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The Consequences of Child Soldiering

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Abstract: Civil conflicts have afflicted a third of all nations and two thirds of Africa since 1991. In many cases, up to a third of male youth (including children) are drawn into armed groups, making soldiering one of the world's most common occupations for the young. Little is known, however, about the impacts of military service on human capital and labor market outcomes due to an absence of data as well as sample selection: recruits are usually self-selected and screened, and may also selectively survive. We assess the impacts of participation in civil war using an original survey from Uganda, where a rebel group's recruitment method provides arguably exogenous variation in conscription. Contrary to the prevailing view that participation in war leads to broad-based 'traumatization', we find that military service primarily hinders long-term economic performance because it is a poor substitute for civilian education and work experience. The most significant impact is upon a recruit's skills and productivity: schooling falls by nearly a year, skilled employment halves, and earnings drop by a third. These impacts are highly robust to relaxation of the assumption of exogenous conscription. Effects are greatest for child soldiers, who lose the most education. There is no observed impact on social capital, and adverse impacts on mental health, while evident, are present in a relative minority.

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I. Introduction

Civil conflict has afflicted a third of all nations and two thirds of Africa since 1991, generating millions of ex-combatants (Marshall & Gurr, 2005). Some of these conflicts involve up to a third of male youth in active combat, many of whom are children. Thus soldiering may be one of the world's most common professions for youth as well as one of the worst forms of child labor.

This purpose of this paper is to understand the impact of war on human capital, the consequences for long-term labor market performance, and by extension the economic recovery and long-term growth of civil war-afflicted country. This concern is a critical one for post-conflict economic development, as adverse impacts on education can take years to recover and damage to health may not recover at all. With so many millions of young ex-combatants, such damage to human capital could therefore hinder their nations' productivity and growth for decades. Moreover, any impact of military service on inequality, aggression and political alienation could threaten a nation's long term stability and growth.[1]

Unfortunately, little is known about the long term effects of participation in armed groups. The dominant view holds that young ex-combatants are traumatized, socially excluded, and violent. The French foreign minister, a keynote speaker at a 2007 child soldiering conference, warned that child soldiers are "a time bomb that threatens stability and growth." They are "lost children," he argued, "lost for peace and lost for the development of their countries" (BBC, 2007). Former child soldiers "are walking ghosts," mourns a recent *New York Times* editorial, "damaged, uneducated pariahs" (NYT, 2006). A growing body of ethnographic and psychological evidence portrays another view, however, finding that resilience rather than traumatization is the norm (Boothby et al., 2006; Shepler, 2005; Wessells, 2006). Yet virtually no representative data or well-identified causal estimates exist to judge either set of claims.

Meanwhile, the micro-level economic consequences of participation in civil war remain unexplored.[2] A small literature has found relatively large and persistent earnings and mortality gaps between veterans and non-veterans in the U.S. and Europe (Angrist, 1990, 1998; Angrist & Krueger, 1994; Hearst et al., 1986; Imbens & van der Klaauw, 1995). Among these, Angrist (1990) finds that white males conscripted

[1] Rational choice theories of conflict suggest that poverty increases the conflict as individuals have more to gain from soldiering when peacetime economic opportunities are poor (Grossman, 2002; Sambanis, 2004; Walter, 2004). Others argue that inequality (such as that between ex- and non-combatants) leads to greater discontent and, ultimately, upheaval (Gurr, 1971).

[2] The literature has tended to focus on the macroeconomic and demographic impacts of conflict. A handful of economists have examined the micro-level impacts of conflict on non-combatants, however. Bellows and Miguel (2006) find that household deaths and displacement lead to greater political participation among Sierra Leonean civilians. Furthermore, Shemyakina (2006) finds an adverse educational impact of conflict on Afghani youth.

into the Vietnam War saw a 15 percent reduction in their long-term earnings due to work experience lost. These studies draw on large datasets with a limited range of labor market outcomes, however, so the nature of the impact and the causal channel are difficult to identify. It is also unclear whether these studies generalize to combatants in developing-country civil wars. The sole survey-based study of the impact of military service in civil war comes from a survey of Sierra Leonean ex-combatants by Humphreys and Weinstein (2004; 2005), who investigate differences in the reintegration success of former fighters. They find little relationship between participation in the national reintegration program and post-war employment, and that an exogenous increase in exposure to violence leads to lower levels of post-war community acceptance but bore little relationship with employability. While such data and findings are important and pioneering, without a non-combatant comparison group our understanding of the impact of military service remains incomplete.

One reason we currently know so little about these impacts is the paucity of data in war zones. To overcome this problem, we conducted a survey of young males during an armed conflict in northern Uganda. Over the past two decades an unpopular rebel group has forcibly recruited tens of thousands of youth to serve in their insurgency. We identified a representative sample of 1100 households and 1,219 young males living in the region before escalation of the war, whereupon a team of local assistants tracked and surveyed 93 percent of the households and 84 percent of their surviving men and boys.

Another challenge in identifying the causal effects of military service is sample selection: ex-fighters are usually a selected group, including those who chose to join and those screened by the armed force. The "ideal" research design would be one where rebel participation was either randomly or exogenously assigned. Evidence presented in this paper suggests that rebel recruitment in northern Uganda resembles just such a terrible case. The survey identifies rural areas where recruitment was large-scale, forcible, and seemingly indiscriminate. With no popular support, small groups of rebels raided homesteads for supplies and recruits, abducting all young males they encountered. Testimony suggests that there was no self-selection into the rebel group and little systematic selection by the group itself, other than by age. The data support such accounts. The survey measures pre-war traits that are generally thought to predict selection into armed groups, and that strongly predict selection into the national military. Levels of these traits are identical across 'abductees' and 'non-abductees', however, and they are individually and jointly insignificant in predicting abduction. Under the assumption that rebel abduction is unconfounded—or independent of outcomes after controlling for pre-abduction traits—average treatment effects can be estimated using non-combatants of the same age and location as counterfactuals for the abducted youth.

The results suggest that the largest and most pervasive impact of abduction is upon education and earnings, largely due to time away from civilian schooling and work experience. The average length of abduction and the drop in educational attainment are almost exactly equal at nine months. This educational deficit impedes labor market success: while formerly abducted youth are just as likely to be employed, they are half as likely to be engaged in skilled work and earn a third lower wages.

The data also support the growing body of ethnographic and psychological evidence that finds most child soldiers and their communities to be resilient. Community acceptance of former abductees is high, and abductees exhibit little difference in aggression and social support. Few "time bombs" and "lost children" are in evidence. Both abductees and non-abductees report relatively few symptoms of emotional distress on average, with abductees reporting only a very slightly higher number. Only five percent of the youth report more than eight of the nineteen potential symptoms of distress, and these youth are disproportionately abductees—in general those that experienced the most extreme violence.

A central concern with such estimates is whether the assumption of unconfoundedness is realistic, as unobserved sources of selection and survival may bias the results. To explore the sensitivity of the causal effects to selection bias, the analysis explicitly relaxes unconfoundedness by allowing for a limited amount of correlation between abduction and unobserved components of the outcomes, using a method proposed by Imbens (2003). Furthermore, the treatment effects are bounded for possibly selective survival using a method proposed by Lee (2005). Both sensitivity analyses imply that moderate amounts of selection and attrition cannot account for more than a fraction of the estimated causal effects.

Another concern is whether our counterfactual—non-abducted youth also in the conflict zone—allows us to estimate the true impact of military service. We do not identify the general impact of war on non-combatants, which is a different 'treatment' altogether. Our approach does identify the incremental impact of military service on youth already in the war zone, which is the relevant policy variable for addressing post-war gaps in reintegration (and which evidence from outside the conflict zone suggests may be more substantial than any impact on non-combatants). We will only misestimate the impact of abduction to the extent that there are externalities for non-abducted youth, a possibility which we discuss.

The evidence from Uganda, however, suggests that organizations concerned with post-conflict recovery ought to be looking through a human capital lens at least as often as a psychological and social (or 'psychosocial') one. The evidence argues for a much more targeted approach to psychosocial care aimed at the most distressed youth, and a larger and broad-based set of economic and educational programs. Such findings are likely to be relevant beyond rebel recruits in Uganda. The results of this study are strikingly similar to that found among a diverse set of war affected populations, from ex-fighters in Sierra Leona, to U.S. veterans, to European and Asian refugee populations.

II. Background

A. War and abduction in northern Uganda

In 1988, a spiritual leader named Joseph Kony assembled the remnants of several failed insurgent groups in northern Uganda into a new guerrilla force, the Lord's Resistance Army, or LRA. The movement is rooted in a longstanding political grievance. Economic power historically rested in the south of Uganda and political and military power in the north (Omara-Otunnu, 1994). In 1986, however, rebels from the south overthrew a government and army dominated by a northern ethnic group, the Acholi. Several Acholi guerrilla forces initially resisted the takeover, but for the most part settled for peace or were defeated by 1988. A handful of these fighters refused to settle, however, and joined forces with Kony (also an Acholi) to continue the fight (Allen, 2005; Doom & Vlassenroot, 1999). Like many African armed groups, the LRA also has a strong spiritual element and cause. Kony is widely believed to possess great spiritual powers, and he and the LRA claim to seek a spiritual cleansing of the nation.

The decision to continue fighting was an unpopular one, however, and the LRA commanded little public support. With little popularity and virtually no material resources, the LRA immediately took to looting homes and abducting youth to maintain supplies and recruits. The Acholi populace, after three years of such abductions and looting, began to organize a defense militia in 1990. To punish them for this betrayal, and to dissuade them from further collaboration with the government, in 1991 Kony ordered the widespread killing and mutilation of Acholi civilians (Behrend, 1999; Branch, 2005).

LRA activity from 1988 to 1994 was fairly low-scale. In 1994 and 1995, however, the neighboring government of Sudan began supplying Kony with weapons and territory upon which to build bases—a direct response to the Ugandan government's support for a rebel group in southern Sudan. This support enlarged and invigorated the LRA, and rebel attacks and abductions escalated dramatically after 1996.

Abduction was large-scale and seemingly indiscriminate. Annan et al. (2006) estimate that more than 66,000 youth have been abducted, most since 1996 and from one of the three Acholi districts: Gulu, Kitgum, and Pader (Figure 1). More than one quarter of males currently aged 14 to 30 in our sample were abducted for more than two weeks (Table 1). Youth were typically taken by small roving groups of rebels conducting night raids on rural homesteads. There are almost no accounts of youth voluntarily joining the LRA. Adolescent males appear to have been the most pliable, reliable and effective forced recruits, and so were disproportionately targeted by the LRA (Blattman & Gates, 2007). Youth under 10 and over 24 tended to be avoided, as seen in Figure 2.

Lengths of abduction ranged from a day to ten years, with an average length of eight months in our sample (Table 1). The vast majority of those taken were tied, beaten, and forced to carry loot and equipment. Youth who failed to escape were trained as fighters and, after a few months, received a gun. Roughly a quarter of abductees eventually became fighters. Many of these were forced to beat or murder civilians—perhaps even their own families—to bind them to the group, to reduce their fear of killing, and to demonstrate the consequences of escape. Eighty percent of abductees eventually escape, almost always during an unsupervised moment, such as in the heat of battle. Of those that have not returned, we estimate that between 4 and 19 percent (or 1 to 4 percent of all abductees) likely remain with the armed group, and that the remainder have not survived.[3] A blanket Amnesty has been granted to all "returnees" and self-reported acceptance rates back into the community are high.

B. Current practice and evidence

The war and the widespread abduction of children have resulted in a massive influx of aid agencies striving to assist the thousands of youth returning from the bush. Many resources have been concentrated on psychosocial trauma among former child soldiers. This focus seems to be driven by abundant anecdotal evidence of traumatization. For instance, one youth interviewed is still haunted by being forced to kill his brother and witness his sister's death: "I started dreaming of [my brother] a week after

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[3] Reports from the media and the military on the number of abductees still in the bush range widely, from 500 to 2500. Using the estimate of 66,000 total abductions (Annan et al., 2006), this suggests that 0.8 to 3.8 percent of abductees remain with the armed group, or 4 to 19 percent of those that did not return from abduction. There are virtually no reports of returnees not returning to their home communities, and it is safely assumed that all who did not return either remain there or have perished.

the incident, and at times I would see him during the day. How I beat him would all re-surface." Re-experiencing the event frequently through nightmares or flashbacks is symptomatic of the most serious forms of distress, as are headaches and chest pains (Annan, 2007).

These anecdotal conclusions of traumatization are bolstered by survey evidence gathered by NGOs. One study that found that abducted youth were "more anxious and depressed, more hostile, less pro-socially active, and less confident" than the non-abducted, although the specific difference was not quantified (MacMullin & Loughry, 2002, p. 9). A second study identified clinical post-traumatic stress in 97 percent of abductees (Derluyn et al., 2004). Such studies raise serious data and identification concerns, however. Both use non-random convenience samples, drawn from youth being assisted by NGOs or town youth called by radio. There are no efforts to examine selection effects; indeed the second study did not employ a comparison group, implicitly using zero symptoms of distress as the counterfactual.

The traumatization paradigm has nevertheless become pervasive in discussions of humanitarian policy. A focus on psychosocial assistance has thus become common to youth post-conflict programs worldwide (e.g. Cohn & Goodwin-Gill, 1994; CSUCS, 2005; ILO, 2003; Machel, 1996; Wessells, 2006).

In general, however, evidence for the traumatization view is limited. Wessels (2006), in a review of the psychological literature on war and child soldiering, suggests that this concentration of trauma and distress in a minority—perhaps 10 to 20 percent—is common to both civilians and combatants in many war-affected populations. Indeed, a small psychological literature emphasizes the resiliency of youth to extreme stressors (e.g. Masten, 2001). It is difficult to draw any conclusions with confidence, however. Few studies pay attention to potential sources of selection bias and endogeneity, and in most the data and analysis are of questionable value. Hollifield et al. (2002), in a review of 394 published psychological studies of war trauma, conclude that the majority "are either descriptive or include quantitative data from instruments that have limited or untested validity and reliability."

Our interviews with former child soldiers provide little support for the trauma paradigm, however. Forty of the surveyed youth were selected for in-depth assessments by a counseling psychologist, including multiple interviews of the youth as well as his family, friends, teachers, and co-workers. As discussed below, these interviews suggest that the typical psychological consequences of abduction are modest, that aggression is low, and that social reintegration is strong. More extreme symptoms of distress appeared to be concentrated in a minority, and while these were concentrated among abductees, they were not limited to this group.

Rather, in the minds of many of the youth and their families it is the interruption of education and employment that of greatest concern. Those coming back from longer abductions also often feel uncomfortable returning to school with youth much younger than them. For instance, a primary school teacher explained that some youth "stayed for a long time in the bush, and when they came back to school, they found themselves older than the others in class." Such students, he continued, "take long to adjust".

This education gap appears to have had serious labor market effects. According to an elder, "the youth who have not been abducted are engaged in different activities like business and vocational work like carpentry, because they had the opportunity to acquire the different skills." Small entrepreneurial activities are the main source of income in northern Uganda. Some require little capital or skill (e.g. collecting firewood), while others require a little capital (e.g. hawking clothes), moderate capital and skill (e.g. a bicycle taxi), or substantial capital and skills (e.g. tailoring). This labor market is highly dynamic, and as youth accumulate skills and capital they move to higher productivity occupations. For example, one youth explained that he began making charcoal from discarded wood. As he accumulated profits, he purchased a bicycle and began a taxi service. With his profits he educated himself in a trade, and later opened a small store. Overall, the interview evidence suggests that abduction interrupts this slow accumulation of skills and capital, trapping youth in low-productivity employment—a pattern strikingly similar to that identified by Angrist (1990; 1998) among U.S. Vietnam veterans.

III. Data and Measurement

In 2005 and 2006 we conducted Phase 1 of the Survey of War Affected Youth (SWAY)—an original, representative survey of 741 youth in northern Uganda, 462 of whom were once with the LRA. The population of interest is male youth currently aged 14 to 30 that were born in rural areas of the Districts of Kitgum and Pader. Surveys were administered by local enumerators in eight sub-counties, or clusters.[4]

[4] A sub-county is an administrative unit that typically encompasses 25 to 100 villages and ranges in population from roughly 10,000 to 40,000 in total. The eight sub-counties studied represent roughly 10 percent of the rural population of Kitgum and Pader. These clusters were not randomly selected as insecurity limited the team to sub-counties that (i) could be reached in less than a 90-minute drive from Kitgum town, and (ii) were visited at least bi-weekly by a partner NGOs and their military escorts. Effort was taken to select sub-counties of varying sizes, and that seemed generally representative of the region as a whole.

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 any migration and mortality. We randomly sampled 1100 households from United Nations World Food Programme (WFP) lists compiled in 2002, and 93 percent of household heads were found and interviewed. Of the 7 percent of households not located, community leaders estimate that one third to one half are "ghost" households—false names placed on the WFP lists that allow a household to collect double rations. The remainder appear to have relocated since 2002. If we assume that a similar number relocated between 1996 and 2001 (and do not appear on the WFP lists), then at least 91 percent of households present in 1996 were interviewed.

Enumerators then worked with household heads to develop a retrospective roster of all youth living in the household in 1996. We chose the year 1996 because it was easily and commonly recalled as the date of the first election since 1980, and because it pre-dates 85 percent of local abductions. A sample of 870 surviving male youth was drawn from this retrospective roster. Ex-combatants were over-sampled.

Of surviving males, 41 percent had moved since 1996, and enumerators attempted to track all migrants. 741 (or 84 percent) of the 870 sampled males were located, including all non-migrants and 70 percent of migrants. We conducted an absentee questionnaire with the families of all 129 unfound youth, collecting extensive data on outcomes and abduction experiences. Basic demographic data were also collected on the 349 youth from the retrospective rosters that had died or not returned from abduction.

The 741 youth that completed the survey provided data on their war experiences as well as current well-being and outcomes. Key variables are described in Table 1. Two aspects of these data are noteworthy. First, war experiences are self-reported and retrospective.[5] Second, the measures of violence, social support, hostility and distress are additive indices of survey questions commonly used for measuring trauma and psychosocial well-being in conflict zones. The questions are adapted from the Northern Uganda Child and Youth Psychosocial Adjustment Scales (MacMullin & Loughry, 2002).[6]

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[5] Some youth have been known to misrepresent themselves as abductees in order to receive aid. We took several measures to guard against such misrepresentation. In community meetings and the interview, the absence of a link between the study and aid was made clear. Moreover, abduction data were taken from the household head and irregularities followed up. Finally, the survey asked more than 200 detailed questions on any abduction, making misrepresentation difficult. Only five percent of abductees raised suspicion, and reclassifying these as "non-abducted" has no material impact on the findings in this paper.

[6] See Annan (2007) for a detailed description of index adaptation and construction. These indices can be constructed additively or through a data reduction technique such as factor analysis. To avoid the cherry-picking of indices, this paper employed a rule of thumb for all index variables that is common in the psychological literature (see Fabrigar et al., 1999). Specifically, questions were selected for inclusion in an index if they were determined to be originating from the same underlying factor, where such common underlying correlation was identified using factor analysis. All questions with factor loadings over 0.3 were included in the index additively. The results reported in this paper are robust to all other methods of index construction.

IV. Empirical Methodology

A. Dealing with endogenous selection into the armed group

The fundamental empirical problem we face is that we cannot observe the outcomes for an ex-combatant had he not received "treatment"—in this instance, abduction. The standard solution to this problem is the counterfactual approach, whereby a relevant control group is found and the 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, of course, and it will be unbiased only when treatment assignment and the potential outcomes are independent.

When comparing ex-combatants to non-combatants we are most concerned that any differences in current outcomes are the result of pre-war characteristics that led to selection into the armed group. To deal with such potential endogeneity, we can look for situations where treatment is conditionally unconfounded—that is, where participation in the armed group is independent of potential outcomes conditional on a set of observed pre-treatment variables (Imbens, 2003; Rosenbaum & Rubin, 1983; Rubin, 1978). The evidence below suggests that LRA abduction presents just such an unlikely case.

Interviews with LRA leaders suggest that the most common types of selection are not present in this case. First, self-selection into the armed group was non-existent in the eight areas surveyed. The LRA's murder and mutilation of civilians in 1991 destroyed what little support they ever enjoyed, and by the early 1990s—the time that Kony's forces expanded operations into the surveyed areas—abduction had become the sole means of acquiring recruits. During field work it proved nearly impossible to find youth who voluntarily joined after 1991, even with the help of community leaders and former rebel leaders.

Second, our interviews with the leaders of these raiding parties suggest that by neither design nor accident did they abduct a select group of youth. From their Sudanese bases, rebels ventured into Uganda for weeks at a time in groups of 15 or 20 fighters. Typical of East Africa, rural Acholi households live in relatively isolated homesteads in their fields rather than villages—arrangements which made them particularly vulnerable to the LRA raiding parties. These parties had two aims: ambushing government forces, and raiding homesteads along their path for food and new recruits. Abductions were large-scale, with thousands of youth taken every year. Rebels usually invaded 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. According to rebel leaders, raiding targets were unplanned and arbitrary. Abduction party leaders claim to have raided whatever homesteads they encountered, regardless of wealth, location, and household composition.

The survey data support these remarkable claims. We gathered retrospective data on pre-war levels of household wealth (land, livestock, and plows), parent's education, father's occupation, and parental death—all measures which have been found to strongly predict participation in armed groups throughout Africa (Cohn & Goodwin-Gill, 1994; Honwana, 2005; Humphreys & Weinstein, 2006). We observe little difference in these pre-war characteristics between abducted and non-abducted youth. The means of each of these pre-war traits for abducted and non-abducted youth are listed in Columns 1 and 2 of Table 2, with unconditional and conditional mean differences calculated in Columns 3 and 4. None of the unconditional differences in means except year of birth are significant at even a 10 percent level, and nearly all differences are close to zero. Conditional mean differences, which control for all other pre-treatment covariates, are generally insignificant as well.

Abducted and non-abducted youth only appear to differ in mean year of birth and mean pre-war household size. This relationship between year of birth (i.e. age) and abduction is expected, as a youth's probability of ever being abducted depended on how many years of the conflict he fell within the LRA's target age range. Moreover, abduction levels varied over the course of the war, so youth of some ages were more vulnerable to abduction than others. Meanwhile the significance of household size is driven entirely by households greater than 25 in number, and perhaps implies that rebel raiders, who traveled in small bands, were less likely to raid such large households as they would be difficult to control

The distribution of predicted probabilities of abduction based on pre-treatment data provides further evidence of unconfoundedness, and is depicted in Figure 3. The probabilities are predicted from a logit regression of abduction on indicator variables for year and location (sub-county) of birth and all pre-war household covariates. The overlap in these predicted probabilities between abductees (the right-hand panel) and non-abductees (the left-hand panel) is substantial. Moreover, any difference in the distributions of abducted versus non-abducted is driven almost exclusively by the respondent's year and location of birth. The addition of other pre-war covariates leaves the distribution of the predicted probabilities almost undisturbed. An F-test of their joint significance yields a (not significant) p-value of 0.18.

Abduction by the LRA can be contrasted to participation in Local Defense Units, or LDU—a voluntary militia under the command of the Ugandan army. Five percent of interviewed youth were current or past LDU members. A comparison of pre-war traits in Table 3, Columns 1 to 4, suggests that militia members come from poorer and more agricultural households. Collectively these covariates have strong predictive power for militia membership. Unlike the case of LRA abduction, a test of the joint significance of all household characteristics in predicting LDU membership yields a highly significant p-value of 0.02. Moreover, even when not statistically significant, the coefficients in the militia regressions are generally more influential. The ability of these pre-war traits to significantly predict militia participation but not abduction is striking, and lends support to the assumption of unconfounded abduction.

B. Dealing with selective attrition and survival

In this study, there are three main types of 'attritors': 12 percent of our target population are unfound migrants, 12 to 15 percent are presumed perished, and 1 to 4 percent are believed to remain with the armed group. This attrition varies by treatment status: abductees comprise all of those with the armed group, are roughly twice as likely to have perished, but are half as likely to be unfound migrants. 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 (Falaris, 2003; Fitzgerald et al., 1998). The tracking success rate of this study meets or exceeds the rates achieved by several 'gold-standard' youth tracking surveys in poor countries (Hamory & Miguel, 2006; Thomas et al., 2001). Even so, we worry that attrition due to war deaths and non-return might be particularly selective.

A partial solution is to examine the attrition on observables, and to correct for observable attrition by re-weighting the sample (Fitzgerald et al., 1998). To do so, enumerators interviewed surviving family members to obtain demographic data on non-surviving youth plus data on current outcomes for any unfound migrants.[7] 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, π_i, to eliminate bias from attrition on observed traits.[8]

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[7] Observed traits used to calculate attrition probabilities for *all* attritors are year and location of birth as well as pre-war household size, father's occupation, and asset holdings. Additional traits collected for unfound migrants include: current activity (employed, in school, in the military, or unemployed); occupational type; educational attainment; literacy; location characteristics (displaced or living outside the home District); and relation to the family member providing the data.

[8] Letting A_i be an indictor variable that equals one for attritors and zero otherwise, and W_i a vector of observed covariates that influence both the outcomes and also the likelihood of attrition, the weights are calculated as:

Even with this correction, however, there remains a risk of bias arising from any unobserved traits that influence survival, abduction, and potential outcomes. Below we discuss the likely direction of any bias and estimate best- and worst-case bounds on the ATE to assess their robustness to potential bias.

C. Estimation

Under the assumption of conditional unconfoundedness, consistent ordinary least squares (OLS) estimates of the ATE can be calculated. A more efficient and consistent approach, however, is a weighted least squares (WLS) regression with weighting on the inverse of a nonparametric estimate of the propensity score (Hirano et al., 2003). Under WLS the regression function for outcome Y is:

$$Y_i = \beta_0 + \tau \bullet T_i + X^S{}_i \bullet \beta_1 + \varepsilon_i, \tag{1}$$

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. The weights used are:

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

ϱ_i and π_i are sampling and attrition weights, and $\hat{e}(v_i)$ is a nonparametric estimate of the propensity score.[9]

One can alternatively estimate the ATE non-parametrically using a matching estimator weighted by the inverse selection probabilities (Abadie & Imbens, 2006). This weighted matching estimator is:

$$\hat{\tau}_M = \frac{1}{N} \sum_{i=1} \rho_i \cdot \pi_i \cdot \left(\frac{T_i \cdot \hat{Y}_{1i}}{\hat{e}(v_i)} - \frac{(1-T_i) \cdot \hat{Y}_{0i}}{1-\hat{e}(v_i)} \right), \tag{2}$$

where $\hat{Y}_{0i}$ equals Y_{0i} if $T_i = 0$, and equals a weighted average of the closest matches if $T_i = 1$; and, likewise, $\hat{Y}_{1i}$ equals Y_{1i} if $T_i = 1$, and equals a weighted average of the closest matches if $T_i = 0$.

V. The Impact of Abduction

Average treatment effects of abduction on ten outcomes are listed in Table 4. The key identifying assumption is that non-abducted youth in the same age group and sub-county are a valid counterfactual for

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$$\pi_i = \left[\frac{\Pr(A_i = 0 \mid T_i, W_i)}{\Pr(A_i = 0 \mid T_i)} \right]^{-1}$$

[9] 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).

surviving abducted youth. OLS estimates are listed in Column 1 and the more efficient semi-parametric WLS estimates in Column 3. Non-parametric matching estimates are more accurate when the distribution of the outcome is non-normal (such as wages), and these are displayed in Column 5. Each entry in Columns 1, 3 and 5 represents a separate regression. In the even-numbered columns we calculate the magnitude of the coefficient relative to the average outcome for non-abducted youth (from Table 1).[10]

A. Average treatment effects

Educational and labor market impacts

Educational attainment lost from abduction is substantial—by WLS, abducted youth attain 0.78 fewer years of education (Table 4, Column 3). The average non-abducted youth only completes seventh grade, so that abduction implies an 11 percent reduction in education attainment (Column 4). Note that this schooling loss corresponds closely to the average length of abduction—eight months, or 0.68 of a year.

The WLS and matching results also suggest that the abducted are 16 or 17 percentage points less likely to report being functionally literate (i.e. able to read a book or newspaper). These figures imply that abductees are nearly twice as likely to be functionally illiterate than non-abductees. The magnitude of the literacy gap is easier to understand once we consider that, in Ugandan schools, functional literacy is usually reached after six to seven years of schooling. Falling below the average level of schooling by a year thus increases the likelihood of poor literacy. For instance, looking at all youth in the sample, having six instead of seven years of schooling is associated with a 22 percentage point fall in functional literacy.

Labor market performance also appears to suffer due to abduction, but in terms of the *quality* of work rather than the *quantity*. Estimates of the abduction's impact on the probability of employment are small and not statistically significant. Thus the abducted appear no more or less likely to have found work. Work found by abductees, however, tends to be of a lower skill and capital-intensity. Eight percent of youth in the sample are engaged in a profession, a vocation, or own their own small business. The WLS estimate suggests that an abducted youth is 4 percentage points less likely than a non-abducted youth (or roughly half as likely) to be engaged in skilled work (Table 4, Columns 3 and 4).

[10] Following the methodology described above, the regression results flexibly control for year and location (sub-county) of birth with dummy variables for year and location as well as year/location interactions. Also included are quartic terms for each pre-treatment household characteristic. Matching estimates match one-for-one exactly on location and four-year age intervals, followed by matching on specific age. All estimates are weighted by inverse sampling and inverse attrition probabilities.

The ATE for the logarithm of wages implies that the abducted are also less productive. The coefficients can be interpreted as the approximate percentage change in wages due to abduction. The WLS result suggests that wages are 22 percent lower for abducted youth, although the result is only statistically significant at the 10 percent level (Table 4, Column 3). The distribution of wages is highly skewed, however, even after a log transformation. A matching estimator makes no parametric assumptions about the distribution of the residual, and thus is more appropriate. The matching estimate suggests that wages are 32 percent lower for the formerly abducted, significant at the 1 percent level (Column 5).[11]

Psychosocial impacts

One risk of combat is that participants are socialized into violence. To test this claim, aggression was measured by two indicators in the survey, one for whether the youth reported being in a physical fight in the past six months (9 percent overall) and a second for self-reported aggressive behaviors such as being quarrelsome, threatening others, and using abusive language (6 percent). The results in Table 4 offer mixed conclusions. While abductees were no more likely to have been in a fight, they were 3 percentage points more likely to report aggressive behaviors—69 percent greater than the non-abducted youth average. While these results may indicate greater hostility among abductees, higher self-reported hostility could also reflect a greater willingness to admit to these behaviors. Moreover, the statistical and substantive significance of the results is quite fragile to specification. Thus we must conclude with great caution that moderate feelings of aggression (or at least its admission) are a consequence of abduction.

Abductees exhibit little evidence of social exclusion, however. Social support was measured by an additive index of 14 forms of support experienced in the previous month (such as someone lending you items, or helping you find work). The impact of abduction on this index appears to be negative but very small and not statistically significant (Table 4, Column 4). Moreover, in results not displayed, the abducted are just as likely to report membership in a church or community organization. In fact, as explored in Blattman (2007), the abducted are *more* likely to be politically active—they are twice as likely to be a minor community leader and a quarter more likely to have voted in a recent national referendum.

[11] A disadvantage of such wage measures, however, is that wages are not observed for 237 unemployed youth (and log wages, furthermore, are undefined for 56 zero wage earners). If abduction is associated with the propensity to be employed or earn zero wages, such 'sample selection' will conflate the direct impact of abduction on wages with the indirect effects on the type of people that would be employed (Heckman, 1979; Lee, 2005). In this case, since abduction is empirically unrelated to both the probability of employment and the likelihood of earning zero wages, such sample selection bias is likely immaterial.

Finally, the measured psychological impacts of abduction appear to be mild on average. We developed an additive index of psychological distress using 19 self-reported symptoms of depression and traumatic stress.[12] The average youth has an index value of 4.2; the highest index value is 16. No norms or scores to determine clinically significant levels of symptoms have been established in this population, and so the severity of the average index value is open to subjective interpretation. The average index of 4.2 indicates a range of symptom profiles, from a youth experiencing approximately four symptoms (i.e., nightmares, difficulty concentrating) frequently to having thirteen symptoms of these symptoms on rare occasion.[13] Based on the in-depth interviews and assessments of youth by the psychologist, the average appears to be associated with mild distress and depression.[14]

Comparing abducted and non-abducted youth, the WLS estimate indicates that abductees exhibit a half-point increase in their index—an increase of roughly 13 percent relative to the non-abducted average of 3.9.[15] The interpretation of such a result is somewhat subjective. Even so, it is difficult to argue that this small difference in symptoms signifies a clinically important difference in populations. Also, if a score of roughly 4 is associated with mild distress, and a score of 6 or more puts one in the top quartile of distress, it follows that an ATE of 0.5 on a base of 3.9 likely represents a fairly modest increase.

Yet while the distress ATE itself is not large, there is evidence that the youth exhibiting the most serious symptoms of distress are disproportionately abductees. For instance, the abducted are nearly three times as likely to be in the top quartile of distress (regression not shown). These findings suggest that we should be interested in differences in the *distribution* of psychological outcomes, especially the top tail. For instance, 20 to 25 percent of former abductees report re-experiencing traumatic events through nightmares and flashbacks, versus 10 to 15 percent of non-abducted youth. Furthermore, 10 percent of abductees report feeling "always sad", compared to 5 percent of their non-abducted peers.

Indeed, the top tail of abducted youths' distribution of distress is much 'fatter' than that of the non-abducted, implying a greater concentration of heightened distress. To see this, we can compare the conditional distributions of distress of abducted and non-abducted youth at different quantiles (via a least-

[12] The symptoms are scaled between zero and one according to their reported intensity. For each of the 19 symptoms, "often" receives a full value of 1, "sometimes" 0.66, "rarely" 0.33, and "never" a zero.
[13] The six most commonly reported symptoms are excessive worrying, bad memories from the past, difficulty concentrating, often finding life difficult, crying when remembering bad memories, and restless nights.
[14] For an extended discussion of the psychological literature, evidence and results, see Annan (2007).
[15] The difference signifies an average increase in frequency of one symptom from 'rarely' to 'often' or the addition of two symptoms experienced rarely.

absolute deviations regression of distress on an abduction indicator and year and location of birth). An abducted youth at the median of his distress distribution reports just 0.66 more symptoms than a non-abducted youth at his median (of 3 symptoms), while an abducted youth at the 90th percentile reports 2 more symptoms of distress, significant at the 5 percent level (regressions not shown).

Below we will see that the concentration of traumatic stress is likely driven by the concentration of extreme violence experienced. Violence rather than abduction appears to be the real "treatment" resulting in distress—a result reinforced by a broad literature on refugee war trauma using similar measures (e.g. Ajdukovic & Ajdukovic, 1998; Dyregrov et al., 2002; Kinzie et al., 1986; Mollica et al., 1997).

A handful of caveats are in order, however. First, these conclusions assume that distress is well-measured by the metrics used. While our qualitative assessments tend to confirm the measures, too little clinical work has been performed in Uganda to have developed standard tests and norms. Second, there is a small risk that some traumatic memories or symptoms have been repressed.[16] Finally, the relatively mild average psychological impacts could be a consequence of the success of humanitarian programs provided to former abductees. If so, our results would underestimate the psychosocial impact of abduction. Unfortunately, this scenario is unlikely, as reintegration programs have been extremely modest in scale and reach.[17] The estimates presented in this paper thus reflect the impact of combat conditional on half of abducted youth receiving the most rudimentary reintegration services.

Robustness

The above results are robust to changes in specification, as seen in Table 5. The WLS estimates of the ATE from Table 4 are reproduced in Column 1 of Table 5 for five outcomes: education, literacy, log wages, hostility, and distress. We can examine the robustness of the ATE exclusion of the pre-war household characteristics (Column 2), further excluding the age and location dummy variables (Column 3), and further still excluding the weights correcting for attrition (Column 4) and selection (Column 5). In general the different specifications yield nearly identical to the original WLS results, save for education

[16] The psychological literature, however, suggests the likelihood of successful repression or avoidance is quite small. Repression of traumatic memories, where observed, has tended to occur among much younger children than in our sample. Moreover, even if a youth actively avoids traumatic memories, this repression will often manifest itself in symptoms measured by the survey, such as nightmares. (DSM) Finally, while psychological distress may manifest itself long after the event, most individuals respond nearly immediately after the traumatic events. See APA (2000).

[17] Only half of abductees passed through the formal care system. Moreover, those who did pass through the centers received only basic medical treatment, family reunification, and non-clinical "counseling"—in essence advice-sharing from social workers. Only 1 in 10 youth were ever revisited (Allen & Schomerus, 2006).

and wages when we omit all weights and controls (although even here we cannot statistically reject equality of the coefficients). If there is selection on observed traits, however, it is almost entirely selection on year and location of birth alone, as expected. Adding age and location controls back into the estimation (Column 6) and attrition weights (Column 7) returns all estimates back to the levels and significance of the WLS estimates in Column 1. Finally, adding back pre-war household traits (Column 8) yields the table 4 OLS estimates. This consistency strengthens our confidence in the unconfoundedness assumption.

B. Treatment effect heterogeneity: Child versus adult combatants

As the sample includes youth abducted at all ages, we can also examine how the above ATEs varied by age of participation, and thereby identify what is different about being a child soldier. To identify this treatment effect heterogeneity, outcomes can be regressed on age of abduction while controlling for year and location of birth indicators, as well as indicator variables for the year of abduction.

The regression results, displayed in Table 6, suggest that older abductees are better educated, with 0.38 additional years of education for every extra year of age before abduction. Older abductees also appear more likely to be engaged in skilled work. Such findings are intuitive—child abductees are more likely to be in school at the time of abduction and have their education interrupted, while young adults are more likely to have finished their schooling, and thus lose work experience rather than education.

Support for this intuition comes from looking at current enrolment rates by age, displayed in Figure 4. The dashed line represents the average probability a youth of that age is currently enrolled in school. Levels of enrolment fall from 95 percent of current 14-year olds to 25 percent of youth currently in their late 20s. Figure 4 also displays the average probability that a formerly abducted youth returned to school. This relationship is illustrated by the solid line, where the horizontal axis represents the age of return from abduction. The enrolment gap is largest among young abductees and closes as age of return rises. While the two lines represent different populations and are not strictly comparable, the pattern suggests that the interruption and termination of schooling is more likely for younger abductees.

There is no significant relationship between log wages and abduction age, however, in spite of the link from age to education. Difficulty in making inferences about treatment effect heterogeneity may be impeded in this case by the limitations of the wage data—the low number of observed wages, the high variability of the measure, and its skewed distribution.

C. Sensitivity Analysis

A remaining concern is the potential for unobserved selection and bias. Several plausible sources exist, including smarter youth "self-selecting" out of the LRA via a better ability to hide, or survival of only the physically strongest. We are especially worried about the selection of "low-types" into the rebel group or from differentially greater attrition of "high-types", leading to overestimation of the ATEs.

Sensitivity to violations of unconfoundedness

To assess the potential for unobserved selection, we can explicitly model relaxations of unconfoundedness. Note that the omission of an unobserved covariate, *U*, will induce a material degree of bias only if it is sufficiently associated with both treatment assignment, *T*, and the outcome, *Y*. To assess the sensitivity of the ATE, we can take a hypothetical *U* with known distribution and calculate the combinations of correlation between *U* and *T* and between *U* and *Y* that would lead the estimated ATE to be biased by a predetermined amount (Imbens, 2003; Rosenbaum & Rubin, 1983). We can then judge whether the existence of such a hypothetical *U* is plausible by benchmarking it against observed covariates, *X*. We employ a parametric model postulating an independent binomial distribution for *U*, a logistic conditional distribution for *T*, and a normal conditional distribution for *Y*. The model is illustrated in Appendix A.

Figure 5 plots each of the observed pre-war controls according to their ability to explain variation in both in *T* (abduction) and *Y* (in this case, education). The vertical axis indicates the influence of each covariate in explaining variation in years of education, and represents the marginal increase in the R^2-statistic from adding each covariate to a regression of education on all other covariates. The horizontal axis indicates the influence of each covariate in explaining additional variation in abduction. Note that the exception of age and location, the observed covariates explain little variation in either *Y* or *T*.

The curve in Figure 5 represents all the combinations of correlation between *U* and *T* and between *U* and *Y* that would be sufficient to reduce the estimated education ATE by half, from 0.78 to 0.39. The curve mapped out in Figure 5 is therefore a threshold, beyond which the hypothetical *U* is influential enough to materially reduce the education ATE. It is also a threshold, incidentally, that would leave the sign and significance of the ATE (and hence our general policy conclusion) intact. Only year of birth—a variable that represents the primary criterion for selection by the armed group as well as variation in rebel abduction activity over time—crosses this hypothetical threshold.[18] Meanwhile, traits that normally influence military recruitment (such as household wealth or orphaning) lie far beneath the threshold—any *U* would need to be roughly 40 times more influential than orphaning, and five times more influential than all the household asset measures to reduce the ATE by half. This analysis suggests that moderate amounts of unobserved selection are unlikely to account for the treatment effects observed.

Bounding the treatment effect for selective survival and non-return

Roughly 16 percent of all youth in the retrospective household roster died or were abducted and did not return. If abductees who died tended to be the weak or less clever, then the average treatment effects we identify are likely understated. Moreover, those who remain with the armed group are likely missing more education and work experience than the average returnee, also contributing to potential underestimation of our economic and educational ATEs. Of course, we simply don't know whether either of these types of selective attrition exist, or by how much.

In such instances Lee (2005) suggests a method of sensitivity analysis whereby "best-case" and "worst-case" scenarios for differential attrition are constructed by trimming the distribution of the outcome in the group with less attrition (in this case the non-abducted). Rates of attrition and bounds for each outcome are listed in Table 7. The best case scenario bound is calculated by dropping non-abductees with the lowest values of the outcome and calculating the 'trimmed' ATE. The worst-case bound is likewise calculated by dropping the best-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 ATE's under the "worst-case" scenario are generally closer to zero and less than robust than the untrimmed ATEs. By the standards of this sensitivity analysis, this worst-case performance is actually quite strong, since not one of these lower bounds changes sign. The loss of statistical significance is not uncommon. The results imply that under austere and implausible dramatic selection, abduction still has the predicted effect on outcomes, albeit at a lower level of statistical significance.

[18] Year of birth (YOB) influences treatment assignment mechanically (as abduction levels changed year to year) as well as because rebel abduction parties targeted adolescents). Thus the distance of YOB from the origin in Figure 5 overstates the role of age as a selection criterion by rebels, and the selection effect induced by age is likely closer to the threshold, not further away.

VI. Heterogeneous Treatment and the Channel of Impact

Abduction has so far been handled as a binary treatment. This approach, however, obscures the diversity of experiences. Abduction length ranged from a day to ten years, and violence varied dramatically. Of 26 violent acts recorded in the survey, abducted youth reported an average of 10.3 and non-abducted youth reported 4.3 (Table 1). Abducted youth were also more likely to be forced to perpetrate violence—17 percent report being forced to steal, beat or kill, versus zero non-abductees.

When treatment is heterogeneous the binary ATE can be interpreted as the average per-unit effect along a response function mapping treatment exposure to outcomes (Angrist & Imbens, 1995). One might prefer, however, to estimate the entire response function, considering abduction length or violence the "true" treatment. Such treatment exposure, however, is likely to be endogenous in this case. Longer or more violent abductions, while idiosyncratic to some degree, are undoubtedly related to unobserved individual traits, even if abduction itself is not.[19]

Nevertheless, estimating the (potentially biased) relationship between our measures of treatment exposure and outcomes are useful from the point of view of understanding the underlying causation. The reduced-form ATEs in Table 4, while informative, tell us little about why these impacts are observed. Together with the correlates of each outcome and the findings on heterogeneity by abduction age, we will see that our estimates of the response function are supportive of some channels over others.

To begin with, the evidence suggests that youth who exhibit the most serious symptoms of psychosocial distress are generally those that experienced the greatest war violence—differences in violence reported by abducted and non-abducted youth can explain virtually all of the difference in distress. Table 8 displays a WLS regression of each outcome on two measures of abduction intensity—an Index of Violence experienced (Column 1) and Years Abducted (Column 2), including abductees only.[20] The index of violence is a linear additive index of the 26 traumatic events reported by respondents. According to these regressions, an additional incident of war trauma is associated with a 0.19 point increase in the index of distress. The average abductee reported 6 more violent acts than non-abductees (Table 1), implying a distress ATE of $6 \times 0.19 = 1.14$. This amount is significantly greater than the distress ATE of 0.51 (Ta-

[19] While endogeneity of length of violence would lead estimates of the response function to be inconsistent, the ATEs in Table 4 remain unbiased. Rather, potentially endogenous exposure simply changes the interpretation of the ATE. If under exogenous exposure the ATE is the average per-unit effect along a response function, under endogenous exposure it is the *ex-ante* expectation of this amount for a youth randomly selected from the prior distribution.

[20] Control variables include pre-treatment covariates, age of abduction, and indicators for the year of return.

ble 4), suggesting that either the marginal effect of additional violence on an average youth may be greater than additional acts of violence, or that the violence coefficient in the regression is biased upwards. The results, however, suggest that the concentration of extreme violence can account for the patterns of distress observed in the sample: a moderate average impact of abduction combined with moderate to serious traumatization among a minority. Such a link between increased exposure to violence and higher emotional distress has been identified among war-affected populations in settings as diverse as Iraq, Cambodia, Rwanda, and Croatia (Ajdukovic & Ajdukovic, 1998; Dyregrov et al., 2002; Dyregrov et al., 2000; Kinzie et al., 1986; Mollica et al., 1997; Paardekooper et al., 1999; Sack et al., 1986).

Meanwhile, anecdotal evidence that it is time away from human capital accumulation rather than violent trauma that accounts for former abductee's decline in economic performance finds support in Table 8. First, time away is strongly correlated with losses in education. Recall that the average length of abduction is nearly identical to the average schooling loss (Table 4). The association between violence and education is also close to zero and statistically insignificant (Table 8). In contrast, the length of abduction is highly correlated with education and literacy levels; each year of abduction is associated with 0.25 years less education and a 9 percentage point reduction in literacy (significant at the one percent level) as well as 3 percentage point decrease in the probability of skilled work (significant at the 10 percent level).

Second, time away only appears to induce education losses among those of schooling age. Recall that the schooling impact of abduction is decreasing in age of abduction, since older abductees are more likely to have finished their studies and thus not have their education interrupted (Table 5). Moreover, in regressions not shown, each year of abduction is associated with 0.80 years less education for those abducted before the age of eighteen, and no distinguishable change for those abducted after 18.

Third, the correlates of wages suggest that time-away from education is by far the most significant determinant of the wage gap between abducted and non-abducted youth. Empirical studies of earnings typically employ a human capital earnings function that expresses wages as a log-linear function of education and experience.[21] Social capital and health are also added to the wage determination equation:

$$ln(Wage)_i = \delta_0 + \delta_1 \cdot Education_i + \delta_2 \cdot Experience_i + \delta_3 \cdot Social\ Capital_i + \delta_4 \cdot Health_i + \mu_i$$

This equation is estimated in Column 1 of Table 9 using the Index of Social Support, an Injury Indicator and the Index of Distress as proxies for social capital, physical and mental health. These estimates sug-

[21] See Mincer (1974) for the theoretical justification of this function. *Experience* is calculated as *Age* – 6 – *Education.*

gest that education and physical health are the strongest observed correlates of wages, followed by experience. While potential measurement error and bias from omitted variables prevent a causal interpretation of these coefficients, the international returns to schooling evidence suggests that the relative magnitude of the education coefficient is likely to be correct to a first order of approximation.[22]

We can obtain a rough measure of the relative influence of each component of human capital in the abduction wage gap by multiplying the earnings function coefficients by the respective abduction ATEs (Table 9, Columns 2 and 3).[23] Education, as the strongest determinant of wages (and a principal casualty of abduction), appears to be the most significant channel by which abduction reduces wages, representing 57 percent of the reduced-form ATE for log wages (Column 4).[24] While this estimate is crude and undoubtedly biased, it is more three times as influential as the experience and physical health measures. Even if dramatically biased, the basic conclusion is the same.

Finally, longer abductions are associated with lower wages, as seen in Table 8. The coefficient on years abducted for log wages suggests that an average abduction (0.68 years) is associated with a $-0.20 \times 0.68 = -0.136$ impact on log wages—62 percent of the log wage ATE from Table 4. Our conclusion is thus analogous to Angrist's (1990) for U.S. Vietnam veterans—that military skills and experience obtained in the LRA appear to be poor substitutes for civilian schooling and work experience.

VII. Conclusions

In this paper we provide some of the first estimates of the nature, magnitude, and distribution of the effects of civil war on youth. The results suggest there are two main impacts of armed group participation operating by two channels—a human capital loss due to time away from schooling and employment, and psychological distress concentrated in those that experience the most violence.

These findings can be juxtaposed against current policy in Uganda, where funding has been focused on broad-based programs for the psychosocial reintegration of former child soldiers. With distress and

[22] For a discussion of estimation bias regarding schooling see Card (1999) and for health see Strauss and Thomas (1998). The literature suggests that ability and attenuation biases are moderate and tend to offset one another (Ashenfelter et al., 2000; Card, 1999). While the coefficient on wages in Table 9 is high relative to rich-country estimates of the returns to schooling, Kruger and Lindahl (2001) suggest that the returns to education are higher in poor countries.

[23] The average length of abduction is used in place of an ATE in Column 2.

[24] Table 8 also suggests that lower wages are associated with more violent abductions. This finding may be driven by the combined influence of violence on the likelihood of an injury and of injuries on wages (Table 9). Violence does not appear to affect wages through the psychological channel, as there is no significant relationship between distress and log wages (Table 9).

aggression concentrated, more targeted and specialized psychosocial services are probably needed for those who have undergone the most severe trauma (abducted or not). The most broad-based impact of abduction, meanwhile, instead appears to be economic and educational, suggesting that more attention and funding should be drawn to this area. Annan et al. (2006) describe a set of conventional humanitarian interventions that could more directly address the needs and gaps identified by this analysis, including: accelerated adult literacy programs; support for private vocational training; expanded access to land and agricultural improvement services; and training and small grants for micro-enterprise development.

A final word on interpretation of the ATEs. Non-abducted youth are adversely affected by war, and it is likely that non-abductees in raided areas had worse schooling outcomes due to fewer schools being open, fewer teachers available, and general income shocks to the household. This situation—where the counterfactual situation is still being affected by war, albeit less dramatically—has two consequences. First, it suggests a potential source of bias that we cannot address—that of any externalities from abduction. For instance, the stress on families of losing one child may have had a negative impact on the psychological health or education of those children who remained. Alternatively, the exit of so many youth from the labor force could conceivably raise the return to labor for those left behind (although in reality such positive externalities seem unlikely). Negative externalities, which seem most plausible, will cause our ATEs to underestimate the true impact of military service in Uganda.

Second, the use of a war-affected counterfactual requires that we interpret the ATEs as capturing the incremental effect of conscription—a bundle of time away and added violence experienced. This counterfactual is the relevant one if we are interested in addressing reintegration gaps—that is, closing any inequality between combatants and non-combatants, or providing reparations beyond that received by other war-affected populations. The disparity between war-exposed youth and unexposed youth is a different impact altogether, probably of equal importance, that is not addressed by this study. It is worth noting, however, that the incremental impact of military service may be substantively more important than the gap between war- and non-war affected populations. National survey data suggest that while the impact of the war on wealth may have been large, the educational impacts has been small—non-abducted youth in the war zone have levels of education and literacy that similar or even *greater* than that in other areas of the country, although household assets are significantly lower. These data and the analysis are discussed in Appendix B. While these comparisons are undoubtedly biased, they do suggest that the gap between non-abducted youth in Acholiland and youth outside the region is not enormous.

Finally, we consider the generalizability of these results. Evidence from a handful of other studies suggests that the findings and conclusions from Ugandan ex-combatants may be more broadly relevant for other situations of conflict. The primacy of persistent economic impacts of war has been identified in quantitative and qualitative work with ex-combatants in Sierra Leone (Humphreys & Weinstein, 2005; Shepler, 2005). Angrist (1990) also finds that white U.S conscripts from the Vietnam War experience a 15 percent decrease in long term earnings, largely due to losses in relevant work experience. Both the impacts and suggested channel are strikingly similar to Uganda.

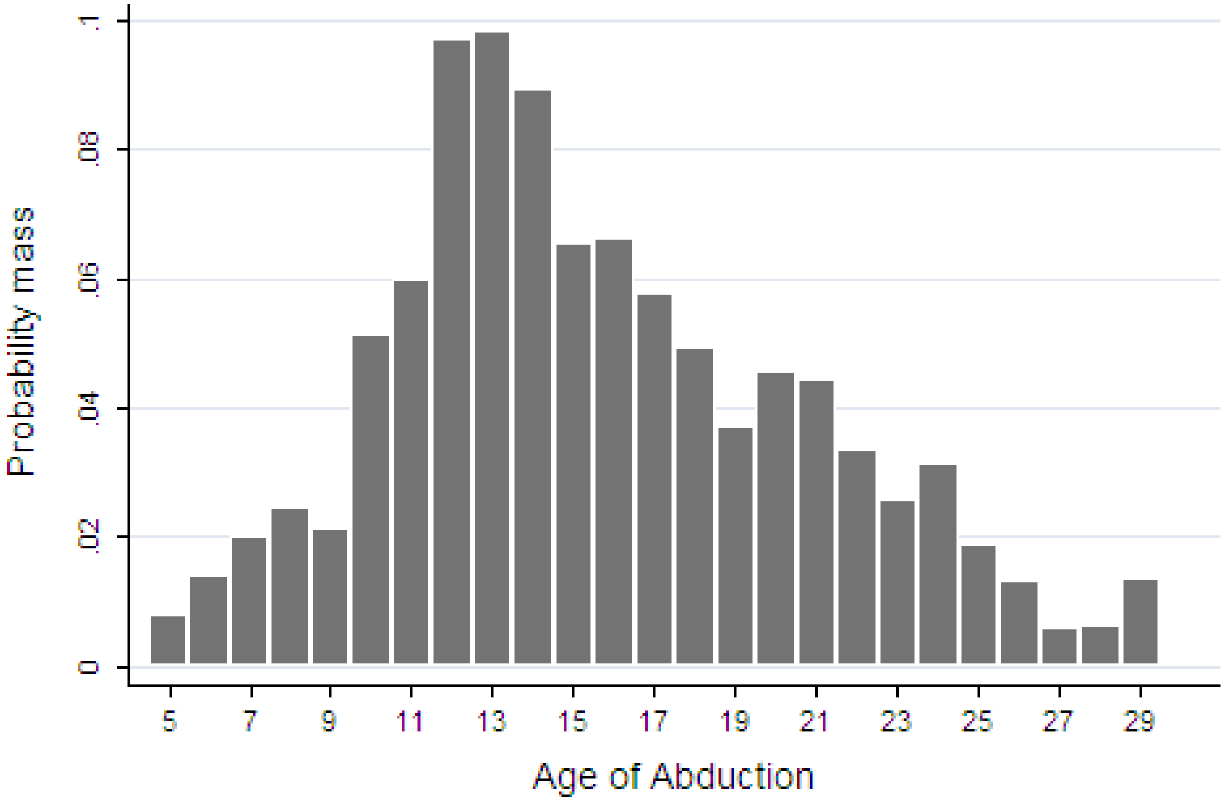
While the Ugandan child soldiering results are most easily generalized to other instances of forcible recruitment, they may actually understate the consequences of voluntary participation in other unpopular armed groups. Volunteer fighters in an unpopular war might see a greater loss in social capital and mental health. Ugandan abductees have not been held accountable for their actions as soldiers, and have been welcomed home with open arms—a remarkable community response that could mute the economic and psychosocial effects. Globally a third of child soldiers are thought to be forcibly recruited (ILO, 2003). For the other two thirds, who might experience more social exclusion upon return, the treatment effects estimated in this paper might be regarded as a minimum impact.

Finally, the 'time away' channel also suggests that the Ugandan findings may be relevant for understanding other forms of child labor or even interruptions of human capital accumulation more generally. For instance, Meng and Gregory (2007) find a sizable reduction in educational attainment among Chinese taken out of school during the Cultural Revolution, although they find little decrease in long-term earnings. Causal estimates of the impact of agricultural child labor on education and earnings in both Vietnam and Tanzania suggest that a doubling of hours employed reduces school enrolment by nearly a third and educational attainment by six percent (Beegle et al., 2004, 2006). In the longer term, however, the authors find that this work experience can augment the child's wages and employment. These examples highlight the importance of the quality of the experience acquired by youth in place of formal education. Child labor or an event like the Cultural Revolution may offer the ability to acquire some measure of human and physical capital, muting the long term impact on economic performance. Child soldiering thus deserves to be singled out as one of the worst forms of child labor not simply because of the obvious risk of injury or death and the terrible experiences of violence, but also because of the dramatic decrease in lifetime earnings ability that comes from the irrelevance of the experience and consequent deficiency in human capital.

Ultimately, however, external validity is difficult to assess because of the paucity of micro-level data in areas of armed conflict. This suggests there is a need for more research in more zones of conflict. For this research to be accurate and comparable, greater attention ought to be paid to representative samples, accounting for attrition, and the careful identification of comparison groups. The aim should be to move from *ad hoc* to evidence-based policy in post-conflict reintegration, redevelopment, and peace-building.

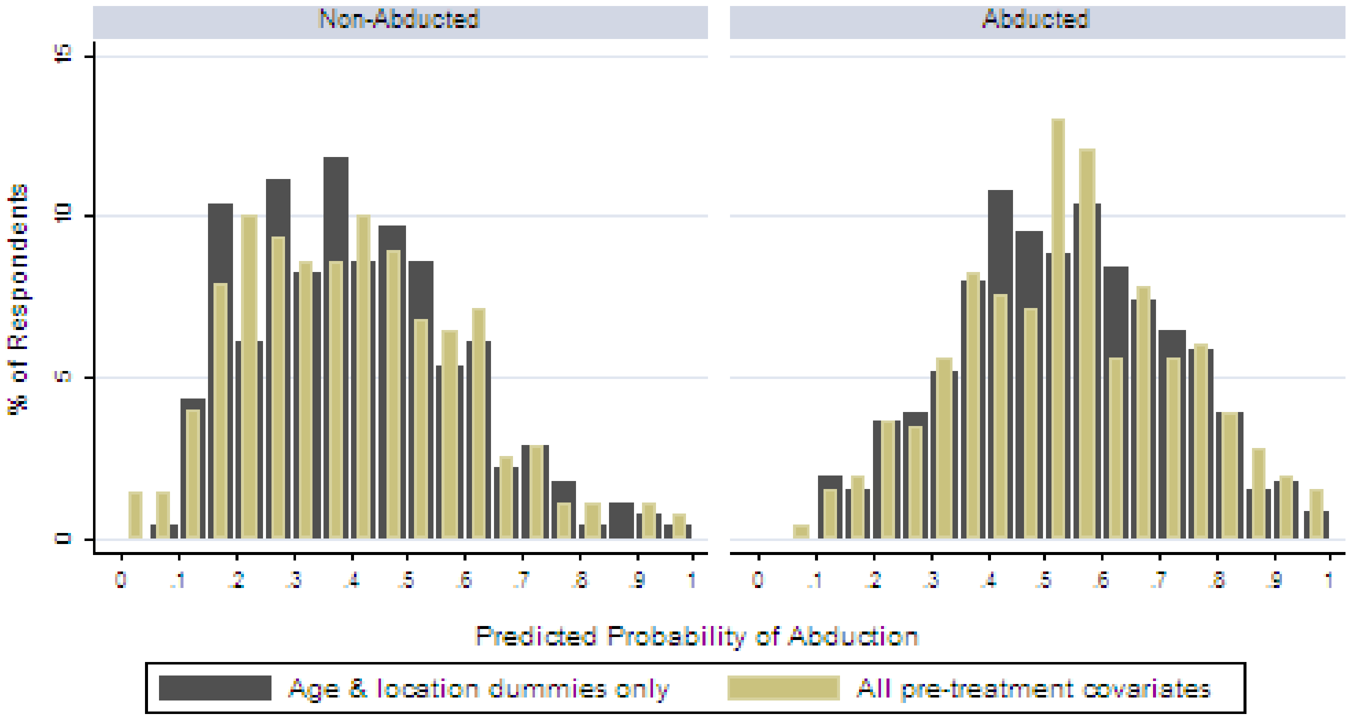
Figure 1: Map of Uganda and survey field site

Figure 2: Distribution of abductions by age at the time of abduction



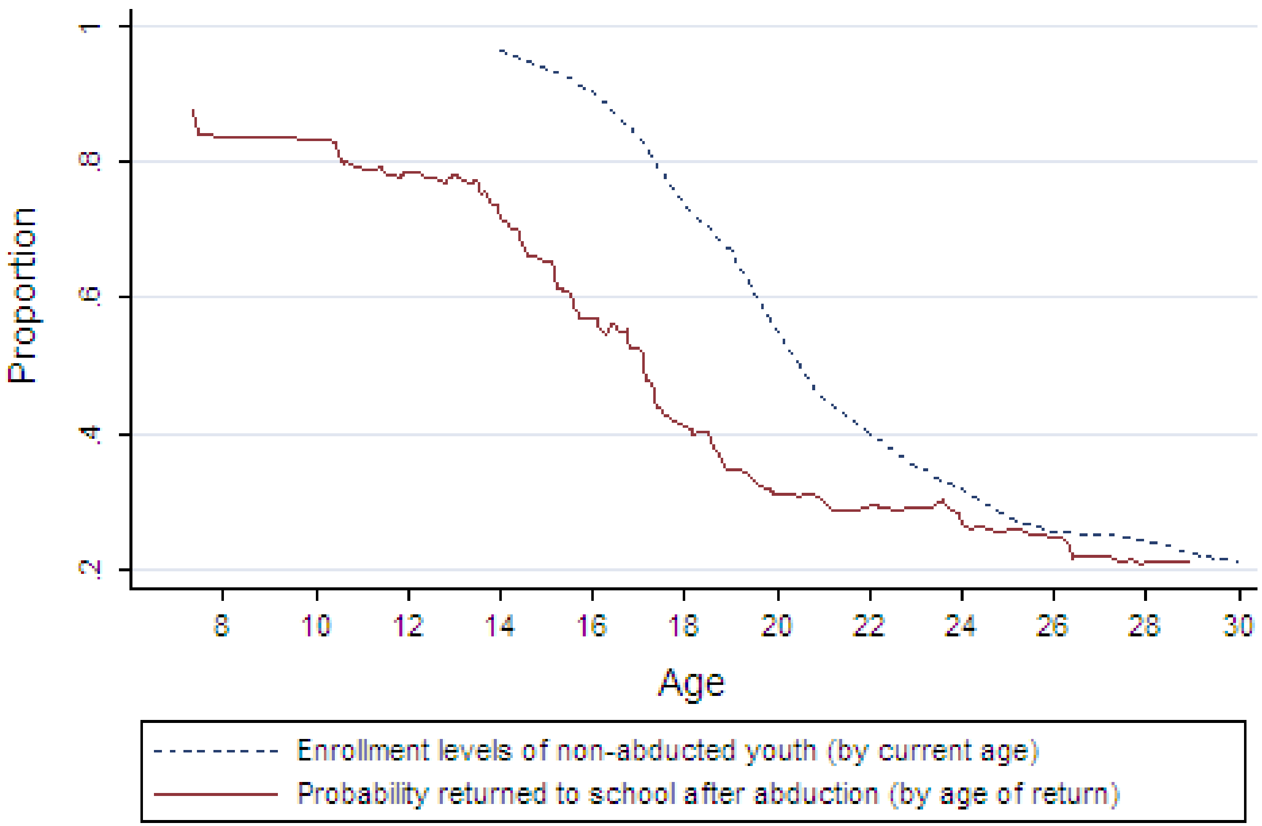
Notes: The bars represent a probability mass function for age at the time of abduction, and so sum to one. The data include absentee youth and youth who have since died or did not return from abduction (collected from the household survey). Where an individual was abducted more than once, all abductions are included.

Figure 3: Distributions of the predicted probability of abduction based on age and location alone versus all pre-treatment covariates (by abduction status)



Notes: N = 741 males aged 14 to 30, weighted by the inverse sampling probability and the inverse attrition probability. Pre-treatment covariates other than age and location dummy variables include mother's and father's education, mother's and father's death in 1996, initial household landholdings and assets, and father's main occupation.

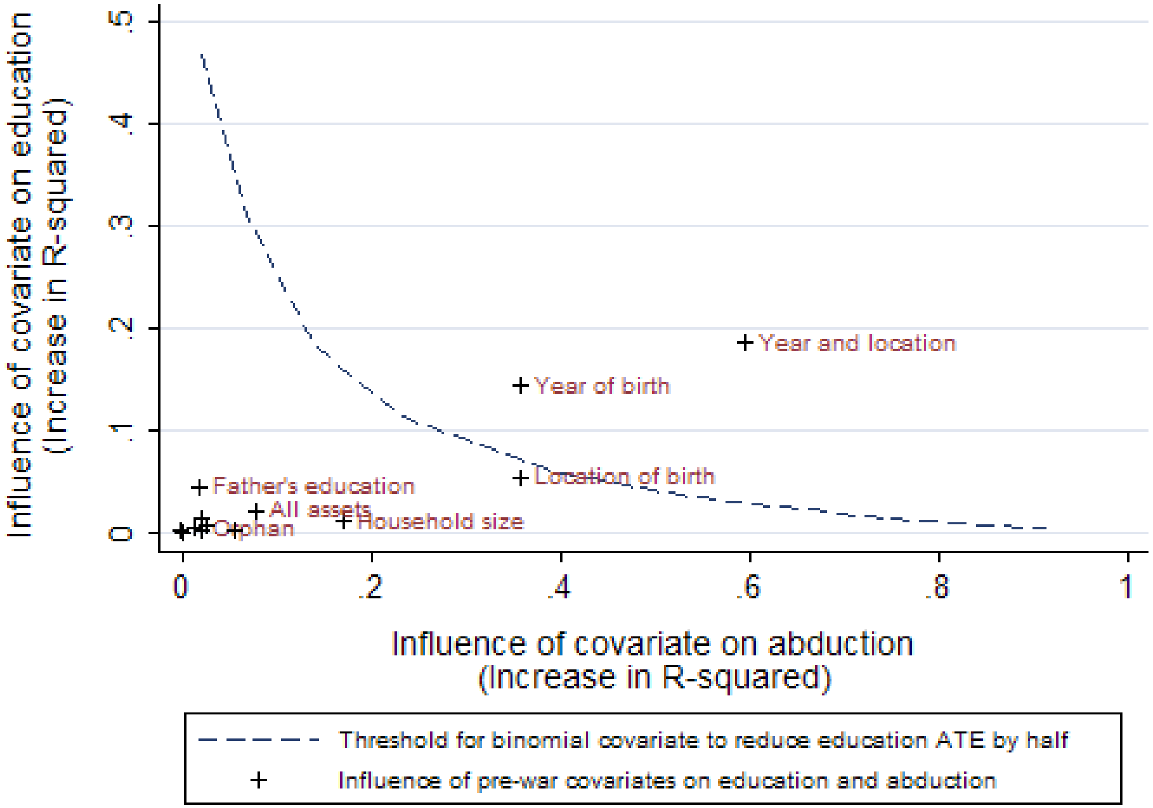
Figure 4: Probability of returning to school after abduction, by age of abduction



Notes: The solid line is a running-mean of the probability that an abducted youth reported returning to school after returning home, by age of return, via symmetric nearest-neighbor smoothing (bandwidth = 0.5). The dashed line is the average probability that a non-abducted youth is currently enrolled in school by current age. While the two lines are not strictly comparable, the wide but closing gap suggests that the younger the age of abduction and return, the more likely a youth is to have his education interrupted. 95 percent confidence intervals (not displayed) suggest that the two averages are statistically significantly different below age 20.

Figure 5: 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 equal to one half of the ATE



Notes: The figure presents the results of the sensitivity analysis following Imbens [2003]. Each × represents a pretreatment covariate, 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 educational attainment. In essence each axis measures the increase (or decrease) in the R^2 statistic from adding that covariate to the regression in question. The downward sloping curve represents the locus of points at which any independent binomial covariate (observed or unobserved) would have sufficient association with both treatment and educational outcomes to reduce the ATE on education by half, from 0.78 to 0.39.

Table 1: Description of key variables: War experiences and post-war outcomes

Variable name	Description	Sample mean [Std Dev]			# of Obs.
		All	Abd	Non-Abd	
War Experiences					
Ever abducted	Indicator for whether the youth reported any abduction experience, no matter how short.	0.44 [0.50]			741
Abducted more than two weeks in total	Indicator for whether the youth reported any abduction experience longer than two weeks.	0.28 [0.45]			741
Months abducted	Length of the respondent's longest abduction, in months.		8.1 [15.2]		462
Age of abduction	Age (in years) at the time of the respondent's longest abduction.		14.7 [4.8]		462
Index of violence	Sum of 26 indicators of violence. Three of the 741 respondents declined to respond.	6.9 [5.0]	10.3 [5.0]	4.3 [2.9]	738
Education & Labor Market Outcomes					
Educational attainment	Highest grade attained, plus any years of vocational training and post-secondary school	7.0 [2.8]	6.8 [2.8]	7.1 [2.8]	741
Indicator for functional literacy	Indicator equaling one if a respondent is able to read a book or a newspaper in any language.	0.71 [0.45]	0.65 [0.48]	0.76 [0.43]	741
Indicator for any work in past month	Indicator equaling one if days employed were greater than zero.	0.63 [0.48]	0.68 [0.47]	0.59 [0.49]	741
Indicator for capital or skill-intensive work	Indicator equaling one if the main occupation is a profession, a vocation, or a small business.	0.08 [0.27]	0.07 [0.25]	0.09 [0.29]	741
Daily wage (in USD)	Gross cash earnings in the past month divided by days employed. 237 respondents were unemployed and thus have no wage data.	1.41 [3.63]	1.29 [2.58]	1.51 [4.36]	504
Psychosocial & Health Outcomes					
Indicator for a physical fight	Indicator equaling one if the respondent reported being a physical fight in the past 6 months.	0.09 [0.3]	0.08 [0.3]	0.09 [0.3]	741
Indicator for hostility	Indicator equaling one if reported being quarrelsome, disrespecting property, using abusive language, or threatening others.	0.06 [0.2]	0.07 [0.3]	0.05 [0.2]	741
Index of social support	Sum of 14 questions on concrete social support received.	5.37 [2.4]	5.39 [2.4]	5.35 [2.4]	741
Index of psychological distress	Sum of 19 survey questions on symptoms of depression and traumatic stress.	4.16 [2.6]	4.49 [2.8]	3.91 [2.4]	741
Indicator for a serious injury	Indicator equaling 1 if the respondent reported moderate or serious difficulty performing basic tasks	0.13 [0.3]	0.17 [0.4]	0.11 [0.3]	741

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

Table 2: Comparison of means: Abducted versus non-abducted youth

	(1)	(2)	(3)	(4)
	Abducted versus non-abducted youth			
Pre-treatment Covariate	**Unconditional mean**		**Difference in means‡**	
	Abducted	**Non-Abducted**	**Unconditional**	**Conditional**
Year of birth†	21.54	20.47	1.08	1.44
	[0.44]	[0.29]	[0.44]**	[0.61]**
Indicator for father a farmer†	0.90	0.90	0.01	-0.03
	[0.01]	[0.03]	[0.02]	[0.03]
Household size in 1996†	8.48	8.81	-0.33	-1.15
	[0.33]	[0.55]	[0.41]	[0.33]***
Landholdings in 1996†	26.78	26.36	0.42	1.00
	[1.48]	[2.44]	[2.10]	[2.41]
Indicator for top 10% of Landholdings†	0.16	0.16	0.00	0.01
	[0.02]	[0.04]	[0.03]	[0.02]
Cattle in 1996†	17.73	12.66	5.07	5.95
	[7.68]	[4.89]	[4.12]	[7.44]
Other livestock in 1996†	14.18	13.23	0.94	1.17
	[2.11]	[3.09]	[2.72]	[0.98]
Indicator for plow ownership in 1996†	0.23	0.19	0.04	0.02
	[0.03]	[0.04]	[0.04]	[0.05]
Indicator for uneducated father	0.12	0.13	-0.02	0.01
	[0.01]	[0.02]	[0.02]	[0.01]
Father's years of schooling	6.11	5.73	0.38	0.22
	[0.19]	[0.27]	[0.34]	[0.25]
Indicator for uneducated mother	0.53	0.55	-0.01	-0.02
	[0.04]	[0.02]	[0.04]	[0.04]
Mother's years of schooling	2.32	2.42	-0.09	-0.10
	[0.23]	[0.16]	[0.28]	[0.28]
Indicator for paternal death before 1996	0.34	0.33	0.00	0.01
	[0.03]	[0.02]	[0.04]	[0.04]
Indicator for maternal death before 1996	0.13	0.12	0.01	0.02
	[0.02]	[0.02]	[0.03]	[0.03]
Indicator for orphaning before 1996	0.07	0.08	0.00	-0.02
	[0.01]	[0.02]	[0.02]	[0.02]

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 3: Comparison of means: Militia volunteers versus non-militia members

	(1)	(2)	(3)	(4)
	Militia versus non-militia members			
Pre-treatment Covariate	Unconditional mean		Difference in means‡	
	Militia	Non-Militia	Unconditional	Conditional
Year of birth	22.94	19.54	3.39	2.67
	0.72	0.41	[0.83]***	[0.69]***
Indicator for father a farmer	0.96	0.89	0.07	0.07
	0.03	0.03	[0.04]*	[0.04]*
Household size in 1996	9.42	8.37	1.05	1.25
	0.83	0.61	0.98	[0.68]*
Landholdings in 1996	15.28	22.35	-7.07	-4.55
	3.02	1.55	[3.02]**	[2.94]
Indicator for top 10% of Landholdings	0.03	0.11	-0.08	-0.07
	0.02	0.02	[0.03]***	[0.03]**
Cattle in 1996	3.29	14.03	-10.73	-6.45
	1.96	7.16	7.13	[2.41]**
Other livestock in 1996	6.23	11.42	-5.20	-4.22
	1.83	2.52	[2.45]**	[2.26]*
Indicator for plow ownership in 1996	0.09	0.19	-0.11	-0.13
	0.04	0.04	[0.06]*	[0.06]**
Indicator for uneducated father	0.07	0.13	-0.05	-0.11
	0.04	0.01	0.04	[0.03]***
Father's years of schooling	6.03	5.89	0.15	0.33
	0.48	0.18	0.50	[0.47]
Indicator for uneducated mother	0.66	0.53	0.13	0.12
	0.11	0.02	0.10	[0.10]
Mother's years of schooling	1.95	2.40	-0.45	-0.32
	0.66	0.13	0.64	[0.66]
Indicator for paternal death before 1996	0.42	0.33	0.09	0.10
	0.09	0.02	0.10	[0.09]
Indicator for maternal death before 1996	0.06	0.13	-0.07	-0.02
	0.05	0.01	0.05	[0.04]
Indicator for orphaning before 1996	0.02	0.08	-0.05	-0.01
	0.02	0.02	[0.03]*	[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%

‡ 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 weights)

Table 4: Estimates of the average treatment effect of abduction

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable	**OLS estimate†**		**WLS estimate‡**		**Matching estimate§**	
	ATE	**%Δ**	**ATE**	**%Δ**	**ATE**	**%Δ**
Educational & Labor Market Outcomes						
Educational attainment	-0.79 [0.14]***	-11%	-0.78 [0.14]***	-11%	-0.73 [0.21]***	-10%
Indicator for functional literacy	-0.15 [0.03]***	-19%	-0.16 [0.03]***	-20%	-0.17 [0.04]***	-22%
Indicator for any employment in the past month	0.02 [0.04]	3%	0.02 [0.04]	3%	-0.01 [0.04]	-2%
Indicator for capital- or skill-intensive work	-0.04 [0.02]***	-47%	-0.04 [0.01]***	-47%	-0.04 [0.02]*	-47%
Log (Daily wage)	-0.22 [0.14]	n.a	-0.22 [0.12]*	n.a	-0.32 [0.12]***	n.a.
Psychosocial & Health Outcomes						
Indicator for physical fights	0.01 [0.02]	13%	0.00 [0.02]	0%	0.00 [0.02]	-5%
Indicator for hostility	0.03 [0.01]***	69%	0.03 [0.01]***	69%	0.03 [0.02]*	73%
Index of social support	-0.23 [0.16]	-4%	-0.24 [0.16]	-4%	-0.22 [0.25]	-4%
Index of psychological distress	0.56 [0.19]***	14%	0.51 [0.19]***	13%	0.44 [0.20]**	11%
Indicator for a serious injury	0.09 [0.02]***	103%	0.09 [0.02]***	103%	0.10 [0.03]***	115%

Notes:
Each entry represents a separate regression
All variables defined and described in Table 1
Treatment is binary and equals 1 if ever abducted and 0 otherwise
The percentage change (%Δ) is calculated as the ATE relative to the mean value for non-abducted youth
Robust standard errors in brackets, clustered by sampling unit (location and abduction status)
* significant at 10%; ** significant at 5%; *** significant at 1%

† Controls in the OLS regressions include age and location dummies, age/location interactions, and pre-treatment individual and household characteristics. Weighted by inverse sampling probability and inverse attrition probability.

‡ Controls in the WLS regressions include age and location dummies, age/location interactions, and pre-treatment individual and household characteristics. Weighted by inverse sampling probability, inverse attrition probability, and inverse propensity score

§ Matching estimates match once for each treatment and control, matching exactly on age group and location, and within age/location cells on age. Weighted by inverse sampling probability, inverse attrition probability, and inverse propensity score

Table 5: Robustness of the WLS treatment effects to observable covariates and weighting schemes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	WLS estimates of ATE under alternative specifications							
Dependent variable	**WLS specification (from Table 3)**	**Minus household pre-war covariates**	**Minus year & location of birth**	**Minus attrition weights**	**Minus selection weights**	**Add year and location of birth**	**Add attrition weights**	**Add household pre-war covariates**
Years of education	-0.78 [0.14]***	-0.79 [0.15]***	-0.87 [0.42]**	-0.96 [0.46]**	-0.45 [0.39]	-0.82 [0.19]***	-0.72 [0.15]***	-0.79 [0.14]***
Indicator for functional literacy	-0.16 [0.03]***	-0.16 [0.03]***	-0.17 [0.06]***	-0.17 [0.06]***	-0.11 [0.04]***	-0.14 [0.03]***	-0.15 [0.03]***	-0.15 [0.03]***
Log (Daily wage)	-0.22 [0.12]*	-0.24 [0.15]	-0.19 [0.15]	-0.25 [0.16]	-0.13 [0.13]	-0.28 [0.13]**	-0.16 [0.13]	-0.22 [0.14]
Indicator for hostility	0.03 [0.01]***	0.03 [0.01]***	0.03 [0.02]*	0.04 [0.02]*	0.03 [0.02]	0.02 [0.01]	0.01 [0.01]	0.03 [0.01]***
Index of psychological distress	0.51 [0.19]***	0.48 [0.19]**	0.44 [0.27]	0.61 [0.25]**	0.67 [0.21]***	0.62 [0.16]***	0.48 [0.20]**	0.56 [0.19]***
Controls and weights employed in estimation:								
Control for pre-war household characteristics	×							×
Control for year and location of birth	×	×				×	×	×
Weights on inverse attrition probability	×	×	×				×	×
Weights on inverse selection probability	×	×	×	×				
Weights on inverse sampling probability	×	×	×	×	×	×	×	×

Notes:
Each coefficient represents a separate WLS regression, each row reporesents a different dependent variable, and each column represents an alternative specification
Treatment is binary and equals 1 if ever abducted and 0 otherwise
Robust standard errors in brackets, clustered by sampling unit (location and abduction status)
* significant at 10%; ** significant at 5%; *** significant at 1%

Table 6: Variation in the average treatment effect by age of abduction

Dependent variable	WLS coeffcient on age of abduction	Obs	R^2
Educational & Labor Market Outcomes			
Educational attainment	0.38 [0.07]***	462	0.45
Indicator for functional literacy	0.03 [0.01]**	462	0.38
Indicator for any employment in past month	-0.01 [0.02]	462	0.33
Indicator for capital- or skill-intensive work	0.01 [0.01]**	462	0.41
Log (Daily wage)	-0.05 [0.05]	288	0.49
Psychosocial & Health Outcomes			
Indicator for physical fights	0.00 [0.01]	462	0.24
Indicator for hostility	0.00 [0.01]	462	0.31
Index of social support	0.02 [0.07]	462	0.38
Index of psychological distress	-0.13 [0.11]	462	0.33
Indicator for a serious injury	-0.02 [0.01]	462	0.30

Notes:
Regression only includes formerly abducted respondents
Each row represents a separate regression
Robust standard errors in brackets, clustered by sampling unit (location and abduction status)
* significant at 10%; ** significant at 5%; *** significant at 1%
All estimates are weighted by inverse sampling, attrition, and selection probabilities
Controls include year and location of birth dummies, pre-war characteristics, and year of return dummies.

Table 7: Treatment effect bounding for selective attrition

	(1)	(2)	(3)	(4)	(5)
	% Missing Data†		Treatment effect bounds§		
Dependent variable	**Non-Abd**	**Abd**	**Untrimmed ATE‡**	**"Best case" attrition bound**	**"Worst case" attrition bound**
Year of education	14%	23%	-0.73 [0.20]***	-1.31 [0.24]***	-0.05 [0.24]
Indicator for functional literacy	14%	23%	-0.13 [0.03]***	-0.22 [0.04]***	-0.12 [0.03]***
Indicator for any employment in past month	14%	23%	0.07 [0.03]**	0.12 [0.04]***	0.00 [0.04]
Indicator for capital or skill-intensive work	31%	30%	-0.04 [0.02]**	-0.04 [0.04]**	-0.04 [0.04]
Log (Daily wage)	58%	56%	-0.23 [0.12]**	-0.38 [0.15]**	-0.08 [0.16]
Indicator for physical fights	31%	30%	0.00 [0.02]	0.00 [0.02]	0.00 [0.03]
Indicator for hostility	31%	30%	0.03 [0.02]*	0.03 [0.02]*	0.02 [0.03]
Index of social support	31%	30%	-0.30 [0.19]	-0.31 [0.26]	-0.30 [0.24]
Index of psychological distress	31%	30%	0.52 [0.19]**	0.52 [0.23]**	0.50 [0.36]
Indicator for a serious injury	34%	36%	0.10 [0.04]**	0.14 [0.06]**	0.08 [0.05]

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 attritors 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 mising 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 weighted means of the abducted and non-abducted groups, and is not a regression estimate. No control variables are used.
§ Best and worst-case bounds are calculated as the difference in the weighted 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 regression estimates.

Table 8: WLS estimates of the impact of abduction length and violence on outcomes

	(1)	(2)	(3)	(4)
	WLS coeffcient on			
Dependent variable	**Index of violence**	**Years abducted**	**Obs**	**R^2**
Educational & Labor Market Outcomes				
Educational attainment	0.01 [0.024]	-0.25 [0.124]**	459	0.50
Indicator for functional literacy	0.00 [0.006]	-0.09 [0.036]**	459	0.45
Indicator for any employment in past month	0.00 [0.007]	-0.02 [0.036]	459	0.36
Indicator for capital- or skill-intensive work	0.00 [0.003]	-0.03 [0.017]*	459	0.46
Log (Daily wage)	-0.02 [0.017]	-0.17 [0.106]	286	0.57
Psychosocial & Health Outcomes				
Indicator for physical fights	0.00 [0.003]	0.01 [0.023]	459	0.29
Indicator for hostility	0.00 [0.004]	-0.03 [0.014]*	459	0.36
Index of social support	0.07 [0.025]**	-0.11 [0.158]	459	0.44
Index of psychological distress	0.19 [0.028]***	0.04 [0.153]	459	0.41
Indicator for a serious injury	0.02 [0.004]***	0.05 [0.020]**	459	0.42

Notes:
Each row represents a separate regression
Robust standard errors in brackets, clustered by location
All estimates are weighted by inverse sampling, attrition, and selection probabilities
Controls include year and location of birth dummies, pre-war characteristics, year of return dummies, and abduction age.
Non-abducted youth are omitted from the regression
* significant at 10%; ** significant at 5%; *** significant at 1%

Table 9: The correlates of wages and the decomposition of the wage ATE

	(1)	(2)	(3)	(4)
Independent variable	**Log(Wage)†**	**ATE (Table 3)**	**(1) × (2)**	**% of Wage ATE‡**
Years of education attained	0.16 [0.02]***	-0.78	-0.12	-57%
Years experience§	0.06 [0.02]***	-0.68§	-0.04	-19%
Index of social support	0.04 [0.02]	-0.24	-0.01	-4%
Indicator for serious injury	-0.38 [0.16]**	0.09	-0.03	-16%
Index of psychological distress	-0.02 [0.02]	0.51	-0.01	-5%
Observations	448			
R-squared	0.17			

Robust standard errors in brackets, clustered by sampling unit (location and abduction status)
* significant at 10%; ** significant at 5%; *** significant at 1%

† This column represents a rough decomposition of wages into the components of human capital: education, experience, social capital, and health. It is calculated as a regression of log wages on measures of human capital, weighted by inverse sample and attrition probabilities. The constant term omitted from table.

‡ Calculated as the result in column (3) divided by the WLS wage ATE from Table 3 (0.22).

§ Experience is calculated as age – years of education – 6. Since there is no defined ATE for Experience, the figure used represents the average abduction length (0.68 years).

Appendices

Appendix A: A model for assessing ATE sensitivity to unobserved covariates

Following Imbens (UBoS, 2003), a simple parametric model for analyzing the sensitivity of a constant treatment effect, τ, to an unobserved covariate, U, is one that postulates a simple binomial distribution for U, a logistic conditional distribution for treatment assignment, T, given U and a vector of pre-treatment variables, X, and finally a normal conditional distribution of the outcome, Y, given U and X:

$$U \sim B(1, 1/2)$$

$$\Pr(T = 1 \mid X, U) = \frac{\exp(\gamma' X + \alpha U)}{1 + \exp(\gamma' X + \alpha U)}$$

$$Y(T) \mid X, U \sim N(\tau T + \beta X + \delta U, \sigma^2)$$

A more general model might allow for covariation between U and X, but as this would reduce the influence of the unobserved covariate, the simpler model offers the more exacting test of the unconfoundedness assumption, and is therefore the one this paper will pursue.

The advantage of this model is that the correlations between U and T and between U and Y are completely summarized by the parameter set (a, δ). For a fixed parameter set, we can estimate $\tau(a, \delta)$ via maximum likelihood. Specifically, we denote $L(\tau, \beta, \sigma^2, \gamma, a, \delta)$ the logarithm of the likelihood function:

$$\sum_{i-1}^{N} \ln \left[\begin{array}{l} \frac{1}{2}\left(\frac{1}{\sqrt{2\pi\sigma^2}}\right) \times \exp\left(-\frac{1}{2\sigma^2}\left(Y_i - \tau T_i - \beta' X_i\right)^2\right) \times \frac{\exp(\gamma' X_i)}{1 + \exp(\gamma' X_i)} \\ + \frac{1}{2}\left(\frac{1}{\sqrt{2\pi\sigma^2}}\right) \times \exp\left(-\frac{1}{2\sigma^2}\left(Y_i - \tau T_i - \beta' X_i - \delta\right)^2\right) \times \frac{\exp(\gamma' X_i + \alpha)}{1 + \exp(\gamma' X_i + \alpha)} \end{array} \right].$$

The sensitivity parameters a and δ do not have an easy interpretation, but they can be transformed into two more easily interpretable quantities. First, the proportion of the previously unexplained variation in Y that is explained by the unobserved covariate U can be represented by $\tilde{R}_Y^2(\alpha, \delta)$—the partial R^2-statistic obtained from adding the hypothetical covariate with fixed (a, δ) to the outcome regression:

$$\tilde{R}_Y^2(\alpha, \delta) = \frac{\hat{\sigma}(0,0) - \hat{\sigma}(\alpha, \delta)}{\hat{\sigma}(0,0)}.$$

This amount is simply the relative change in the unexplained sum of squares from adding U to the outcome regression.

Second, the proportion of the previously unexplained variation in the logistic latent index model, $\Pr(T = 1 \mid X, U)$, that is explained by the unobserved covariate U can be represented by the term $\tilde{R}_T^2(\alpha,\delta)$—the partial R²-statistic obtained from adding the hypothetical covariate with fixed (a,δ) to the outcome regression:

$$\tilde{R}_T^2(\alpha,\delta) = \frac{\hat{\psi}(\alpha,\delta) - \hat{\psi}(0,0)}{\hat{\psi}(0,0)},$$

where $\hat{\psi}(\alpha,\delta)$ represents the unexplained sum of squares in the latent index regression.[25] This procedure is implemented in Figure 5 for the education outcome, and is described in the text.

Appendix B: The impact of war on non-abducted youth

The treatment effects estimated in Table 4 identified the incremental impact of conscription on already war-affected youth. The impact of war on non-abducted youth is not known, but could be estimated if we possessed a comparable sample of youth outside the war zone. Unfortunately, a valid counterfactual is not available. We can turn, however, to national survey data for a very rough assessment of the impact of war. The 2002/03 Uganda National Household Survey (e.g. Collier, 1999; Ghobarah et al., 2003) collected data on more than 8000 youth, excluding the war-affected (Acholi) districts. Three education measures (years of schooling, enrolment, and illiteracy) and three household asset indicators (mobile phone, bicycle, and radio ownership) were measured by both the national survey and the survey conducted by the author.

Age-adjusted mean differences between non-abducted youth in the Acholi region (from the author's dataset) and youth in four other Uganda regions (other Northern districts, as well as Uganda's Central, Eastern and Western regions) suggest that economically Acholi youth are substantially behind their Ugandan peers, but educationally remain roughly on par. Appendix Table 1 displays the mean of each education and asset measure for non-abducted Acholi youth in the survey sample, as well as the results of a regression of the measure on indicators for each region and age. The coefficients on each region thus indicate the age-adjusted mean difference between Acholi youth and non-Acholi youth. We see that educational attainment among the Acholi sample appears to be higher than in Central and Western Region, comparable to the other Northern districts, and less than in Eastern region (Column 1). School enrolment is higher and illiteracy is lower in the Acholi sample than in all other regions, however (Columns 2 and 3). The results may be driven by difficulties and biases in cross-regional and cross-dataset

[25] This is actually a slight over-simplification. There is in fact no natural R^2 or partial R^2 for the treatment indicator regression, and in fact Imbens uses the explained variation in the latent index in a latent index representation.

comparison. They also do not account for school quality differences, which may be large. They are nevertheless consistent with the explanation that, with war's diminishment of economic opportunities, youth in Acholiland may have elected to remain longer in school. At the very least, the war does not appear to have set Acholi youth far behind their Ugandan peers.

In terms of wealth, the data suggest that Acholi youth have access to fewer assets. Mobile phone and bicycle ownership are mildly lower in the Acholi sample (Columns 4 and 5) while radio access is dramatically lower (Column 6). Unfortunately, national earnings data for youth are not available.

Appendix Table 1: Education and wealth differences between non-abducted Acholi and other Ugandan youth

	(1)	(2)	(3)	(4)	(5)	(6)
	Education meaures			**Asset ownership indicator**		
	Years education	**Indicator for school enrolment**	**Illiteracy indicator**	**Mobile phone**	**Bicycle**	**Radio**
Non-abducted sample mean (Acholi region)	7.13	0.67	0.08	0.14	0.45	0.36
Age-adjusted mean in non-Acholi regions (from 2002/03 UNHS) relative to Acholi non-abducted sample mean:						
Non-Acholi north	-0.16	-0.03	0.07	-0.01	0.00	0.24
	[0.14]	[0.02]**	[0.02]***	[0.02]	[0.02]	[0.02]***
Central region	-0.45	-0.19	0.05	0.05	0.04	0.49
	[0.14]***	[0.02]***	[0.02]***	[0.02]***	[0.02]**	[0.02]***
Eastern region	0.27	-0.03	0.08	0.04	-0.02	0.39
	[0.14]*	[0.02]**	[0.02]***	[0.01]**	[0.02]	[0.02]***
Western region	-0.46	-0.14	0.04	0.01	-0.03	0.43
	[0.14]***	[0.02]***	[0.02]***	[0.02]	[0.02]	[0.02]***
Observations	8683	8692	5112	8819	8820	8820
R-squared	0.12	0.45	0.03	0.01	0.02	0.17

Standard errors in brackets
* significant at 10%; ** significant at 5%; *** significant at 1%
Non-Acholi data come from the 2002/03 Uganda National Household Survey, which due to the conflict excluded the Acholi region.
The mean differences come from a regression of the dependent variable on regional dummies and dummies for age at the time of survey.

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