic of treatment and control group, we used two versions of the questionnaire: one version included all items in the list experiment including the sensitive item, the other version only included the control items. An equal number of both questionnaire versions was printed on paper and randomly assigned to respondents.

Pre-testing. Prior to the actual fieldwork, a pilot survey was carried out and 26 pre-test interviews collected. In these 26 pre-test interviews, 14 respondents were male and 12 female. Age ranged from 24 to 63 with a mean of 42.6 years. Sixteen respondents were Tamil, six Sinhalese, and four Moor. The pilot was carried out in nine different districts (Colombo, Galle, Anuradhapura, Hambanthota, Nuwaraeliya, Batticaloa, Vavuniya, Mullaitivu, and Killinochchi). These pre-test interviews were not formally analyzed but general experiences and feedback from the pilot survey were taken into consideration for the final questionnaire. A workshop was held in the beginning of February 2016 with the Tamil research assistants of the Northern and Eastern Provinces to discuss issues and difficulties they faced in collecting data. A similar workshop was held for the Sinhala enumerators. Major concerns were the interview duration and the sensitive nature of some of the questions on war experiences. The questionnaire was revised accordingly.

Survey administration. The survey was administered through face-to-face interviews in both the Sinhala and Tamil languages. A total of 30 graduate research assistants consisting of both males and females, Tamil and Sinhala, carried out the interviews. They were given extensive instructions in a training workshop held for the research assistants and field coordinators in the beginning of January 2016. The

workshop focused on interviewing techniques, research ethics, sample selection, and the selected Grama Niladhari (GN) divisions (the lowest administrative units). The actual fieldwork in Tamil-dominated regions was conducted by Tamil enumerators, started in February 2016, and was completed in May 2016. Fieldwork in Sinhala regions was carried out by Sinhala research assistants, also started in February 2016, and was completed in July 2016. A group of field coordinators were deployed to supervise the data collection process during and after the fieldwork, including spot-checks to ensure sampled households had indeed been visited. In addition, local senior researchers and one of the authors visited the field sites during the fieldwork period in order to ensure the quality of the data collection.

Research ethics and participant compensation. The respondents were thoroughly briefed before the interviews. This included the objectives of the research study and research ethics (voluntary participation as well as confidentiality and privacy of information). Informed consent was obtained from each respondent prior to conducting the interview. Interviews took between 45 minutes to one hour. At the end of the interview, respondents were given a list with the contact details of psychologists and doctors in nearby hospitals, in case they required assistance. Respondents also received an umbrella as a gift of appreciation for their participation.

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