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War and local collective action in Sierra Leone

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ABSTRACT

We study the brutal 1991–2002 Sierra Leone civil war using nationally representative household data on conflict experiences, postwar economic outcomes, local politics and collective action. Individuals whose households directly experienced more intense war violence are robustly more likely to attend community meetings, more likely to join local political and community groups, and more likely to vote. Tests using prewar controls and alternative samples suggest that selection into victimization is unlikely to be driving the results. More speculatively, the findings could help partially explain the rapid postwar political and economic recoveries observed in Sierra Leone and after several other recent African civil wars.

1. Introduction

This paper analyzes a novel nationally representative dataset from postwar Sierra Leone with the goal of better understanding the short-run economic and political impacts of civil war. Political violence has been a prominent feature of recent African history: over two-thirds of sub-Saharan African countries experienced a civil conflict episode since 1980. Some scholars claim that these wars have played a major role in the region's disappointing recent economic performance. For example, a recent World Bank report claims: "[t]he legacy effects of civil war are usually so adverse that they cannot reasonably be viewed as social progress…[Civil war] has been development in reverse" (World Bank 2003: 32). Yet the rapid postwar recovery experiences of some African countries after brutal civil wars – notably, Mozambique and Uganda – suggest that wars need not always have persistent negative economic consequences: in the decade following the end of their wars, Mozambique and Uganda experienced annual per capita income growth of 3.9% and 4.6%, respectively, well above the African average (United Nations, 2004).

Other recent research has shown that the long-run effects of war on population and economic growth are typically minor. Studies that focus on United States bombing – including in Japan (Davis and Weinstein 2002), Germany (Brakman et al., 2004) and Vietnam (Miguel and Roland 2005) – find few if any persistent impacts of the bombing on local population or economic performance. To the extent that war impacts are limited to the destruction of capital, these findings are consistent with the predictions of the neoclassical growth model, which predicts rapid catch-up growth postwar.

However, the neoclassical growth model has little to say about the impact of war on institutions, politics, and social norms, and it is plausible that effects along these dimensions could be more substantial and longer lasting than physical capital investment impacts. Historians have argued that wars can generate large impacts on both national and local institutions. Tilly (1975) finds that wars historically promoted state formation and nation building in Europe, ultimately strengthening institutional capacity and promoting economic development. Yet different types of conflicts could also have varying legacies. For instance, international wars against a common external foe plausibly lead to more positive institutional legacies than civil wars that heighten social divisions. A broader definition of institutions might include the social equilibrium reached by individual rational actors, including what some call social capital (Sobel, 2002). Alesina and LaFerrara (2002) find that Americans who had a traumatic experience – in their case, health problems, divorce, or financial troubles – in the previous year are much less likely to claim they "trust" others in surveys. In experimental economics evidence from Honduras, Castillo and Carter (2005) find that people in locales that experienced extensive destruction from Hurricane Mitch shared significantly more of the "pie" with their partner in a Dictator Game, suggesting that traumatic experiences could also have a positive impact on altruism or local cooperation norms.

At the individual level, the experience of being a victim of war violence could also profoundly change individual beliefs, values, and preferences. A psychological literature has documented some of these individual responses to conflict-related trauma. Studies often focus on the adverse legacies of post-traumatic stress syndrome (e.g. Dyregrov et al., 2002), but a subset of the literature now also explores

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positive responses to trauma, so-called post-traumatic growth theory (Tedeschi and Calhoun 1996; Powell et al., 2003), including changes in political action and beliefs. For example, Israelis who survived the Holocaust are more religious, more optimistic and at the same time have more extreme political views (Carmil and Breznitz, 1991), while Palestinians who personally survived aerial attacks are more likely to engage in political activism (Punamaki et al., 1997). Yet one key limitation of this literature is the use of small respondent samples of unknown representativeness. Experimental economics has produced evidence that individuals have a taste for punishing social norm violations, a dynamic likely to be relevant for civil war victims. This taste for punishing norm violators, which appears to have neural–physiological underpinnings (de Quervain et al., 2004), is consistent with preferences for equity (Fehr and Schmidt 1999) and could affect local collective action success by lowering the cost of sanctioning free riders.

Unfortunately, the extreme scarcity of micro survey data from contemporary conflict and post-conflict societies has limited research progress on these questions. One exceptional aspect of this project is the availability of high quality nationally representative household data from Sierra Leone containing detailed information on household experiences with war violence as well as on immediate postwar political and collective action behaviors, in addition to the more standard socioeconomic questions. The main empirical results focus on the individual-level analysis made possible by this unusual dataset. We also draw on other new Sierra Leone data to estimate relationships at the more aggregated chiefdom level.[1]

In our main result, we find that individuals whose households directly experienced war violence are much more active political and civic participants than non-victims. War victims are significantly more likely to register to vote (by 2.6 percentage points), attend community meetings (by 6.5 percentage points), participate in local political and community groups, and contribute to local public goods (serving on a local primary school committee). Yet two or three years after the end of the war, there are – perhaps surprisingly – no lasting impacts on household socioeconomic status measures, including asset ownership, income earning activities, as well as consumption expenditures and child nutrition. This finding of no lingering socioeconomic impacts differs from Akresh et al. (2007) and Blattman and Annan (2007), both of whom find negative socioeconomic legacies of violent conflict in the Burundi and Uganda wars, respectively.

This suggests that the increased political mobilization is not due to socioeconomic differences, but rather reflects a direct relationship between victimization and postwar behaviors. In a related result, Blattman (2007) finds that former child soldiers in Uganda are significantly more likely to vote than other youth, and importantly shows that experiencing violence (but not perpetrating violence) increases voting among the former soldiers, which is consistent with our evidence on victimization. Earlier work using only a subset of the dataset assembled for this paper and using chiefdom-level variables finds broadly similar impacts on collective action outcomes (Bellows and Miguel 2006). The current paper's contribution over Bellows and Miguel (2006) lies in the use of individual-level data, a far broader range of outcome variables (from new datasets), and more extensive discussion of econometric estimation issues, implications of the findings, as well as the Sierra Leone context.

All of our regression specifications include enumeration area fixed effects. These fixed effects allow us isolate the variation in violence experienced across neighbors within the same village. Anecdotal evidence suggests that although some villages experienced more violent attacks than others, many rebel attacks were characterized by indiscriminant violence against individuals within villages. Yet even after including fixed effects, a key econometric issue remains in establishing a causal effect of victimization: the possibility that politically active individuals within a village were singled out for violence during the war. Such targeting could potentially generate spurious relationships due to omitted variable bias. We explore this possibility in two separate tests, and both tests indicate that systematic individual selection into victimization is unlikely to be driving our estimates. First, we show that our main finding – that victimized households are more politically active than non-victims – is statistically robust, and does not change in magnitude, in specifications with a rich set of household controls including prewar socioeconomic characteristics and community group leadership roles.

Second, and equally important, the estimates are undiminished among subsamples in which victimization is arguably more random than in the full sample: among youth (who were too young to have demonstrated prewar community leadership and thus less likely to be singled out) and among people living in areas with infrequent exposure to rebel groups, where rebel violence within villages was more likely to be indiscriminate.

Civil war experiences are transformative for many and our analysis suggests that one short-run legacy is increasing individual political participation, community activism, and local public good provision. As we discuss in the conclusion, this finding echoes the observations of other scholars of Sierra Leone and speaks to the remarkable resilience of ordinary Sierra Leoneans. More speculatively, this paper also contributes to the recent debate on the underlying causes of Africa's woeful recent economic performance, and speaks against claims that civil war's legacies are always major long-run impediments to African economic and political development. We speculate on how far the results may generalize in the conclusion.

2. The Sierra Leone civil war

Sierra Leone was ravaged by a civil war that started in 1991 and lasted until January 2002. An estimated 50,000 Sierra Leoneans were killed, over half of the population was displaced from their homes, and thousands were victims of amputations, rapes, and assaults (Human Rights Watch 1999).

2.1. Origins of the war

Just before the war, Sierra Leone had the second lowest living standards of any country in the world (United Nations 1993). For the preceding two decades the country had been ruled by dictators who enriched themselves through illicit deals involving diamonds, while doing next to nothing to provide needed services such as health care and education (Reno 1995). Partially as a result of the widespread discontent towards the corruption and ineffectiveness of the government, a small group of rebels, who had entered the country from Liberia in 1991, were successful in recruiting disenfranchised youth to rise up violently against the status quo. As their numbers swelled by early 1992, these rebels, known as the Revolutionary United Front (RUF), spread the armed conflict to all parts of the country. Some scholars have claimed that the initial motivations of the RUF were idealistic and that the early rebels were guided by a strong sense of political grievances related to the failings of the corrupt regime (Richards 1996).

Another important factor in the RUF's original motivations was access to Sierra Leone's diamond wealth. Mining diamonds in Sierra Leone requires no heavy machinery or technology, since these alluvial stones sit close to the surface in dried riverbeds, so any armed group that controlled a diamond-rich area could extract and then sell the diamonds for large profits. All armed groups participated to some extent in diamond smuggling during the conflict, and the control of these diamond areas was an important objective for all groups. David

[1] The chiefdoms in Sierra Leone are administrative units that were formalized by the British in the 1930s. These colonial boundaries remain salient today as most people identify their residential location by the chiefdom. The average chiefdom has roughly 20,000 people.

Keen notes that "[a]ny battles were largely restricted to the areas with the richest diamond deposits" (Keen 2005: 212), and we find some quantitative evidence consistent with this below. Additionally, since large-scale diamond smuggling was possible so long as the country remained in chaos, profits from these "blood diamonds" represented an important incentive for armed groups to prolong the war (Keen 2005: 50).

In contrast to most popular media coverage on African civil wars, neither ethnic nor religious divisions played a central role in the Sierra Leone conflict. The RUF rebels targeted people from every ethnic group and throughout the country, and statistical analysis of documented human rights violations shows that no ethnic group was disproportionately represented among RUF victims (Conibere et al., 2004). There is also no evidence that levels of civilian abuse were higher when a particular armed faction and the community were predominantly from different ethnic groups (Humphries and Weinstein 2006: 438).

2.2. The Revolutionary United Front (RUF) and the Sierra Leone Army (SLA)

Although there were many different actors in the decade-long war, the majority of the violence was perpetrated by the RUF: the official government truth and reconciliation commission, which documented war atrocities, reports that RUF fighters committed over 70% of all human rights abuses (Conibere et al., 2004). Our own analysis of the No Peace Without Justice (NPWJ) conflict mapping project, which is a comprehensive record of all reported armed violence during the war, similarly concludes that 75% of all attacks and battles involved the RUF as the primary fighting force (Smith et al., 2004). The following incidents recorded in the NPWJ report are fairly typical of the brutal and seemingly arbitrary RUF raids on civilians:

> "In the early hours of 27 May 1997, the town of Karina (Biriwa Chiefdom, Bombali District) was attacked by RUF/AFRC forces carrying guns and other weapons. Soldiers surrounded the central mosque and killed 10 civilians celebrating the Muslim feast of 'Jonbedeh'… An unknown number of people were injured trying to escape. RUF/AFRC forces raped an unknown number of women, and abducted 30 young civilian men and women. During the attack, numerous houses were burned down." (p. 133)
> "RUF forces attack Koi town (Nongowa Chiefdom, Kenema District) early one morning in mid-1994, reportedly to terrorize the inhabitants. … They fired indiscriminately and many civilians were killed and others were wounded. The town was looted and people were forced to carry the stolen property to Peyama." (p. 303)
> "On 11 March 1998, RUF/AFRC forces attacked the headquarter town of Jagbwema (Fiama Chiefdom, Kono District). RUF/AFRC forces entered the town firing indiscriminately. More than 70 houses were burnt and the town was massively looted. During the night, the RUF/AFRC forces abducted three people, including the Town Chief, who were all later killed. On 24 March 1998, RUF/AFRC forces coming from Jagbwema attacked Yeanoh, shooting and killing many people." (p. 361)
> "On 26 December 1994, RUF forces attacked Mattru on the Rail (Tikonko Chiefdom, Bo District) in the afternoon, mutilating civilians' arms and legs. The RUF then opened sporadic gunfire on the civilians, killing many people, looting their property and burning down their houses. They also abducted civilian youths who they conscripted into the RUF forces." (p. 395)

The degree of targeting of community leaders or political opponents is important in the later analysis. It is useful here to distinguish between regions where the RUF did not establish permanent bases and thus mainly resorted to raids like those described above, versus regions with permanent bases that were occupied for extended periods, often years. The ability to systematically single out particular types of civilians is inherently related to the length of RUF occupation. The NPWJ report indicates that slightly more than half of all chiefdoms (86 of 152 chiefdoms) never had permanent RUF bases. In the analysis below we sometimes restrict attention to these areas to estimate war impacts in this subsample where RUF violence against civilians was more likely to be indiscriminate.

One feature of the fighting that has drawn attention from international observers is the cooperation between the rebels and the Sierra Leone Army (SLA). These two groups coordinated their movements to avoid direct battles, and at times worked out mutually beneficial profit sharing arrangements in diamond areas. This was especially true following the 1997 coup that formally brought elements of the SLA and RUF together into a national coalition government called the Armed Forces Revolutionary Council, or AFRC (Keen 2005). Some soldiers apparently fought for the SLA by day and the RUF by night. As a result, the main victims of the violence were civilians, who were terrorized not only by the RUF but also by the army that was supposed to protect them.

2.3. Civil Defense Forces (CDF)

In order to protect themselves from the terror of RUF and SLA fighters, many communities organized local fighting groups that became known collectively as the Civil Defense Forces (CDF). CDF fighters were overwhelmingly civilians and relied primarily on local fundraising for supplies. While there were numerous manifestations throughout the country, the command and organization of CDFs were often linked to traditional chiefly authorities. For example, the largest CDF, known as the *kamajors*, were an outgrowth of traditional hunter groups found in the country's largest ethnic group, the Mende (Ferme 2001).

Whether part of the CDF movement or not, there are many accounts of ordinary civilians going to heroic lengths to protect themselves from RUF attacks. One such account from Allister Sparks (2003: 309), an international observer in the 1996 Sierra Leone presidential election (held during a brief lull in the violence), describes how the citizens of Kenema Town bravely resisted the RUF to exercise their right to vote:

> "The polling stations were due to open at 7 am on 26 February, but at exactly 6.15 am the rattle of small-arms fire broke out around the centre. … For two-and-a-half hours the firefight raged. At times the rebels ran close past our building and we could hear them shouting: 'No election! No election!' between their bursts of AK-47 fire. Then, indistinctly at first but gradually increasing in volume, we heard a counter-chant coming from the direction of the town: 'We want vote! We want vote!' Thousands of people were pouring into the streets, and as the chanting crowd swelled they ran through the town waving palm leaves. … Whether it was this display of public courage or a successful counterattack by the local military was unclear, but the rebels began to withdraw and the shooting subsided. As the observers made their way gingerly into the town, crowds lining the streets yelled impatiently at us: 'Bring the boxes. We want vote!' The polling stations opened late, some not until the afternoon, but electoral officials worked frantically to open extra stations, and by the time the polls closed at 6 pm nearly every registered adult in Kenema had voted." (p. 309)

The CDF continues to be admired within Sierra Leone. However, late in the conflict when their power and numbers had grown, some CDF units lost discipline and they too began to abuse civilians and enter the illicit diamonds trade, although to less of an extent than the RUF or SLA (Keen 2005: 268). The rise of the CDF is illustrative of two points raised in the Introduction: first, that war creates influential

new institutions, and second, that many Sierra Leoneans responded to violence with an increased desire to assert their political rights rather than resignation (as illustrated concretely in the above account). We return to both points in the analysis below.

Following the brutal 1999 rebel attack on Freetown, a large deployment of United Kingdom and United Nations troops finally brought an end to the war. These foreign troops conducted a disarmament campaign and secured a peace treaty in early 2002. Donor and non-governmental organization (NGO) assistance has since played a major role in reconstructing physical infrastructure, resettling internally displaced people (almost all of whom had returned home by 2003), and funding government expenditures. National elections for a president and members of parliament were held in 2002, and local government elections – the first in over 30 years – in 2004. Additional rounds of national and local elections were held in 2007 and 2008, respectively.

3. Empirical strategy

The literature discussed in the introduction suggests that there are at least two plausible channels through which violence may impact postwar behavior. First, the trauma associated with violence could change individual beliefs, identities, values, and preferences. Second, conflict could give rise to new institutions or social norms. We investigate these two possible channels by first examining the relationship between individual-level victimization and postwar behaviors, and then by examining the relationship between conflict intensity and postwar outcomes at the aggregated chiefdom level.

Before discussing the details of the estimation, an important caveat to the empirical strategy is worth emphasizing: we focus on local comparisons across individuals and across chiefdoms, and cannot estimate the overall impact of the Sierra Leone civil war on the nation as a whole. The data do not permit the estimation of national impacts because no suitable counterfactual exists.[2] This caveat is important, as the net national effect of the war could be negative even in the presence of any positive local victimization impacts that we estimate, or vice versa.

3.1. Individual-level estimation strategy

The individual-level analysis compares individuals who suffered from different degrees of violence during the conflict. These specifications include enumeration area fixed effects.[3] The fixed effects absorb any variation in conflict intensity across villages, and the remaining variation essentially distinguishes neighbors within the same village. As the accounts in section 2.2 demonstrate, RUF attacks on villages were sometimes brief, chaotic, and indiscriminate affairs, providing little opportunity for precise targeting or selection of victims; we discuss exceptions to this characterization below.

A second advantage of including enumeration area fixed effects is that the effective within-village comparison groups are small, typically consisting of a handful of inter-related local families, and are relatively homogeneous. Our surveys indicate that rural Sierra Leonean villages consist almost entirely of subsistence farmers, and with the notable exception of traditional chiefly families, there is typically no conspicuous landowning elite for the RUF to target.

However, the identifying assumption in our estimation – that victimization within villages is close to random conditional on observable characteristics – may not hold if there was systematic targeting by fighters along some household dimension. For instance, in the case that targeting was related to prewar community leadership, which may in turn be related to postwar political behaviors, an omitted variable bias could lead us to overstate the importance of victimization.

We carry out two tests to assess the importance of such variables. The first way to assess the importance of selection is to include extensive individual and household characteristics as control variables. We rely on two sets of characteristics that are likely to be related to postwar political and socioeconomic outcomes. The first set includes postwar characteristics that are unlikely to have changed as a result of the war, for instance, adult educational attainment, membership in local chiefly families, and demographic characteristics (gender and age). The second set includes characteristics of the prewar household, which were collected by asking respondents retrospective questions about their household in 1990, immediately before the start of the war, including information on community group leadership activities.[4] To the extent that the coefficients of interest are robust to the inclusion of these variables, this provides further evidence that selection is unlikely to be driving the main results.

Although these two sets of controls provide a substantial amount of household level information – perhaps uniquely so in an African civil war context – they may not fully control for all relevant characteristics, so the possibility of some omitted variable bias remains. We gauge the relative importance of omitted variable bias by investigating how the coefficients of interest change with the inclusion of the additional explanatory variables. If including controls substantially attenuates the coefficient estimates on victimization, then it is possible that inclusion of more controls would reduce the estimated effect even further. If, on the other hand, the inclusion of controls has no effect on coefficient estimate magnitudes, then we can be more confident in suggesting a causal interpretation to the estimated relationship. Following Altonji et al. (2005), we formalize this intuition and derive the ratio of the "influence" of omitted variables relative to the observed control variables that would be needed to fully explain away our victimization result. In this set-up, the omitted variable is the individual's personality or "type": individuals who are "leader"-types, "aggressive"-types, "courageous"-types, etc. could be targeted during the war, but there is no way for the econometrician to control for this. Given the intuitive importance of our rich set of observed control variables, and especially the prewar leadership and chief controls, a large ratio would imply that the result cannot be plausibly explained entirely away by the unobserved type of the individual.[5]

The second approach in dealing with selection is to restrict attention to subsamples for which there was less targeted violence. The first such subsample contains respondents who were too young to be prewar community leaders, those 30 years old or younger in 2005 (and 15 or younger in 1990, right before the war).[6] These individuals could not have been targeted for violence as a result of already being community leaders themselves prewar (or in the early war years), thus it is more plausible that treatment effects among this subgroup primarily reflect violence impacts rather than selection. This is not a perfect test since these youths may have been targeted because their parents were community leaders, for instance, and leadership abilities may be somewhat correlated across generations. Nonetheless focusing on this youth subsample should considerably weaken the link between unobserved individual political activism and targeted violence.

The second subgroup for which targeted violence within a village was likely to be less systematic is among respondents who lived in areas without permanent RUF bases. As discussed above, the ability of

[2] Liberia shares similar geography, history and culture with Sierra Leone, but Liberia was also experiencing a civil war during this time so it cannot be used as a peacetime counterfactual.

[3] In most rural settings an enumeration area corresponds to a single village, but in some instances one enumeration area contains two small villages. In the urban settings an enumeration area is equivalent to a block or a neighborhood. For the GoBifo sample (discussed below) all enumeration areas correspond to exactly one village.

[4] This second set is only available for a subset of the respondents. Details are discussed further in section 5 and in the notes to Tables 2, 3 and 4.

[5] Full details are provided in the supplementary online appendix.

[6] It is extremely rare for individuals under 20 years of age to be community leaders in our data. We thank Rachel Glennerster for suggesting this test.

the RUF to target particular individuals often depended on the attackers' in-depth knowledge of or repeated interaction with a specific village. This was more likely to occur in villages in close proximity to RUF bases. For villages far from bases, RUF attacks were more sporadic and less organized. As with the youth subgroup, focusing on those enumeration areas far from an RUF base reduces concerns about selective targeting of violence.

The final sample that we use comes from a different dataset, the GoBifo Survey. While not nationally representative, GoBifo contains the same rich information on conflict experiences and postwar outcomes as our main national household survey. We replicate the results using this separate dataset to ensure that the findings are not artifacts of any survey design problems in the main dataset. GoBifo is discussed in more detail in the following section.

We admittedly cannot rule out that some targeting of politically active households occurred. But we feel that the evidence from our multi-pronged empirical strategy – which includes enumeration area fixed effects, the use of extensive control variables (including prewar activism controls), and analysis of alternative datasets and of multiple subsamples where violence targeting was less systematic – makes a plausible case that the relationship we document is informative of the causal effect of conflict victimization on postwar household behaviors and outcomes.

A remaining concern is that the results could be driven by selective migration of individuals who experienced violence. Unfortunately, we do not have high frequency data on locations, violence, and outcomes during the conflict, which would allow us to fully address this concern. Yet selective migration seems unlikely to be a leading factor empirically: the correlation between conflict victimization and individual migration between chiefdoms is very low, at only 0.07, and not statistically significant. We also partially address this concern below by restricting analysis to the subsample of individuals who lived in the same chiefdom both before and after the war, and find that our main results are robust.

3.2. Chiefdom-level estimation strategy

There may also be war impacts at levels more aggregated than the household due to changes in local institutions or social norms brought on by the conflict. We investigate these effects by comparing chiefdoms that experienced different levels of conflict intensity among the 152 chiefdoms in the analysis. In the chiefdom-level analysis, we rely on a rich set of local characteristics as explanatory variables to ensure we are isolating violence effects. These controls include the number of diamond mines, roads, population density, and in some specifications prewar socioeconomic measures. Additionally, district fixed effects are included to account for broader regional variation in unobservable characteristics; there are 13 districts in the sample.

4. Data

Data on individual war experiences is extremely rare and this has limited research progress in estimating civil war impacts. The wide array of individual data on conflict experiences and postwar outcomes makes ours among the most comprehensive datasets from a post-conflict society.

4.1. Individual-level data

We make use of data from three household surveys that were collected soon after the war ended. Two of the surveys are nationally representative[7] and were conducted by the Institutional Reform and Capacity Building Project (IRCBP).[8] These surveys, conducted in 2005 and 2007, were repeated cross-sections of households in the same enumeration areas for both rounds. In most of the regressions we pool the data from the 2005 and 2007 surveys and conduct the analysis on the joint sample, which contains data on 10,471 households in 539 enumeration areas. The third survey was conducted as baseline data for a government assistance program called "GoBifo" and it covers only selected wards within two districts.[9]

These three surveys all contain detailed questions on household war victimization experiences. The IRCBP survey contains the following three retrospective questions: "Were any members of your household killed during the conflict?" "Were any members injured or maimed during the conflict?" and "Were any members made refugees during the war?" We create a victimization index as the average of responses to these violence related questions (Table 1, panel A); as we discuss below, breaking the index down into its component questions does not substantively change the results. Victimization rates are high, seemingly too high given war casualties and refugee flows documented in other sources, but we believe many respondents' interpretation of the questions as referring to extended family members is a key explanation.

One important limitation of our data is that all of the evidence on victimization is self-reported. This raises concerns about systematic response bias, including possible over-reporting of victimization. That said, there is no ready solution to this problem and the detailed individual victimization data represents a significant improvement over other studies and is a major feature of this paper.

The data also includes information on household assets, some respondent characteristics (including education), and multiple measures of political engagement, voting, participation in collective action, and self-expressed levels of trust and cooperation (Table 1, panels B, C, D, and E). The IRCBP and GoBifo surveys were conducted independently using different sampling frames, so carrying out the analysis on both provides a robustness check.

4.2. Chiefdom-level data

In addition to the household-survey based measure of conflict victimization mentioned above, we use the number of reported attacks and battles within each chiefdom as another violence measure. The number of attacks and battles is related to, but distinct from, the household reports of victimization, as it also includes the battles between troops that did not directly involve civilians. The 2004 No Peace Without Justice (NPWJ) conflict mapping project compiled all reports by human rights organization and the media on the location and intensity of violence during the conflict (Table 1, panel F). We construct a measure of attacks and battles from the descriptions included in this report. The correlation across the household victimization measure and the number of attacks and battles at the chiefdom level is moderate, at 0.3. Note that the two measures of conflict-related violence are broadly analogous to the two types of commonly used crime data, crime victimization data versus official crime reports.

Additional chiefdom-level data is constructed from multiple sources. The 2004 Sierra Leone Integrated Household Survey provides data on nutrition, education and socioeconomic outcomes (Table 1, panel G). The 2005 School Survey provides data on the quality, monitoring and funding of local education facilities (Table 1, panel H). The 2003 Sierra Leone Data Encyclopedia provides information on the number of non-governmental organization (NGO) projects in each

[7] There is one important exception to this: the capital Freetown is excluded from the analysis. Freetown is Sierra Leone's only large city and its local institutions and history are quite different from the rest of the country. For instance, the chief system does not exist in Freetown.

[8] The IRCBP is affiliated with the government of Sierra Leone and its primary role is to support the ongoing decentralization of government services.

[9] The location of sample enumeration areas for the IRCBP and GoBifo surveys is presented in Appendix Figs. 1 and 2, respectively, and details on these surveys can be found in Appendix B, all of which are found in the supplementary online appendix.

Table 1
Descriptive statistics.

	Individual-level data			
	IRCBP[a]		GoBifo[b]	
	2005 and 2007		2005	
	Mean	(SD)	Mean	(SD)
Panel A: household experience with conflict				
Were any members of your HH killed?	0.44	(0.50)	0.20	(0.40)
Were any members of your HH injured/maimed?	0.35	(0.48)	0.23	(0.42)
Were any members of your HH made refugees?	0.38	(0.49)	0.97	(0.17)
Were any children from your HH captured?			0.23	(0.42)
Were any women from your HH captured?			0.10	(0.30)
Was your house burned down?			0.39	(0.49)
Household conflict victimization index *(Average of above variables)*	0.39	(0.34)	0.35	(0.19)
Panel B: postwar local institutions politics				
Did you attend any community meetings in past year?	0.57	(0.49)	0.70	(0.46)
Are you a member of a social group?	0.57	(0.50)	0.34	(0.47)
Are you a member of a political group?	0.14	(0.35)		
Are you a member of a school management committee?	0.21	(0.41)	0.23	(0.42)
Did you vote/register to vote in either recent election?	0.94	(0.23)	0.92	(0.27)
Did you participate in road brushing in the past year?	0.37	(0.48)	0.72	(0.45)
Do you know when the next election will be held?	0.39	(0.49)	0.22	(0.41)
Can you correctly name your Local Councilor?	0.42	(0.49)	0.47	(0.50)
Can you correctly name your Paramount Chief?	0.77	(0.42)	0.77	(0.42)
Panel C: postwar social capital				
Do you trust other members of your community?	0.83	(0.38)	0.59	(0.28)
Do you trust people from outside your community?	0.44	(0.50)		
Are you a member of a church/ mosque group/Have you attended church/mosque in the past month?	0.56	(0.50)	0.93	(0.26)
Panel D: postwar socioeconomic outcomes				
Does your household own a stove?	0.04	(0.20)	0.10	(0.30)
Does your household own a radio?	0.51	(0.50)	0.36	(0.48)
Does your house have a tin/zinc roof?	0.62	(0.48)	0.56	(0.50)
Panel E: respondent controls				
Female	0.49	(0.50)	0.48	(0.50)
Age	42.10	(15.5)	40.62	(16.4)
Have you ever been in school?	0.31	(0.46)	0.23	(0.42)
Traditional authority household?	0.16	(0.37)	0.25	(0.43)
Panel F: 1990 household controls				
Member of 1990 HH was a traditional leader	0.19	(0.39)		
Member of 1990 HH was a community leader	0.33	(0.47)		
Head of 1990 HH had any education	0.23	(0.42)		
Number of household observations	10,471		2692	
Number of districts; chiefdoms; EAs/villages	13; 152; 539		2; 13; 235	

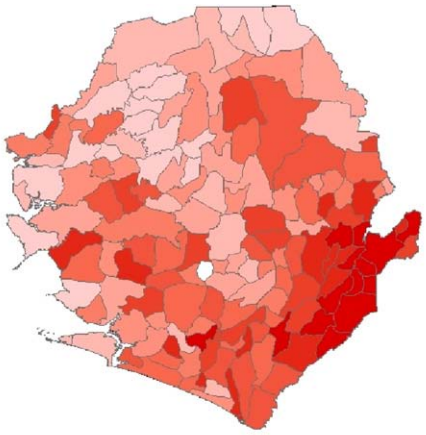
	Chiefdom-level data	
	2005	
	Mean	(SD)
Panel F: chiefdom conflict victimization		
Chiefdom conflict victimization index *(Chiefdom average of IRCBP conflict index)*[a]	0.46	(0.17)
Number of attacks and battles in chiefdom, 1991–2002[c]	9.41	(9.67)
Panel G: postwar socioeconomic outcomes		
Average log per capita expenditure *(Leones)*, 2004[d]	13.07	(0.50)
Proportion of children enrolled in school *(ages 5–18)*, 2004[d]	0.66	(0.18)
Average BMI for children *(ages 0–5)*	21.88	(8.27)
Panel H: postwar education and local public goods		
Proportion of teachers absent on day of school survey[h]	0.25	(0.14)
Proportion of schools receiving financial/in-kind resources from community[h]	0.47	(0.42)
Proportion of schools receiving financial/in-kind resources from donors/NGOs[h]	0.48	(0.42)
Proportion of schools visited by a traditional authority in the last year[h]	0.74	(0.38)
Proportion of adults in chiefdom who have ever been to school, 2004[d]	0.31	(0.18)
Total number of NGO projects in the chiefdom[e]	45.05	(43.41)
Panel I: prewar socioeconomic and geographic controls		
Average log per capita expenditure *(Leones)*, 1989[f]	7.95	(0.70)
Proportion of children enrolled in school *(ages 5–18)*, 1989[f]	0.29	(0.20)
Number of diamond mines *(per chiefdom)*[g]	2.62	(5.49)
Road density *(km of road per sq km of land area)*[g]	0.09	(0.07)
Log distance to Freetown *(km)*[g]	11.93	(0.59)
Log population density *(people per sq km)*, 1985[g]	3.75	(0.75)
Number of districts; chiefdoms	13; 152	

Sources: ([a]) Institutional Reform and Capacity Building Project, 2005 and 2007 Household Surveys; ([b]) GoBifo Household Survey, 2005; ([c]) No Peace Without Justice Conflict Mapping Report, 2004; ([d]) Sierra Leone Integrated Household Survey, 2003–2004; ([e]) Encyclopedia of Sierra Leone, Sierra Leone Information Systems, 2003; ([f]) Sierra Leone Household Survey, 1989; ([g]) GIS Data, Government of Sierra Leone, 2002 and ([h]) Sierra Leone School Monitoring Survey, 2005.
There are some differences in questions across the IRCBP and GoBifo surveys. First, some questions in the IRCBP survey were not included in the GoBifo survey and vice versa. Second, the wording of some questions is different. These are indicated in the table and include: (1) The 2005 IRCBP survey asks "Did you register to vote for either of the last two elections?"; the GoBifo survey asks "Did you vote in either of the last two elections?". The 2007 IRCBP survey does not contain a voting question because it was held before the election. (2) The IRCBP surveys ask "Are you a member of a church/ mosque group?"; the GoBifo survey asks "Have you attended church/mosque in the past month?". (3) The IRCBP surveys ask "How much do you trust members of your community?"; the GoBifo asks three hypothetical questions that measure trust in different situations, and the average of those three questions is the overall trust measure. (4) The IRCBP surveys ask whether the respondent or anybody from the household holds a position of traditional authority. The GoBifo survey asks whether members of the household are eligible to hold such positions, which does not necessarily imply that they do hold these positions.
Freetown (the capital city) is excluded from every sample. Due to survey sampling design, there are 117 observations for the 2004 socioeconomic variables (source ([d])), 64 observations for the 1989 socioeconomic variables (source ([f])), and 104 observations for the school survey data (source ([h])).

chiefdom (Table 1, panel H). A GIS dataset provides information on the location of diamond mines, roads, and population density (Table 1, panel I). The 1989 Sierra Leone Household Survey provides the only existing data we are aware of on prewar household socioeconomic conditions (Table 1, panel I).[10]

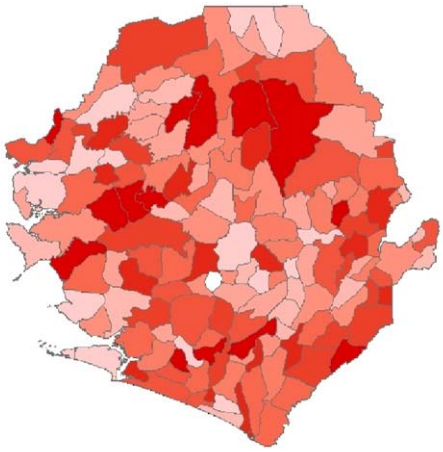
The variation in chiefdom-level civilian victimization is presented in Fig. 1. As expected, violence is concentrated in the eastern part of the country near Liberia, but some violence was experienced in all regions. Fig. 2 presents the residuals of the victimization index after the district means have been subtracted off; this measure of local violence is effectively used in specifications that include district fixed effects. As is apparent from the figure, subtracting off district averages emphasizes the considerable variation in violence across neighboring chiefdoms.

[10] The sample for the 1989 household survey includes fewer than half of all chiefdoms in the country. The documentation for the dataset is incomplete, in part because of the chaos and destruction of the civil war, making it impossible to know how exactly this sample was chosen. Further details on all of the data sources and on variable construction are provided in the supplementary online data appendix.



Notes: The Conflict Victimization Index is the chiefdom average of three conflict related questions in the IRCBF survey. Chiefdoms are shaded in deciles according to the value of the conflict index. Data is missing for Gbonkolenken chiefdom, leaving a sample size of 151 chiefdoms.

Fig. 1. Chiefdom conflict victimization index.



Notes: The residuals in this figure are from a regression of the conflict index on a set of district fixed effects. Thus, this map shows the variation being used in all of the specifications that include district fixed effects. Chiefdoms are shaded in deciles according to the value of these residuals. Data is again missing for Gbonkolenken chiefdom.

Fig. 2. Residuals of chiefdom conflict victimization index.

5. Individual-level results

We document that individuals that experienced more direct civil war victimization are significantly more likely to be politically mobilized and engaged in local collective action than other individuals, but do not appear to be significantly different in terms of assets or religiosity. Before turning to these results, though, we first investigate the correlations between victimization and household characteristics.

5.1. Correlates with victimization

According to many academic accounts of the war, the one group that the RUF succeeded in targeting for attacks were members of traditional authority (chief) households, who were well-known and visible in their localities, and the closest equivalent to a local elite (Keen 2005; Richards 1996). There are also many media accounts of chiefs being leading targets, as part of an RUF attempt to bring down the "corrupt" existing social order that chiefs represented.

It is thus unsurprising that traditional authority households are statistically more likely to have experienced violence during the war (Table 2, regressions 1 and 2). In addition, respondents who lived with a community leader (i.e. a women's group, a youth group, or a farmer's group leader) in 1990 are significantly more likely to have experienced violence during the war (regression 2), again suggesting that local leaders were at greater risk of experiencing violence.[11] In all later regressions, we control for these variables. Other potential correlates with violence, including respondent gender, age and education, are not significantly related to victimization during the war (an *F*-test on the joint significance of these terms fails to reject the null hypothesis of no effect).[12]

[11] We have also run these regressions on the two alternative subsamples that are the focus of Table 6, and find that some targeting of chiefs and prewar community leaders also occurred within those two subsamples. The usefulness of the subsamples as a robustness check in the main analysis relies on the assumption that there was less selection on unobservables in those subsamples.

[12] Verwimp (2005) documents a systematic relationship between socioeconomic status and being a perpetrator of violence in the Rwanda genocide. The focus of the current study is on victims of violence (rather than perpetrators), which could explain the discrepancy in findings.

Table 2
Household/respondent characteristics and conflict victimization.

	Dependent variable: conflict victimization index	
	IRCBP	
	2005 and 2007	2007
Explanatory variables	(1)	(2)
Respondent is female	0.0086	0.0069
	(0.0057)	(0.0086)
Respondent age	0.0000	−0.0002
	(0.0002)	(0.0003)
Respondent has any education	0.0024	0.0018
	(0.0076)	(0.0134)
Traditional authority household	0.0458***	0.0443**
	(0.0092)	(0.0138)
1990 Household head had any education		−0.0157
		(0.0137)
1990 Household had a traditional leader		0.003
		(0.014)
1990 Household had a community leader		0.0354**
		(0.0121)
R-squared	0.343	0.324
Observations	10,471	5193
Enumeration area/Year fixed effects	X	X

Notes: Column (1) includes pooled data from the 2005 and 2007 rounds of the IRCBP survey. Column (2) includes additional controls for household characteristics in 1990. These controls were not included in the 2005 survey, so the sample is restricted to the 2007 survey respondents.
The conflict victimization index is the average of three questions: Was anybody from your household killed during the conflict? Was anybody from your household maimed/injured during the conflict? Was anybody from your household made a refugee during the conflict? Traditional authority household is a variable that is equal to one if the respondent or anybody from the household currently holds a position of traditional authority.
Enumeration area–Year fixed effects are included in all specifications. Robust standard errors are reported. Standard errors are clustered at the Enumeration area–Year level. Significantly different than zero at *90% confidence, **95% confidence, and ***99% confidence.

Table 3
Community meetings and conflict victimization.

	Dependent variable: did you attend any community meetings in the past year?			
	IRCBP			
	2005 and 2007		2007	
Explanatory variables	(1)	(2)	(3)	(4)
Conflict victimization index	0.0704***	0.0652***	0.0775***	0.0686***
	(0.0164)	(0.0165)	(0.0253)	(0.0246)
Respondent is female		−0.1300***		−0.1276***
		(0.0084)		(0.0126)
Respondent age		0.0003		0.0002
		(0.0003)		(0.0005)
Respondent has any education		0.0590***		0.0466**
		(0.0108)		(0.0194)
Traditional authority household		0.0928***		0.0647***
		(0.0128)		(0.0194)
1990 Household head had any education				0.0205
				(0.0199)
1990 Household had a traditional leader				0.1054***
				(0.0217)
1990 Household had a community leader				−0.0067
				(0.0169)
R-squared	0.361	0.391	0.267	0.298
Observations	10,471	10,471	5193	5193
Enumeration area/Year fixed effects	X	X	X	X

Notes: Columns (1) and (2) include pooled data from the 2005 and 2007 rounds of the IRCBP survey. Columns (3) and (4) restrict the sample to only 2007 data. Column (4) includes additional controls for household characteristics in 1990. These controls were not included in the 2005 survey.
The construction of the conflict victimization index is as described in the notes to Table 2. Enumeration area–Year fixed effects are included in all specifications. Robust standard errors are reported. Standard errors are clustered at the Enumeration area/Year level. Significantly different than zero at *90% confidence, **95% confidence, and ***99% confidence.

Table 4
Social group membership and conflict victimization.

	Dependent variable: are you a member of a social group?			
	IRCBP			
	2005 and 2007		2007	
Explanatory variables	(1)	(2)	(3)	(4)
Conflict victimization index	0.0711***	0.0655***	0.1151***	0.0988***
	(0.0167)	(0.0166)	(0.0249)	(0.0246)
Respondent is female		−0.0517***		−0.0308***
		(0.0086)		(0.0139)
Respondent age		−0.0031***		−0.0045***
		(0.0003)		(0.0005)
Respondent has any education		0.0308***		0.0272
		(0.0110)		(0.0198)
Traditional authority household		0.1155***		0.0751***
		(0.0129)		(0.0192)
1990 Household head had any education				−0.0172
				(0.0210)
1990 Household had a traditional leader				0.0786***
				(0.0210)
1990 Household had a community leader				0.0510***
				(0.0181)
R-squared	0.328	0.344	0.221	0.252
Observations	10,471	10,471	5193	5193
Enumeration area/Year fixed effects	X	X	X	X

Notes: Columns (1) and (2) include pooled data from the 2005 and 2007 rounds of the IRCBP survey. Columns (3) and (4) restrict the sample to only 2007 data. Column (4) includes additional controls for household characteristics in 1990. These controls were not included in the 2005 survey.
The construction of the conflict victimization index is as described in the notes to Table 2. Social group membership is defined as being a member of either a women's group, a youth group, or a farmer's group.
Enumeration area–Year fixed effects are included in all specifications. Robust standard errors are reported. Standard errors are clustered at the Enumeration area/Year level. Significantly different than zero at *90% confidence, **95% confidence, and ***99% confidence.

5.2. Armed conflict and civic participation

We begin with a detailed analysis of the relationship between conflict victimization and three specific postwar individual behaviors: attendance at community meetings, membership in a social group,[13] and membership in a political group. We focus on these three outcome variables because they were collected in both the 2005 and 2007 survey rounds, allowing us to explore the full range of specifications; due to changes in the questionnaires across rounds, other variables are available for either 2005 or 2007 (see the notes to Table 6).

Household conflict victimization is positively and significantly related to respondent community meeting attendance postwar (Table 3), whether the respondent is a member of a social group (Table 4), and also whether the respondent is a member of political group (Table 5). The point estimates on victimization are remarkably stable across specifications with different sets of household controls: an increase from zero to one in the household conflict victimization index (which corresponds to going from no violence to experiencing all three types of violence listed above) is associated with an approximately 6.9 percentage point increase in the probability of attending a community meeting (Table 3, regression 4), on average village meeting attendance of 70%. The analogous effects are a 9.9

[13] Social groups include women's groups, youth groups, and farmer's groups. A respondent is said to be a member of a social group if he/she reports being a member of any of these groups.

Table 5
Political group membership and conflict victimization.

	Dependent variable: are you a member of a political group?			
	IRCBP			
	2005 and 2007		2007	
Explanatory variables	(1)	(2)	(3)	(4)
Conflict victimization index	0.0602*** (0.0124)	0.0568*** (0.0122)	0.0409*** (0.0129)	0.0339*** (0.0124)
Respondent is female		−0.0437*** (0.0059)		−0.0194*** (0.0068)
Respondent age		0.0003 (0.0002)		0.0001 (0.0002)
Respondent has any education		0.0430*** (0.0088)		0.0292*** (0.0105)
Traditional authority household		0.0659*** (0.0104)		0.0445*** (0.0115)
1990 Household head had any education				0.0199* (0.0111)
1990 Household had a traditional leader				0.0402*** (0.0123)
1990 Household had a community leader				0.0156* (0.0091)
R-squared	0.275	0.288	0.272	0.293
Observations	10,471	10,471	5193	5193
Enumeration area/Year fixed effects	X	X	X	X

Notes: Columns (1) and (2) include pooled data from the 2005 and 2007 rounds of the IRCBP survey. Columns (3) and (4) restrict the sample to only 2007 data. Column (4) includes additional controls for household characteristics in 1990. These controls were not included in the 2005 survey.
The construction of the conflict victimization index is as described in the notes to Table 2.
Enumeration area–Year fixed effects are included in all specifications. Robust standard errors are reported. Standard errors are clustered at the Enumeration area/Year level. Significantly different than zero at *90% confidence, **95% confidence, and ***99% confidence.

percentage point increase in social group membership and a 3.4 point increase in political group membership (Tables 4 and 5, regression 4).

Other determinants of meeting attendance and group membership that emerge in Tables 3–5 are sensible in the Sierra Leonean context. Women are relatively less involved in the political process – specifically, both less likely to attend community meetings and to be group members – while more educated respondents and those from traditional authority households both attend more meetings and join more groups. Perhaps due to the prominence of youth community groups in Sierra Leone, young adults are more likely to be members of a social group than older adults.

The robustness of the coefficient estimates to the inclusion of additional household controls provides a first piece of evidence that omitted variables alone are not driving the results. Even with the inclusion of the prewar 1990 household controls, all three postwar behavioral outcomes are positively related to conflict victimization. More importantly, for meeting attendance and social group membership, the inclusion of the controls attenuates the magnitude of the coefficient estimate only slightly. As discussed in section 3 (and the supplementary online appendix), we can use the amount of attenuation to estimate the relative importance of the omitted variable required to explain away the entire effect. Specifically, for meeting attendance, including socioeconomic household controls attenuates the coefficient by 0.0052 (difference between columns 1 and 2 in Table 3) from 0.0704, which is the estimated coefficient for the pooled sample without any controls. The inclusion of prewar household controls attenuates the effect by 0.0089 (difference between columns 3 and 4 in Table 3) from 0.0775, which is the estimated coefficient for the 2007 sample without any controls. We estimate that the amount of targeting on unobserved variables would have to be over 13 times greater than the amount of targeting on observed socioeconomic variables and over 9 times greater than the amount of targeting on prewar household characteristics to explain away the entire meeting attendance effect. Given that together the socioeconomic characteristics and the prewar 1990 characteristics constitute an exceptionally rich set of observed household controls, this seems highly unlikely.

The estimated ratios for the remaining two outcomes – social group participation and political group membership – are similarly large, ranging from 5 to 17, suggesting that those effects cannot easily be explained by unobserved variables.[14] We conclude that the addition of other control variables would not likely completely eliminate the main relationship between conflict victimization and postwar activism.

5.3. Alternative subsamples

We next estimate these relationships for different subsamples that are less likely to be affected by selective targeting of violence (in Table 6). Each entry in Table 6 is from a separate regression, and contains the coefficient estimate on conflict victimization (in a specification analogous to regression 2 in Tables 3–5, which includes enumeration area fixed effects and controls for respondent gender, age, education, and traditional authority household). The coefficient on victimization is presented for the full sample in the first column, the youth sample in the second column, the sample not living near an RUF base in the third column, and the GoBifo survey sample in the fourth column.

Conflict victimization is positively and significantly related to community meeting attendance in each of the four alternative subsamples (Table 6, row 1). The coefficient is in fact somewhat larger in magnitude in the alternative subsamples, with the youth subsample having the largest coefficient. The robustness of this result provides further evidence against targeting along omitted variables. The relationships between victimization and the other two variables we focused on above – social group membership and political group membership – are less striking but nevertheless also point away from the omitted variable bias interpretation. The magnitude of the coefficients in the case of social group membership is remarkably stable across the different subsamples. The relationship between social group membership and conflict is robust in the "no RUF bases sample" and the GoBifo sample (row 2). Although the point estimate is positive in the youth sample, it is no longer statistically significant, due to increased standard errors in this smaller sample. The impact on political group membership is somewhat smaller in the alternative subsamples, though still significant at 95% confidence in the "no RUF bases" subsample (row 3). Even in subsamples where selective targeting is less likely, there generally remains a positive relationship between victimization and postwar civic activism.

Selective migration is not a leading concern since the measure of war violence exposure is captured at the household level, and thus adheres to respondents wherever they move, and since violence exposure is only weakly correlated with residential mobility from 1990 to the postwar period. Yet to partially address remaining concerns, we re-run all of Table 6 only among individuals who are living in the same chiefdom postwar as in 1990, and the results are very similar; to illustrate with community meeting attendance as the dependent variable, the point estimate on the conflict victimization index is 0.0553 (statistically significant at 90% confidence, results not shown).

[14] In an analogous set-up, Altonji et al. (2005) estimate a ratio of 3.55, and they interpret that (much smaller) ratio as evidence that unobservables are unlikely to explain away their entire effect of Catholic school attendance on outcomes.

Table 6
Household level postwar outcomes and conflict victimization.

Dependent variables	Conflict victimization index: coefficient (s.e.) IRCBP			GoBifo
	Full sample	Youth sample	No RUF bases	Full sample
	(1)	(2)	(3)	(4)
Panel A: mobilization, group membership, and voting				
1. Did you attend a community meeting in the past year?	0.0652*** (0.0165)	0.1446*** (0.0334)	0.0762*** (0.0238)	0.1261*** (0.0477)
2. Are you a member of a social group?	0.0655*** (0.0166)	0.0558 (0.0353)	0.0610*** (0.0238)	0.1017*** (0.0445)
3. Are you a member of a political group?	0.0568*** (0.0122)	0.0194 (0.0228)	0.0367** (0.0181)	– –
4. Are you a member of a school management committee?	0.0383** (0.0149)	0.0632** (0.0271)	0.0351* (0.0206)	0.1430*** (0.0520)
5. Did you vote/register to vote in the past election?†	0.0255*** (0.0087)	0.0536** (0.0229)	0.0224* (0.0129)	0.0874*** (0.0281)
6. Did you participate in road brushing in the past year?††	0.0235 (0.0229)	0.0626 (0.0476)	−0.0146 (0.0338)	0.2214*** (0.0498)
Panel B: political knowledge				
7. Do you know when the next election will be held?††	0.0490** (0.0212)	0.0620 (0.0439)	0.0097 (0.0274)	0.0481 (0.0416)
8. Can you correctly name the Local Councilor?††	0.0326 (0.0218)	0.0279 (0.0431)	0.0439 (0.0317)	0.1347*** (0.0499)
9. Can you correctly name the Paramount Chief?††	0.0254 (0.0169)	0.0879** (0.0399)	0.0144 (0.0240)	0.1006** (0.0439)
Observations	10,471	3031	5155	2694
Enumeration area/Year fixed effects	X	X	X	X
Panel C: trust and church membership				
10. Do you trust people from outside your community? (0 = low trust, 1 = high trust)	0.0569*** (0.0181)	0.0832*** (0.0372)	0.0750*** (0.0256)	– –
11. Do you trust other members of your community? (0 = low trust, 1 = high trust)	−0.0085 (0.0131)	0.0320 (0.0288)	−0.0245 (0.0183)	0.0662** (0.0334)
12. Are you a member of a church/mosque group/Have you attended church/mosque in the past month?	0.0046 (0.0179)	−0.0235 (0.0371)	0.0505* (0.0248)	0.0532* (0.0319)
Panel D: socioeconomic outcomes				
13. Does your household have a stove?†	−0.0015 (0.0101)	−0.0144 (0.0295)	−0.0059 (0.0148)	0.0622* (0.0363)
14. Does your household own a radio?	−0.0378** (0.0160)	−0.0170 (0.0352)	−0.0434** (0.0217)	0.0521 (0.0579)
15. Does your household have a tin roof?††	−0.0045 (0.0191)	−0.0461 (0.0410)	−0.0454 (0.0285)	−0.0159 (0.0477)
Observations	10,471	3031	5155	2694
Enumeration area/Year fixed effects	X	X	X	X

Notes: Each entry is the coefficient (s.e.) on the conflict victimization index in a separate OLS regression. The specification is analogous to (2) in Tables 4–6. All regressions include controls for education, gender, age, and traditional authority household. Enumeration area/Year fixed effects are included in (1), (2) and (3) and village fixed effects are included in (4). Controls for 1990 household characteristics are not included, as they are not available for all surveys.
The IRCBP samples in (1), (2), and (3) include pooled data from 2005 and 2007 surveys. Due to differences in questionnaires across surveys, some rows contain data from only 1 year. Specifically, rows marked with (†) only have 2005 IRCBP data (n = 5278, 1479, and 2601 in (1), (2) and (3), respectively); and rows marked with (††) only have 2007 IRCBP data (n = 5193, 1552, and 2554 in (1), (2), and (3) respectively).
Robust standard errors are reported. Standard errors are clustered at the Enumeration area–Year level in (1), (2), and (3) and at the village level in (4). Significantly different than zero at *90% confidence, **95% confidence, and ***99% confidence.

5.4. Other postwar civic and political outcomes

We next expand the analysis to consider a more complete set of postwar outcomes, and these additional measures of political mobilization echo the findings above. Membership on school management committees is positively and significantly related to conflict experience in all four subsamples (Table 6, row 4). People who experienced more violence are significantly more likely to have registered to vote or to have voted in recent elections in all four subsamples (row 5).[15] The relationship between violence victimization and participation in road brushing, a locally organized activity to keep bush paths between villages passable, is positive and statistically significant in the GoBifo sample (though not statistically significant in the IRCBP sample, row 6). These findings on school committees and road maintenance are important, as they appear to confirm that increased political mobilization is producing more local public goods and not just creating deadlock within communities.[16]

The evidence on political knowledge is less consistent, pointing to a weak positive association with victimization (Table 6, panel B).[17] All twelve coefficient estimates in this panel – for three different political knowledge measures, in four specifications each – are positive, and four out of the twelve are statistically different than zero at over 95% confidence, while five others have t-statistics greater than one.

Self-reported trust is of interest since it could reflect heightened levels of cooperation (what some scholars call "social capital"). The relationship between war victimization and self-reported trust for outsiders (not from this community) is unexpected: violence exposure appears to make people *more* trusting of those from outside their community (Table 6, row 10). However, while trust for members of a respondent's own community is also positively and significantly related to victimization in the GoBifo sample, it is not statistically significant in the IRCBP samples (row 11). Similarly, while there is some evidence for a positive relationship between conflict victimization and postwar religiosity in some specifications, the relationship is not robust across samples (row 12).

The final portion of the analysis turns to socioeconomic status as measured by household assets. Perhaps surprisingly, we find no conclusive evidence of a relationship between conflict victimization and household socioeconomic measures in the postwar period. The relationship between victimization and owning a stove is actually positive in the GoBifo sample, but not statistically significant in the IRCBP samples (Table 6, row 13). In contrast, the relationship between victimization and owning a radio is negative but small in two of the three IRCBP samples, though not significant for the GoBifo sample (row 14). Having a tin roof (an indicator of relative prosperity) is not related to conflict victimization in any of the subsamples (row 15). Taken together, there is no decisive evidence that individuals who experienced more conflict victimization are either systematically better or worse off along observable asset ownership several years after the war; further evidence on socioeconomic impacts is provided below in the chiefdom-level analysis.

5.5. Additional specifications

We next investigate the possibility of heterogeneous effects of violence victimization for different population subgroups. When the

[15] IRCBP survey respondents were asked if they had registered to vote, while GoBifo respondents were asked if they had voted. While not exactly the same, these two dependent variables are analyzed together for the sake of parsimony. We also find that females are less likely to vote while education is weakly related to voting (not shown).
[16] Olson (1984) has noted that increased political mobilization could give rise to small, exclusive coalitions that lobby for narrowly targeted policies that do not benefit society at large. That type of mobilization could have a negative aggregate effect.
[17] Systematic response bias may be a concern with some self-reported measures, but is unlikely to be a concern with these political awareness questions, for which a response is either correct or incorrect.

explanatory variables for female, education, age, and traditional authority are interacted with the household victimization index, the coefficient estimates on these interaction terms are not generally statistically significant for the outcome measures in Table 6. The point estimate on the interaction of violence victimization with a youth indicator is often positive, and sometimes marginally significant (as suggested by the findings in column 2), suggesting larger political mobilization impacts among youth, but this result is not robustly significant across samples, outcomes and specifications (regressions not shown).

It is also theoretically possible that effects could differ across different types of victimization, e.g., physical assault versus residential displacement, if these experiences are associated with different degrees of personal trauma. Yet, perhaps surprisingly, no single input into the conflict victimization index is a more important determinant of postwar behaviors than the others: when the three distinct components of the victimization index are included as separate independent variables, an *F*-test on the null hypothesis that the corresponding coefficients are all equal cannot be rejected at 95% confidence for any of the outcomes in Table 6 for the full IRCBP sample (results not shown).

6. Chiefdom-level results

6.1. Chiefdom level correlates with violence intensity

We next investigate the relationship between war intensity and various factors thought to have contributed to the war. The most robust finding is that chiefdoms with diamond mines witnessed significantly more attacks and battles. In all specifications, including those with district fixed effects and controls for 1989 socioeconomic status, the relationship is large, positive, and statistically significant (Table 7, regressions 1–3). Our data thus confirms the widely held view that diamonds were related to local fighting intensity. Other geographic controls, including road density, distance to Freetown (the capital) and population density are only weakly related to both measures of violence. We find no significant relationship between diamonds and household reports of victimization (regressions 4–6). Humphries and Weinstein (2006: 444) similarly find no relationship between diamond mines and brutality towards civilians, in data reported postwar by the fighting units themselves. It appears that the fighting around diamond mines primarily involved soldiers and did not disproportionately affect civilians in those areas.

Turning to other factors, prewar 1989 school enrollment is negatively related to civilian victimization (Table 7, regression 6). This is consistent with the explanation that violence was more severe in areas with poor public services, possibly due to more severe political grievances in those areas (Richards 2003), or possibly fewer youth employment opportunities (Collier and Hoeffler 2004). Finally, we find that 1989 average log per capita consumption expenditures are positively related to the number of chiefdom attacks and battles, consistent with the explanation that lootable resources attracted armed groups (Collier and Hoeffler 2004). While we do not place too much emphasis on these 1989 data because the sample size falls to just 64 chiefdoms, it provides suggestive evidence that prewar socioeconomic conditions are associated with later violence.

6.2. Chiefdom-level violence and outcomes

Chiefdom-level violence intensity is not robustly correlated with postwar outcomes in terms of socioeconomic measures. We find no substantial lingering negative effects of the war on 2004 consumption expenditure levels using either measure of conflict violence (the average conflict victimization, or the number of attacks and battles). The specifications include geographic controls, district fixed effects, and finally controls for prewar 1989 log per capita expenditures (Table 8, regressions 1–3). If anything, areas that suffered from more violence victimization have slightly higher postwar consumption, although effects are never statistically significant. One possible partial explanation for the rapid postwar economic recovery is improved soil fertility: land was often left fallow in areas that experienced more violence and population displacement, and this could have resulted in temporarily higher postwar yields there, although this of course remains speculative in the absence of longitudinal soil data. In contrast, the number of diamond mines in the district is robustly positively associated with higher local living standards in all specifications, as expected, given the key role of the mining sector in the Sierra Leonean economy.

We next estimate the relationship between conflict and other socioeconomic and public goods outcomes, in a specification that includes all 152 chiefdoms and controls for district fixed effects and chiefdom geographic characteristics (Table 9); results are similar with 1989 prewar per capita log expenditure control although the sample is considerably smaller in that case (not shown). Neither 2004 log per capita consumption expenditures (reproducing the Table 8 result), proportion of children enrolled in school, nor child body mass index

Table 7
Chiefdom-level correlations with conflict intensity.

Explanatory variable	Dependent variable: number of attacks and battles			Dependent variable: conflict victimization index		
	(1)	(2)	(3)	(4)	(5)	(6)
Number of diamond mines	0.395***	0.325***	0.364***	−0.0016	0.0010	0.0011
	(0.078)	(0.074)	(0.088)	(0.0024)	(0.0014)	(0.0011)
Road density	19.01*	5.34	−25.13	0.0937	−0.2294	0.4028
	(10.55)	(15.23)	(29.73)	(0.1851)	(0.1587)	(0.3853)
Log distance to Freetown	−1.934	0.5571	4.636	0.127**	0.071*	0.097
	(1.792)	(1.878)	(5.241)	(0.048)	(0.038)	(0.073)
Log population density, 1985	−0.322	0.208	1.903	0.0268	−0.0057	0.0728**
	(1.287)	(0.938)	(2.408)	(0.0268)	(0.0138)	(0.0316)
Proportion children in school, 1989			5.012			−0.212**
			(14.30)			(0.101)
Log per capita expenditure, 1989			3.76**			0.0104
			(1.71)			(0.0269)
R-squared	0.069	0.211	0.337	0.182	0.621	0.707
Observations	152	152	64	152	152	64
District fixed effects		X	X		X	X

Notes: Additional controls in all regressions include number of chiefdom non-diamond mines and the river density. In regressions (2), (3), (5), and (6) district fixed effects are included for Tonkolili, Pujehun, Port Loko, Moyamba, Kono, Koinadugu, Kono, Kenema, Kambia, Bonthe, Bombali, and Bo Districts; Western Area Rural District is the omitted district. Robust standard errors are reported. Standard errors are clustered at the district level in all regressions. Significantly different than zero at *90% confidence, **95% confidence, and ***99% confidence. The coefficient on log per capita expenditure in column (3) is robust to excluding Western Area Rural from the regression sample.

Table 8
2004 Chiefdom log per capita expenditure and conflict victimization.

Explanatory variable	Dependent variable: log per capita expenditures, 2004		
	(1)	(2)	(3)
Chiefdom conflict victimization index	0.522	0.423	0.395
	(0.472)	(0.384)	(0.542)
Number of attacks and battles	−0.0052	−0.0063	−0.0037
	(0.0042)	(0.0063)	(0.010)
Number of diamond mines	0.0278***	0.0245***	0.0161**
	(0.0033)	(0.0040)	(0.0058)
Road density	0.295	0.804	0.689
	(0.734)	(0.523)	(1.064)
Log distance to Freetown	−0.3192**	−0.1198	0.3292
	(0.1071)	(0.1198)	(0.1852)
Log population density, 1985	−0.0940**	−0.0164	−0.0494***
	(0.0406)	(0.0396)	(0.0128)
Proportion children in school, 1989			0.0433
			(0.3498)
Log per capita expenditure, 1989			0.0645
			(0.0830)
R-squared	0.223	0.475	0.648
Observations	117	117	55
District fixed effects		X	X

Notes: Robust standard errors are reported. Standard errors are clustered at the district level in all regressions. Significantly different than zero at *90% confidence, **95% confidence, and ***99% confidence. Due to sampling in the 2004 household survey, the sample size is smaller than the full sample of 152 chiefdoms. Additional controls in all regressions include number of chiefdom non-diamond mines and the river density. In regressions (2) and (3) district fixed effects are included for Tonkolili, Pujehun, Port Loko, Moyamba, Kono, Koinadugu, Kono, Kenema, Kambia, Bonthe, Bombali, and Bo Districts; Western Area Rural District is the omitted district.

Table 9
Chiefdom-level outcomes and conflict victimization.

Dependent variables	Chiefdom conflict victimization index: coefficient (std. error)
Panel A: postwar socioeconomic outcomes	
1. Log per capita expenditure, 2004	0.423
	(0.384)
2. Proportion children enrolled in school, 2004	0.187
	(0.169)
3. BMI for children, 2004	3.61
	(8.03)
Panel B: school Quality Outcomes, 2005	
4. Proportion of teachers absent on day of survey	−0.022
	(0.160)
5. Proportion of schools receiving financial/in-kind resources from community	0.653**
	(0.313)
6. Proportion of schools receiving financial/in-kind resources from donors or NGOs	−0.088
	(0.361)
7. Proportion of schools visited by a traditional authority in past year	−0.276
	(0.416)
Panel C: adult education and NGO projects in 2004	
8. Proportion of adults with any education, 2004	0.036
	(0.089)
9. Total number of NGO projects	−31.47*
	(16.30)
District fixed effects	X

Notes: Each coefficient and standard error is from a separate OLS regression. Robust standard errors are reported. Standard errors are clustered at the district level in all regressions. Significantly different than zero at *90% confidence, **95% confidence, and ***99% confidence.
Due to sampling in the various household surveys, there are 117 observations in rows 1–3; 104 chiefdoms in rows 4–7; and 152 chiefdoms in rows 8–9. The number of NGO projects in row 9 includes all reported education, health, and agriculture NGO projects. The specification in these regressions is equivalent to regression (2) in Table 8: additional explanatory variables include number of attacks and battles, number of diamond mines, road density, log distance to Freetown, log population density in 1985, number of non-diamond mines, and river density, district fixed effects are also included (see notes in Table 8).

(BMI) are significantly associated with conflict victimization in a chiefdom (Table 9, panel A). Conflict victimization is also not significantly related to local primary schooling outcomes, including teacher attendance, outside assistance, visits by chiefs, or local educational attainment levels (panels B and C). The one exception is that chiefdoms with greater civilian victimization were significantly more likely to have successful community fundraising for their primary schools (row 5), echoing the individual-level collective action findings (in Table 6). This change is apparently not driven by different education levels among parents in war-affected chiefdoms (row 8).

One concern with the chiefdom-level results is that chiefdoms heavily affected by the war could have received increased amounts of NGO and donor funding in the postwar period. Not only do war-affected chiefdoms not get more NGO projects, we find that they may even receive relatively fewer projects (Table 9, row 9). This could, in part, be due to the fact that some of the most conflict-affected areas were not declared safe for aid workers until up to a year or more after other regions.

A second issue is whether chiefdom-level impacts were larger in areas where chiefs were themselves attacked or killed in the violence. We used the NPWJ report to construct an indicator variable for this type of violence against traditional authorities, but when this measure is included as an additional explanatory variable we find that the coefficient estimate is not statistically significant at traditional confidence levels for any of the collective action variables (regressions not shown). Thus attacks on traditional leaders do not appear to be the key drivers of the political and collective action impacts we estimate, consistent with the view that individual-level changes are key.

7. Conclusion

Using unique nationally representative household data for a postwar society, we find that individuals who directly experienced violence during the recent Sierra Leone civil war are no different in terms of postwar socioeconomic status, but they display dramatically higher levels of political mobilization and engagement, as well as higher local public goods contributions, than non-victims. Conflict victims' households are more likely to attend community meetings and join social and political groups, more likely to register to vote, and to sometimes participate in school committees and road maintenance.

This relationship at the individual level is remarkably robust across two survey datasets and multiple specifications with different controls. We admittedly cannot completely rule out the possibility of some omitted variable bias, in that the types of people victimized were those who would have become postwar local leaders anyway and this remains a concern for interpretation. Yet there is no evidence that controlling for education, chief status, or prewar community leadership changes the results. Additional tests – namely, demonstrating robustness in the youth subsample and in chiefdoms without permanent RUF bases – also argue against the hypothesis that the selective targeting of community leaders is the main driver of our empirical results.

It is noteworthy that differences across individuals within the same village are so pronounced given the fact that everybody, at a minimum, witnessed extreme acts of violence during the Sierra Leone civil war, even if their household did not experience it directly. The gap we find between those who directly experienced violence and others provides evidence that personal experience is much more influential than mere observation in shaping subsequent behavior. This finding resonates with similar evidence provided by Simonsohn et al. (2006), who show in the lab that behavior is much more responsive to individuals' own personal experiences.

While our finding that victimization leads to increased political mobilization is perhaps surprising given recent evidence that personal trauma can reduce social capital (Alesina and LaFerrara 2002), it is

broadly in line with a growing body of work on the determinants of U. S. political participation. Traditionally many studies on this topic have focused on how costs and other economic factors influence participation choices, but these models have been largely unsuccessful in explaining key behaviors such as voter turnout (Green and Shapiro 1994). In response to the failure of these rational choice models, there is growing interest in how psychological and social factors affect political participation. For example, Green and Gerber (2008) find that subtle changes in the framing of political messages often have major impacts on turnout; Hastings et al. (2006) find that parents who lost in school choice lotteries are significantly more likely to vote in subsequent school board elections, compared to parents who won; and Achen and Bartels (2004) claim that voters who suffered a seemingly random local misfortune – including local floods, shark attacks, or flu epidemics – tend to punish political incumbents in later elections. Given the large voting impacts among school lottery losers and in towns with a shark attack, a finding that political activism increases among civil war victims seems intuitively plausible.

Many Sierra Leone scholars argue that the civil war transformed society and culture. David Keen (2005: 170) claims that the "experience of displacement and to some extent the exposure to aid organizations seems to have produced a heightened awareness among many ordinary Sierra Leoneans", and among youths in particular. Ferme also discusses the potential to forge something positive out of the horrors of war: "[Sierra Leonans] have sometimes turned [social instability] into a creative, though violent, opportunity to refashion themselves vis-à-vis their own institutions" (2002: 228). In a striking measure of the extent of political change in the country, Sierra Leone's free and fair multiparty national elections were decisively won by the political opposition in 2007, completing the first peaceful transfer of power since the 1960s.

More speculatively, the increased local political mobilization we document could potentially help promote future economic development in Sierra Leone rather than hinder it. We find that contributions to some local public goods are higher among war victims. These individual contributions cannot simply be interpreted as a response to increased local problems since enumeration area fixed effects control for any village-wide needs, but rather appear to reflect changes in individual preferences and values. On average, experience with war violence mobilizes people and turns them into community activists, rather than demoralizing them. If this results in better local infrastructure (roads) and services (education), it would be one positive legacy of the conflict.

More research clearly needs to be done to understand the legacies of civil wars in Africa, especially since our empirical strategy only provides evidence on localized conflict impacts rather than overall national effects. Further research should also investigate whether similar patterns hold in societies emerging from civil wars fought along ethnic or religious lines, unlike the Sierra Leone war where the main fighting groups and victims were multiethnic.

Yet our findings may begin to make sense of the rapid economic growth and political consolidation that other African countries have experienced following protracted civil wars. The humanitarian costs of civil wars are horrific but it appears their legacies need not be catastrophic.

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Appendix A. Supplementary data

Supplementary data associated with this article can be found, in the online version, at doi:10.1016/j.jpubeco.2009.07.012.

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