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. Little is known, however, about the impacts of military service 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 '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 human capital and productivity: schooling falls by nearly a year, skilled employment halves, and earnings drop by a third. These impacts are highly robust to relaxations of the assumption of exogenous conscription. Effects are greatest for child soldiers, who lose the most education. Only a minority evidence psychological and social problems. Our findings parallel those from other forms of child labor and related schooling interruptions.

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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 and Gurr, 2005]. Some of these conflicts involve up to a third of male youth in active combat, many of whom are children. In fact, children were believed to be serving in 72 armed forces in 20 countries as of 2002 [Achverina and Reich, 2006]. Thus soldiering may be one of the world's most common professions as well as one of the worst forms of child labor.

Little is known, however, about the long term impacts of participation in armed groups. With so many millions of ex-combatants, identifying the nature, magnitude and distribution of these impacts is a critical concern. Human capital can takes years to re-accumulate (assuming it rebounds at all) and with so many young ex-combatants, any interruption of education or damage to health could hinder a nation's productivity and growth for decades to come. Moreover, any impact of military service on inequality, aggression and political alienation could threaten a nation's stability and growth. Rational choice theories of conflict suggest that poverty increases the likelihood of conflict, as individuals have relatively more to gain from soldiering when peacetime economic opportunities are poor [Sambanis, 2004; Walter, 2004; Grossman & Kim, 1995]. Others argue that inequality (such as that arising between ex- and non-combatants) leads to greater discontent and, ultimately, upheaval [Gurr 1970]. The security concerns surrounding child soldiers are particularly widespread. The French foreign minister, a keynote speaker at a 2007 child soldiering conference, warned that former child soldiers are "a time bomb that threatens stability and growth in Africa and beyond." They are "lost children," he argued, "lost for peace and lost for the development of their countries" [BBC News, 2007]. The view that young ex-combatants are traumatized and potentially dangerous is one that pervades not only the press but post-conflict policy as well.

Little evidence exists to support or deny any of these claims, however. A principal reason is the paucity of data in war zones. To overcome this problem, we conducted a large-scale survey of young men and boys during a war in northern Uganda. Over the past two decades an unpopular rebel group has abducted tens of thousands of male youth to serve in their insurgency. More than 1000 households and 741 youth (including 462 former recruits) were interviewed by the authors and a team of local research assistants. To be sure to account for those youth that did not survive or who migrated away, respondents were sampled from pre-war household rosters. Out-migrants were tracked across the country, and data were gathered on the unfound and non-surviving youth from surviving family.

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. This paper uses arguably exogenous variation in rebel recruitment practices to attempt to overcome the usual problems of selection into armed groups. The "ideal" research design would be one where rebel participation was randomly assigned. Evidence presented in this paper suggests that rebel recruitment in northern Uganda resembles such a terrible case. The survey identifies eight rural areas where recruitment was large-scale, forcible, and seemingly indiscriminate. With no popular support, small groups of rebels raided rural homesteads for supplies and recruits, abducting all young men and boys they encountered. Testimony from rebel leaders and abductees suggests that there was no self-selection into the armed group and little systematic selection by the armed group itself, other than by their year and location of birth. The survey 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 in Uganda. Levels of these traits are identical across abducted and non-abducted youth, however, and they are individually and jointly insignificant in predicting the probably of abduction.

Under the assumption that rebel abduction is unconfounded—that is, independent of outcomes after controlling for pre-treatment characteristics—average treatment effects can be estimated using non-combatants of the same age and location as counterfactuals for the abducted youth. The main results suggest that the most pervasive impact of abduction is upon education and earnings, impacts that come as a consequence of time spent with the rebel group in the bush rather in school or acquiring employment experience. Both the average length of abduction and the average loss of schooling are almost exactly equal, at roughly nine months. This schooling loss is largest among those abducted at younger ages, as they are more likely to have their education interrupted and appear less likely to return to school after abduction. Former abductees are also more than twice as likely to have suffered a serious injury. Altogether these human capital deficits reduce by nearly half the likelihood that a former abducted youth is later engaged in skilled work, and reduce by a third the average wage earned.

In contrast, we find little support for the prevailing view that brutal military service leads to broad-based psychological distress, social exclusion, and aggression. The average psychological impacts for combatants appear mild to moderate, with serious distress concentrated in the minority that personally experienced extreme violence—roughly a sixth of abductees. Moreover, the average impacts of abduction on aggression and social exclusion are weak or non-existent, and community acceptance is high. Finally, as detailed in Blattman [2007b], political participation seems to increase as a result of abduction; former abductees are one quarter more likely to vote and are twice as likely to hold a minor community leadership position. Few "time bombs" and "lost children" are in evidence.

A central concern, of course, is whether our assumption of unconfoundedness is realistic. While observed selection is negligible, unobserved sources of selection may still remain, biasing the results. To explore the sensitivity of the our causal effects to confounding variables, the analysis explicitly relaxes unconfoundedness by allowing for a limited amount of correlation between treatment assignment (i.e. abduction) and unobserved components of the outcomes, using methods proposed by Imbens [2003] and Rosenbaum and Rubin [1983]. 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 would not account for more than a fraction of the estimated causal effects.

A final concern is what broader relevance these findings have beyond the reintegration of forcible recruits in Africa. In this regard it is worth noting that the main conclusions—long term declines in economic performance due to interrupted human capital—are strikingly similar to studies of former Sierra Leonean guerrillas and U.S. veterans. Moreover, the resiliency of youth to war violence is echoed by the psychological literature on ex-combatants as well as refugees. Finally, the conclusions appear to have much in common with other interruptions of schooling, from child labor to China's Cultural Revolution.

Today, with peace on the horizon in Uganda, international donors are preparing to spend hundreds of millions of dollars on post-war aid. Current reintegration programs are principally psychological and social in their focus ('psychosocial' in aid lingo)—an approach common to youth post-conflict programs worldwide [CSUCS, 2005; ILO, 2003; Machel, 1996; Cohn and Goodwin-Gill, 1994] The evidence from Uganda suggests that increased attention to closing the economic and educational gaps between former combatants and civilians should be a priority of post-conflict development policy.

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 and 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 [Branch, 2005; Behrend, 1999]. Thus from 1991 onwards, Kony's war was waged not only against the government but against the Acholi people.

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 since 1996 has been large-scale and seemingly indiscriminate. Annan, Blattman and Horton [2006] estimate more than 60,000 youth have been abducted at some time. Most have been taken since 1996, and nearly all abductees come from one of the three Acholi districts: Gulu, Kitgum, and Pader (see Figure 1). Blattman [2007a] provides evidence that adolescent males were the most pliable, reliable and effective forcible recruits, and so were disproportionately targeted by the rebels. Age was the only given criteria for selection into the armed group, with youth under 10 and over 24 avoided (see Figure 2). Youth were typically abducted by small roving groups of rebels conducting night raids on rural homesteads. There are almost no accounts of youth voluntarily joining the LRA.

Lengths of abduction ranged from a day to ten years, with half taken for at least four months. The vast majority of those taken were tied and beaten, and a fifth was forced to commit great acts of violence. Youth who failed to escape were trained as fighters and, after a few months, were given a gun for raiding. Roughly a quarter of abductees eventually became fighters. Many of these are forced to beat or murder civilians—perhaps even their own friends or families—in order to bind them to the group, to reduce their fear of killing, and to demonstrate the consequences of escape. Four-fifths eventually escape, almost always during an unsupervised moment (such as in the heat of battle). The remainder can, tragically, be assumed perished as relatively few remain with the LRA at this time. A blanket Amnesty has been granted to all "returnees" and self-reported acceptance rates back into the community are high.

B. Popular perceptions of abduction's consequences

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 rebels. Resources have been concentrated on what is seen to be the most crucial issue: 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 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 [Annan, 2007]. According to the same youth, "I also used to see my sister. She would appear to me when she was worried about me. And when she comes, tears just roll down my cheeks."

The perception that former child soldiers are traumatized, socially dislocated, and potentially aggressive is common. One story that has attained the status of rural legend in Uganda is that of a former abductee who returns to his mother and calmly kills her with a knife when asked what he learned while with the rebels. Such perceptions are not limited to Uganda. The United Nations envoy for children and armed conflict recently expressed her concern that, in the absence of any intervention, former child soldiers may "grow up to become a lost generation of migrant professional killers." [O'Neill, 2007]

Interviews conducted by the author and a psychologist provide little support for these beliefs, however. Interviews suggest that the average psychological consequences of abduction are modest, aggression low, and social reintegration strong [Annan, 2007; Annan et al., 2006]. In the minds of many youth and their families it is the interruption of education and employment that is their 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".

The resulting education gap appears to have had serious labor market consequences. According to a village 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 activities require little capital or skill (e.g. collecting firewood), while others require minor capital (e.g. hawking clothes), moderate capital and skill (e.g. a bicycle taxi or brick-making), or substantial capital and skills (e.g. tailoring or construction). This labor market is highly entrepreneurial, and as youth accumulate skills and capital they quickly move to higher productivity occupations. For example, one youth interviewed explained that, before displacement, he had been making charcoal from discarded wood. As he accumulated profits, he purchased a bicycle and began a taxi service. With savings from this activity he educated himself in a trade, and later started a small enterprise. Overall, the interview evidence suggests that abduction interrupts this slow accumulation of skills and capital, trapping youth in low-productivity employment.

III. Existing evidence

What little is known about the impact of combat on youth comes largely from interview-based, ethnographic case studies [e.g. Honwana, 2006; Shepler, 2005; Cohn and Goodwin-Gill, 1994]. Just two studies, by Allen and Schomerus [2006] and ILO [2003], collect data on large (albeit non-representative) samples of combatants under 18. The general conclusion that emerges from this literature is the prevalence of psychological trauma and social dislocation among former child soldiers. Economic and political outcomes are seldom measured, however, and so the relative impact is unclear.

A large medical literature has also studied the psychological impact of war violence on youth, focusing especially on the prevalence of disorders such as depression, post-traumatic stress disorder (PTSD), and aggression.[1] For instance, in northern Uganda, MacMullin and Loughry [2002] found that abducted youth were more anxious, depressed, hostile, and socially inactive than the non-abducted. Yet while in-

[1] For instance, see Dyregrove et al. [2002], Margolis & Gordis [2000], Mollica et al. [1997], Ajdukovic and Ajdukovic [1998], Husain et al. [1998], and Kinzie et al. [1986].

sightful, such studies seldom use representative samples or address attrition and selection bias. Moreover, only psychological and social measures are typically explored. Finally, few employ comparison groups, often implicitly using zero symptoms of disorders as the counterfactual outcome.

Economists and political scientists have meanwhile produced little theory or evidence on the individual economic and political consequences of violent conflict. Economists have focused on the country-level impacts for physical capital [Collier, 1999] as well as institutions and social capital [Bellows and Miguel, 2006]. Political scientists, meanwhile, have concentrated on the consequences for macro-level institutional formation [Herbst, 2000; Tilly, 1992; Zartman, 1975] and mortality [Ghobarah et al., 2003].

Only a handful of studies have tackled micro-level impacts. A small economics literature has estimated the impact of military service on U.S veterans, using exogenous variation in recruitment practices to overcome the problems of self-selection and screening [e.g. Angrist, 1998, 1990; Angrist and Krueger, 1994]. Angrist [1990] finds that white veterans saw a 15 percent reduction in their earnings as a result of involuntary recruitment—an impact due, he argues, to work experience lost. Higher levels of mortality and drug use have also been noted among veterans [Hearst et al., 1990, 1986]. It is unclear, however, whether these developed-country studies generalize to combatants in African civil wars.

The sole survey-based study of the impact of military service in civil war comes from Humphreys and Weinstein [2005], in Sierra Leone. The authors identify exogenous variation in violence experienced and find a negative impact on post-conflict social and economic reintegration. Without a non-combatant comparison group, however, our understanding of the impact of military service remains incomplete.

IV. 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.[2] 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 clusters (by sub-county) with ex-combatants over-sampled. A similar survey of females is currently underway.

The survey sought to select its respondents from a sample frame of youth living in the region before the conflict in order to minimize sample attrition due to the heightened migration and mortality in war.

[2] For details of SWAY, including copies of the questionnaires and the dataset, see www.sway-uganda.org.

We sampled 1100 households from United Nations World Food Programme lists compiled in 2002, and 92.5 percent of household heads were found and interviewed.[3] 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 recalled as the date of the first election since 1980, and because it predates 85 percent of local abductions. The sample was then drawn from this retrospective roster of youth.

Migrants were then followed across the country to their current location. Of the 870 surviving male youth sampled from the retrospective household rosters, 41 percent had moved since 1996. 741 of these youth (or 84 percent) were located, including virtually all non-migrants and 70 percent of migrants. We conducted an absentee questionnaire with the families of all 129 unfound youth, collecting extensive education, employment, family, and abduction data in order to adjust for observed attrition. Demographic data were collected on the 349 youth from the retrospective rosters that had died since 1996.[4]

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 Harvard Trauma Questionnaire, a widely used scale, and the Northern Uganda Child and Youth Psychosocial Adjustment Scales, a questionnaire specifically designed for northern Uganda. Both scales were adapted to the context by one of the authors [see Annan, 2007].[6]

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[3] Potential selection arises from the 7.5 percent of households not located, as well as from the fact that the sample frame dates from 2002 (by which time many households may have had the opportunity to out-migrate). The potential for bias is very difficult to assess, but there are several reasons to believe that it is not a serious concern. First, a third to a half of the 7.5 percent of unidentified households are thought to be "ghost" households—false names placed on the United Nations lists that allow a community leader to collect double rations. Second, interviews with community leaders suggest that very few households left the region entirely before 2002—most left family members (especially parents) behind, who remain on the lists. Many migrants also took pains to get onto these lists in 2002 even when away in order to increase the food rations going to their families. These individuals would thus be captured on the retrospective rosters. Finally, as will be demonstrated below, attrition among sampled youth appears to have been relatively unselective, and corrections for observable sources of attrition have a negligible impact on the findings. It seems plausible (although nevertheless unknowable) that household attrition is similarly unselective.

[4] Those youth who have not returned from abduction were presumed perished, since very few abductees remain with the LRA.

[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] These indices can be constructed in a number of different ways, including additively or through a data reduction technique such as principal components or factor analysis. The results reported in this paper are highly robust to the method of index construction. To avoid the cherry-picking of indices, this paper employed a simple rule of thumb for all index variables based on common practice in the psychosocial literature [e.g. Fabrigar et al., 1999]. Questions were selected for inclusion in an index

V. 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, recruitment into the armed group. A 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. In the context of military service, we are concerned that pre-war differences among youth influence both long-term outcomes as well as selection into the armed group (whether self-selection or selection by the armed group itself).

To deal with such potential endogeneity, one 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, 2006; Rosenbaum and Rubin, 1983a; Rubin, 1978]. The evidence below suggests that LRA abduction presents just such an unlikely case. If so, consistent ordinary least squares (OLS) estimates of the ATE can be calculated under the assumption of conditional unconfoundedness. A more efficient and consistent approach than OLS, however, is a weighted least squares (WLS) regression with weighting on the inverse of a nonparametric estimate of the propensity score [Hirano et al., 2000]. This estimation method is illustrated in Appendix A.

The assumption of conditional unconfoundedness requires careful assessment of the potential for self-selection as well as intended and unintended selection by the LRA itself. Interviews with LRA leaders and survey data 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.

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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.

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, nearly all 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 predict participation in armed groups throughout Africa [Honwana, 2006; Humphreys and Weinstein, 2006; Cohn and Goodwin-Gill, 1994]. 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 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 p-value of 0.18 (not statistically significant).

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

Even if abduction is independent of potential outcomes, any association between the likelihood of attrition and potential outcomes can still bias any estimates of the ATE. In this study, there are two main types of 'attritors': non-survivors and 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 [e.g. Fitzgerald et al, 1998, Falaris 2003]. The tracking success rate of this study, 84 percent, meets or exceeds the rates achieved by several 'gold-standard' youth tracking surveys in poor countries [e.g. Hamory and Miguel, 2006; Duncan et al., 2001]. Even so, differential attrition rates by treatment status still raise some concern; mortality rates were double among the abducted, while out-migration rates were double among the non-abducted.

To correct for attrition on observables, enumerators collected demographic data and data on current activities and well-being from the surviving family members of any attritors. Following Fitzgerald et al.

[1998], these data were used to calculate attrition probabilities, and regression estimates are weighted by the inverse of these attrition probabilities, π_i, to eliminate bias from attrition on observed traits.[7] Even with this correction, however, there remains a risk of bias arising from any unobserved traits that influence survival, abduction, and potential outcomes. In the following section the paper bounds the ATE with best- and worst-case scenarios to see if the estimates are robust to such potential bias.

VI. The Average Impact of Abduction

Under the assumption that rebel abduction is unconfounded, we calculate treatment effects for ten outcomes in Table 4. OLS estimates are listed in Column 1, where each entry represents a separate regression. Semi-parametric WLS estimates, which are as consistent but more efficient than OLS, are displayed in Column 3. Finally, non-parametric matching estimates are displayed in Column 5.[8] In each case, the even-numbered columns calculate the magnitude of the ATE relative to the performance of non-abducted youth (listed in Table 1).

A. Educational and labor market impacts

Education lost from abduction is not only substantial, but is nearly equal in duration to the average length of abduction. The abducted have 0.78 years less schooling by WLS and 0.73 fewer years by the matching technique. These estimates correspond closely to the average length of abduction—eight months, or 0.68 of a year. With levels of education among non-abducted youth only seven years on average, abduction implies an 11 percent average reduction in educational attainment (Table 4, Column 4).

The WLS and matching results also suggest that the abducted are 16 or 17 percentage points less likely to report being functionally literate—that is, 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

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[7] 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:

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

[8] Following the methodology described above, the regression results flexibly control for year and location of birth with dummy variables for year and location, as well as year/location interactions. Also included as controls are quartic terms for each of the pre-treatment household characteristics. 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.

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 and matching estimates both suggest 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.

The wage ATE implies that the abducted are also less productive. The wage distribution is skewed, and so the results in Table 4 employ a log transformation of wages. 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. The matching estimator, however, makes no parametric assumptions about the distribution of the residual, and is more accurate given the continued skew in the residuals. It suggests that wages are 32 percent lower for the formerly abducted, a result that is significant at the 1 percent level.[9]

B. 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 indicating whether the youth had been in a physical fight in the previous six months (9 percent of youth in total) and another indicating self-reported aggressive behavior such as being quarrelsome, threatening others, disrespecting others' property, and using abusive language (6 percent). The results in Table 4 offer mixed evidence on aggression. While abducted respondents were no more likely to have been in a fight in the past 6 months, they were 3 percentage points more likely to report other hostile behaviors and attitudes—69 percent more likely than non-abducted

[9] 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 [Lee, 2005; Heckman, 1979]. 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. Still, the wage ATE must be interpreted as the ATE conditional on having found employment

youth. While these results may indicate greater hostility among abductees, higher self-reported hostility could simply 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 socialization into violence, or at least its admission, is a consequence of abduction.

Abductees exhibit little evidence of social exclusion, however. Social exclusion was measured by an additive index of 14 forms of support received in the previous month (such as someone lending you items, comforting you when sad, or helping you find work). The impact of abduction on this index of social exclusion appears to be small and not statistically significant (Table 4). Moreover, in results not displayed, the abducted are also no more or less likely to report membership in a church, school or community organization. Furthermore, as illustrated in Blattman [2007b], the abducted are much more likely to participate in community and political life, being more than twice a likely to be a minor community leader and a quarter more likely to have voted in a recent referendum.

Finally, the psychological impacts of abduction appear to be moderate. The index of psychological distress is based on an additive index of 19 symptoms of depression and traumatic stress, scaled between zero and one according to their reported intensity.[10] The average youth has an index value of 4.2; the highest index value is 16. The WLS and matching estimates of the ATE indicate that abducted youth exhibit a half-point increase in their index—an increase of roughly 13 percent relative to the non-abducted average, or 0.68 standard deviations. The measurement and interpretation of such a result is admittedly challenging. Analysis presented in Annan [2007] suggests that the average level of psychological distress in this population is mild to moderate, and that the average difference between abducted and non-abducted youth is relatively modest.

Yet while the average psychological impact is not large, the most traumatized youth appear to be disproportionately former abductees. For instance, in regressions not displayed, the abducted are nearly three times as likely to have a measure of distress above the 75th percentile (i.e. 6 or more reported symptoms). These findings suggest that we should be interested in differences in the *distribution* of psychological outcomes, especially the top tail. The top tail in this case represents severe symptoms of emotional distress [Annan, 2007]. Re-experiencing traumatic events through nightmares and flashbacks figure strongly in the index, and 20 to 25 percent of former abductees report such symptoms (versus 10 to 15

[10] For each of the 19 symptoms, "often" receives a full value of 1, "sometimes" 0.66, "rarely" 0.33, and "never" a zero.

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 appears much ‘fatter’ than that of the non-abducted, implying a greater concentration of moderate to serious mental disorders. One way to see this is to compare the conditional distributions of distress of abducted and non-abducted youth at different quantiles (via a least-absolute deviations regression of distress on an abduction indicator and indicators for year and location of birth). In regressions not shown, the median abducted youth exhibits a distress score that is 0.66 points greater than the median non-abducted youth (who has a median score of 3), a difference that is significant at the 5 percent level. An abducted youth at the 90th percentile of his distribution, however, reports 2 greater symptoms of distress than a non-abducted youth in the same position, also significant at the 5 percent level. In-depth interviews of a sub-sample of 30 of these youth and their family and community members suggests that these youth in the upper tail exhibit serious impairment in their functioning because of symptoms such as chronic nightmares, flashbacks, withdrawal, and hyper-arousal [Annan, 2007].

Could the relatively moderate average psychological impact and the absence of an adverse social impact be explained simply by the success of the aid and reintegration programs provided to former abductees? If so, our results would underestimate the causal impact of war and violence. This scenario seems unlikely, however. Unfortunately, reintegration programs have been modest in scale and reach. Only half of abducted youth passed through the formal care system [Annan et al., 2006]. Moreover, abductees who did pass through the centers received only basic medical treatment, assistance with family reunification, and “counseling”—in essence advice-sharing from relatively untrained social workers. Only 1 in 10 youth were later followed up. The estimates presented in this paper thus reflect the impact of combat conditional on half of abducted youth receiving only the most rudimentary reintegration services.

C. Interpretation of the treatment effects

Abduction has so far been handled as a binary treatment. This approach, however, obscures the diversity of military experiences. In Uganda, abduction length ranged from a day to ten years, and violence varied dramatically. When treatment is heterogeneous the binary ATE can be interpreted as the average per-unit effect along a response function mapping treatment exposure (e.g. length or violence) to outcomes [Angrist and Imbens, 1995]. One might prefer, however, to estimate the entire response function.

One way to approximate this function is to examine the association between outcomes and units of exposure—results that will be reviewed in Section 7.

Treatment exposure, however, is likely to be endogenous. Longer or more violent abductions, while idiosyncratic to some degree, are undoubtedly related to unobserved individual traits. While OLS estimates of the response function will therefore be inconsistent, the above ATEs are not. Rather, potentially endogenous exposure requires a more nuanced 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.

Finally, note that both abducted and non-abducted youth are adversely affected by the war, and so the ATEs capture the incremental effect of conscription—a bundle of time away and added violence experienced. The disparity between war-exposed youth and unexposed youth is a different impact altogether, and is not addressed by this study. National survey data, however, 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 similar to that in other areas of the country, although household assets are significantly lower. These data are discussed in Appendix C.

D. Child versus adult combatants

As the sample includes youth abducted at all ages, we can 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.[11]

The regression results, displayed in Table 5, suggest that older abductees are better educated, with 0.4 additional years of education for every extra year of age before abduction. Older abductees also appear more likely to be engaged in skilled work today. Such findings are intuitive—child abductees are more

[11] Dummy variables for year of return are included in the regression because of arise in the average age of abduction over the conflict. Evidence in Blattman [2007a] and Annan et al. [2006] suggests that as the LRA expanded operations and abductions they widened the pool of abductees by age. Thus, all other things equal, younger abductees have been back from abduction longer than older ones. Since length of time back is associated with greater reintegration, the coefficients on age of abduction in this simple regression may be biased towards zero. A continuous "time back" variable yields similar results. Note that selection bias by age is not an obvious concern. Just as abduction is uncorrelated with the pre-war covariates, age of abduction is also independent of all observables Blattman [2007a].

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.[12] 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.

While education and skill-intensity are associated with age of abduction, there is curiously little significant relationship between log wages and abduction age. With more education and a higher likelihood of skilled employment, we might expect that adult returnees would exhibit higher wages. The point estimate, however, is negative and statistically insignificant. 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.

E. Robustness to alternative specifications

The above results are robust to changes in specification, as seen in Table 6. The WLS estimates of the ATE from Table 4 are reproduced in Column 1 of Table 6 for five outcomes: education, literacy, log wages, hostility, and distress. We can examine the robustness of the ATE to dropping the pre-war household characteristics (Column 2), further dropping the age and location dummy variables (Column 3), and further still dropping the weights correcting for attrition (Column 4) and selection (Column 5).

In general the coefficients on each outcome vary relatively little as a consequence of the changes. The estimated ATEs are nearly identical to the original WLS results, save for education and wages when we omit all weights and controls (Column 5). The drop in the education and wage coefficients suggests that there is some selection on observable traits, even though we cannot statistically reject equality of the coefficients. If there is selection on observed traits, however, it is almost entirely selection on year and loca-

[12] The solid line is calculated from a locally-weighted regression of an indicator for returning to school after abduction on the age of return with a bandwidth of 0.5.

tion 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 pre-war household characteristics to the regressions (Column 8) yields the OLS estimates from Table 4. Overall, the relative consistency of ATEs (after accounting for age) strengthens confidence in the unconfoundedness assumption.

VII. Sensitivity Analysis

The greatest remaining concern is the potential for unobserved selection into the rebel group. 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 mainly worried about bias that leads to overestimation of the ATEs—bias that would arise from the systematic selection of "low-types" into the rebel group, or from differentially greater death or attrition of "high-type" abductees.

A. Sensitivity to violations of unconfoundedness

To assess the potential for unobserved selection bias, we can explicitly model relaxations of the unconfoundedness assumption. A first method of sensitivity analysis, suggested by Imbens [2003], starts from the observation that to induce selection bias, an observed covariate, X, must be correlated with both treatment assignment, T, and the outcome of interest, Y. Moreover, X must be sufficiently correlated with both T and Y to induce a degree of bias worthy of concern. The same argument applies to any unobserved covariate, U. Therefore, to assess the sensitivity of the ATE to a hypothetical U with known distribution, we can calculate the combinations of correlation between U and T and between U and Y that would lead the estimated treatment to be biased by a fixed amount. We can then judge whether the existence and influence of such a hypothetical covariate is plausible by benchmarking it against observed covariates. This analysis employs a parametric model that postulates a simple binomial distribution for U, a logistic conditional distribution for treatment, and a normal conditional distribution for the outcome. The maximum likelihood model is illustrated in Appendix B.

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 the covariate in question to a regression of education on all other covariates. The horizontal axis indicates the influence of each covariate in explaining additional variation in abduction. With the exception of age and location, the observed covariates explain little variation in either Y or T—a fact which accounts for the unresponsiveness of the ATE to their exclusion or inclusion in Table 6.

The downward sloping 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 U in question is modeled as a binomial variable independent of all other covariates. The curve mapped out in Figure 5 is therefore a threshold, beyond which the hypothetical U is influential enough to reduce the treatment effect by a significant amount. 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.[13] Moreover, the traits that normally influence military recruitment such as household wealth or orphaning lie far beneath the threshold. To cross it, an unobserved covariate would need to be roughly 40 times more influential than orphaning, and five times more influential than all the household asset measures combined. Even then the ATE would only be reduced by half. This sensitivity analysis thus suggests that moderate amounts of unobserved selection are unlikely to account for the treatment effects observed.

B. Bounding the treatment effect for selective survival

A second method of sensitivity analysis can be examined, one suited to accounting for any bias from selective attrition. In a method proposed by Lee [2005], "best-case" and "worst-case" scenarios for differential attrition are constructed by trimming the distribution of the outcome in the group with less attrition, in this case the non-abducted (Table 7). The worst case scenario bound is calculated by dropping those with the highest values of the outcome and calculating the 'trimmed' ATE. The best-case bound is likewise calculated by dropping the worst-performing non-abducted youth. The greatest amount of data is missing from wages, since we do not observe wages for the unemployed (Columns 1 and 2). The least data is missing for schooling and injuries, since these data were collected on unfound migrants from their families. Lee's method compares the untrimmed ATE (Column 3) to the trimmed means—the best and

[13] Since year of birth influences treatment assignment mechanically (because abduction levels changed year to year) as well as explicitly (because rebel abduction parties targeted adolescents) the distance of the Year of Birth variable from the origin in Figure 5 likely overstates the role of age as an explicit selection criterion by rebels. The selection induced by rebel attempts to abduct only specific age grups is likely closer to the threshold, not further away.

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. However, not one of these lower bounds changes sign, implying even under austere assumptions, abduction has the predicted effect on outcomes.

VIII. Channel of Impact

Reduced-form estimation of treatment effects, while informative, tell us little about why these impacts are observed. The interview evidence (reviewed in an earlier section) suggests that it is time away from human capital accumulation rather than violent trauma that accounts for the general decline in economic performance observed among former LRA recruits. Causal identification of this channel of impact is unfortunately not possible due largely to a problem of under-identification—we have only one source of exogenous variation (abduction) and many potentially endogenous channels. Even so, the combined evidence—the correlates of each outcome, patterns of treatment effect heterogeneity, and the response to heterogeneous treatments—are supportive of some channels of impact 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. Control variables include pre-treatment covariates, age of abduction, and indicators for the year of return. The index of violence is a linear additive index of 12 traumatic events reported by respondents. According to these regressions, an additional incident of war trauma is associated with a 0.34 point increase in the index of distress. The average abductee reported 7 of the events—2.2 more violent events than non-abducted youth (Table 1). This difference implies a distress ATE of $2.2 \times 0.34 = 0.75$—greater (but statistically indistinguishable) from the actual ATE of 0.51. This relationship is only suggestive, however, as the coefficients in Table 8 are vulnerable to omitted variable bias from unobserved determinants of both violence and distress. These 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.

Meanwhile, other evidence suggests that the adverse educational and labor market impacts are most consistent with a simple 'time away' or human capital accumulation channel and have little to do with violent trauma. That is, military skills and experience obtained in the LRA appear to be poor substitutes for civilian schooling and work experience. 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 0.31 years less education, significant at the one 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.[14] 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 suggest 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.[15]

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

[14] See Mincer [1974] for the theoretical justification of this function. *Experience* is calculated as *Age* – 6 – *Education*.
[15] 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 [e.g. Card, 1999; Ashenfelter et al., 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.

(Table 9, Columns 2 and 3).[16] 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).[17] 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.

IX. 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 traumatization concentrated in those that experience the most violence.

These findings can be juxtaposed against current policy in Uganda, where funding has been directed largely towards the psychosocial reintegration of former child soldiers. The largest and most prevalent impact of abduction, however, 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: 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.

Implicit in these policy proposals, however, is the assumption that governmental and non-governmental organizations can address the gaps in education and employment at least as effectively as the psychosocial gaps. If not, then focusing limited resources on psychosocial assistance would remain a sensible aid strategy. Little evidence on the relatively effectiveness of post-conflict educational, eco-

[16] Since an ATE is not defined for the experience term (which is simply age minus years education minus 6), the average length of abduction is used in place of an ATE in Column 2.

[17] 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 little significant relationship between distress and the logarithm of wage (see Table 9).

nomic, and psychosocial interventions exists—a situation which suggests that one of the most important avenues of future research is post-conflict program evaluation. In the meantime, however, with increased donor funding to northern Uganda such hard choices probably do not need to be made.

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 and 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. The link between increased exposure to violence and higher emotional distress has been identified among youth in settings as diverse as Iraq [Dyregrove et al., 2002], Cambodia [Mollica et al., 1998], Rwanda [Dyregrove et al., 2000], and Croatia [Ajdukovic & Ajdukovic, 1998]. The low average levels of distress are also consistent with some psychological literature which emphasizes the resiliency of youth to extreme stressors [e.g. Masten, 2001]; however, there is a wide variation in reports of symptom prevalence for war-affected populations [Holifield et al., 2002].

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. Abducted children in Uganda have not been held accountable for their actions as soldiers, and have been welcomed home with open arms. This remarkable community response most likely mutes the economic and psychological impacts. Globally a third of child soldiers are thought to be forcibly recruited [ILO, 2003]. For the other two thirds of child soldiers, 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 [Beegle et al., 2005] and Tanzania [Beegle et al., 2007] suggest that a doubling of hours employed reduces school enrolment by nearly a third and educational attainment by six percent. 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.

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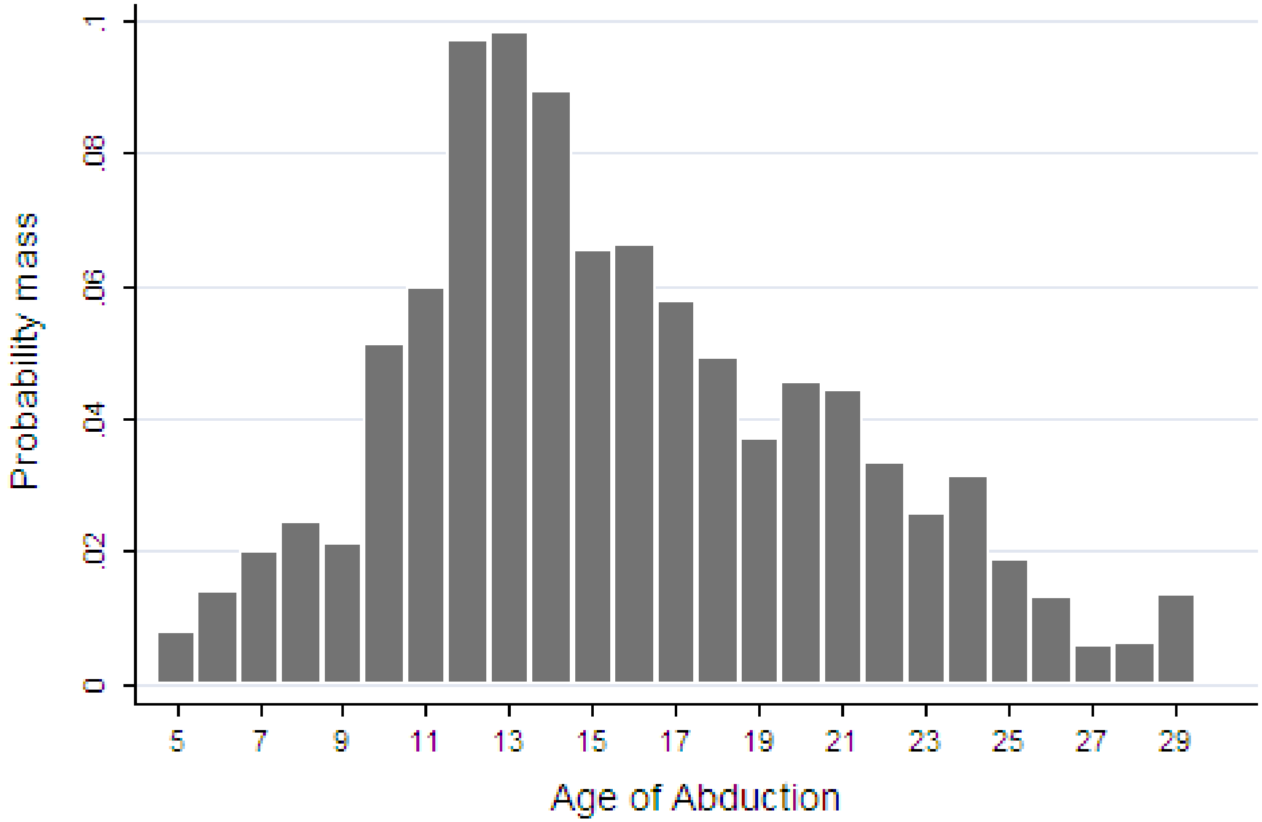
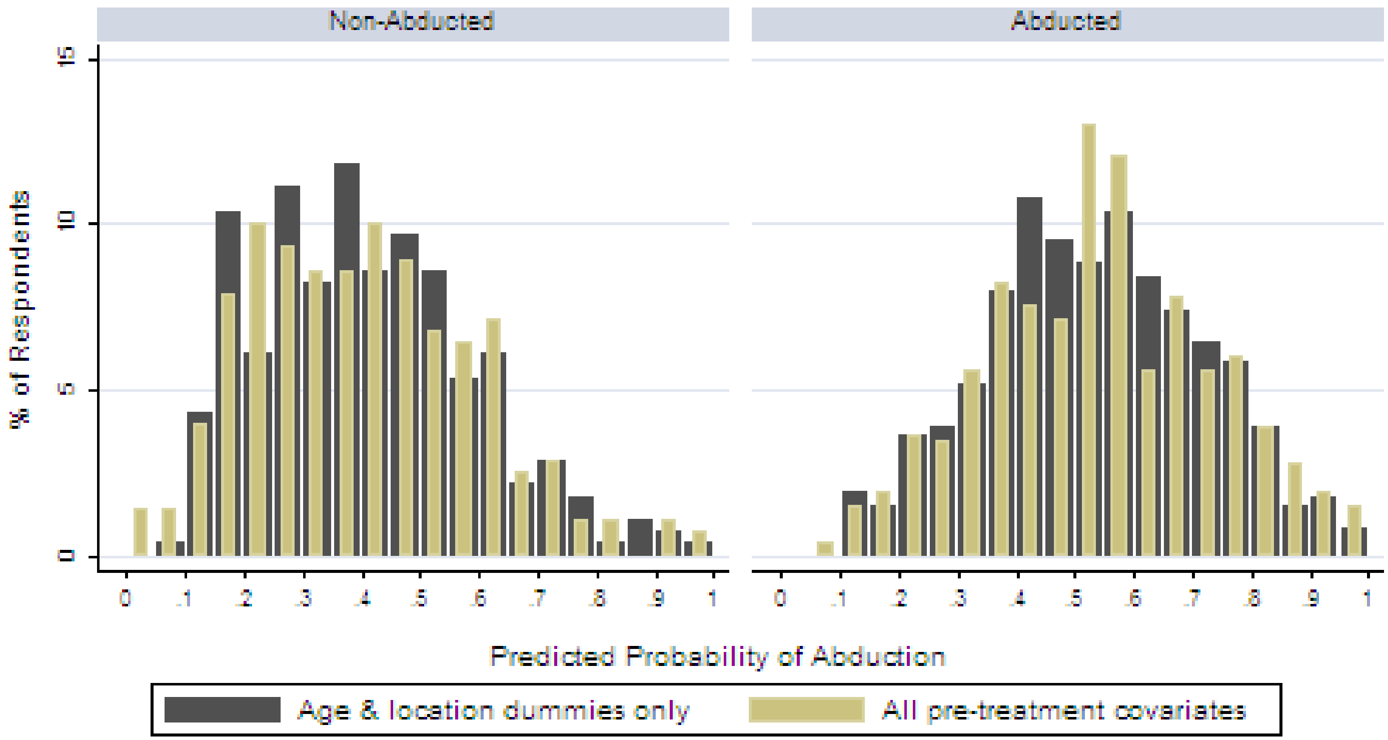
Figure 1: Map of Uganda and survey field site

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



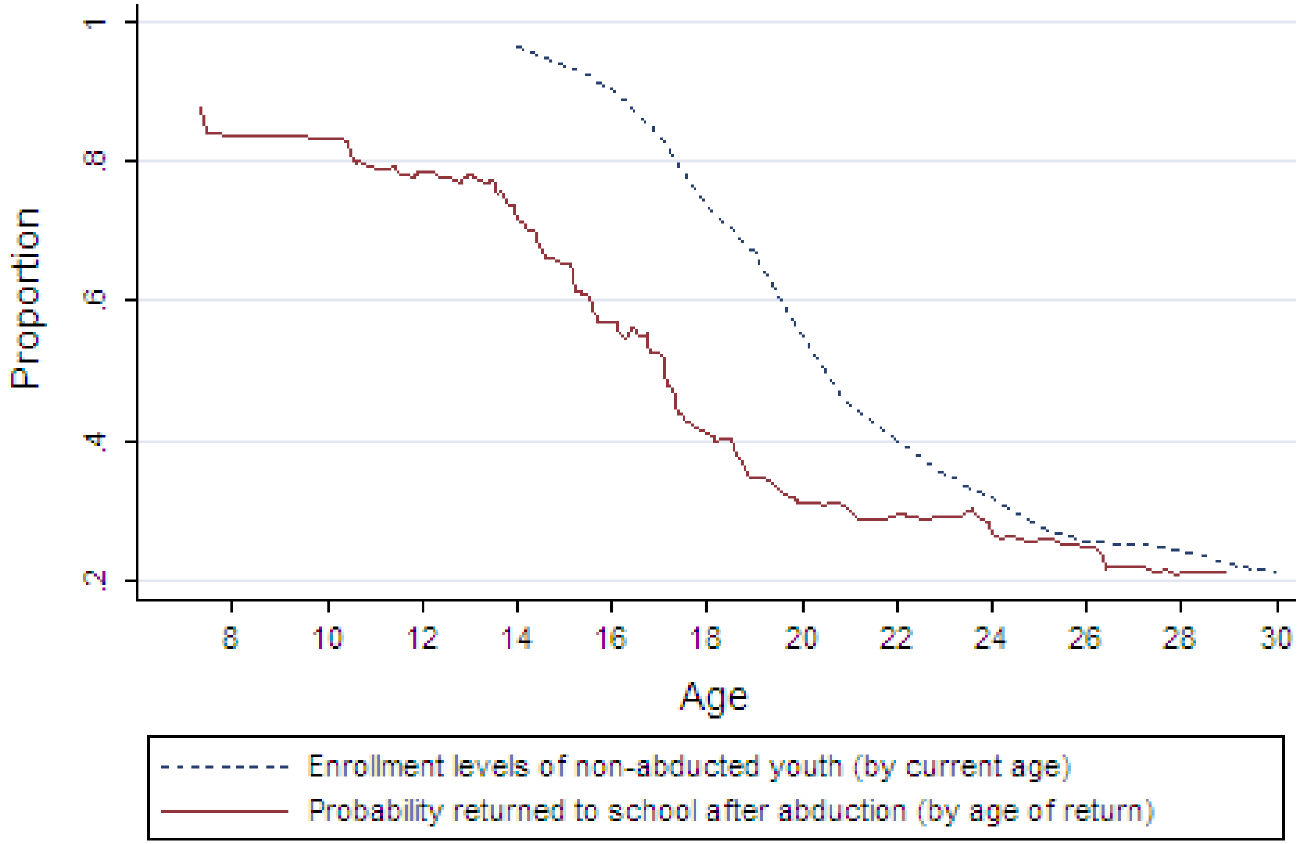
Notes: The bars represent a probability mass function for age at the time of abduction, and so sum to one. The data include absentee youth and youth who have since died or did not return from abduction (collected from the household survey). Where an individual was abducted more than once, all abductions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

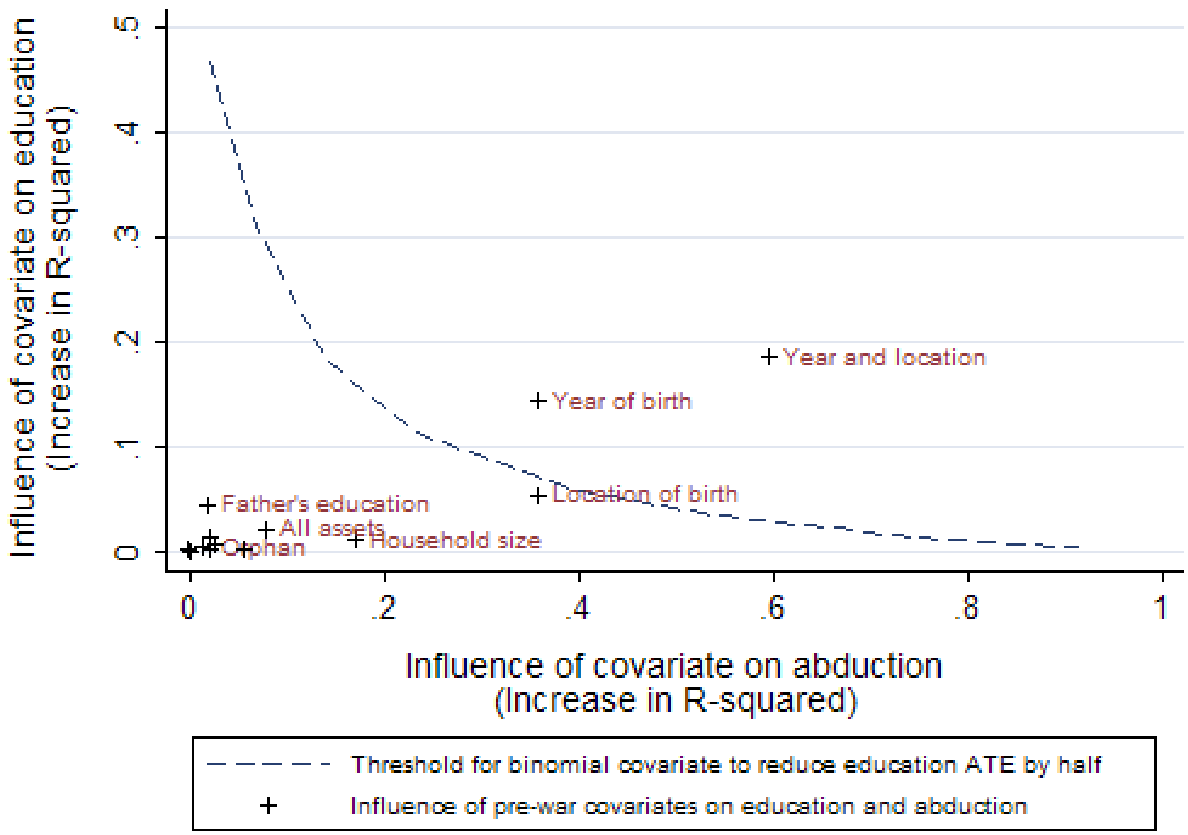
Figure 4: Probability of returning to school after abduction, by age of abduction



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					
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 12 indicators of violence. Three of the 741 respondents declined to respond.	4.3 [3.4]	6.9 [3.1]	2.2 [2.1]	738
Education & Labor Market Outcomes					
Years of education	Number of years of education (including tertiary and vocational training)	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 ocupation 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 perrforming 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						
Years of education	-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
‡ 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 scor
§ 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: 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			
Years of education	0.39 [0.08]***	462	0.39
Indicator for functional literacy	0.04 [0.01]***	462	0.3
Indicator for any employment in past month	-0.01 [0.02]	462	0.26
Indicator for capital- or skill-intensive work	0.01 [0.01]*	462	0.28
Log (Daily wage)	-0.05 [0.04]	288	0.41
Psychosocial & Health Outcomes			
Indicator for physical fights	0.00 [0.01]	462	0.19
Indicator for hostility	0.00 [0.01]	462	0.24
Index of social support	0.00 [0.06]	462	0.3
Index of psychological distress	-0.16 [0.11]	462	0.27
Indicator for a serious injury	-0.02 [0.01]*	462	0.21

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 6: 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 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)
Dependent variable	**WLS coeffcient on**		**Obs**	$\mathbf{R^2}$
	Index of violence	**Years abducted**		
Educational & Labor Market Outcomes				
Years of education	0.01	-0.31	459	0.48
	[0.04]	[0.09]***		
Indicator for functional literacy	-0.01	-0.09	459	0.40
	[0.01]	[0.03]***		
Indicator for any employment in past month	-0.01	0.00	459	0.34
	[0.01]	[0.03]		
Indicator for capital- or skill-intensive work	0.00	-0.02	459	0.42
	[0.00]	[0.02]		
Log (Daily wage)	-0.06	-0.20	286	0.54
	[0.03]**	[0.11]*		
Psychosocial & Health Outcomes				
Indicator for physical fights	0.01	0.01	459	0.31
	[0.00]*	[0.02]		
Indicator for hostility	0.00	-0.01	459	0.32
	[0.01]	[0.01]		
Index of social support	0.02	-0.02	459	0.37
	[0.03]	[0.13]		
Index of psychological distress	0.34	0.09	459	0.37
	[0.05]***	[0.17]		
Indicator for a serious injury	0.03	0.05	459	0.37
	[0.01]***	[0.02]**		

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: Semi-parametric WLS estimation of the average treatment effect

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.[20]

One can alternatively estimate the ATE non-parametrically using a matching estimator weighted by the inverse selection probabilities [Abadie and 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 $T_i = 0$.

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

Following Imbens [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:

$$U \sim B(1, \tfrac{1}{2})$$

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

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

[20] A series estimator for the propensity score achieves the efficiency bound [Hirano et al., 2000]. It requires linear regression of treatment assignment on each covariate in X. Those covariates that pass a threshold t-statistic (e.g. 1.00) 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., 2000].

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(τ, β, σ^2, γ, a, δ) 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 $\tilde{R}^2_Y(\alpha,\delta)$—the partial R^2-statistic obtained from adding the hypothetical covariate with fixed (a, δ) to the outcome regression:

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

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 | X, U), that is explained by the unobserved covariate U can be represented by the term $\tilde{R}^2_T(\alpha,\delta)$—the partial R^2-statistic obtained from adding the hypothetical covariate with fixed (a,δ) to the outcome regression:

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

where $\hat{\psi}(\alpha,\delta)$ represents the unexplained sum of squares in the latent index regression.[21] This procedure is implemented in Figure 5 for the education outcome, and is described in the text.

[21] This is actually a slight over-simplification. As detailed in Imbens (2003), there is in fact no natural R^2 or partial R^2 for the treatment indicator regression, and in fact he uses the explained variation in the latent index in a latent index representation.

Appendix C: 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 (UBS, 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 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.