Supplementary Appendix

The Political Consequences of Wartime Sexual Violence: Evidence from a List Experiment

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González, B. & Traunmüller, R. (2023). The Political Consequences of Wartime Sexual Violence: Evidence from a List Experiment. *Journal of Peace Research*, Online First.

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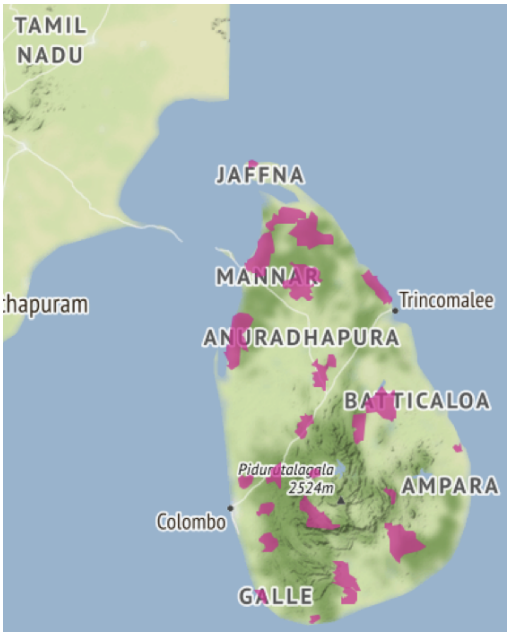
A.1 Information on the Survey

This section contains information about the survey that Traunmüller, Kijewski and Freitag (2019) carried out in Sri Lanka in the first half of 2016, seven years after the armed conflict between the Sri Lankan state and the LTTE had ended.

1) **The sample.** The survey took place across all twenty-five districts of Sri Lanka, including the Northern and Eastern Provinces at the center 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, twenty-four 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 eighteen 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, nonresponse was not an issue. Tamils were over sampled to guarantee reliable estimates for this important ethnic minority group. Figure A.1 shows the distribution of the randomly sampled areas.

FIGURE A.1. Map of Sampled Areas in Sri Lanka.



Note: Areas in light red show the randomly sampled Grama Niladhari (GN) divisions for the survey.

2) **The list experiment.** The survey followed the existing literature to design and implement the list experiment (Corstange, 2009; Blair & Imai, 2012; Glynn, 2013). For the list experiment, respondents were randomly assigned to either treatment or control group and then presented with a list of survey items. Whereas the control

group only received a number of control items, the treatment group additionally received the sensitive item along with those control items.

The list experiment asked the following question: "*Now we would like to ask you some more questions about what happened during the war. Please refer to the following list and tell me how many of these experiences happened to you during the war. Please don't tell me which specific statements you believe to be true, only how many.*"

The interviewers then showed the respondents a list with the following items:

[1] I won money in a lottery or competition.

[2] I was involved in an accident.

[3] I received help from a stranger.

The list shown to the treatment group also included the sensitive item:

[4] I was personally sexually assaulted.

All respondents were asked to count the number of items that apply to them or with which they agree. The reasoning behind this indirect question format is that victims of sexual violence understand the anonymity granted by it in the interview situation. Since respondents do not have to say openly whether the sensitive item applies and the interviewer has no way of knowing, victims are less likely to under-report their experience due to feelings of shame or fear of legal consequences. Traunmüller, Kijewski and Freitag (2019) provide further information on randomization and sample balance.

3) Test of No Design Effect. In addition to randomization, the analysis of list experiments rests on two other assumptions (Blair & 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 the list experiments and thus supports the assumption (see table A.1). The Bonferroni-corrected p-value is 1.

TABLE A.1. Test of No Design Effect in List Experiment

	Est.	S.E.
$p_i(Y_i(0) = 0, Z_i = 1)$	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	

4) **The direct question.** The questionnaire also included a direct question on sexual violence as part of a survey battery capturing various direct war experiences. The direct question was asked after the list experiment (Eady, 2017). The list experiment and the direct question were part of different sections in the questionnaire, with five questions/item batteries between them to avoid any priming effects. Moreover, respondents had the option to skip these questions; thus only those who were willing to speak about their wartime experiences provided a response.

   The question read: "*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?*" The answers to the item "You becoming sexually assaulted" were coded "1" for "yes" and "0" for "no" to the item.

   We use this additional item to compare the results between direct and indirect measures of wartime sexual violence in A.4.1 and A.4.3 below.

5) **The measure of sexual violence.** The measure used to capture sexual violence remains quite general and thus follows a definition that is broader than the frequently used legal definition of the International Criminal Court (2000), which includes rape, sexual slavery, forced pregnancy or sterilization, and abortion. Similar to Wood (2009) and the *Sexual Violence in Armed Conflict* (SVAC) data set (Cohen & Nordås, 2014), it also captures acts of related physical violence such as sexual mutilation and sexual torture. Following Leiby (2009), the measure also includes experiences that do not involve direct physical violence such as sexual humiliation and sexual coercion.

   In addition, the list experiment is silent about the perpetrators of the act of sexual violence. Thus, while it clearly refers to experiences that "happened during the war," it is not restricted to conflict-related sexual violence committed by armed groups. It is likely to also elicit experiences of sexual violence committed by intimate partners, acquaintances, and strangers. The list experiment thus relies on an inclusive and low-threshold definition of sexual violence, which is in line with the definition stated at the Conflict-Sexual Violence Special Report by the United Nations (2019).

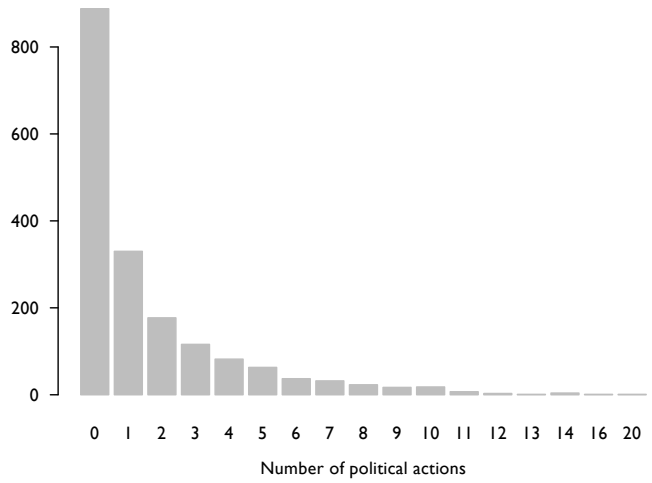
A.2 Information on Outcome and Control Variables: Survey Questions and Coding

1) **The outcome variable: Political participation.** The survey question for political participation read "*During the last twelve months, have you done any of the following?*" Then respondents were then presented with a list of twenty-two binary items ranging from attending political meetings to participating in illegal protests. The items are (sorted in descending order by percentages in "yes" category, given in parentheses): attended a political meeting/rally (35.3); contacted a local government official (17.8); participated in party activities (17.6); contacted a

politician (13.9); done voluntary work for a party (11.6); worked for the campaign of a candidate for office (8.5); contacted a political organization (7.9); signed a petition (6.2); worked in a political party or action group (5.7); boycotted certain products (5.2); visited websites of political organizations or candidates (3.6); worn or displayed a campaign badge/sticker (3.2); taken part in a lawful public demonstration (3.2); participated in political activities over the Internet (3.1); donated money to a party (2.9); deliberately bought certain products for political, ethical, or environmental reasons (2.8); raised funds (2.2); worked in another political organization or association (2.1); taken part in a strike (1.9); cancelled vote at the voting station (1.9); posted political content on Twitter (1.0); or, participated in illegal protest activities (0.9).

We operationalized political participation as an additive index of these twenty-two different political actions. Figure A.2.1 shows the frequency of the additive index for political participation.

FIGURE A.2.1. The Outcome Variable: Political Participation



Note: The graph shows the frequency of the number of political actions that respondents engaged in during the twelve months prior to the survey.

2) Control variables: Survey questions and coding. Table A.2.2 outlines information on the wording and the coding of the survey items used as control variables in the main models.

TABLE A.2.2. Control Variables: Survey Items and Coding Criteria

Variable	Wording and Coding
Gender	"[Interviewer: Please indicate the sex of the respondent]" Male 0 Female 1
Age	"How old are you? Please indicate in years."
Education	"What is the highest level of school education you have achieved?" *Categorical Coding:* No formal level of education 0 Primary school 1 Junior secondary school (until Grade 9) 2 GCE O Level (Grade 10-11)3 GCE A Level (Grade 12-13) 4 BA level or equivalent 5 MA level or equivalent 6 Doctoral level or equivalent 7 Low education 0-1 Medium education 2-3
Ethnicity	"How would you describe your ethnic identity?" *Dummy Coding:* Sinhalese 0/1 Sri Lankan Tamil 0/1 Indian Tamil 0/1 Moor 0/1
Province	*Categorical Coding:* Western 1 Central 2 Southern 3 North Western 4 North Central 5 Uva 6 Sabaragamuwa 7 Eastern 8 Northern 9
Prewar: Political involvement	"In the time before the war, how often were politics discussed at your house?" Never 1 From time to time 2 Often 3 Always 4
Prewar: NGO	"Before the war, did you or any member of your close family work as a humanitarian worker or for an NGO?" No 0 Yes 1

TABLE A.2.2. Control Variables: Survey Items and Coding Criteria (*continued*)

Variable	Wording and Coding
Assisted military group	"Did you assist the Sri Lankan army or other military groups during the war?" No 0 Yes 1
Displaced	"Did you or any members of your household have to move as a result of the conflict?" No 0 Yes 1
Other traumatic experience	"Now we would like to ask you some questions about what happened between 1983 and 2009. These are not questions about your feelings, they are questions about what happened to you and what you experienced. We know that this is very personal, and we are troubled to ask, but we hope that you will think that it will be important to study how many people in this country had such experiences, and how they are related to people's attitudes now. Did you see or witness with your own eyes and ears a war-related event that involved actual or threatened death or injury to you or any member of your household to which you responded with intense fear, helplessness, or horror?" No 0 Yes 1

A.3 Additional Information on List Experiments

To provide some intuition on how the sensitive item outcome is predicted, first note that while we cannot observe an individual's sensitive outcome, the *joint distribution*, $\pi(C_i, Z_i^*)$, of control items C_i and the latent sensitive item Z_i^* *is identified* and can be estimated (Glynn 2010, Blair and Imai 2012). For instance, $\pi(2,1)$ says that the respondent affirms two control items and the sensitive item. This joint distribution *completely characterizes* each respondent's *type* for the purpose of statistical modeling.

To identify *each type* of respondent, we rely on the comparison between treatment ($T_i = 1$) and control group ($T_i = 0$). Assume a list experiment with J=4 list items as in the present study (i.e. a design with 3 control items + 1 sensitive item). A respondent in the *control group* who answers "3" can either be of type $\pi(3,1)$ (i.e. experience the sensitive item but was not asked about it) or of type $\pi(3,0)$ (i.e. no experience). A respondent in the *treatment group* who answers "3" can either be of type $\pi(2,1)$ (i.e. experience the sensitive item) or of type $\pi(3,0)$ (i.e. no experience). For respondents of this group, an answer of "4" and "0" perfectly identifies types $\pi(3,1)$ (i.e. experience) and $\pi(0,0)$ (i.e. no experience). For each remaining type, experiencing the sensitive item, $Z^* = 1$, is identified by comparing oberservable proportions $Pr(C_i \leq y|T_i = 0) - Pr(C_i \leq y|T_i = 1)$. For instance, the individual probability of experiencing the sensitive item when the observed answer is "3" is $Pr(C_i \leq 3|T_i = 0) - Pr(C_i \leq 3|T_i = 1)$. This is $1 - Pr(C_i \leq 3|T_i = 1)$ because in the *control group* everybody has $y \leq 3$ as they were not asked the sensitive item. This, in turn, is $1 - (1 - Pr(\pi(3,1)|T_i = 1))$

which we know since in the *treatment group* $Pr(\pi(3,1))$ is observed as the probability of answering "4". The same applies to the remaining categories.

A.4 Additional Analyses

This section provides information on additional analyses on the political consequences of sexual violence in Sri Lanka.

1) **The prevalence of wartime sexual violence.** We replicate Traunmüller, Kijewski and Freitag (2019) approach to obtain the prevalence of sexual violence. We thus compare the responses between treatment and control groups—without the respondents having to disclose their choice of item. We then drawn inferences on the sensitive item. Since randomization ensures that treatment and control units are the same in all observable and unobservable characteristics, any difference in response must be attributed to the sensitive item.

   Table A.4.1 shows the mean number of list items affirmed by the treatment (.44) and the control (.31) groups. This results in an estimated difference-in-means of 13.4% of the sample that experienced personal sexual assault during the time of war. Table A.4.1 also compares the results from the list experiment to the direct question item, where only 1.4% of respondents admit to a personal experience of sexual violence. The difference between the unobtrusive measure and the direct item is 12%. The list experiment thus revealed a prevalence of wartime sexual violence that is ten times higher than a direct question did.

Table A.4.1. Prevalence of Wartime Sexual Violence: Result of the List Experiment

	List Experiment		Direct Item	
Mean Number of Items Treatment Group	.441	(.024)		
Mean Number of Items Control Group	.307	(.019)		
Estimated % Experiencing Sexual Assault	13.4 %	(3.1)	1.4 %	(0.3)
Difference Indirect/Direct	12.0%	(3.1)		
Number of Observations	1800		1424	

Notes: The table displays point estimates and standard errors in parentheses. The difference in the number of observations for the list experiment and the direct item is due to participants not answering the direct question.

2) Factor Structure of Social and Political Participation Items.

TABLE A.4.2. The Factor Structure of Social and Political Participation Items

	Factor			
	1	2	3	4
Youth societies		0.89		
Cultural organisation		0.87		
Environmental or human rights organisation		0.80		
Sports club		0.75		0.45
Leisure or hobby organisation		0.73		
Religious organisation		0.71		
Interest group		0.53		
Charity or social-welfare organisation		0.39		-0.36
Participate in party activities	1.08		-0.30	
Worked for the campaign of a candidate for office	0.99			
Worked in a political party or action group	0.97			
Do voluntary work for a party	0.91			
Contacted a politician	0.89			
Worn or displayed a campaign badge/sticker	0.72			
Attended a political meeting/rally	0.70			
Contacted an political organisation	0.68			
Contacted a local government official	0.67			
Donate money to a party	0.56			
Taken part in a lawful public demonstration	0.43			
Deliberately bought certain products			0.91	
Boycotted certain products			0.78	
Participated in illegal protest activities			0.73	
Raised funds			0.63	
Signed a petition			0.62	
Cancel vote at the voting station			0.42	
Worked in another political organization or association	0.38		0.34	
Taken part in a strike			0.30	
Participated in political activities over the internet	0.35			0.81
Visited websites of political organizations or candidates				0.73
Twitter political content				0.71

Note: The table displays factor loadings $> |.30|$. Results are based on tetrachoric correlations and promax rotation.

3) Survivors' political participation. For the main estimations, we followed the multivariate modelling strategy proposed by Imai, Park & Greene (2015). This simultaneously models the response to the sensitive item, the control items, and the outcome of interest. In particular, we include the predicted responses regarding experiences of sexual violence as an explanatory variable in the regression model of political participation (outcome equation). Table A.4.3 presents the results for each of these equations. The first column refers to the outcome equation for political participation (see Figure 1 in the main text for the visualization of the effects). The second, third, and fourth columns present the (simultaneous) equation results for the latent sensitive item, the control items, and the direct item respectively. We estimate this joint model using the EM algorithm implemented in the R package "list" (Blair et al., 2015).

4) Survivors' political participation using a direct measure of wartime sexual violence. It is instructive to contrast our findings to a simple linear

TABLE A.4.3. Multivariate Results on Sexual Violence Victims' Political Participation by Individual Equations

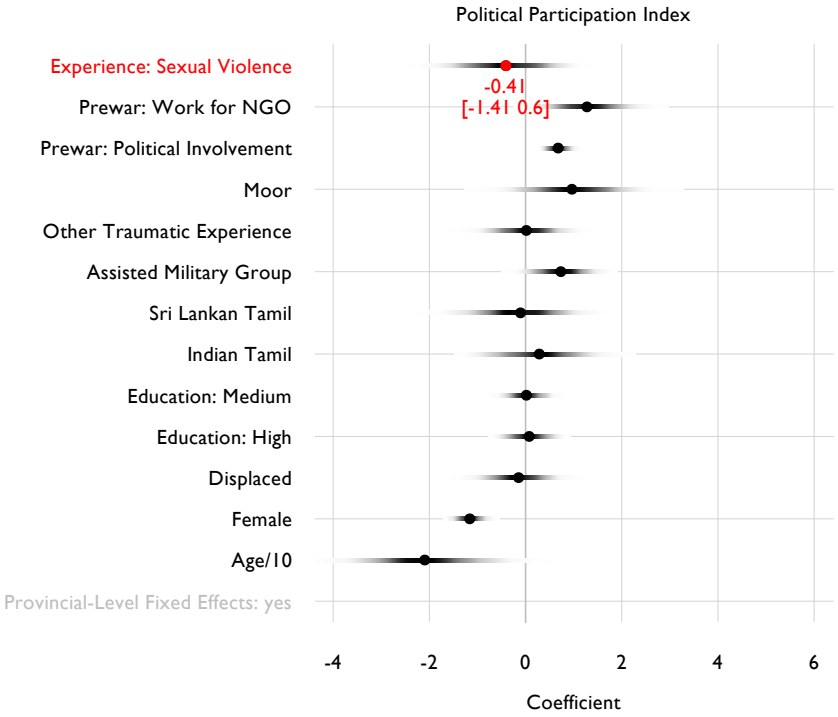
			Items					
Equations:	Outcome		Sensitive		Control		Direct	
Intercept	1.61	(0.53)	-1.53	(1.18)	-3.05	(0.41)	2.55	(0.58)
Female	-0.70	(0.14)	-1.41	(0.38)	-0.07	(0.11)	-1.16	(0.16)
Age/10	-1.52	(0.56)	-0.93	(1.33)	0.07	(0.43)	-2.10	(0.66)
Education: Medium	0.04	(0.20)	0.15	(0.50)	-0.11	(0.15)	0.02	(0.20)
Education: High	-0.05	(0.24)	0.33	(0.55)	-0.01	(0.18)	0.08	(0.25)
Ethnicity: Sri Lanka Tamil	0.20	(0.39)	-0.85	(0.81)	0.61	(0.28)	-0.10	(0.53)
Ethnicity: Indian Tamil	0.18	(0.34)	0.80	(0.64)	0.07	(0.26)	0.29	(0.52)
Ethnicity: Sri Lanka Moor	0.51	(0.39)	0.03	(0.88)	-0.09	(0.33)	0.96	(0.58)
Province: Central	-0.07	(0.32)	-1.68	(0.75)	-0.50	(0.39)	-0.57	(0.41)
Province: Southern	-0.14	(0.34)	-0.27	(0.62)	0.56	(0.33)	-0.17	(0.36)
Province: North Western	-0.51	(0.40)	-1.79	(1.03)	0.03	(0.39)	-0.75	(0.46)
Province: North Central	-0.19	(0.39)	-0.36	(0.68)	-1.05	(0.47)	-0.21	(0.39)
Province: Uva	-0.82	(0.47)	-0.38	(0.73)	1.77	(0.32)	-0.54	(0.48)
Province: Sabaragamuwa	-0.24	(0.36)	-0.66	(0.72)	-0.14	(0.37)	-0.40	(0.39)
Province: Eastern	0.29	(0.44)	-0.18	(1.03)	0.37	(0.32)	0.26	(0.57)
Province: Northern	0.06	(0.51)	-1.80	(1.28)	-0.27	(0.33)	-0.26	(0.63)
Assisted Military Group	0.24	(0.32)	0.81	(0.60)	0.58	(0.20)	0.73	(0.32)
Other Traumatic Experience	0.29	(0.24)	-0.05	(0.61)	0.13	(0.18)	0.02	(0.36)
Displaced	-0.56	(0.28)	0.59	(0.70)	0.52	(0.22)	-0.14	(0.35)
Prewar: Political Involvement	0.57	(0.14)	0.28	(0.31)	0.37	(0.10)	0.68	(0.14)
Prewar: NGO	0.67	(0.33)	1.65	(0.70)	0.12	(0.24)	1.28	(0.42)
Wartime Sexual Violence (Indirect Item)	5.52	(0.22)	–	–	–	–	–	–
Wartime Sexual Violence (Direct Item)	–	–	–	–	–	–	-0.41	(0.51)
N	1081		1081		1081		915	

Notes: The table displays point estimates and standard errors in parentheses. For a visualization of the effects from the model on the "Outcome" equation and the "Direct Item" equation, see Figure 1 (in the manuscript) and Figure A.4.3 below.

model which relies on a direct measure of experiencing wartime sexual violence as a predictor for political participation. Figure A.4.3 visualizes the results based on the estimations reported in Table A.4.3, last columns. This analysis yields the results we would have generated without our experimental measure of wartime sexual violence. The coefficient for survivors of such violence is much smaller, negative, and nonsignificant ($\beta = -0.41[-1.41, 0.59]$). This demonstrates the benefits of list experiments in assessing the consequences of wartime sexual violence. The effects of the remaining individual covariates are more or less robust. A notable exception is collaboration with a military group, which is now positively related to postconflict political participation ($\beta = 0.73[0.10, 1.36]$). This effect is spurious, because LTTE collaborators and their family members were among the high-risk group regarding wartime sexual violence—which is not apparent when using a direct measure rather than a list experiment (Traunmüller, Kijewski and Freitag, 2019).

5) **Survivors' type of political participation.** We analyzed how survivors of wartime sexual violence try to influence political outcomes to better understand the rationale behind the mobilizing effect of the latter. We subjected the twenty-two items that constitute the political participation index to factor analysis. Table 1 (in the manuscript) shows the results. There are three underlying factors that

FIGURE A.4.3. The Effect of Wartime Sexual Violence on Political Participation Using a Direct Measure of Sexual Violence



Note: Coefficients refer to the difference in reported political activities, and are based on the estimations reported in Table A.4.3. Full inferential uncertainties were obtained by simulation.

best explain the variation in the type of political participation. The first gathers together political items that relate to *institutionalized* political participation. The second factor subsumes more *noninstitutionalized* forms of political participation. The third and final one corresponds to political participation *online*.

6) **Survivors' participation in elections.** We also estimated whether the experience of wartime sexual violence is related to turnout in presidential, parliamentary, and local elections. Table A.4.4 presents the results. Victims of wartime sexual violence are not more likely to cast a vote in elections that nonvictims.

TABLE A.4.4. The Effect of Wartime Sexual Violence on Election Turnout

	Elections					
	Presidential		Parliamentary		Local	
Intercept	0.25	(0.07)	0.67	(0.06)	0.03	(0.08)
Female	-0.01	(0.02)	-0.01	(0.02)	0.00	(0.02)
Age/10	0.06	(0.08)	0.46	(0.08)	0.43	(0.09)
Education: Medium	-0.02	(0.03)	0.02	(0.03)	-0.02	(0.04)
Education: High	-0.02	(0.04)	-0.00	(0.03)	-0.03	(0.04)
Ethnicity: Tamil	0.54	(0.04)	0.04	(0.03)	0.54	(0.05)
Eastern Province	0.20	(0.03)	0.05	(0.03)	0.22	(0.03)
Assisted Military Group	0.02	(0.08)	0.07	(0.08)	-0.06	(0.06)
Other Traumatic Experience	0.10	(0.03)	-0.02	(0.03)	0.06	(0.03)
Displaced	0.11	(0.03)	-0.01	(0.03)	0.14	(0.03)
Wartime Sexual Violence	-0.06	(0.08)	0.03	(0.08)	0.03	(0.08)
Prewar: Political Involvement	-0.06	(0.02)	0.00	(0.02)	-0.08	(0.02)
Prewar: NGO	0.16	(0.07)	-0.02	(0.04)	0.19	(0.06)
N	1,081		1,081		1,081	

Note: The table displays point estimates and standard errors in parentheses.

A.5 Sensitivity Analysis

A.5.1 Sensitivity to Violation of the 'No Liars' Assumption

The results of the list experiment build on the crucial assumption that participants respond truthfully to the sensitive item on wartime sexual violence. While this 'no liars' assumption cannot be tested, we can assess the sensitivity of our results to its potential violation. In particular, we can accommodate the possibility of 'floor effects' in answers to the list experiment (Blair & Imai, 2012). A floor effect occurs when respondents whose truthful answer would only affirm the sensitive item, report instead that none apply out of fear that their true experience would be revealed. This may happen if the baseline items have low prevalence, which is the case in our list experiment as indicated by the low average number of affirmed list items. Intuitively, such an effect should lead to an underestimate of the true proportion of victims of wartime sexual violence and affect our results.

Following the suggestion of Blair and Imai (2012), we first estimate the population proportion of potential liars using an intercept-only MLE model of wartime sexual violence *allowing for floor effects*. Results are reported below, on Table A.5.1. This yields a very large estimated proportion of potential 'floor' liars of 61 percent. However, the inferential uncertainty is too large for this estimate to be meaningful. Second, we estimate a simple intercept-only MLE model *without floor effects*. Comparing both models we find that, in fact, the model fit of the floor effect model shows little improvement to that of the model without floor effects (BIC of 2955 vs. 2957). There are, however, differences in the prevalence estimates. The MLE estimate of wartime sexual violence without floor effects shows a prevalence of 12.3 percent (95% CI: [8.7, 17.1]). Accounting for floor effects, the MLE estimate increases considerably to 21.0 percent (95% CI: [14.3, 29.8]). This suggests that the 'no liar' assumption may be problematic. But, how problematic?

TABLE A.5.1. Comparison of models with and without floor effects (MLE).

	Est.	SE	Est.	SE
Sensitive Item Equation				
Intercept	-1.963	(0.199)	-1.326	(0.238)
Prevalance: Wartime Sexual Violence	12.3		21.0	
Floor Effects	No		Yes	
Floor Effect Equation				
Intercept	–	–	0.468	(0.455)
Prevalance: Floor Liars	–	–	61.5	
N	1800		1800	
Log Likelihood	-1463.370		-1454.885	
BIC	2957		2955	

To assess the robustness of our results to the violation of the 'no liars' assumption, we conducted a simulation based sensitivity analysis. Based on the difference in prevalence estimates between the MLE models with and without floor effects, we expect a proportion of liars of $21.0 - 12.3 = 8.7$ percent (i.e. respondents who could be potential victims but still lied about their experience because of floor effects). To incorporate this share in the outcome model of political participation, we randomly declared respondents as liars by drawing from a Bernoulli distribution with success probability of $\pi = .087$ (i.e. the share of potential victims who lied because of floor effects). For all such randomly created liars that are in the treatment group and report an item count lower than 4 we then assign an affirmative answer to the sensitive item by adding 1 to their response. We repeat this procedure a total of $S = 1,000$ times, creating a different random set of liars and survivors of wartime sexual violence each time. For each of these S simulations we then estimate the effect of wartime sexual violence on political participation using the same specification as in the main result and yielding a total of 1,000 effect estimates. The variation in effect estimates reflects our uncertainty about who the liars are and gives a sense of how this uncertainty may change our results. We find that 95% of all simulated effects lie between 5.17 and 5.67, as shown in Figure 2, in the manuscript. This effect range is well within the confidence interval of the original estimate (β=5.52 [5.09, 5.96]). In other words, it is very unlikely that the presence of liars and therefore the violation of the 'no liars' assumption jeopardizes our main result.

A.5.2 Sensitivity to Unobserved Confounding

Following Cinelli & Hazlett (2020), we can re-express the two quantities in this product in more intuitive terms as hypothetical *partial* R^2s. First, the amount of variance left in political participation when taking into account all controls X, which is explained by the unobserved confounder, $R^2_{Y\sim U|X}$. And second, the amount of variance left in experiencing sexual violence when taking into account all controls, which is explained by the unobserved confounder, $R^2_{Z^*\sim U|X}$. The bias from unobserved confounding can then be derived as:

$$Bias = SE_{Z^*} * \sqrt{\frac{R^2_{Y\sim U|X} R^2_{Z^*\sim U|X}}{1-R^2_{Z^*\sim U|X}}(df)},$$

where SE_{Z^*} is the standard error of the coefficient for wartime sexual violence's effect on political participation and *df* the degrees of freedom of the respective regression equation.

A.5.3 Sensitivity to Sample Selection Due to Loss of Observations

In our main model specification, we control for prewar measures of political involvement and civic engagement to establish credible causal effects. However, this came with the cost of ignoring almost a third of the sample that refused to answer the survey module on prewar experiences, raising concerns about sample selectivity. Yet, it could be the case that those who refused to answer were more likely to have experienced sexual assault or, perhaps, were motivated not to respond because their prewar involvement was of a certain type. To address these concerns, we conducted separate analyses for the sub-sample that refused to answer the prewar module, the sub-sample that answered it, and the complete sample.

We first assess whether the different sub-samples differ in their degree of victimization. Table A.5.2 below provides the respective prevalence from the list experiment for those who refused to answer the prewar module (7%, SE: 5%, N=558), those who answered it (17%, SE: 4%, N=1,201) and the complete sample (13%, SE: 3%, N=1,800). We find that while the prevalence among those who refused to answer the prewar module is estimated only with high uncertainty (as we would expect, given the small sample size), it is only half the size of the prevalence in the remaining samples. Thus, it is not the case that it is more likely for victims to refuse to answer the prewar module. More importantly, the prevalence of wartime sexual violence in the sample that answered the prewar module is not significantly different from the other two samples.

TABLE A.5.2. Prevalence of wartime sexual violence for different sub-samples based on prewar political behaviour.

	N	Est.	SE
Refused Prewar Survey Module: Prevalence of Wartime Sexual Violence	558	0.07	(0.05)
Answered Prewar Survey Module: Prevalence of Wartime Sexual Violence	1201	0.17	(0.04)
Complete Sample: Prevalence of Wartime Sexual Violence	1800	0.13	(0.03)

To test whether sample selectivity plagues our effect estimates of wartime sexual violence, we ran two additional model specifications. The first model was run on the restricted sample who provided answers to their prewar behavior but *without* the two prewar control variables (N=1,081). Comparing this model to our original model tells us how the prewar controls affect our estimate for the effect of wartime sexual violence. The second model uses the full sample (and by necessity does not include the prewar controls, N=1,674). Comparing this second model to the first thus tells us something about the potential influence of sample selectivity.

We report the results of these two models in Table A.5.3 below. We find that omitting prewar variables from the model changes our original effect estimate very little: $\beta = 5.60$, [5.17, 6.03] (the original estimate was $\beta = 5.52$ [5.09, 5.96]).

Re-running the model on the full sample yields a somewhat larger effect estimate: $\beta = 6.40$ [6.09, 6.71]. Taken together, this suggests that the restricted sample consists of respondents with higher base-line participation (intercept estimate of 2.44 political actions) than the full sample (intercept estimate of 1.53 political actions) and, if anything, reacts less strongly to the experience of wartime sexual violence. Put differently, our original estimate is conservative and not threatened by issues of sample selectivity.

TABLE A.5.3. Alternative models for different sub-samples based on prewar political behaviour.

	Restricted sample w/o prewar controls		Full sample w/o pre-war controls	
	Est.	SE	Est.	SE
Intercept	2.44	(0.49)	1.53	(0.33)
Female	-0.77	(0.14)	-0.70	(0.11)
Age/10	-1.21	(0.57)	0.10	(0.39)
Education: Medium	0.12	(0.20)	0.15	(0.17)
Education: High	0.05	(0.24)	0.02	(0.19)
Ethnicity: Sri Lanka Tamil	-0.07	(0.39)	0.59	(0.28)
Ethnicity: Indian Tamil	-0.16	(0.31)	0.28	(0.20)
Ethnicity: Sri Lanka Moor	0.40	(0.39)	0.83	(0.28)
Province: Central	-0.00	(0.31)	-0.05	(0.22)
Province: Southern	-0.19	(0.34)	0.08	(0.22)
Province: North Western	-0.37	(0.40)	-0.54	(0.30)
Province: North Central	-0.21	(0.40)	0.03	(0.26)
Province: Uva	-0.50	(0.47)	-0.75	(0.24)
Province: Sabaragamuwa	-0.23	(0.37)	-0.24	(0.26)
Province: Eastern	0.24	(0.43)	-0.30	(0.33)
Province: Northern	-0.06	(0.51)	-0.36	(0.38)
Assisted Military Group	0.33	(0.31)	0.15	(0.24)
Other Traumatic Experiences	0.29	(0.24)	0.18	(0.20)
Displaced	-0.40	(0.28)	-0.29	(0.21)
Wartime Sexual Violence (Indirect Item)	5.60	(0.22)	6.40	(0.16)
N	1081		1674	

One could also ask whether those respondents who *refused* to answer the module about pre-war behaviors are impacted differently by experiences of wartime sexual violence. Unfortunately due to computational issues, we cannot estimate the same model specification on this sub-sample (there are perfect collinearities in the design matrix). Instead, we propose to compare the simple bivariate effect of wartime sexual violence on political participation across the three samples. The results are reported in Table A.5.4 below. We find that those who refused to answer the pre-war module have a lower base-rate level of political participation (intercept estimate 0.98) and actually show a greater effect for wartime sexual violence (β=7.57 [7.12, 8.02]) than the other samples. Again, our original estimate is conservative.

TABLE A.5.4. Alternative bivariate models for different sub-samples based on prewar political behaviour.

	N	Est.	SE
Refused Prewar Survey Module: Intercept (Baseline Political Participation)	558	0.98	(0.12)
Answered Prewar Survey Module: Intercept (Baseline Political Participation)	1201	1.27	(0.08)
Complete Sample: Intercept (Baseline Political Participation)	1800	1.19	(0.07)
Refused Prewar Survey Module: Effect of Wartime Sexual Violence	558	7.57	(0.23)
Answered Prewar Survey Module: Effect of Wartime Sexual Violence	1201	6.23	(0.18)
Complete Sample: Effect of Wartime Sexual Violence	1800	6.64	(0.14)

A.5.4 Sensitivity to Sample Selection Due to Unsampled Population

Another potential threat to causal inference is the possibility that less politically active individuals are more likely to die, be displaced, or flee the country (Gilligan, Pasquale and Samii, 2014). Thus, to test whether the effect that experiencing wartime sexual has on political participation is due to selection bias, we need to understand how this effect differs for the unsampled population. We first note that unit nonresponse was virtually absent in our study. This excludes the possibility that victims who are particularly politically passive also refused to participate in the survey. Still, the survey sample would not have covered Sri Lankan refugees at the time it took place—who, according to the United Nations High Commissioner for Refugees (2016), then amounted to 117,447 persons. When added to the 2016 Sri Lankan mid-year population of 21.2 million, this amounts to $p_{unsampled}$=0.6% of the total population.

We engage again in counterfactual reasoning about what the unsampled population might look like and how this would impact on our findings to understand whether our results are driven by a selection effect. The logical possible upper bound for a scenario that runs against our findings would be a situation where everyone from the unsampled population experienced wartime sexual violence, politically participated at the maximum level if they did not experience sexual violence, and completely refrained from participating if they did. Formally, participation without experience of wartime sexual violence would amount to Y_{0i}=22 and with experience hereof to Y_{1i}=0 for all i, thus resulting in an average treatment effect of $ATE_{unsampled}$=-22 for the unsampled population. To investigate how this logical upper bound impacts the overall effect of wartime sexual violence on political participation, we look at the weighted average of this effect: $ATE = p_{sampled}ATE_{sampled} + p_{unsampled}ATE_{unsampled} = .994 \times 5.52 + .006 \times (-22) \approx 5.35$. Clearly, accounting for the potential issue of selection does not nullify a large and positive effect of experiencing wartime sexual violence on subsequent political participation. Therefore, we reject any selection bias in our results.

A.6 Probing the Mechanisms

This section provides the estimation outputs for the causal mediation analyses that help us evaluate the potential theoretical mechanisms connecting experiences of wartime sexual violence to political participation.

1) **Causal mediation analyses.** We evaluate several theoretical mechanisms connecting experiences of wartime sexual violence to political participation. We do so through causal mediation analysis. We decompose the effect of wartime sexual violence on political participation into two parts: an indirect or mediated effect that runs via a hypothesized mechanism, and a direct effect that captures all possible remaining influences on political behavior.

   We first assess whether experiences of wartime sexual violence lead to changes in social preferences (in-group favoritism, altruistic social preferences, and post-traumatic growth) and in social networks (measured as active social participation in civic associations). We then model respondents' political participation using a second equation including all variables as before, but now including social preferences and networks as predictors of political participation.

2) **Formal definition of causal quantities.** In the following, we give a formal description of our causal mechanisms and define more precisely how we think about the effect of wartime sexual violence on political participation. Let $Y_i(t)$ denote the potential value of the outcome for unit i under the treatment condition $T_i = t$. In our context, this specifies an individual's participation, given his or her experience of sexual violence during the conflict. $Y_i(t = 1)$ is the level of participation for an individual who personally experienced sexual violence and $Y_i(t = 0)$ how the exact same individual would participate if they had not had this experience. The effect of wartime sexual violence is then captured by $\tau_i(t) \equiv Y_i(t = 1) - Y_i(t = 0)$, the difference in individual i's behavior with and without experience of sexual assault. Of course, we can only ever observe one of i's potential participation choices at a given point in time; since we are usually interested in the whole population, the actual quantity of interest is the average treatment effect (ATE): $\bar{\tau}(t) \equiv E[Y_i(t = 1) - Y_i(t = 0)]$ across all individuals.

   In the causal mediation analysis, we now decompose this total effect into two parts: an indirect or mediated effect that runs via individuals' social preferences or social networks—and thus captures the theoretical mechanism—and a direct effect that captures all possible remaining influences on individuals' participation (cf. Imai et al., 2011). Formally, we introduce $M_i(t)$ to denote the potential *mediator* of unit i under the treatment condition $T_i = t$, i.e. individual i's social preferences or networks. Thus, $M_i(t = 1)$ is an individual's preference (network) if they have experienced sexual violence and $M_i(t = 0)$ is the same individual's preference (network) if they have not. Then $Y_i(t, m)$ is the potential outcome if the treatment takes the value t and the mediator the value m. In our case, how a respondent participates given a particular experience and his or her particular preference (network).

We can now define the *indirect* or *causal mediation effect* as $\delta_i(t) \equiv Y_i(t, M_i(1)) - Y_i(t, M_i(0))$, for unit i and treatment status $t = 0, 1$. We are thus interested in the difference in respondents' political participation that would occur given an experience of sexual violence and the political action that would occur under the same condition but where the individuals' social preference (network) is different—namely as it would be without this experience. Put differently, this is the effect wartime sexual violence has on an individual's political participation that is only due to the change in social preference (network).

By fixing the experience and only changing the individual's social preference (network) we isolate our hypothesized mechanism from all other possible mechanisms through which wartime sexual violence may impact on participation (Imai et al. 2011). Again, we are interested in the ACME $\bar{\delta}(t)$, i.e. how the population participates compared to how the population would participate if we changed their social preference (network), while holding their actual experience constant.

There may exist alternative ways in which experiences of wartime sexual violence impact on individuals' political action. These alternatives are captured by the *natural direct effect* $\xi_i(t) \equiv Y_i(1, M_i(t)) - Y_i(0, M_i(t))$, for unit i and treatment status $t = 0, 1$. This is the change in an individual's political participation when altering the experience but holding his or her social preference (network) constant. The ANDE $\bar{\xi}(t)$, therefore captures all effects of wartime sexual violence that impact on political participation but which do not work through respondents' social preferences (networks).

3) **Social networks as mediators of wartime sexual violence and political participation.** The fourth theoretical mechanism relates to social networks rather than individual preferences. To evaluate whether the political consequences of wartime sexual violence are due to mobilization in social networks, we rely on a simple social participation index. Like the previous mechanisms, Table A.6.1 shows the estimations for the two stages of the causal mediation analysis. We exclude variables on prewar political involvement and social participation to be consistent with previous analyses. The results are different to the alternative social preference mechanisms that we test below. Active participation in social networks significantly mediates the relationship between experiences of wartime sexual violence and political participation (see also Figure 4 in the main text).

4) **Alternative mechanisms: Social preferences as mediators of wartime sexual violence and political participation.** We identify three social preference mechanisms: in-group favoritism, altruistic social preferences, and post-traumatic growth. We report results below. Across tables, the mediator is first the outcome variable and then a predictor for political participation. We exclude variables on prewar political involvement and social participation due to computational issues with perfect linear dependencies. The results suggest that none of the three social preference mechanisms account for the connection between individual experiences

TABLE A.6.1. Causal Meditation Analysis for *Social Networks*

	Mediator		Outcome	
	Social Networks		Political Participation	
(Intercept)	0.19	(0.18)	1.56	(0.43)
Female	-0.11	(0.06)	-0.67	(0.13)
Age/10	0.04	(0.23)	-1.48	(0.52)
Education: Medium	0.10	(0.08)	0.01	(0.19)
Education: High	0.30	(0.09)	-0.12	(0.23)
Ethnicity: Tamil	0.05	(0.08)	0.13	(0.16)
Eastern Province	-0.35	(0.10)	0.50	(0.19)
Assisted Military Group	-0.10	(0.11)	0.08	(0.33)
Other Traumatic Experience	0.12	(0.07)	0.40	(0.23)
Displaced	-0.17	(0.08)	-0.47	(0.23)
Prewar: Political Involvement	0.10	0.05	0.44	(0.12)
Prewar: NGO	0.57	0.08	0.59	(0.34)
Wartime Sexual Violence	2.74	(0.09)	5.60	(0.22)
Social Networks	–	–	0.32	(0.07)
N	1,081		1,081	

Note: The table displays point estimates and standard errors within parentheses.

of wartime sexual violence and higher levels of political participation in postwar Sri Lanka.

In-group favoritism—The first mechanism brought forward by existing work suggests that victims of sexual violence take up political action because of increased *social identification* and *in-group favoritism*. We measure this using several items of ethnic trust. 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 in-group favoritism from their trust toward the following ethnic groups: Sinhalese, Sri Lankan Tamil, Indian Tamil, and Muslim Moor. In-group favoritism is thus the difference between the amount of trust in their own ethnic group minus their average trust in the remaining three ethnic groups. Perhaps unsurprisingly, most Sri Lankans tend to trust their own ethnic in-group more than their ethnic out-groups (see A.5 a description of these variable).

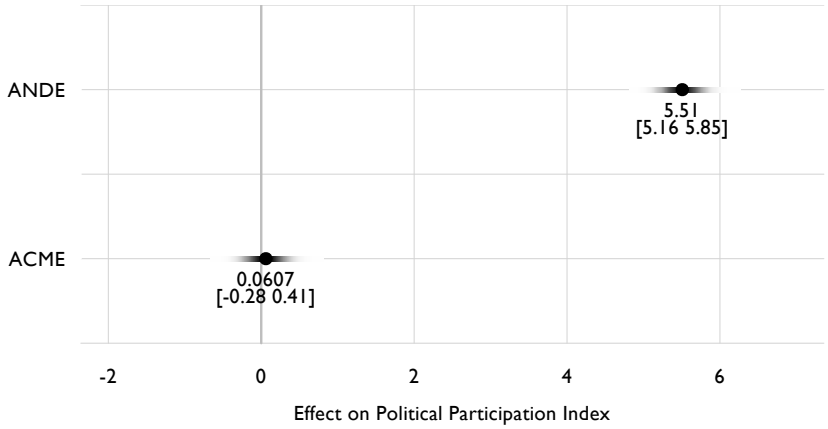
Table A.6.2 and Figure A.6.1 show the estimations for the two stages of the causal mediation analysis for social identification. We find that survivors of wartime sexual violence do not show significantly higher levels of in-group favoritism. Further, in-group favoritism is not a significant predictor of political participation. As a result there is no significant mediation effect (ACME: 0.06 [-0.28, 0.41]), but there is a strong and significant direct effect (ANDE: 5.51 [5.16, 5.85]) of wartime sexual violence on political participation. In sum, social identification and in-group favoritism do not mediate the effect of wartime sexual violence on political participation.

TABLE A.6.2. First Alternative Mechanism: Causal Meditation Analysis for *In-Group Favoritism*

	Mediator		Outcome	
	In-Group Favoritism		Political Participation	
(Intercept)	2.21	(0.36)	1.63	(0.45)
Female	0.26	(0.12)	-0.77	(0.14)
Age/10	-0.39	(0.46)	-1.62	(0.53)
Education: Medium	0.19	(0.16)	0.06	(0.19)
Education: High	-0.16	(0.19)	-0.01	(0.23)
Ethnicity: Tamil	-0.89	(0.17)	0.22	(0.17)
Eastern Province	-0.94	(0.17)	0.38	(0.19)
Assisted Military Group	0.46	(0.29)	0.39	(0.33)
Displaced	0.64	(0.22)	-0.54	(0.23)
Other Traumatic Experience	0.10	(0.19)	0.40	(0.22)
Prewar: Political Involvement	–	–	0.43	(0.12)
Prewar: NGO	–	–	0.82	(0.31)
Wartime Sexual Violence	0.12	(0.36)	5.74	(0.22)
In-Group Favoritism	–	–	0.07	(0.04)
N	1,062		1,062	

Note: The table displays point estimates and standard errors in parentheses.

FIGURE A.6.1. First Alternative Mechanism: In-group Favoritism Does Not Mediate the Effect of Wartime Sexual Violence on Political Participation



Note: The graph shows the ACME and the ANDE of in-group favoritism on political participation along with inferential uncertainty. The 95% confidence intervals in square brackets. Simulations are based on the estimations for the causal mediation analysis reported in Table A.6.2.

Altruism—The second mechanism posited by the literature is *altruistic social preferences*. To capture survivors' altruistic motivations in political participation, as a means to shape postconflict policies and reconciliation, we rely on two survey items. Respondents were asked how far they agreed with the following statements, using a four-point scale: The first reads, *"In order to avoid such violent conflicts in the future, everyone now needs to use their political voice to solve problems peacefully"*; the second, *"Now, after the war, it is time for my ethnic group to improve relations with other ethnic groups in this country."* The two items are

only weakly correlated ($r = .2$), and thus capture different aspects of postconflict politics (see section A.5 below for descriptive information on these survey items).

Table A.6.3, Table A.6.4 and Figure A.6.2 show the estimations for the two stages of the causal mediation analysis for the altruistic social preferences mechanism. We find that experience of wartime sexual violence significantly increases the desire for peaceful problem-solving but is unrelated to the desire for improving inter-ethnic relations. However, whereas the preference for the latter is related to higher levels of political participation, the preference for peaceful problem-solving after the war is not. Taken together, neither of these two altruistic social preferences are credible mediators of the effect of wartime sexual violence on political involvement.

TABLE A.6.3. Second Alternative Mechanism: Causal Meditation Analysis for *Political Voice*

	Mediator		Outcome	
	Political Voice		Political Participation	
(Intercept)	1.92	(0.14)	1.88	(0.47)
Female	-0.06	(0.04)	-0.71	(0.14)
Age/10	0.21	(0.17)	-1.52	(0.52)
Education: Medium	0.06	(0.06)	0.06	(0.19)
Education: High	0.08	(0.07)	0.03	(0.23)
Ethnicity: Tamil	0.36	(0.06)	0.24	(0.17)
Eastern Province	0.20	(0.07)	0.36	(0.20)
Assisted Military Group	-0.11	(0.11)	0.19	(0.33)
Displaced	-0.10	(0.07)	-0.54	(0.22)
Other Traumatic Experience	0.01	(0.06)	0.41	(0.22)
Prewar: Political Involvement	–	–	0.42	(0.13)
Prewar: NGO	–	–	0.29	(0.35)
Wartime Sexual Violence	0.12	(0.15)	5.83	(0.22)
Political Voice to Solve Problems	–	–	-0.08	(0.10)
N	1,071		1,071	

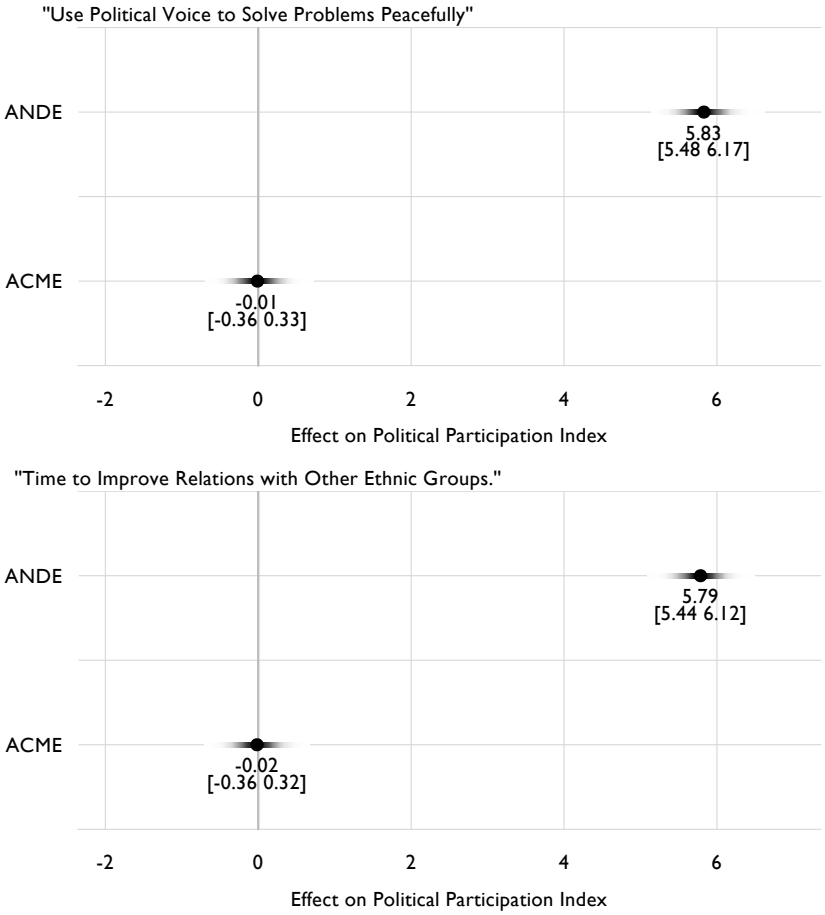
Note: The table displays point estimates and standard errors in parentheses.

TABLE A.6.4. Second Alternative Mechanism: Causal Meditation Analysis for *Improved Ethnic Relations*

	Mediator		Outcome	
	Improved Ethnic Relations		Political Participation	
(Intercept)	1.80	(0.15)	1.30	(0.47)
Female	-0.07	(0.05)	-0.69	(0.14)
Age/10	0.08	(0.19)	-1.61	(0.52)
Education: Medium	0.05	(0.07)	0.06	(0.19)
Education: High	-0.05	(0.08)	0.01	(0.23)
Ethnicity: Tamil	0.20	(0.07)	0.17	(0.17)
Eastern Province	0.69	(0.08)	0.23	(0.20)
Assisted Military Group	-0.02	(0.13)	0.22	(0.34)
Displaced	-0.33	(0.09)	-0.50	(0.22)
Other Traumatic Experience	0.09	(0.08)	0.39	(0.22)
Prewar: Political Involvement	–	–	0.46	(0.12)
Prewar: NGO	–	–	0.70	(0.32)
Wartime Sexual Violence	-0.08	(0.16)	5.72	(0.22)
Improved Ethnic Relations	–	–	0.22	(0.09)
N	1,073		1,073	

Note: The table displays point estimates and standard errors in parentheses.

FIGURE A.6.2. Second Alternative Mechanism: Altruistic Social Preferences Do Not Mediate the Effect of Sexual Violence on Political Participation



Note: The graphs show the ACME and the ANDE of political voice and improved inter-ethnic relation on political participation along with inferential uncertainty. The 95% confidence intervals in square brackets. Simulations are based on the estimations for the causal mediation analysis reported in Table A.6.3 and Table A.6.4 respectively.

Post-traumatic growth—The third and final social preference mechanism is the aforementioned *post-traumatic growth*. To measure this, we rely on a short form of the Post-Traumatic Growth Inventory (Cann et al., 2010; Tedeschi and Calhoun, 1996). This scale averages ten items included in the survey, which ask about personal changes that have occurred as a result of experiences during the war. A Cronbach's $\alpha = .93$ indicates very good scale reliability. However, since only respondents that actually reported any negative experiences during wartime were asked these items, the number of observations available for analysis drops considerably (leaving us with $N = 685$).

Table A.6.5 and Figure A.6.3 show the estimations for post-traumatic growth. We find that personal experience of wartime sexual violence is strongly and significantly related to higher levels of post-traumatic growth ($\beta = 1.39$ [0.98, 1.80]). Survivors of sexual violence report that their general outlook on life has changed for the better as a result of their traumatic experience. However, the post-traumatic growth scale does not reliably predict political participation. Figure A.6.3 presents the ACME

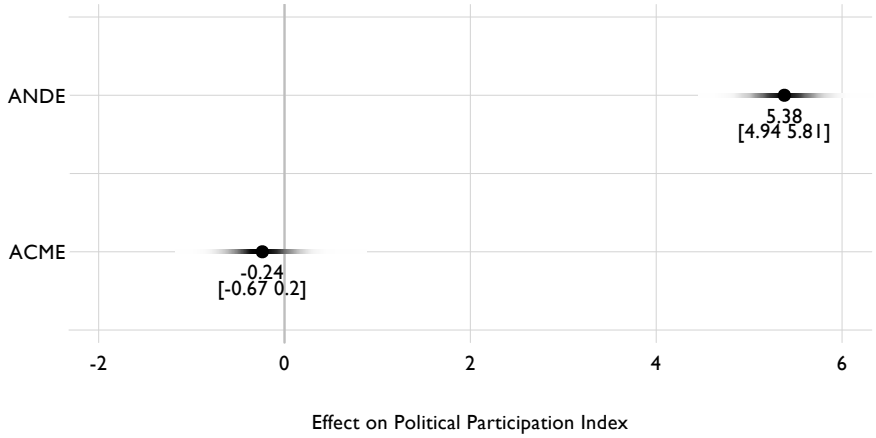
and ANDE of the relationship between sexual violence, post-traumatic growth, and political participation. While the estimated ANDE is again sizable and statistically significant (5.37 [4.94, 5.81]), the estimated ACME is not (-0.24 [-0.67, 0.20]). This suggests that, like with the other possible explanations, post-traumatic growth does not connect the individual experience of wartime sexual violence to a higher level of political participation.

TABLE A.6.5. Third Alternative Mechanism: Causal Meditation Analysis for *Post-Traumatic Growth*

	Mediator		Outcome	
	Post-Traumatic Growth		Political Participation	
(Intercept)	1.96	(0.19)	2.33	(0.62)
Female	-0.22	(0.07)	-0.70	(0.19)
Age/10	-0.18	(0.28)	-1.67	(0.70)
Education: Medium	-0.11	(0.09)	-0.09	(0.25)
Education: High	0.07	(0.11)	-0.05	(0.29)
Ethnicity: Tamil	0.01	(0.10)	0.05	(0.22)
Eastern Province	-0.54	(0.09)	0.01	(0.27)
Assisted Military Group	-0.26	(0.14)	0.26	(0.38)
Other Traumatic Experience	1.12	(0.13)	0.33	(0.30)
Displaced	0.65	(0.14)	-0.10	(0.31)
Prewar: Political Involvement	–	–	0.40	(0.17)
Prewar: NGO	–	–	1.02	(0.36)
Wartime Sexual Violence	1.39	(0.21)	5.89	(0.29)
Post-Traumatic Growth	–	–	-0.14	(0.12)
N	685		685	

Notes: The table displays point estimates and standard errors in parentheses. The post-traumatic growth items were only asked to respondents who provided information on their wartime experiences. This explains the massive drop in the number of observations.

FIGURE A.6.3. Third Alternative Mechanism: Post-Traumatic Growth Does Not Mediate the Effect of Sexual Violence on Political Participation



Note: The graph shows the ACME and the ANDE of post-traumatic growth on political participation along with inferential uncertainty. The 95% confidence intervals in square brackets. Simulations are based on the estimations for the causal mediation analysis reported in Table A.6.5.

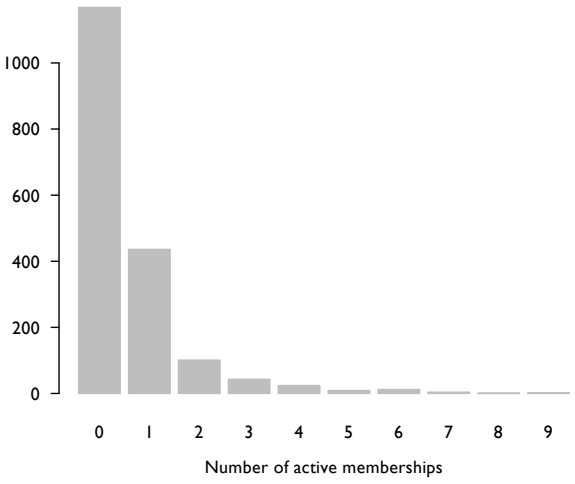
A.7 Additional Figures on Mediating Factors

This section provides further descriptions of the mediator variables used in the causal mediation analyses to test the mechanism of social networks (measured as active social participation in civic associations) and the alternative mechanisms of social preferences (in-group favoritism, altruistic social preferences, and post-traumatic growth) on the political consequences of wartime sexual violence in Sri Lanka.

1) **Social networks.** We operationalize social networks as active social participation in civic associations. The survey question read: "*Now I am going to read off 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. The complete list is as follows (by percentages of active members, in parentheses): charity or social welfare organization (e.g. women's societies, Samurdhi, 21.9); other organization (e.g. donation society, community development society, 28.6); sports club or outdoor activities club (7.9); religious organization (7.3); youth societies (5.2); interest group or trade union (3.9); political parties (3.1); cultural organization (music etc., 1.7); environmental or human rights organization (1.1); and, leisure or hobby organization (0.3). We ignore 'political parties' and 'other organization' to avoid tautological relations with political participation and add all remaining active memberships into an additive scale ranging from 0 to 8 (with a mean of 0.49 and SD of 0.90). Figure A.7.1 shows the frequency of active memberships respondents hold.

FIGURE A.7.1. Active Social Participation in Civic Associations

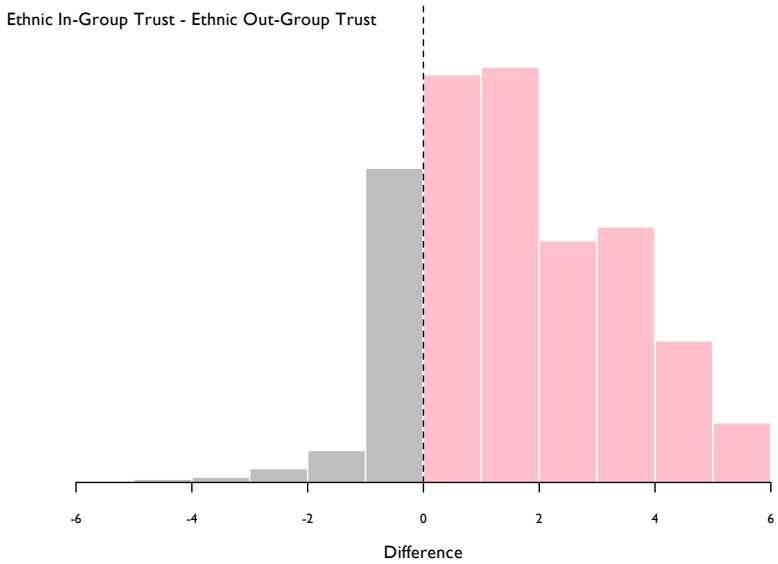


Note: The graph shows the frequency of respondents' active memberships in social organizations.

2) **Social identification and in-group favoritism.** We use information from the survey to operationalize social identification and in-group favoritism. 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 in-group favoritism from their trust toward the following ethnic groups: Sinhalese, Sri Lankan Tamil, Indian Tamil, and Muslim Moor. In-group favoritism is thus the difference between the amount of trust in their own ethnic group minus their average trust in the remaining three ethnic out-groups. Figure A.7.2 shows the distribution of this variable.

FIGURE A.7.2. Social Identification and In-group Favoritism

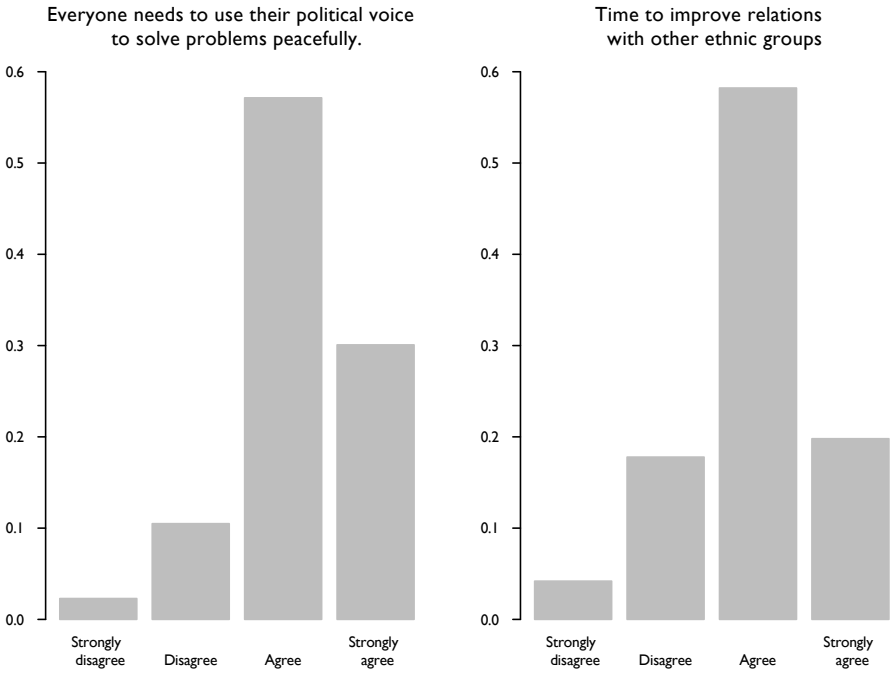


Notes: Numbers on the x-axis depict the differences between in- and out-group trust. Positive numbers represent more in-group trust (red bars), while negative ones denote out-group trust (grey bars). Most Sri Lankans have higher trust in their own ethnic in-group than in any other out-groups.

3) **Altruistic social preferences.** We use two items from the survey to operationalize altruistic social preferences. Respondents were asked how far they agreed with the following statements using a four-point scale: The first read, "*In order to avoid such violent conflicts in the future, everyone now needs to use their political voice to solve problems peacefully.*" The second statement read, "*Now, after the war, it is time for my ethnic group to improve relations with other ethnic groups in this country.*" Figure A.7.3 below shows the distribution of these variables.

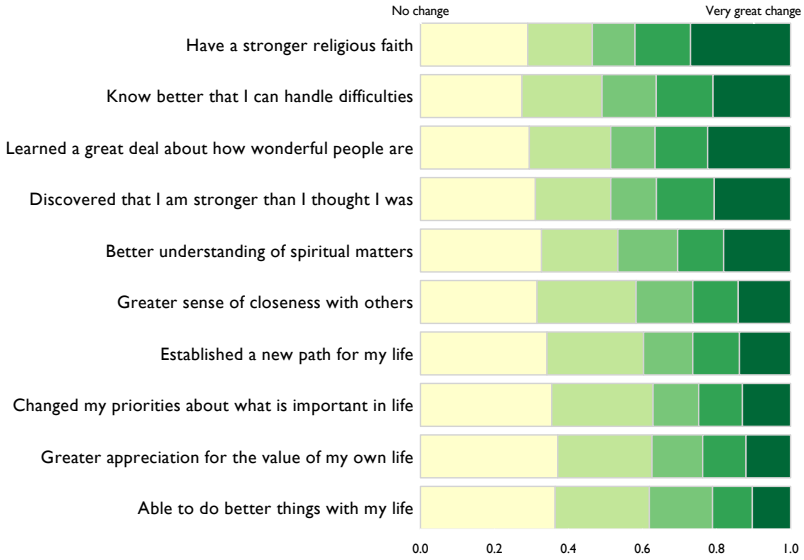
4) **Post-traumatic growth.** To measure post-traumatic growth we rely on a short form of the Post-Traumatic Growth Inventory (Cann et al., 2010; Tedeschi and Calhoun, 1996). This scale averages ten items, included in the survey, which ask for personal changes that have occurred as a result of experiencing the war. Figure A.7.4 shows these ten items and the average personal level of changed experienced. Based on these ten items, we run a correlation test to assess how similar they are at capturing post-traumatic growth. The result shows Cronbach's α=.93. This high level of correlation suggesting that the post-traumatic growth index offers very good scale reliability.

FIGURE A.7.3. Altruistic Social Preferences: Political Voice (left) and Improved Inter-Ethnic Relations (right)



Notes: The graphs shows the distribution of responses for each survey item operationalizing altruistic social preferences. The two items are weakly correlated ($r = .2$), capturing different aspects of postconflict politics.

FIGURE A.7.4. Post-Traumatic Growth



Notes: For each survey item on personal change occurring as a result of experiencing war (y-axis), the x-axis depicts the average level thereof. A higher number indicates more dramatic change.

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