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## From Violence to Voting: War and political participation in Uganda

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**Abstract:** What is the political legacy of violent conflict? This paper presents evidence for a link between war, violence and increased individual political participation and leadership among former combatants and victims of violence, and uses this link to understand the deeper determinants of individual political behavior. The setting is northern Uganda, where rebel recruitment methods generated quasiexperimental variation in who became a rebel conscript and who did not. Original survey data shows that the exogenous element of conscription (by abduction) leads to significantly greater political participation later in life. The principal determinant of this increased political participation, moreover, appears to be war violence experienced. Meanwhile, abduction and violence do not appear to affect multiple non-political types of community participation. I show that these patterns are not easily explained by models of participation based on simple rational preferences, social preferences, mobilization by elites, or information availability. Only 'expressive' theories of participation appear consistent with the patterns observed, whereby exposure to violence augments the value a person places on the act of political expression itself. The implications for general theories of political participation are discussed.

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## 1. Introduction

What is the political legacy of a violent civil war? Can perpetrators and victims of violence become productive citizens once the fighting stops? Policy-makers appear pessimistic. A World Bank document suggests that the impacts of civil war are often so adverse that they “cannot reasonably be viewed as social progress” (2003: 32). There is particular concern (and scattered evidence) that former combatants are especially likely to lead lives of crime, violence, and exclusion. The French foreign minister, addressing a recent UN conference, spoke of young ex-soldiers as “a time bomb that threatens stability and growth” (BBC, 2007). A *New York Times* (2006) editorial lamented that such youth return as “damaged, uneducated pariahs”. Reintegration experts worry that former combatants face a life of crime and banditry (Spear, 2006) and that these youth remain alienated and “at war” in their own minds (Richards et al., 2003). If these commentators are correct, then the rebuilding of civil society and democracy will be all the more challenging and unlikely in post-conflict states, and may even contribute to the well-known ‘conflict trap’ (Collier, 2003; 2007).

Not all of the evidence is so gloomy. A small literature ties victimization by war violence to greater political and collective action. Wood (2003) argues that government violence in El Salvador prompted its’ victims to support and even join opposition forces out of moral outrage. Bellows and Miguel (2007) find that war-related displacement or deaths in the family lead to greater political participation and awareness in Sierra Leonean households. Likewise, psychologists find that victims of violence are in general resilient, and that exposure has even led to political activism among groups such as Jewish Holocaust survivors (Carmil and Breznitz, 1991) and Palestinian victims of bombardment (Punamaki et al., 1997). Toure (2002) argues that the civil war in Liberia saw the birth of a robust indigenous civil society and human rights organizations. Little of this evidence, however, demonstrates a clear causal link from violence to political engagement, and little of it deals with the perpetrators of violence.<sup>3</sup>

This paper employs new data and an unfortunate natural experiment in Uganda to show that combat experiences and exposure to war violence lead to *greater* political participation and engagement among young men formerly in an armed group. This is potentially good news for policy makers in civil war-torn nations. For social scientists, however, it presents a puzzle: why should violence influence political partic-

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<sup>3</sup> There are some exceptions. Humphreys and Weinstein (2007) document the determinants of demobilization and reintegration in Sierra Leone, and note that 62 percent of their survey respondents reported attitudes supportive of the government and democracy.

ipation? The answer can expand our understanding of the determinants of individual political behavior more generally, and test competing theories of participation.

Uganda provides a natural, albeit tragic, testing ground for theories about the individual impacts of war. A low-scale guerrilla war has plagued the north of the country for nearly twenty years. This paper will show that patterns of rebel recruitment appear to have generated nearly exogenous variation in participation in warfare and violence. Over the past two decades tens of thousands of adolescent and young adult males have been forcibly recruited, or abducted, by the Lord's Resistance Army (LRA). Abduction was large-scale and, according to rebel leaders themselves, indiscriminate. Survey data support this unusual claim, and suggest that rebel conscription is exogenous conditional on year and location of birth.

If abduction is indeed conditionally exogenous, causal estimates of its impact on later-life outcomes such as social and political participation can be identified. The results in this paper suggest that forced recruitment leads to *greater* political participation—a 22 percent increase in the likelihood of voting, a more than doubling of the likelihood of being a community leader or holding a political job, and a 73 percent increase in the likelihood of being a member of a peace-promoting organization. Abduction, however, does not seem to systematically affect non-political forms of social participation, such as community group membership or public goods management, suggesting that the impact of conscription is uniquely political.

Of course, conscription simply represents a package of war experiences—violence experienced, violence perpetrated, military training, indoctrination, time away from school and work—and it is these experiences that probably account for any long term impacts we observe. Exploring the effect of such (potentially endogenous) experiences among the abducted, this paper shows that violence, in particular violence received, can account for the bulk of the impact of abduction on participation. No other war experiences are so significantly and consistently associated with both participation and abduction.

Why should we expect abduction and violence to have any impact on an individual's political expression at all? Almost none of the dominant theories of political participation appear consistent with the patterns we observe. First, there is little evidence that abduction or violence reduces the shoe leather costs of participation, making simple rationalist explanations unattractive. Second, there is no relationship between abduction, violence, and non-political forms of participation and volunteering, suggesting that the channel of impact is not the augmentation of “social” preferences by violence.

Third, there is no indication that abductees are more likely to be targeted for mobilization by political elites. One reason is that there is little evidence of such activities on the ground, especially since competing political organizations in Uganda have little presence in the internal displacement camps where most of the population resides. More importantly, political participation is associated with the number of acts of violence experienced, a trait which is difficult for others to observe. If formerly abducted youth are being mobilized by elites, we would expect mobilization to be associated with more easily observed traits (such as abduction itself, or abduction length) rather than past experiences of violence.

The patterns we observe are consistent, however, with “expressive” theories of participation, whereby voters and leaders are motivated to participate because violence augments the inherent value placed on political expression. The expressive interpretation should be accepted with caution, if only because it runs the risk of being axiomatic, and because it is difficult to demonstrate directly or disprove. Indeed, the case for expressive preferences presented in this paper is based primarily on the elimination of alternative explanations. As we will see, however, the expressive interpretation has intuitive appeal, is consistent with the evidence, and is supported by growing a body of evidence in politics and psychology.

The findings are relevant to at least two important literatures in political science and economic development. First, the evidence contributes to our understanding of the general determinants of political participation and collective action, providing some support to proponents of expressive behavior. The empirical literature offers too few examples of exogenous variation in the determinants of political behavior, especially outside of the OECD. Understanding the impact of violence on micro-politics is particularly important in the developing world, as a third of developing countries (and two thirds of African ones) have experienced a societal conflict since the end of the Cold War (Marshall and Gurr, 2005).

The findings also complement a macro-level literature linking war and political and institutional change. Wantchekon (2004) argues that nearly forty percent of all civil wars that took place from 1945 to 1993 resulted in an improvement in the level of democracy, generally when warlords saw democratization in their interests. Tilly (1992) argued that European wars of conquest led to the creation of centralized states when rulers in need of taxes and recruits built bureaucracies and bargained with subject populations. Conversely, in Africa, Herbst (2000) has suggested that state weakness is a product of too little warfare. This macro-level literature tends to explain the link from war to state-building or democracy as a result of interactions between group actors (such as states, elites, warlords, and subject popula-

tions). The results in this paper suggest that war could also push a population towards more participatory politics and collective action at the micro-level—an exciting possibility for further research.

## 2. Violence and political participation in theory

Social science has yet to produce a standard and empirically-supported theory of political participation. One of the most vexing issues is exemplified by the “paradox of voting”: in large elections, the chance that a single vote will change the outcome is so unlikely that the expected private benefit to voting is zero, and so even a small cost of voting should deter a rational individual from participating (Riker and Ordeshook, 1968; Downs, 1957). Yet voters do turn out in large numbers, confounding rationalists.<sup>4</sup>

Three main adjustments to the rational model have been offered to overcome the paradox. One set of theories suggest that voters have *social preferences* and consider the benefit of their vote to others in their rational calculus.<sup>5</sup> A second set propose that a consumption benefit is received from the act of voting itself, and are known as *expressive* theories for the emphasis they place on the inherent value of expressing one’s preferences.<sup>6</sup> Scattered evidence, largely from the US, suggests several patterns consistent with such expressive voting behavior.<sup>7</sup>

A third set of theories argue that political leaders are able to *mobilize* voters by applying social pressure, attention, or material goods from political leaders (Shachar and Nalebuff, 1999; Uhlaner, 1989). Experimental and non-experimental evidence suggests that personal requests and shaming are effective in turning out U.S. voters (Gerber and Green 2000; Green and Gerber, 2004; Verba et al., 2000). In the context of an African state dominated by a single powerful party, political actors may be able to offer credible promises of patronage, political appointments, or other selective incentives to encourage people to turn out to vote, or to become a community leader.

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<sup>4</sup> Reviews of this literature include Feddersen (2004), Dhillon and Peralta (2002), and Aldrich (1993).

<sup>5</sup> e.g. Edlin, Gelman, and Kaplan (2007), Fowler (2006), Feddersen and Sandroni (2002), and Harsanyi (1992, 1977).

<sup>6</sup> For instance when individuals value the preservation of democracy (Downs, 1957), feel a civic duty to vote (Blais, 2000; Riker and Ordeshook, 1968), or receive psychological gains from voting with one’s preferences or ideological affiliation (Schuessler, 2000; Brennan and Buchanan, 1984; Fiorina, 1976)

<sup>7</sup> Surveys of U.S. voters suggest several regularities: that the propensity to vote is associated with expressive acts such making a donation to the election commission; that the likelihood of voting is greater among ideologues than moderates; that pre-election feelings about candidates influence vote choice; and that individuals vote to show disapproval of an disfavored candidate (Greene and Nelson, 2002; Copeland and Laband, 2002; Kan and Yang, 2001). Several mock voting exercises also suggest that altruistic voting is more likely when the chance of influencing the vote is small (Fischer, 1996; Carter and Guerrette, 1992).

Finally, a fourth set of *information-based* explanations propose that better educated and informed voters are more likely to participate, although the theoretical rationale is not clear.<sup>8</sup> Rather, the argument is based on the well-established correlation between voting and education in the U.S. (e.g. Verba et al., 2000) and the causal impact of news media exposure on U.S. voter turnout (George and Waldfogel, forthcoming; DellaVigna and Kaplan, 2006; Gentzkow, 2006).

A smaller literature has sought to explain participation in acts such as protest, community meetings, and rebellion.<sup>9</sup> Analogous to the paradox of voting, the challenge faced in explaining such active participation is in most cases a problem of collective action—participation is individually costly, while many of the benefits are shared regardless of participation (Olson, 1965). To explain such participation, scholars typically look for the provision of selective benefits of a material or social nature.<sup>10</sup>

Selective incentives are not always apparent, however, and in these cases expressive motives—ideology, grievances, and moral outrage—are convincingly proposed as an alternative solution to the collective action problem. For instance, expressive values are commonly cited by activists (e.g. Verba et al., 2000) and ideology is frequently observed to be associated with membership in political associations (Leighley, 1995).<sup>11</sup> In the context of violent rebellion, Gurr (1971), Scott (1976), and Wood (2003) argue that grievances and moral outrage are the primary motivators of participation. For instance, in her landmark study of insurgent collective action in El Salvador, Elisabeth Wood (2003) dissects the motivations of civilians that aided rebels (or became rebels themselves) and finds rational actor, class-based, and other conventional theories of participation are wholly unsupported. Rather, in her analysis, civilians were prompted to life-threatening collective action out of a sense of moral outrage and changing conceptions of justice in the wake of government violence against them and their family members and communities.

There are several reasons to be cautious about such expressive interpretations. First, expressive rationales could be developed by individuals after the fact to justify their actions. Third, causal identification

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<sup>8</sup> Feddersen and Pesendorfer (1999, 1996) propose a model where uninformed independent voters find it strategically optimal to abstain and delegate their vote to more informed voters.

<sup>9</sup> This paper follows Verba et al. (2000) in conceiving of political participation more broadly, as acts that are “intended to have the consequence of influencing the choice of governing official or the policies they make and implement” (pp. 245).

<sup>10</sup> In the context of rebellion, for instance, Lichbach (1995) emphasizes material incentives in the decision to engage in violent collective action. Alternatively, Petersen (2001), Ostrom (1990), Taylor (1988), and Popkin (1988, 1979) emphasizes social groups, norms and institutions, while Weinstein (2006) finds evidence for both material and social selective incentives.

<sup>11</sup> Other examples, especially concerning protest, include Muller et al. (1991), who find a correlation between dissatisfaction with public goods provision and protesting, as well as similar studies in West Germany and the U.S. argue that protesters receive psychological selective incentives from valuing public goods (e.g. Opp, 1988; Klosko et al., 1987; Muller and Opp, 1986).

is often poor, demonstrating correlation and not causation.<sup>12</sup> Third, the available studies are still limited in number and (with the exception of the rebellion literature) are based on experiences in the U.S. and Europe alone. Nevertheless, ideology, grievances, and moral outrage frequently offer a plausible and even convincing explanation for otherwise inexplicable collective action, and cannot be easily dismissed.

### **The link from war and violence to participation**

Each of the above theories offers a potential mechanism for linking war, violence, and participation. According to the simple rational model, abduction can influence participation if it results in differential private costs of participation. Abduction in Uganda resulted in decreased migration, diminished education and economic opportunities, and increased injuries and psychological distress among abductees (Blattman & Annan, 2007), all of which could alter the calculus of voting by lowering its cost among former abductees.<sup>13</sup> For community leadership in particular, abduction could also have a positive impact if it is associated with training or experience in leading others. Of course, abduction could diminish leadership if it is associated with social stigmatization.

Abduction and violence could also explain voting behavior in a mobilization model if it met two conditions: first, if it is easily observed by political leaders; and second, if exposure to abduction or violence augments the ease of mobilization. Abduction in northern Uganda is associated with lower wealth and employment, lower literacy and education, and higher levels of distress (Blattman and Annan, 2007), each of which could make an abductee more susceptible to vote buying or pressure. To the extent that information-based theories are influential, however, lower education and literacy should have the opposite impact, leading to lower turnout among abductees.

Violence could also affect participation if it influences “social” or “expressive” preferences. For instance, violence could directly affect psychological features of the individual. Post-traumatic growth theory in psychology supports the notion that positive political and psychological responses to war violence are common, especially when young (e.g. Powell et al., 2003; Tedeschi and Calhoun, 1996). Some

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<sup>12</sup> This is especially true of the voter turnout literature. A vast number of U.S. voter surveys find a strong correlation between participation and socio-economic traits such as income and education.<sup>12</sup> These findings have supported a “socio-economic status model”, where participation is thought to be influenced by individual resources and civic attitudes (e.g. Verba et al., 2000, Verba and Nie, 1972). Yet such studies are vulnerable to misspecification and causal identification problems, and are not empirically supported in non-Western contexts (e.g. Mattes and Bratton, 2003).

<sup>13</sup> Lower migration levels imply less re-registration and travel to vote, and stronger community connections (potentially needed for leading). Lower earnings and wealth may lower the opportunity cost of voting or being a community leader. Serious injuries or psychological distress, meanwhile, may make the act of voting or leading itself more costly or difficult.

social preference theorists such as Edlin, Gelman, and Kaplan (2007) distrust such appeals to variation in psychological traits and preferences, however. In their model, one's social preference is a function only of the probability of one's vote being pivotal and the size of the constituency—a simple setup that is consistent with a broad range of turnout patterns across time and space. Unfortunately, such parsimonious models cannot explain the significant variation in turnout within a particular country and election.

It is difficult to predict, however, how violence should affect psychology and preferences. On the one hand, if adversity stimulates solidarity, grievances and moral outrage, then participation should be increasing in the intensity of exposure to that adversity. On the other hand, if adversity results in discouragement or disenfranchisement, then participation will diminish with violent exposure.

A small but growing body of evidence suggests that the former case is dominant, including the post-traumatic growth theory discussed above. Psychologists also routinely find youth resilient to violence and other trauma (e.g. Dyregrove et al., 2002; Masten, 2001; Ajdukovic & Ajdukovic, 1998; Nader et al., 1993). Other evidence suggests that voters respond to other types of negative shocks with increased participation. Bloom and Price (1975) show that U.S. voters are more likely to vote following negative macroeconomic outcomes than positive ones, while Hastings et al. (2006) find that parents of school lottery losers were more likely to vote in later school board election than those of winners. A related literature has focused on how voters punish incumbent politicians for bad macroeconomic performance and reward them for good, even when those events are beyond political control (Lewis-Beck, 1988; Markus, 1988; Kramer, 1971). Voters even appear to punish incumbents for natural disasters, droughts, and shark attacks (Achen and Bartels, 2004).

### **3. War, abduction, and politics in northern Uganda**

To examine the impact of war and violence on participation, this paper draws on the experiences of youth embroiled in the twenty-year war in northern Uganda.

The war in Uganda has both spiritual and political roots. In 1988, a spirit medium named Joseph Kony assembled the remnants of several failed insurgent groups from northern Uganda into a new guerrilla force, the Lord's Resistance Army, or LRA.<sup>14</sup> Locally Kony is believed to possess great spiritual powers,

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<sup>14</sup> This account is based on Allen (2005), Behrend (1999), Doom and Vlassenroot (1999), Finnström (2003), Lamwaka (2002), and Omara-Otunnu (1994).



and his stated goal is to seek a spiritual cleansing of the nation. Kony's movement, however, is also rooted in a longstanding political grievance and economic disparity between northern ethnic groups (including the Acholi, to which he and the bulk of the LRA belong) and ethnic groups from south-central Uganda. Following Independence, northern peoples came to dominate the military while southerners dominated the commercial sector, and until 1986 Uganda was governed by a series of brutal dictators from the north. In 1986, however, rebels from the southwest of the country led by Yoweri Museveni overthrew an Acholi-dominated government. Several guerrilla forces in the north initially resisted the takeover, but for the most part settled for peace or were defeated by 1988. The handful of fighters that would not settle for peace gathered under Kony to continue the fight.

In spite of widespread antipathy for Museveni among the Acholi, Kony and the LRA attracted little popular support. The poverty and unpopularity of Kony's movement limited his military options and ultimately accounts for the LRA's total dependence on forcible recruitment. From its earliest days the rebels looted homes and abducted youth to obtain supplies and recruits. Many Acholi responded by joining a government-sponsored local defense militia. To punish them for this betrayal, and to dissuade them from further collaboration, Kony ordered the massacre and mutilation of civilians. Thus from 1991 the war was waged not only against the government but against the Acholi populace at large.

In 1994 the Sudanese government began supplying the LRA with supplies, weapons and territory upon which to build bases—support that enlarged and invigorated a small and weak LRA. Abduction from 1995 to 2004 was large-scale and indiscriminate, with at least 60,000 youth estimated to have been taken by the LRA for at least a day (Annan et al., 2006). The majority of these abductees were adolescent males, though men and women of all ages were commonly taken.

Twenty percent of male abductees did not return and sadly can be presumed perished (as few remain with the LRA). The remaining 80 percent escaped, were released, or were rescued after periods of a day up to ten years. Roughly half of these 'returnees' reported to and were demobilized by the Ugandan army (the UPDF), and two in five returnees passed through a 'reception center' that provided basic health services, family relocation, and reinsertion. In 2006 the Government of Uganda and the LRA reached a truce and peace talks continue.

The two decades of instability and economic destruction in the north stand in stark contrast to the success and stability of the rest of Uganda. Outside Acholiland, violence has abated, infrastructure has

expanded, HIV infection rates have fallen, and economic growth has been a robust 6 percent for the past decade (Government of Uganda, 2007). Moreover, the country has become gradually more free and democratic. President Museveni introduced single-party democracy in 1996, and was elected and re-elected in 1996, 2001 and 2006 under moderately free and fair elections.<sup>15</sup>

In 2005, Museveni proposed constitutional amendments which would allow for multi-party democracy as well as eliminate term limits, allowing him to run again. A peaceful national referendum was held in August 2005 on the question of multi-party politics—just two weeks before our survey began.<sup>16</sup> 47 percent of eligible voters turned out, with 92 percent voting in favor of the amendments (IFES, 2007).

#### 4. Data & measurement

The data come from Phase I of the Survey of War Affected Youth, or SWAY—an original, representative survey of 741 rural male youth (ages 14 to 30) in the Acholi districts of Kitgum and Pader, Uganda. Both districts (and thus the full sample) are ethnically, linguistically, and religiously homogeneous.<sup>17</sup> Surveys were administered by local enumerators in eight rural sub-counties between September 2005 and March 2006. Former abductees were over-sampled, with 462 interviewed in total.

The survey sought to select its respondents from a sample frame of youth living in the region *before* the conflict in order to minimize sample attrition due to the migration and mortality. 1100 households were sampled from U.N. World Food Programme lists compiled in 2002, and 92.5 percent of these household heads were successfully tracked down and interviewed.<sup>18</sup> Enumerators then worked with household heads to develop a retrospective roster of all youth living in the household in 1996. The year 1996 was chosen as it was easily recalled as the date of the first election since 1980, and because it dates to the time of the war's escalation (and pre-dates 85 percent of local abductions).

A sample of 870 surviving youth was drawn from this retrospective roster of youth. Of these youth, 41 percent had moved since 1996 and were followed across the country to their current location. 741 of

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<sup>15</sup> Official election results suggest that Museveni received 59 percent of the 2006 national vote share. (IFES, 2007)

<sup>16</sup> This referendum asked voters: "Do you agree to open up the political space to allow those who wish to join different organizations/parties to do so to compete for political power?"

<sup>17</sup> Virtually the entire population of the Districts are ethnically and linguistically Acholi (Luo) and all virtually are Christian (although these a mix of Catholic, Anglican, Evangelical, and Pentecostal faiths).

<sup>18</sup> Potential selection arises from the 7.5 percent of households not located, as well as from the fact that the sample frame dates from 2002 (by which time many households may have had the opportunity to out-migrate). Interviews with community leaders suggest that very few households left the region entirely before 2002—most left family members (especially parents) behind, who remain on the lists. Many migrants also took pains to get onto these lists in 2002 even when away to increase food rations.

sampled youth (or 84 percent) were located, including virtually all non-migrants and 70 percent of migrants. Absentee questionnaires were conducted with the families of all 129 unfound young men, collecting extensive data on current outcomes and abduction experiences in order to adjust for observed attrition. Demographic data were also collected on the 349 youth from the retrospective roster that had died or not returned from abduction.

## Measuring war and abduction experiences

The survey collected self-reported, retrospective information on war and abduction experiences, described in Table 1. More than two in five male youth reported an *Abduction* of any length. Many of these abductions were short, usually because the youth was too young or too old to be kept as a recruit, and so was quickly released after showing the way or carrying looted goods. Indeed, a third of abductions were less than two weeks in length, and just half were longer than six weeks. The number of *Months abducted* ranged as long as 120 (or 10 years) in the sample, with the average abduction lasting 8.5 months.

Even short abductions could be quite traumatic, however; youth abducted two weeks or fewer reported experiencing nearly seven violent acts on average. The survey asked respondents directly about 26 specific *Violent acts experienced*, including 12 *Violent acts received*, 9 *Violent acts perpetrated* by the respondent himself, and 5 *Violent acts upon the family* of the respondent.<sup>19</sup> The average youth reported 6.6 violent acts experienced, with abductees reporting 9.8 acts to non-abductees' 4.2 acts (Table 1). Other war experiences are displayed in Table 1, including a youth's *Age at abduction* and percentages that *Carried a firearm* or held a *Leadership role* in the rebel group.

## Measuring participation

Current socio-political outcomes are listed in Table 2. Our main measures of political participation are voting, community leadership, and holding of political jobs. 44 percent of the youth over age 18

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<sup>19</sup> Acts received include: witnessing an attack or battle; witnessing beatings or torture of others; witnessing a killing; witnessing a massacre; witnessing rape; witnessing the torching of occupied homes; forced labor; receiving a severe beating; being attacked with a weapon; being tied or locked up; and receiving a serious injury in a battle or attack. Acts perpetrated include: forced to kill a soldier; forced to beat a civilian; forced to beat a family member or friend; forced to kill a civilian; forced to kill a family member or friend; forced to have sex with a woman; and forced to abuse dead bodies. Violence upon family includes: a parent was abducted; another family member was abducted; a family member was injured in combat or by landmines; and, a parent was murdered or died violently.

*Voted in the 2005 referendum* (with the 238 underage youth omitted from the voting analysis).<sup>20</sup> Four percent of youth also report that they are currently a *Community mobilizer*—volunteer members of the community who are responsible for gathering the community together for political and community meetings.<sup>21</sup> This role is one of the most common forms of leadership in the community among adolescents and young adults. The youth were also asked about other forms of political employment, but only 4 of the respondents (0.4 percent) reported holding such a *Political job*.

Other indicators of community participation and collective action were also recorded. 42 percent report *Membership in at least one community group*, including *Peace groups* (7 percent), *Water management committees* (1.3 percent), *Cultural groups* (16 percent), *Sports teams* (12 percent), *Farmer’s cooperatives* (9 percent), *School clubs and committees* (5 percent), and *Church or bible study groups* (18 percent).<sup>22</sup> 81 percent also *Attend church regularly*, and 4 percent of youth *Volunteer for a community organization*. Finally, an important part of social life involves cooperation with and obedience to elders. 7 percent indicated that they “sometimes” or “often” *Disobeyed elders*.

## 5. The causal impact of abduction on participation

Estimating the impacts of military service and war violence is a challenging task. In the case of recruitment into armed groups, combatants are usually unlike non-combatants in unobservable ways, and a comparison of their behavior is likely to conflate the impact of war with any pre-existing differences that led the youth to join or be selected by the armed group. We are particularly concerned that characteristics typically associated with participation in armed groups (such as poverty, social exclusion, ideological commitment, or malleability) are traits that also affect social consciousness or political activity.

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<sup>20</sup> Selection into voting eligibility is based on an observed and exogenous factor, year of birth, and classifying underage youth as “missing” does not bias the results, but merely changes the interpretation of the treatment effect as the impact conditional being eligible to vote.

<sup>21</sup> Each five years, or when a position otherwise becomes available, a community meeting is held and a call is made for nominations. Nominees are given a chance to give a short speech, and are typically elected by a show of hands or by lining up behind the nominees. Community mobilizers are unpaid, although they may occasionally receive small tokens of thanks (e.g. food rations or household items) from the NGOs for which they mobilize community members.

<sup>22</sup> Virtually all report themselves as Christian: 71 percent Catholic, 19 percent Anglican, 9 percent Pentecostal or Evangelical, and less than one percent other (either “No religion” or traditional religion)

## **Empirical strategy**

One solution to this potential endogeneity is the counterfactual approach, whereby a relevant control group is found for the war-affected (or “treated”) individuals. The impact, or average treatment effect (ATE), is estimated by taking the difference in the outcomes of the treated and controls (Imbens, 2004; Rubin, 1974). The estimated ATE is only as reliable as the counterfactual, and it will be unbiased only when treatment assignment (in this instance, rebel recruitment) is conditionally unconfounded. That is, any selection into treatment must be on observed and measured characteristics and otherwise independent of potential outcomes (Imbens, 2004; Rosenbaum and Rubin, 1983; Rubin, 1978).

### *Evidence for the conditional unconfoundedness of abduction in Uganda*

In most armed conflicts, such stringent identification conditions would be unlikely to hold. Evidence from northern Uganda, however, suggests that the LRA’s large-scale and indiscriminate use of abduction and violence tragically provide a natural experiment where abduction and (to a lesser extent) violence experienced are unrelated to personal characteristics and potential participation.

Rebel testimonies provide the first indication that the most common types of selection into armed groups are not present in the case of the LRA. First, volunteering for the LRA (i.e. self-selection into the armed group) was virtually unknown; volunteers likely comprised less than 0.5 percent of all LRA recruits. Nearly all of these volunteers joined before 1991, however, and the majority appear to have come from the neighboring district of Gulu, however, so none were captured in our sample.

Second, interviews with the leaders of LRA raiding parties suggest that by neither design nor accident did they abduct a select group of youth. Abduction targets were unplanned and arbitrary, and homesteads were raided regardless of wealth and household composition. From their Sudanese bases, rebels ventured into Uganda for several weeks at a time in small groups of roughly 15 fighters. Raiding parties had two aims: ambushing government forces, and raiding homesteads along their path for food and new recruits. Typical of East Africa, nearly all Acholi households live in relatively isolated homesteads in their fields, arrangements which made them particularly vulnerable to LRA raids. Rebels usually invaded such homesteads at night, abducting all able-bodied members of the household to carry looted goods. These abduction parties were under instruction to release only young children and older adults, but to keep all adolescent and young adult males. Fewer than 5 percent of males abducted between the ages of 10 and 24 report being released. Abductions were large-scale, with thousands of youth taken every year.

The survey data support such claims of indiscriminate abduction. The survey gathered data on pre-war levels of household wealth (including land, livestock, and plows), parent's education, father's occupation, and parental death—traits that are believed to be reliable predictors of participation in armed groups in Africa (e.g. Honwana, 2006; Humphreys and Weinstein, 2006; Cohn and Goodwin-Gill, 1994).

We observe little difference in these pre-war traits by abduction status, yet these same traits are strong predictors of participation in the national military. Examine first the means of each of these pre-war traits for abducted and non-abducted youth (Columns 1 and 2 of Table 3) as well as the unconditional and conditional mean differences between the two groups (Columns 3 and 4). Excepting year of birth, nearly all of the unconditional differences are close to zero and none are significant at even the 10 percent level. Conditional mean differences, which control for all other pre-treatment covariates, are generally insignificant as well. Abducted youth differ only by year of birth and pre-war household size. The explanatory power of year of birth is expected, as a youth's probability of ever being abducted depended on how many years of the conflict he fell within the rebels' target age range. Abduction levels also varied over time, and so youth of some ages were vulnerable for longer than others. Meanwhile, the significance of household size is driven entirely by households greater than 25 in number, which perhaps implies that the small bands of raiders were hesitant to raid large and difficult-to-control households.<sup>23</sup>

The inability of these pre-war traits to predict abduction can be contrasted with their success in predicting government military service: participation in a voluntary government militia, or Local Defense Units (LDU). Five percent of youth were current or past LDU members. A comparison of pre-war traits in Table 3, Columns 5 to 8, suggests that militia members came from poorer and more agricultural households. Collectively our pre-war covariates strongly predict government militia membership—a test of the joint significance of all pre-war traits in predicting LDU membership yields a p-value of 0.02. Moreover, the coefficients in the militia participation regressions are much more sizable than in the abduction likelihood ones. The ability of these pre-war traits to significantly predict militia participation but not abduction is striking, and lends support to the case for unconfounded abduction.

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<sup>23</sup> All of the difference in the distributions of abducted and non-abducted youth is driven by year and location of birth. The addition of other pre-war covariates to a logit regression of abduction on age and location indicators leaves the distribution of the predicted probabilities undisturbed. An F-test of their joint significance yields a p-value of 0.18 (not statistically significant).

### *Dealing with selective attrition and survival*

A final challenge is that any association between participation and war experiences may be biased by selective attrition. In this study, there are two main types of ‘attritors’: non-survivors and unfound migrants. We are concerned because our estimates of the impact of abduction will be biased if personal qualities that determine survival also influence later social and political behavior. Plausible candidates include intelligence, self-confidence, or the tenacity to resist abduction. In general, studies of survey attrition in developing countries have concluded that attrition due to death or migration has little impact on coefficient estimates, even with attrition rates up to 50 percent (e.g. Fitzgerald et al., 1998; Falaris, 2003). The tracking success rate of this study, 84 percent, meets or exceeds the rates achieved by several ‘gold-standard’ youth tracking surveys in poor countries (e.g. Hamory and Miguel, 2006; Thomas et al., 2001). Even so, differential attrition rates by treatment status still raise some concern; mortality rates were double among the abducted, while out-migration rates were double among the non-abducted.

To correct for attrition on observables, enumerators collected demographic data and data on current activities and well-being from the surviving family members of any attritors. Following Fitzgerald et al. (1998), these data were used to calculate attrition probabilities, and regression estimates are weighted by the inverse of these attrition probabilities to eliminate bias from attrition on observed traits. Even with this correction, however, there remains a risk of bias arising from any unobserved traits that influence survival, abduction, and potential outcomes. In the sensitivity analysis below, the ATE is bounded with best- and worst-case scenarios to see if the estimates are robust to such potential bias.

### **Results: The ATE of abduction**

Assuming conditional unconfoundedness, consistent estimates of the ATE can be calculated using an index model such as the probit. A more efficient and consistent approach, however, is to weight on the inverse of a nonparametric estimate of the propensity score (Hirano et al., 2003). In this instance,  $Y^*$  is a latent variable describing an individual  $i$ 's propensity for participation, and the researcher observes a binary participation outcome,  $Y$ . The treatment effect can be estimated by the following regression:

$$P(Y_i = 1) = \Phi(\tau \cdot T_i + X_i^S \cdot \beta_i), \tag{1}$$

where the treatment indicator  $T$  equals one if youth  $i$  was abducted, and the  $X^S$  are the subset of covariates  $X$  that are significantly correlated with  $Y$ , conditional on treatment.<sup>24</sup> The resulting ATEs, represented by  $\tau$  and summarized in Table 4, suggest that abduction causes higher political participation and activity but has little effect on non-political forms of political participation.<sup>25</sup>

To begin, abduction leads to an increase of 8.5 percentage points in the likelihood that an eligible youth (one who is 18 or older) voted in the 2005 referendum (Column 2), and is significant at the one percent level. Since just 39 percent of eligible non-abducted youth voted (Column 1), this ATE represents a 22 percent increase in voting (Column 3).

Abduction also leads to a 3.3 percentage point increase in the likelihood that a youth is a community mobilizer. Relative to the non-abducted mean of 2 percent, this represents a 145 percent increase, also significant at the one percent level.

Abduction is also associated with an eightfold increase in the likelihood of holding a political job. This estimate, however, is statistically significant at only the ten percent level, and is based on only four respondents reporting such employment (three of whom are former abductees). We therefore must be cautious about the causal effect on political jobs. Nevertheless, the direction and magnitude of the result is certainly consistent with the other political results.

Turning to others forms of community participation, the causal impacts of abduction on group membership and church membership in Table 4 are generally small and not statistically significant (even though large numbers of both abducted and non-abducted youth participate). Looking at specific groups

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<sup>24</sup> The weights used are:

$$\omega_i = \omega(T_i, v_i, \rho_i) = \rho_i \cdot \pi_i \cdot \left( \frac{T_i}{\hat{e}(v_i)} + \frac{1-T_i}{1-\hat{e}(v_i)} \right)$$

where  $\rho_i$  and  $\pi_i$  are sampling and attrition weights, and  $\hat{e}(v_i)$  is a nonparametric estimate of the propensity score. A series estimator for the propensity score achieves the efficiency bound (Hirano et al., 2003). It requires linear regression of treatment assignment on each covariate in  $X$ . Those covariates that pass a threshold t-statistic of 1.0 are included in  $X^S$ . Inverse selection weights are normalized so that differences between the inverse  $\hat{e}(v)$  and one sum to one within each treatment group. The  $v_i$  are the subset of the covariates  $X_i$  that have substantial correlation with the treatment (Hirano et al., 2003).

<sup>25</sup> An alternative approach would stack the ATE regression equations for each participation outcome and run the regressions simultaneously as seemingly unrelated regressions (SURs). This process can offer efficiency gains as well as allow simultaneous testing of hypotheses (such as the nonzero impact of abduction on political participation and the zero impact of abduction on non-political participation). An SUR model with a *linear* age term as a control for conditional unconfoundedness produces results that are consistent with those in Table 4, and confirms the joint significance of political participation (and non-significance of non-political participation). However, an SUR model that accounts for the non-linear selection effect of year and location of birth does not achieve convergence. Accounting for these non-linear selection effects are of singular importance, and so the less biased (but less efficient) individually-estimated regression estimates are displayed in Table 4.



or activities, the difference in group participation between abductees and non-abductees is generally small and never statistically significant.

Only in one instance, peace groups, is there a significant impact of abduction: abducted youth are 3.8 percentage points more likely to participate in such groups, an increase of 73 percent over non-abductees. Peace groups are clubs of youth that stage cultural dances, dramatic presentations, debates, and talks, often with peace-building or reconciliation themes. There are three possible explanations for this causal effect. First, some youth join or start these clubs independently, while others are organized by schools and international NGOs. In both cases formerly abducted youth may be targeted to take part to facilitate reintegration. Second, higher participation by the abducted could also reflect a disproportionate interest in peace activities or in signaling their peacefulness to the community. Third, an interest in peace clubs (and not other cultural or community groups) could reflect the same predilection for political participation we observed in voting and leadership.

Finally, there is little evidence of an impact on social relations. Abductees were 3.6 percent more likely to disobey elders, but the estimate is not statistically significant. Moreover, as discussed in Blattman and Annan (2007), abductees report almost no difference in levels of aggression and social support.<sup>26</sup>

#### *Robustness of the ATEs*

The ATE estimates in Table 4 are all highly robust to alternative regression models, controls, and weights. The ATEs for five of the outcomes are recalculated under alternative models in Table 5. The original ATEs (reproduced in Column 1 of Table 5) are robust to the removal of pre-war household traits (Column 2), and removal of year and location of birth (Column 3), although standard errors increase and statistical significance diminishes somewhat with no controls whatsoever. Reintroducing the control variables, the original results are robust to omission of the selection, or inverse propensity score, weights (Column 4), as well as elimination of the attrition correction (Column 5). The unweighted regressions are further robust to elimination of pre-war controls (Column 6) and again the elimination of

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<sup>26</sup> It is worth noting that the above results do not prove that non-political forms of participation are not impacted by abduction. Rather, we merely fail to reject that (null) hypothesis. Several of estimated ATEs on non-political participation in Table 4 are positive, but have a low level of statistical significance. If non-political forms of participation are systematically noisier or less variant than political participation, then we may not have the statistical power to identify a positive relationship between abduction and non-political participation. Even if true, however, the basic conclusions of the analysis hold; abduction affects political participation more consistently and more substantively than non-political forms of participation. The relative impact of abduction is unlikely to be disproven by more statistical power.

age and location controls (Columns 7 and 8). The latter regression is a simple difference of means, and implies similar, and in fact larger, treatment effects than displayed in Table 4. Use of alternative models, such as the logit or linear probability models, given similar results as well (not displayed).

### *Sensitivity of the identification strategy*

In spite of the evidence presented above, several plausible sources of unobserved selection into the LRA exist, including less clever youth “self-selecting” into the LRA because of a poorer ability to hide, or survival of only the physically strongest. Such selection could lead to overestimation of the ATEs—bias that would arise from the systematic selection of more politically active youth into the rebel group, or from differentially greater death or attrition of less politically-inclined abductees. While there is no obvious reason for either case to be true, it cannot be proven otherwise. What can be estimated, however, is the degree of selection that would be necessary to generate the ATEs we observe, which can then be judged as plausible or implausible. Two means of such sensitivity analysis are presented in an Appendix. A first method, based on Imbens (2003), explicitly model relaxations of the unconfoundedness assumption and finds that moderate amounts of unobserved selection are highly unlikely to account for the treatment effects observed (see the Appendix and Figure 1). A second method, based on Lee (2005), estimates “best-case” and “worst-case” scenarios for attrition, and finds that even in the (unlikely) worst-case, the sign of the treatment effects remain intact (see the Appendix and Table 10).

## **6. Unpacking the causal effects using heterogeneous treatments**

Evidence that conscription into a rebel force is associated with political engagement rather than apathy or exclusion is important and useful information, but the reasons for such a relationship—the causal mechanism—is even more interesting. One means of uncovering this mechanism is to examine the effects of heterogeneity. Abduction by the LRA represents a bundle of experiences, including exposure to varying violence, time away from school and work, military training, indoctrination, and leadership. To the extent that it is these underlying and varying experiences that account for the observed relationship between abduction and violence, we should observe a significant correlation between their incidence and political participation. The results suggest that the principal mechanism by which abduction impacts political participation is through violent acts received.

## Empirical strategy

In order to unpack the causal impact of abduction on participation, we can confine our analysis to the abducted alone and examine treatment heterogeneity. Specifically, we can estimate a probit model of a participation indicator,  $Y$ , on a set of observable and measured war experiences, including our measures of violence,  $\mathbf{V}$ , abduction length,  $L$ , and a vector of other abduction experiences,  $\mathbf{Z}$ , including age of abduction, having carried a firearm, and holding a leadership position (defined and summarized in Table 1):

$$P(Y_i = 1) = \Phi(\mathbf{V}_i \cdot \boldsymbol{\delta}_1 + \delta_2 \cdot \ln(L)_i + \mathbf{Z}_i \cdot \boldsymbol{\delta}_3 + \mathbf{X}_i \cdot \boldsymbol{\delta}_4 + \mu_i) \quad \text{if } T_i = 1. \quad (2)$$

For the estimated coefficients on the elements of  $\mathbf{V}$ ,  $L$ , and  $\mathbf{Z}$  to have a causal interpretation, their incidence must be assumed to be conditionally unconfounded. Yet while abduction itself was shown to be arguably exogenous, these war experiences are less plausibly so. The length of abduction, a youth's ease of indoctrination, or his inclination to commit violence are plausibly related to underlying traits unobservable to the researcher. If these traits are themselves associated with later social and political participation, then any relationship between participation and war experiences will conflate the effect of these pre-existing differences with the causal impact of the war experience itself. A linear regression of violent acts experienced on pre-abduction traits, for instance, suggests that these variables are of some but weak influence (Table 6).<sup>27</sup> The coefficients are substantively quite small, suggesting that selection into violence is minor. We are not fully confident that violence is unconfounded, however, and so the results from Equation 2 must be interpreted with caution.<sup>28</sup>

In examining the influence of violence experienced we should also consider the potential effects of systematic measurement error. Our measures of violence record incidence and not frequency, and so will understate the number of acts ever experienced. Moreover, some individuals may be hesitant to report the most extreme acts of violence, especially violence perpetrated.<sup>29</sup> If true, the consequence of such measurement error will be to: (i) increase standard errors (reducing the precision of any tests); and (ii)

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<sup>27</sup> In Column 2 of Table 4, the regression allows for non-linear relations between the pre-war traits and violence, through the use of fourth-order terms and dummy variables. Four orders of household size are jointly significant at the one percent level, a dummy for plow ownership is significant at the 10 percent level, and all pre-war traits are jointly significant at the 1 percent level, although in all cases the selection appears substantively small.

<sup>28</sup> We are most concerned about overstating the relationship between violence and participation, a situation that would arise from unobserved factors that are significantly associated with both violence and participation in the same direction.

<sup>29</sup> The direction and magnitude of any such reporting error is unknown, of course. Based on personal interviews and experience with formerly abducted young males, the author's impression is that a moderate degree of under-reporting of violence perpetrated is not unlikely because of shame or timidity. It is also conceivable that those in "public life" are less like to report perpetrating violence, and may even be more likely to report victimization. Based on personal interviews, the author feels this possibility is unlikely, especially in the context of a leadership position as junior as that of a community mobilizer.

bias the coefficient on violence towards zero. Thus the estimates should be considered a lower bound on the influence of violence on participation.

## Results

The largest and most consistent determinant of political participation in the sample is the number of violent acts received. Among the abducted, each additional act of violence received is associated with a 2.9 percentage point increase in the probability of voting and a 1.2 percentage point increase in the probability of being a community mobilizer (Table 7, Columns 1 and 2). The average abducted youth reports 4.3 more acts of violence received than non-abducted youth, implying that violence received from abduction is associated a 12.5 percentage point increase in voting (from a non-abducted base of 39 percent) and a 5.2 percentage point increase in community leadership (from a base of 2 percent). These effects are roughly comparable to (albeit greater than) the respective ATEs from Table 4.

Meanwhile, violence received is weakly and inconsistently related to non-political participation (see Table 7, Columns 3 to 13). Group membership is positively but not statistically significantly associated with violence received. Looking at individual activities and groups, sports team membership and church attendance are negatively correlated with violence received, while being a member of a farmer's group, school club, or church group are positively correlated (albeit sometimes weakly). The other of participation forms have little significant association with violence received.

Violent acts perpetrated are negatively correlated with political participation, although neither point estimate is statistically significant. As noted earlier, this coefficient could be biased towards zero to the extent that it is systematically underreported. Only a dramatic amount of underreporting could make violence perpetrated as influential as violence received, however.

Violent acts upon the family, however, are strongly positively associated with both voting and mobilization, although the result is significant only in the mobilization case. The overall effect and explanatory power of family violence is substantively small, though, since the average number of acts of family violence is small in number (2.0, from Table 1) and since the difference between abducted and non-abducted youth is very small (about 0.2 acts).

Violence perpetrated is, however, strongly associated with being a member of a peace group and with church attendance. The effect is substantively largest for peace group membership, however; the average abducted youth reports one act of violence perpetrated, which is associated with a 1.9 percentage point

increase in peace group membership on a non-abducted base of 5 percent—almost half of the treatment effect seen in Table 4.

Longer abductions seem to be associated with lower levels of political and especially group participation, although not always significantly. After controlling for violence, the natural logarithm of abduction length is inversely (but not statistically significantly) associated political participation. This inverse relationship is larger and more statistically significant in the case of community group participation, especially cultural groups, farmer’s groups, and school groups. These general results hold for alternative transformations of abduction length (not displayed).

Turning to the other measured war experiences, none are as robustly and as consistently related to our measures of political participation as violence received. Abduction age is hardly associated with any form of participation. Having ever carried a firearm in the LRA is not associated with political participation either, although it is positively associated with cultural group membership and disobeying elders.

Having held a rank or similar leadership position in the LRA (just 7 percent of our sample, in all cases very junior ranks such as sergeant or lieutenant) is negatively associated with several forms of participation—voting, community group membership, church attendance, and disobedience of elders. Leadership in the rebel group is positively associated with community mobilization, on the other hand, but not statistically significantly so in this specification. In other specifications, the relationship is occasionally significant. It is unclear, however, whether this is a causal effect of leadership experience in the LRA on later leadership, or whether pre-abduction leadership aptitude is driving both behaviors. Interviews with a reception center social worker, who also worked as an election poll supervisor during the previous two elections, suggest that these leadership experiences are not immaterial. In his experience, formerly abducted youth “feel like they can take control of their lives.” Former abductees, he continued, “are subjected to hardship where... they mature very fast.” Moreover, they “comfortably speak their views in a group of people or a crowd.” Such a view suggests that leadership may explain part of the treatment effect. The part it explains, however, may be quite small owing to the rarity of formal leadership experience in the LRA.

#### *Further evidence on the causal mechanism*

The above analysis suggests that violent acts received are the principal mechanism by which abduction impacts political participation. This proposition can be further tested: if violent acts received are the

primary causal mechanism, they should eliminate the explanatory power of abduction when included in the abduction ATE regression (Equation 1). The coefficient on abduction should fall to zero, and the coefficient on violent acts received should be positive and significant.

We test the hypothesis on three dependent variables: voting, community mobilization, and community group membership (see Table 8). For each dependent variable, the abduction ATE regression from Table 4 is replicated in the first column, and the second column introduces violent acts received and length of abduction to the regression. Months abducted is used in tandem with violent acts received as in order to reduce (but not eliminate) potential bias from omitted variables and endogeneity.

In the case of voting, the coefficient on abduction falls from 0.085 to 0.040 when violent acts are introduced, and the coefficient on violent acts is 0.018 (Table 8, Columns 1 and 2). Individually both coefficients are not significant, but they are jointly significant at the 1 percent level. This pattern is consistent with the hypothesis that violence “explains” the impact of abduction. Nevertheless, we cannot reject the possibility that abduction holds explanatory power over voting independent of violent acts received.

The results are more conclusive for community mobilization, where the coefficient on abduction falls from 0.033 (significant at the one percent level) to 0.010 (and not significant) when violence is introduced. The coefficient on violence, meanwhile, is 0.007 (significant at the one percent level). Abducted youth report four more acts of violence than their non-abducted peers, implying a 2.8 percentage point increase in the likelihood of being a mobilizer that is almost identical to the 2.3 percentage point drop in the abduction coefficient.

According to our hypothesis, neither abduction nor violence should be associated with non-political forms of participation. The results in Table 8 are consistent with this conjecture. Looking at regressions of community group membership on abduction alone (Column 5) and with violence received (Column 6) we find all coefficients to be small and individually and jointly not statistically significant.

## **7. Alternative explanations and mechanisms**

The mechanism linking violence and participation is difficult to test directly, not least because social and expressive preferences or the mobilization activities of leaders are difficult to observe and measure. The qualitative and quantitative evidence can nevertheless help us weigh competing accounts.

The mobilization of youth by elites or political parties is common across Africa, and it is conceivable that formerly abducted youth could have been targeted for voter turnout and community leadership by these elites. Several patterns and pieces of evidence, however, suggest that this explanation is unlikely. At the time of the survey, Uganda was a single party state. The ruling party was intensely unpopular in the Acholi region (in multi-party Presidential elections the following year, fewer than 5 percent supported the ruling party) and few mobilization efforts were observed by the author or his team of local research assistants, especially of formerly abducted youth. At the time of voting, the future opposition parties were not allowed to organize or mobilize, and did not visibly do so (in fact, mobilization of any nature on voting day is outlawed). Moreover, the entire survey population was internally displaced at the time of survey, and ruling party and opposition party political organizations were weak at best in the displacement camps. None of the interviewed community leaders could recall any significant voter mobilization activities, and none agreed with the idea that formerly abducted youth were targeted by elites (and, if anything, suggested the opposite might be true). Elections to become a community mobilizer, meanwhile, are held outside the normal political cycle on a sporadic and periodic basis, and are unrelated to political party or affiliation. No community leaders interviewed could recall interference from political elites or discussion of party affiliations during these informal elections.

The ability of political elites to mobilize formerly abducted youth is also contingent upon the ability of political leaders to identify their target group. In northern Uganda, however, voting and being a community mobilizer are not correlated with abduction experiences that are easily observed or common knowledge in the community, such as whether a youth has been abducted, for how long, or whether he received an injury there. Rather, participation is primarily associated with exposure to violence, which is difficult to observe and often known only to the youth in question.

Other patterns in the data also fail to support the rational actor and social preferences theories. The social preferences model from Edlin et al. (2007) predicts that individuals who vote should also be more likely to make other social contributions, such as charitable donations. While donations are not especially relevant in a displaced persons camp, we do measure contributions to public goods (such as participation in school and water management committees) and volunteering for an NGO. Yet as we saw in Tables 4 and 5, none are positively and significantly associated with abduction or violence in the sample.

The relationship between leadership in the rebel group and becoming a community mobilizer might be construed as evidence for the rational model of participation. In this view, leadership experience is a part of military training, and either augments the private returns to leadership or reduces the private cost. (Of course, an argument could be made that such experience augments social or expressive preferences. Such is the hazard of preference-based explanations.) Regardless of the specific mechanism at work, the evidence suggests that leadership experience can indeed account for at least part of the observed impact on later community leadership.<sup>30</sup> Such experience cannot, however, account for the larger and more robust relationship between violence and mobilization, and so is only a partial explanation.

Finally, note that from a mechanical point of view, any other factor that could plausibly lead to the impact of abduction on political participation must meet three conditions: (1) it must differ between abducted and non-abducted youth (that is, there must be a significant treatment effect of abduction on the factor itself); (2) it must also be a determinant of voting and community leadership; and (3) it must not influence non-political forms of participation. Each of the theories of participation we have discussed possess a number of plausible proxies in our data that can be tested against these three criteria. The proxies are not exhaustive, but together the patterns we observe fail to support the mobilization, rational actor, and social preferences explanations for the abduction treatment effect.

For the simple rational model we desire measures of the individual's shoe leather and opportunity costs of participation. Proxies for shoe leather costs include indicators for no longer living in one's district of origin—for instance having *Migrated to a town* or *Migrated out of district*. We proxy for health conditions that impede participation using an *Injury indicator* and an indicator for being in the *Top quartile of emotional distress* proxy. Finally, the opportunity cost of voting is proxied by an *asset index*, *days employed*, and *gross earnings*.

The social preferences model, as noted, supposes that the individual holds social or altruistic preferences. As noted above, in this setting we might expect such preferences to be associated with *Membership in community groups* and *Volunteering for an NGO*, and possibly with *Church attendance* as well. The survey also contained a psychosocial questionnaire that measured culturally appropriate pro-social attitudes and behaviors—including whether the youth feels that he is helpful to elders, helpful to younger youth, cares

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<sup>30</sup> While its role may be overestimated by endogenous selection into leadership, its role could also be underestimated in that there is leadership training and experience that we do not observe.



for his peers, shares freely with other youth, and enjoys of community participation. An indicator for being in the *Bottom quartile of pro-social behavior* might capture whether a youth places value on his community. Finally, we might expect an inverse relationship between social preferences and social exclusion, measured by an *Index of 17 forms of social support received* and an indicator for reporting *Poor family relations*.

Last, levels of political information and understanding, central to information-based explanations of participation, can be proxied by indicators for *Functional literacy*, *Radio ownership*, and *Educational attainment*.

Table 9 assesses the relative explanatory power of each of these potential proxies for the participation cost, social preferences, or information-based theories. Our indices of violence are included as well. The results are striking. None of the proxies meet more than one of the three criteria. Indeed, even after controlling for these factors, violence continues to explain the vast bulk of the observed treatment effect—approximately 77 percent of the voting ATE and 145 percent of the mobilization ATE versus a (statistically not significant) -22 percent of the community group membership ATE.

The impacts of abduction on violence received and each of the proxies mentioned above is displayed in Column 1 of Table 9.<sup>31</sup> The coefficients from a probit regression of the political participation measures on violence received and each of the proxies are displayed in Columns 2 and 4.<sup>32</sup> Finally, the relative influence of each determinant on the voting and leadership ATEs is listed in Columns 3 and 5 (calculated as the abduction ATE in Column 1, multiplied by the relationship with participation in Column 2 or 4, divided by the relevant ATE from Table 4).

In virtually all the cases where abduction has a substantive and significant impact on a factor (such as educational attainment), the relationship between that determinant and voting or mobilization is small and statistically insignificant. Where the determinant has a robust relationship with participation (such as that between earnings and voting), there is a small and statistically not significant impact of abduction on the determinant. As a consequence, for the vast majority of potential determinants, their ability to explain the ATE appears close to zero (Columns 3 and 5). In the case of mobilization, serious injuries are the only factor that exhibit a statistically significant relationship with both abduction and participation, but this relationship is negative (abduction decreases the likelihood of participation when it causes injury) and relatively modest (equivalent to -19 percent of the mobilization ATE).

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<sup>31</sup> As with the participation ATEs, they represent the coefficient on abduction in a regression of each determinant on an abduction indicator, year and location of birth indicators and interactions, and pre-war household traits.

<sup>32</sup> Year and location of birth indicators and interactions are included in also both these regressions but are not displayed.

All the above estimates are undoubtedly prone to bias from omitted variables, measurement error or endogeneity. Even if greatly underestimated, however, their ability to explain abduction's impact on participation would continue to be low.

The clearest exception is violence received. Abductees report 4.3 more acts of violence received than non-abductees (significant at the one percent level). Moreover, each act of violence inflicted is associated with a 1.5 percentage point increase in voting and a 1.1 percentage point increase in mobilization activities. This excess violence corresponds to 77 percent of the voting ATE and 145 percent of the mobilization ATE—much larger magnitudes than the estimated influence of the other factors (none of which display statistical significance in both abduction and participation impacts). Violence is so influential relative to the other potential determinants that even dramatic amounts of bias from potential unobservables or endogeneity would not likely change violence's central role in determining participation.

## 8. Discussion and Conclusions

What are we to conclude from this analysis? The data imply a large and robust causal impact of abduction on political participation in northern Uganda, mediated it seems by violence received. Several theories of behavior—simple rationality, social preference, mobilization, and information-based explanations—could in theory generate the abduction-participation link. None are supported by the patterns we observe, however. We do not see an impact of violence perpetrated on participation, and we do not observe any form of violence impacting non-political forms of participation. There is no evidence of elite mobilization of abducted youth (or other victims of violence), and there is no education-voting association like that we see in the U.S. While there is some evidence of leadership training influencing participation, it seems to explain only a small fraction, and explains mobilization alone. A higher incidence of injuries also seems to influence participation, but acts in the opposite direction of the average treatment effect of abduction that we observe, and so only moderates the powerful influence of violence.

The only major theoretical account of participation that is consistent with these facts is that of expressive voting. By this account, exposure to war violence creates grievances that augment the inherent value individuals place upon political expression, motivating them to increase voting and community leadership. As discussed above, this interpretation is shared with a growing body of political and psychological research linking violence to political activism via psychological growth and transformation.

The expressive explanation, however, is only a residual one. It is simply a label given to a broad category of unexplained behavior based on an under-researched association between violence and psychological growth. More work remains to be done to measure and test expressive behaviors before we can be fully confident of the conclusions in this paper. In the absence of such tests, and without conclusive evidence against the alternative mechanisms, we must accept the expressive interpretation with caution.

Five additional caveats are in order. First, as discussed earlier, the violence–participation relationship could be biased upwards by pre-existing characteristics that lead to both victimization by violence and later political expression—such as a defiant or independent character.

Second, the number of political outcomes available in the survey, particularly political ones, is quite small. Thus we should take caution in generalizing the findings to political participation generally, as the determinants of different forms of political participation may be quite different.

Third, the results in this paper arise from data on male youth alone, and so are not necessarily generalizable to females or older adults, or to other contexts. Even so, the similarity between the Ugandan results in this paper and those from refugees and victims of war violence in Sierra Leone, Israel, and Palestine (discussed above) suggest some degree of external validity outside Uganda. This external validity may not extend to all political variables, however. For instance, Humphreys and Weinstein (2007) examine attitudes to democracy among Sierra Leonean combatants and find an inverse (albeit statistically not significant) relationship between the abusiveness of one’s military unit and democratic attitudes. Generalization of the results awaits more data collection in more situations of violence and conflict.

Finally, if we accept the expressive explanation, a more important question remains unanswered: that is, if violence leads to expressive participation, why has this participation been peaceful and productive rather than contentious? Any number of explanations is possible—the opportunities for free and effective political action in Uganda, or a declining tolerance for insurgency. Each is plausible but not easily tested with these data. The decision to work within rather than outside the system is one of the most important micro-political decisions to understand, and is likewise a productive area for future research.

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## Appendix: Sensitivity analysis

### Relaxation of the assumption of conditional unconfoundedness in estimating the ATE

One means of sensitivity analysis, suggested by Imbens (2003), explicitly models relaxations of the unconfoundedness assumption in the voting ATE regression. To induce selection bias an observed covariate,  $X$ , must be sufficiently correlated with both treatment assignment,  $T$ , and the outcome of interest,  $Y$ , to induce a degree of bias worthy of concern. The same argument applies to a hypothetical unobserved covariate,  $U$ . By making some simple assumptions about the distribution of  $U$  we can calculate all the possible combinations of correlation between  $U$  and  $T$  and between  $U$  and  $Y$  that would lead the ATE estimate to be biased by a fixed amount and judge whether the existence and influence of such a  $U$  is plausible by benchmarking it against observed covariates.

Figure 1 plots each of the observed pre-war controls ( $X$ ) according to their ability to explain variation in abduction ( $T$ ) and our principal dependent variable of interest, voting ( $Y$ ). The vertical axis indicates the influence of each element of  $X$  in explaining variation in the likelihood that a youth voted in 2005 ( $Y = 1$ ). Specifically, the axis represents the marginal increase in the  $R^2$ -statistic from adding the covariate in question to a regression of  $Y$  on all other covariates. The horizontal axis indicates the influence of each element of  $X$  in explaining additional variation in abduction. Only the year and location of birth indicators (the  $\bullet$ 's in Figure 1) display substantial influence over both abduction and the potential outcome, voting. Meanwhile the pre-war household covariates (the  $+$ 's in Figure 1) explain little variation in either  $Y$  or  $T$  (a fact which accounts for the unresponsiveness of the ATE to their exclusion in Table 5).

The downward sloping curve in Figure 1 represents all the combinations of correlation between  $U$  and  $T$  and between  $U$  and  $Y$  that would be sufficient to reduce the estimated voting abduction ATE by half of the coefficient estimate (in Table 4). The  $U$  in question is modeled as a binomial variable independent of all other covariates that is assumed to have a logistic conditional distribution with both  $Y$  and  $T$ . The curve is therefore a threshold, beyond which the hypothetical  $U$  is influential enough to reduce the treatment effect by such a significant amount. It is also a threshold, we should observe, that (despite the dramatic hypothetical endogeneity) leaves the sign of the ATE intact.



The traits that normally influence military recruitment such as household wealth (point 13) or orphaning (point 7) lie far beneath the threshold. Only year and location of birth—variables that represent the primary criterion for selection by the armed group as well as variation in rebel abduction activity over time—crosses this hypothetical threshold. Even all of the pre-war covariates combined (point 14) do not near the threshold.

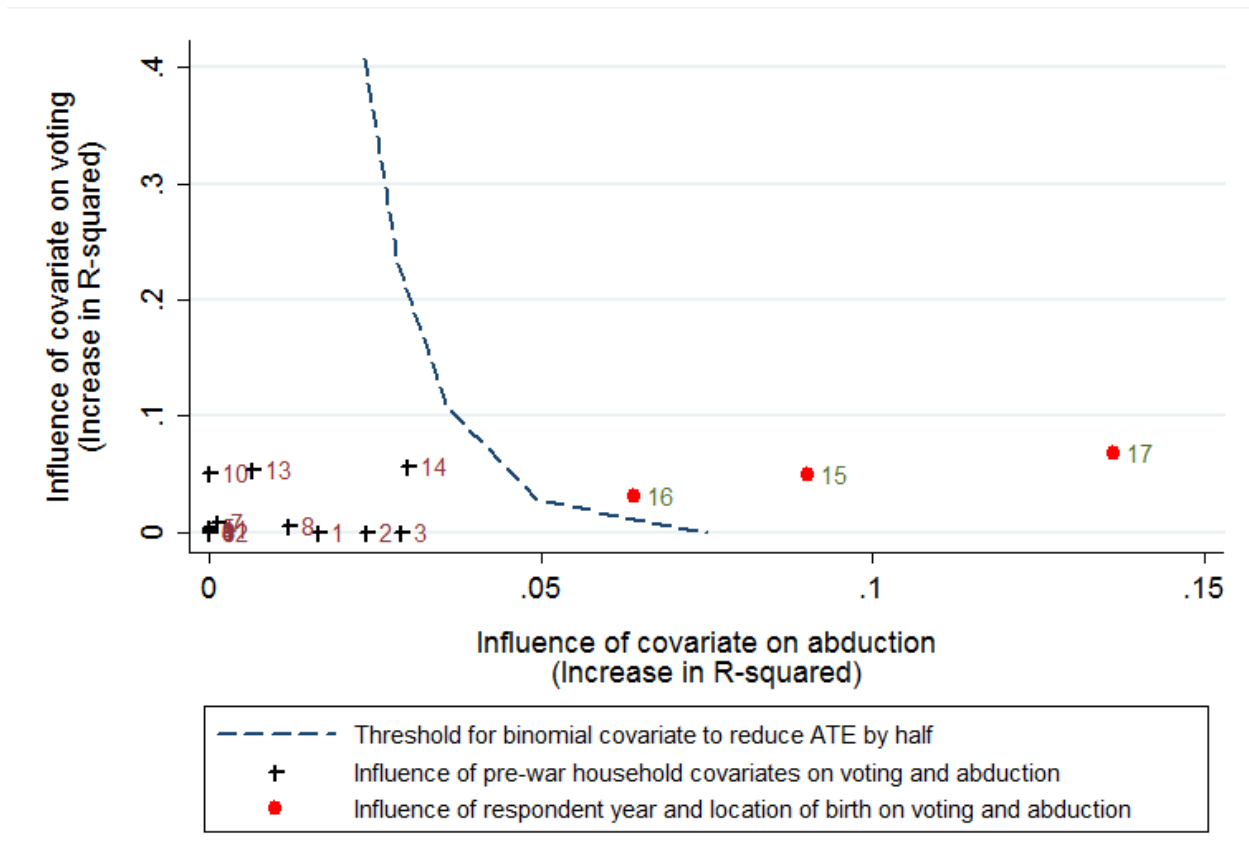
Thus an unobserved source of selection would have to be more influential than all observed household covariates, and nearly as influential as some of the primary determinants of abduction (location and age), *and even then the sign and magnitude of the average treatment effect would still be positive*. Similar results apply for more and less extreme selection thresholds. Overall, this sensitivity analysis thus suggests that moderate amounts of unobserved selection are unlikely to account for the ATE of abduction on political participation.

### **Bounding the ATE for selective attrition**

A second method of sensitivity analysis can be used to assess the potential bias from selective attrition. In a method proposed by Lee (2005), “best-case” and “worst-case” scenarios for differential attrition are constructed by trimming the distribution of the outcome in the group with less attrition, which in this case the non-abducted (see Table 10). The worst case scenario bound is calculated by dropping those with the highest values of the outcome and calculating the ‘trimmed’ ATE. The best-case bound is likewise calculated by dropping the worst-performing non-abducted youth. Lee’s method compares the untrimmed ATE (Column 3) to the trimmed means—the best and worst case scenarios (Columns 4 and 5). The ATEs under the “best-case” scenario are larger than (and at least as robust as) the untrimmed ATEs. The ATEs under the “worst-case” scenario are generally closer to zero and less than robust than the untrimmed ATEs. However, not one of these lower bounds changes sign, implying even under austere assumptions, abduction has the predicted effect on outcomes.

**Figure 1: Impact of relaxing the assumption of unconfoundedness**

Selection bias that would be induced by the omission of an observed covariates relative to the threshold where a independent binomial covariate would induce selection bias sufficient to reduce the ATE by half



Notes: The figure presents the results of the sensitivity analysis following Imbens (2003). Each + and • represents a pre-treatment covariate (listed below), plotted according to its additional explanatory power for treatment assignment (on the horizontal axis) and its explanatory power for the outcome (vertical axis), which in this case is the voting indicator. Each axis measures the change in the R<sup>2</sup> statistic from adding the covariate to the ATE regression. The downward sloping curve represents the locus of points at which any independent binomial unobserved covariate would have sufficient association with both treatment and the outcome to reduce the ATE on voting by half.

Influence of observed covariates (numbers refer to + and • in Figure 1):

- |   |   |
|---|---|
| 1. Mother's Education                   | 10. Household cattle stock in 1996        |
| 2. Father's Education                   | 11. Household other livestock in 1996     |
| 3. Mother's & Father's Education        | 12. Household plow ownership in 1996      |
| 4. Father died before 1996              | 13. All household assets in 1996          |
| 5. Mother died before 1996              | 14. All household characteristics         |
| 6. Both parents died before 1996        | 15. Respondent year of birth              |
| 7. One or both parents died before 1996 | 16. Respondent location of birth          |
| 8. Household size in 1996               | 17. Respondent year and location of birth |
| 9. Household landholdings in 1996       |   |

**Table 1: Summary statistics on war experiences**

Variable	Description	(1)	(2)	(3)	(4)	(5)	(6)
		Sample Mean [ <i>Std Dev</i> ]			Min	Max	Obs
		All	Non-abd	Abd			
Ever abducted	Indicator for ever having been abducted by the LRA for any length of time	0.44 [0.50]	n.a.	n.a.	0	1	741
Months abducted	Total length of the respondent's abductions, in months.	8.5 [15.7]	n.a.	8.5 [15.7]	0	137	462
Age abducted	Age at the time of first abduction	14.7 [4.8]	n.a.	14.7 [4.8]	5	29	462
Violent acts experienced (of 26)	Total number of all 26 possible violent events reported (data incomplete for three youth)	6.6 [4.7]	4.2 [2.9]	9.8 [4.8]	0	24	738
Violent acts received (of 12)	Reported number of the 13 violent events inflicted by others upon youth	4.2 [3.3]	2.3 [2.2]	6.6 [2.9]	0	12	738
Violent acts perpetrated (of 9)	Reported number of the 8 violent events committed by the youth upon others	0.5 [1.3]	0.0 [0.2]	1.1 [1.8]	0	8	738
Violent acts upon family (of 5)	Reported number of the 5 violent events inflicted by others upon the youth's family	2.0 [1.4]	1.9 [1.4]	2.1 [1.4]	0	5	739
Leadership position	Indicator for ever having received a rank or leading other youth while with the LRA	0.07 [0.25]	n.a.	0.07 [0.25]	0	1	462
Carried own firearm	Indicator for having been given and allowed to keep (to 'slep with') a firearm by LRA	0.28 [0.45]	n.a.	0.28 [0.45]	0	1	462

*Note:* Sample means weighted by inverse sampling and inverse attrition probabilities

**Table 2: Summary statistics on social and political participation**

Variable	Description	(1)	(2)	(3)	(4)	(5)	(6)
		Sample Mean [ <i>Std Dev</i> ]			Min	Max	Obs
		All	Non-abd	Abd			
Current age	Self-reported age in years	19.72 [5.11]	19.20 [5.03]	20.40 [5.15]	14	30	741
Voted in 2005	Indicator for voting in 2005 referendum (238 youth under age 18 classified as "missing")	0.44 [0.50]	0.39 [0.49]	0.50 [0.50]	0	1	533
Political employment	Indicator for reporting a political position as a main occupation	0.004 [0.06]	0.001 [0.04]	0.007 [0.08]	0	1	741
Community mobilizer	Indicator for currently being a "community mobilizer"	0.039 [0.19]	0.023 [0.15]	0.061 [0.24]	0	1	741
Any community group member	Indicator for being a member in any one of the above seven community groups	0.42 [0.49]	0.41 [0.49]	0.44 [0.50]	0	1	741
Peace group member	Indicator for currently being a member of a peace group	0.07 [0.25]	0.05 [0.22]	0.08 [0.28]	0	1	741
Water committee or group member	Indicator for currently being a member of a water committee	0.013 [0.11]	0.017 [0.13]	0.007 [0.08]	0	1	741
Cultural group member	Indicator for currently being a member of a drama, music, or dance performance group	0.16 [0.37]	0.16 [0.37]	0.15 [0.36]	0	1	741
Sporting group or team member	Indicator for currently being a member of a sports team or group	0.12 [0.32]	0.13 [0.34]	0.09 [0.29]	0	1	741
Farmer's cooperative member	Indicator for currently being a member of a farmer's cooperative.	0.09 [0.29]	0.09 [0.28]	0.10 [0.30]	0	1	741
School club or committee member	Indicator for currently a school prefect or member of a school committee/club	0.05 [0.22]	0.04 [0.21]	0.06 [0.23]	0	1	741
Church or bible group member	Indicator for being a member of a church or bible study group	0.18 [0.38]	0.18 [0.38]	0.18 [0.38]	0	1	741
Attends church	Indicator for attending church "often"	0.81 [0.39]	0.81 [0.39]	0.81 [0.39]	0	1	741
NGO volunteer	Indicator for currently being a volunteer for a non-governmental organization	0.04 [0.20]	0.03 [0.18]	0.06 [0.23]	0	1	741
Disobeys elders	Indicator for disobeying parents, teachers or elders "sometimes" or "often"	0.07 [0.26]	0.07 [0.25]	0.08 [0.27]	0	1	741

*Note:* Sample means weighted by inverse sampling and inverse attrition probabilities

**Table 3: Determinants of LRA abduction and recruitment into government militias**

Pre-treatment Covariate	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Abducted versus non-abducted youth				Militia versus non-militia members			
	Unconditional mean		Difference in means <sup>‡</sup>		Unconditional mean		Difference in means <sup>‡</sup>	
	Abd	Non-Abd	Unconditional	Conditional	Abd	Non-Abd	Unconditional	Conditional
Year of birth <sup>†</sup>	21.54 [0.44]	20.47 [0.29]	1.08 [0.44]**	1.44 [0.61]**	22.94 0.72	19.54 0.41	3.39 [0.83]***	2.67 [0.69]***
Indicator for father a farmer <sup>†</sup>	0.90 [0.01]	0.90 [0.03]	0.01 [0.02]	-0.03 [0.03]	0.96 0.03	0.89 0.03	0.07 [0.04]*	0.07 [0.04]*
Household size in 1996 <sup>†</sup>	8.48 [0.33]	8.81 [0.55]	-0.33 [0.41]	-1.15 [0.33]***	9.42 0.83	8.37 0.61	1.05 0.98	1.25 [0.68]*
Landholdings in 1996 <sup>†</sup>	26.78 [1.48]	26.36 [2.44]	0.42 [2.10]	1.00 [2.41]	15.28 3.02	22.35 1.55	-7.07 [3.02]**	-4.55 [2.94]
Top 10% of Landholdings <sup>†</sup>	0.16 [0.02]	0.16 [0.04]	0.00 [0.03]	0.01 [0.02]	0.03 0.02	0.11 0.02	-0.08 [0.03]***	-0.07 [0.03]**
Cattle in 1996 <sup>†</sup>	17.73 [7.68]	12.66 [4.89]	5.07 [4.12]	5.95 [7.44]	3.29 1.96	14.03 7.16	-10.73 7.13	-6.45 [2.41]**
Other livestock in 1996 <sup>†</sup>	14.18 [2.11]	13.23 [3.09]	0.94 [2.72]	1.17 [0.98]	6.23 1.83	11.42 2.52	-5.20 [2.45]**	-4.22 [2.26]*
Owned plow in 1996 <sup>†</sup>	0.23 [0.03]	0.19 [0.04]	0.04 [0.04]	0.02 [0.05]	0.09 0.04	0.19 0.04	-0.11 [0.06]*	-0.13 [0.06]**
Uneducated father	0.12 [0.01]	0.13 [0.02]	-0.02 [0.02]	0.01 [0.01]	0.07 0.04	0.13 0.01	-0.05 0.04	-0.11 [0.03]***
Father's years of schooling	6.11 [0.19]	5.73 [0.27]	0.38 [0.34]	0.22 [0.25]	6.03 0.48	5.89 0.18	0.15 0.50	0.33 [0.47]
Uneducated mother	0.53 [0.04]	0.55 [0.02]	-0.01 [0.04]	-0.02 [0.04]	0.66 0.11	0.53 0.02	0.13 0.10	0.12 [0.10]
Mother's years of schooling	2.32 [0.23]	2.42 [0.16]	-0.09 [0.28]	-0.10 [0.28]	1.95 0.66	2.40 0.13	-0.45 0.64	-0.32 [0.66]
Paternal death before 1996	0.34 [0.03]	0.33 [0.02]	0.00 [0.04]	0.01 [0.04]	0.42 0.09	0.33 0.02	0.09 0.10	0.10 [0.09]
Maternal death before 1996	0.13 [0.02]	0.12 [0.02]	0.01 [0.03]	0.02 [0.03]	0.06 0.05	0.13 0.01	-0.07 0.05	-0.02 [0.04]
Orphaned before 1996	0.07 [0.01]	0.08 [0.02]	0.00 [0.02]	-0.02 [0.02]	0.02 0.02	0.08 0.02	-0.05 [0.03]*	-0.01 [0.03]

**Notes:**

Robust standard errors in brackets, clustered by location

All estimates weighted by inverse sampling probabilities and inverse attrition probabilities

\* significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%

† Mean differences include data from unfound and non-surviving youth, and omit inverse attrition weights.

‡ The unconditional difference is a simple difference in means, while the conditional difference is the coefficient on abduction from a weighted least squares regression of the covariate on abduction and all other pre-war covariates (weighted by inverse sampling and attrition probabilities).

**Table 4: Impact of abduction on social and political participation**

	(1)	(2)	(3)
<b>Dependent Variable</b>	<b>Participation of Non-Abducted Youth</b>	<b>Marginal Impact of Abduction (ATE)<sup>†</sup></b>	<b>% Change</b>
Voted in 2005	0.39	0.085 [0.028]***	22%
Political employment	0.001	0.012 [0.007]*	833%
Community mobilizer	0.02	0.033 [0.011]***	145%
Any community group member	0.41	-0.007 [0.040]	-2%
Peace group member	0.05	0.038 [0.016]**	73%
Water committee/group member	0.02	-0.012 [0.007]	-70%
Cultural group member	0.16	-0.028 [0.030]	-17%
Sporting group/team member	0.13	-0.029 [0.022]	-21%
Farmer's cooperative member	0.09	0.007 [0.017]	8%
School club/committee member	0.04	0.019 [0.016]	43%
Church or bible study group member	0.18	0.017 [0.029]	10%
Attends church	0.81	0.01 [0.029]	1%
NGO volunteer	0.03	0.006 [0.013]	18%
Disobeys elders	0.07	0.036 [0.024]	54%

**Notes:**

Each item in Column 2 is the product of a separate regression

All variables defined and described in Table 2

Robust standard errors in brackets, clustered by primary sampling unit (location and abduction status)

† The ATE is calculated as the coefficient on an abduction dummy variable in a weighted probit regression of the dependent variable on the abduction dummy, age (including the square and cube), location dummy variables, and pre-war household traits. The regression is weighted on inverse selection, sampling, and attrition probabilities

\* significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%

**Table 5: Robustness of abduction impacts to alternative regression models**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent variable	Abduction ATE (from Table 4)	Alternative estimation models						Difference of means
		Alternative controls	Alternative weights	Alternative controls & weights				
Voted in 2005	0.085 [0.028]***	0.106 [0.035]***	0.113 [0.064]*	0.108 [0.031]***	0.102 [0.027]***	0.096 [0.033]***	0.075 [0.044]*	0.115 [0.055]**
Community mobilizer	0.033 [0.011]***	0.033 [0.011]***	0.038 [0.013]***	0.037 [0.012]***	0.034 [0.012]***	0.032 [0.012]***	0.045 [0.021]**	0.042 [0.017]**
Any community group member	-0.007 [0.040]	0.025 [0.037]	0.022 [0.057]	0.019 [0.036]	-0.008 [0.037]	-0.003 [0.039]	-0.006 [0.051]	0.009 [0.051]
Peace group member	0.038 [0.016]**	0.036 [0.017]**	0.03 [0.027]	0.04 [0.018]**	0.045 [0.017]**	0.038 [0.018]**	0.037 [0.023]	0.029 [0.022]
Cultural group member	-0.028 [0.030]	0.015 [0.035]	-0.007 [0.038]	0.006 [0.032]	0.008 [0.041]	0.016 [0.044]	-0.004 [0.041]	0.006 [0.044]
<b>Weights</b>								
Selection correction	×	×	×					
Attition correction	×	×	×	×				
<b>Controls included:</b>								
Year of birth dummies	×	×		×	×	×	×	
Location of birth dummies	×	×		×	×	×		
Household traits in 1996	×			×	×			

Each term is the coefficient on an abduction indicator from a weighted probit regression of the dependent variable on an abduction indicator and the listed controls.

All variables defined and described in Table 2

Robust standard errors in brackets, clustered by sampling unit (location and abduction status)

\* significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%

**Table 6: Determinants of violence experienced**

	(1)	(2)
	<b>Violent acts experienced</b>	
	<b>Simple linear regression coefficients</b>	<b>p-value of higher order regression</b>
Indicator for father a farmer	1.13 [0.511]**	0.055*
Household size in 1996	0.02 [0.046]	0.111
Landholdings in 1996	0.00 [0.005]	0.232
Cattle in 1996	0.01 [0.004]	0.280
Other livestock in 1996	0.00 [0.006]	0.381
Owned plow in 1996	-1.18 [0.544]**	.010***
Father's years of schooling	-0.07 [0.045]	0.378
Mother's years of schooling	0.03 [0.060]	0.020**
Paternal death before 1996	0.21 [0.463]	0.828
Maternal death before 1996	-0.04 [0.500]	0.508
Year of birth dummies	not displayed	0.004***
Location of birth dummies	not displayed	0.989
Observations	738	738
R-squared	0.08	0.19
Joint test of significance (p-value)		
All 1996 household traits	0.3857	0.000***
All variables	0.019**	0.195

**Notes:**

Robust standard errors in brackets, clustered by location

All estimates weighted by inverse sampling probabilities and inverse attrition probabilities

\* significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%



**Table 7: Impact of war experiences on social and political participation (former abductees only)**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
	Voted in 2005	Community mobilizer	Any community group member	Peace group member	Water committee/group member	Cultural group member	Sports group/team member	Farmer's group member	School group member	Church or bible study group member	Attends church	NGO volunteer	Disobeys elders
Violent acts received	0.029 [0.012]**	0.012 [0.004]**	0.020 [0.012]	0.001 [0.008]	-0.016 [0.018]	0.013 [0.008]	-0.015 [0.006]**	0.024 [0.006]**	0.011 [0.006]*	0.015 [0.008]*	-0.021 [0.012]*	0.002 [0.005]	0.008 [0.005]
Violent acts perpetrated	-0.005 [0.019]	-0.011 [0.007]	0.004 [0.017]	0.019 [0.009]**	0.015 [0.020]	0.001 [0.010]	0.013 [0.010]	0.007 [0.008]	0.007 [0.006]	0.013 [0.012]	0.030 [0.012]**	-0.013 [0.007]*	0.010 [0.008]
Violent acts upon family	0.017 [0.022]	0.020 [0.004]**	0.028 [0.035]	0.003 [0.011]	-0.089 [0.078]	0.021 [0.014]	0.014 [0.011]	-0.019 [0.012]	0.007 [0.009]	0.007 [0.019]	0.021 [0.018]	0.013 [0.007]*	0.005 [0.009]
ln(Months abducted)	-0.039 [0.025]	-0.008 [0.007]	-0.050 [0.017]**	-0.019 [0.010]*	0.008 [0.009]	-0.044 [0.011]**	0.007 [0.009]	-0.025 [0.010]**	-0.018 [0.007]**	-0.017 [0.016]	0.032 [0.012]**	0.008 [0.011]	-0.018 [0.010]*
Age abducted	0.016 [0.008]*	-0.006 [0.004]	-0.007 [0.011]	0.000 [0.003]	-0.002 [0.004]	0.000 [0.006]	-0.002 [0.003]	0.000 [0.006]	0.002 [0.005]	0.005 [0.011]	0.005 [0.005]	0.003 [0.004]	0.004 [0.005]
Carried own firearm	0.066 [0.065]	-0.002 [0.028]	0.139 [0.068]*	0.083 [0.048]*	-0.007 [0.035]	0.143 [0.039]**	0.016 [0.023]	0.022 [0.051]	-0.006 [0.039]	-0.005 [0.066]	-0.091 [0.059]	-0.002 [0.040]	0.110 [0.052]**
Leadership position	-0.203 [0.106]*	0.008 [0.048]	-0.133 [0.064]*	-0.036 [0.025]	n.a.	0.040 [0.071]	-0.069 [0.015]**	-0.036 [0.063]	n.a.	-0.113 [0.045]**	-0.180 [0.091]*	-0.012 [0.047]	-0.070 [0.027]**
Observations	344	459	459	459	459	459	459	459	459	459	459	459	459
Controls not displayed:													
Age (three orders)	x	x	x	x	x	x	x	x	x	x	x	x	x
Location of birth dummies	x	x	x	x	x	x	x	x	x	x	x	x	x
Household traits in 1996	x	x	x	x	x	x	x	x	x	x	x	x	x

Each column represents a separate regression

All variables defined and described in Tables 1 and 2

Robust standard errors in brackets, clustered by location

\* significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%

**Table 8: Impact of war violence on participation (all youth)**

	(1)	(2)	(3)	(4)	(5)	(6)
	<b>Voted in 2005</b>		<b>Community mobilizer</b>		<b>Any group member</b>	
Ever abducted	0.085 [0.028]***	0.040 [0.053]	0.033 [0.011]***	0.010 [0.015]	-0.007 [0.040]	-0.028 [0.069]
Violent acts received		0.018 [0.013]		0.007 [0.002]***		0.014 [0.010]
Abduction $\times$ Violent acts						
Months abducted		-0.004 [0.001]***		0.000 [0.001]		-0.005 [0.002]**
Observations	533	532	741	739	741	739
Joint significance of violence and abduction terms (p-value)		0.003***		0.001***	0.215	0.215
Controls included:						
Age (three orders)	×	×	×	×	×	×
Location of birth dummies	×	×	×	×	×	×
Household traits in 1996	×	×	×	×	×	×

All variables defined and described in Tables 1 and 2

Robust standard errors in brackets, clustered by location

\* significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%

**Table 9: Relative explanatory power of the correlates of participation**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	<b>Impact of Abduction (ATE)<sup>†</sup></b>	<b>Voted in 2005 referendum</b>		<b>Community mobilizer</b>		<b>Any group member</b>	
		<b>Correlation</b>	<b>% of ATE<sup>‡</sup></b>	<b>Correlation</b>	<b>% of ATE<sup>‡</sup></b>	<b>Correlation</b>	<b>% of ATE<sup>‡</sup></b>
Violent acts received	4.346 [0.219]***	0.015 [0.007]**	77%	0.011 [0.003]***	145%	0.002 [0.007]	-22%
Violent acts perpetrated	1.072 [0.117]***	-0.006 [0.020]	-8%	-0.015 [0.007]**	-49%	0.003 [0.012]	3%
Violent acts upon family	0.271 [0.085]***	-0.004 [0.017]	-1%	0.026 [0.007]***	21%	0.016 [0.021]	3%
Currently lives in town	0.013 [0.039]	0.018 [0.054]	0%	-0.037 [0.027]	-1%	-0.158 [0.044]***	6%
Currently lives outside home district	0.003 [0.027]	-0.144 [0.079]*	-1%	-0.015 [0.033]	0%	-0.188 [0.082]**	-14%
Serious injury	0.085 [0.022]***	-0.06 [0.047]	-6%	-0.074 [0.021]***	-19%	-0.05 [0.042]	-43%
Top quartile of emotional distress	0.104 [0.033]***	-0.02 [0.068]	-2%	0.017 [0.022]	5%	-0.054 [0.039]	-19%
Asset index	-0.084 [0.017]***	0.015 [0.140]	-1%	0.022 [0.058]	-6%	0.006 [0.099]	1%
Days employed in past four weeks	0.104 [0.572]	0.001 [0.003]	0%	0.001 [0.001]	0%	0.009 [0.002]***	0%
Gross cash earnings in past 4 weeks (USD)	-2.733 [2.010]	-0.001 [0.001]*	3%	0 [0.001]	0%	-0.001 [0.001]*	0%
Community group membership	0.008 [0.039]	0.021 [0.036]	0%	0.069 [0.024]***	2%		
Volunteers for an NGO	0.002 [0.012]	0.271 [0.070]***	1%	0.001 [0.036]	0%		0%
Attends church	0.009 [0.025]	0.141 [0.046]***	1%	0.01 [0.022]	0%		0%
Bottom quartile of pro-social behavior	0.082 [0.034]**	0.014 [0.066]	1%	0.037 [0.033]	9%	0.226 [0.066]***	-44%
Index of social support	-0.225 [0.176]	-0.004 [0.008]	1%	0.001 [0.004]	-1%	0.02 [0.011]*	-3%
Indicator for poor family relations	0.026 [0.021]	-0.03 [0.114]	-1%	0.091 [0.044]**	7%	0.028 [0.105]	4%
Indicator for functional literacy	-0.163 [0.033]***	0 [0.053]	0%	0.003 [0.028]	-1%	0.05 [0.054]	0%
Radio ownership	-0.057 [0.028]*	-0.027 [0.050]	2%	-0.011 [0.026]	2%	0.056 [0.059]	-14%
Educational attainment in years	-0.765 [0.140]***	-0.002 [0.007]	2%	-0.005 [0.003]	12%	0.014 [0.009]	-4%
Observations		531		738		738	
Additional controls (not displayed)							
Year of birth dummies	×	×		×		×	
Location of birth dummies	×	×		×		×	
Year and location of birth interactions	×	×		×		×	
Household traits in 1996	×	×		×		×	

Robust standard errors in brackets, clustered by sampling unit (location and abduction status)

\* significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%

Figures in bold represent correlates that exhibit a statistically significant ATE (at the 5 percent level) in Column 1 and a statistically

† Each item in Column 1 is a separate regression. Each ATE is calculated as the coefficient on an abduction indicator variable in an

‡ Calculated as the ATE in Column 1 multiplied by the coefficient in Column 2, divided by the relevant ATE in Table 4.

**Table 10: Best case and worst case attrition bounds on treatment effects**

Dependent variable	Percent missing <sup>†</sup>		Treatment effect bounds		
	Non-Abd	Abd	Untrimmed ATE <sup>‡</sup>	"Best case" bound <sup>§</sup>	"Worst case" bound <sup>§</sup>
Voted in 2005	33%	35%	0.098 [0.047]**	0.122 [0.058]**	0.084 [0.055]
Community mobilizer	28%	30%	0.042 [0.017]**	0.067 [0.044]	0.042 [0.0167]**
NGO volunteer	28%	30%	0.011 [0.016]	0.038 [0.043]	0.010 [0.017]
Attends church	28%	30%	-0.018 [0.030]	-0.013 [0.031]	-0.037 [0.046]
Community group member	28%	30%	0.009 [0.038]	0.024 [0.045]	-0.002 [0.043]
Community group memberships	28%	30%	0.007 [0.078]	0.086 [0.121]	-0.011 [0.085]
School club member	28%	30%	0.014 [0.018]	0.038 [0.044]	0.013 [0.018]
Water committee member	28%	30%	-0.014 [0.010]	0.010 [0.005]**	-0.015 [0.011]
Disobeys elders	28%	30%	0.019 [0.019]	0.045 [0.044]	0.018 [0.020]
In fight	28%	30%	-0.004 [0.020]	0.022 [0.044]	-0.005 [0.020]

**Notes:**

Each row represents the results of the trimming procedure suggested by Lee (2005) to account for selective attrition and survival

Treatment is binary and equals 1 if ever abducted and 0 otherwise

Standard errors in brackets, but are not clustered or heteroskedastic-robust

All estimates are weighted by inverse sampling probabilities and inverse propensity scores

\* significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%

† Missing youth include attriters and non-survivors. 31% of non-abducted youth and 30% of abducted youth are missing. Data collected from families on the education, employment status, and major injuries of migrant youth reduce these missing percentages to 14% and 23%. In the case of wages, additional observations are missing due to unemployed youth.

‡ The untrimmed ATE is the difference in the means of the abducted and non-abducted groups, and is not a regression estimate. No control variables are used. The means are analogous to the WLS estimates in Column 4 of Table 4.

§ Best and worst-case bounds are calculated as the difference in the means of the abducted and non-abducted groups after 'trimming' the top or the bottom of the distribution of the outcome variable in the treatment group with less attrition. They are not