The Consequences of Child Soldiering*

March 2009

Forthcoming in the *Review of Economics and Statistics*

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* **Acknowledgements:** Macartan Humphreys, Edward Miguel, Chalmer Thompson, and Jeremy Weinstein deserve special thanks for their input and guidance. For comments we also thank David Albouy, Tim Allen, Matias Cattaneo, Bryan Graham, Chang-Tai Hsieh, Guido Imbens, Pamela Jakiela, Seema Jayachandran, Stathis Kalyvas, Kory Kroft, David Lee, David Leonard, David Lynch, Lauren Morris-MacLean, Devin Pope, Gerard Roland, Amos Sawyer, Thomas Sexton, Harvey Weinstein, our anonymous referees, and numerous seminar participants. For data collection we thank Roger Horton, Okot Godfrey, and our field research assistants. Robert Blair provided excellent research assistance. Logistical support was supplied by AVSI Uganda and UNICEF Uganda. Military escorts were provided by the Uganda People's Defense Force. Financial support was primarily received from UNICEF, the John D. and Catherine T. MacArthur Foundation, the United States Institute of Peace, and the Harry Frank Guggenheim Foundation.

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Abstract:

Civil conflicts have afflicted a third of all nations and two thirds of Africa since 1991, in some cases drawing a third of male youth into the conflict. Little is known, however, about the impacts of military service on human capital and labor market outcomes due to an absence of data as well as sample selection: recruits are usually self-selected and screened, and may also selectively survive. We assess the impacts of participation in civil war using an original survey from Uganda, where a rebel group's recruitment method provides arguably exogenous variation in conscription. The economic and educational effects are widespread and persistent: schooling falls by nearly a year, skilled employment halves, and earnings drop by a third. Educational losses are closely associated with time away, suggesting that military service is a poor substitute for civilian education. Meanwhile, we find that those reporting the most psychological distress have been exposed to the most severe war violence and are disproportionately, but not only, former combatants.

1. Introduction

This paper assesses the impact of combat on the human capital of Ugandan youth, the consequences for lifetime labor market performance, and lessons for the economic recovery of civil war-torn countries. Civil conflict has afflicted a third of all nations and two thirds of Africa since 1991 (Marshall and Gurr 2005). The recovery of children and young adults is a critical concern in these post-war economies; lost education can take years to regain, and physical and psychological trauma may be long-lasting. Some of these conflicts involve up to a third of male youth in active combat, many of whom are children under 18. With so many millions of young ex-combatants, injuries to human capital could hinder a nation's productivity and growth for decades. Moreover, any impact of military service on inequality, aggression and alienation could threaten a nation's long term stability.

The effects of combat are uncertain. The dominant view holds that these youth are traumatized, violent, social pariahs. Speaking at a 2007 U.N. conference, the French foreign minister warned that young ex-combatants are "a time bomb that threatens stability and growth, …lost for peace and lost for the development of their countries" (BBC 2007). Former child soldiers in particular are "damaged, uneducated pariahs," says a recent *New York Times* editorial (NYT 2006). A growing body of ethnographic evidence portrays another view, however, finding that resilience rather than disabling psychological trauma is the norm (e.g. Shepler 2005; Boothby et al. 2006; Wessells 2006).

Virtually no representative data or well-identified causal estimates exist to judge either set of claims (Blattman and Miguel forthcoming). A small literature has found large and persistent earnings and mortality gaps between veterans and non-veterans in the U.S. and Europe (e.g. Hearst et al. 1986; Angrist 1990; Angrist and Krueger 1994; Imbens and van der Klaauw 1995; Angrist 1998). For instance, Angrist (1990) finds that white American males conscripted into the Vietnam War saw a 15% reduction in their long-term earnings due to work experience lost, while Costa and Kahn (forthcoming) use data on U.S. Civil War veterans to link wartime stress to higher mortality later in life. The impacts of combat, however, are not uniformly bad; both U.S. studies, for instance, find

that military service increased the human capital and lifetime earnings of black Americans—sad evidence of their otherwise poor alternatives.

Unfortunately, these findings don't generalize easily to the developing countries where most civil wars rage. They also draw on datasets with a limited range of outcomes, and so the full nature of the impact (and the causal channel) is hard to see. The sole survey-based study of the impact of civil war combat comes from a pioneering survey of Sierra Leonean ex-combatants by Humphreys and Weinstein (2004; 2007). They find that increased exposure to violence is associated with lower community acceptance, but not with employability. Without a non-combatant comparison group, however, our understanding of the impact of military service remains incomplete.

One reason we know so little about these impacts is the paucity of data in war zones. To overcome this problem, we conducted a survey during the war in northern Uganda, where for 20 years an unpopular rebel group has forcibly recruited tens of thousands of youth.

A second challenge in identifying the causal effects of military service is selection: ex-fighters are usually a select group, including those who chose to join and those screened by the armed force. The "ideal" research design would be one where rebel participation was randomly or exogenously assigned. We argue that forced recruitment in Uganda resembles just such a terrible case. Under the assumption that abduction is conditionally unconfounded, causal impacts can be estimated using non-combatants of the same year and place of birth as counterfactuals. Naturally, unobserved sources of selection and survival could bias the results. Hence we explicitly model the sensitivity of the treatment effects to unobserved selection to show that moderate to large amounts of selective abduction and attrition cannot account for the causal effects nor change our general conclusions.

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

The data also support a growing body of ethnographic evidence that finds ex-soldiers to be socially and psychologically resilient. Community acceptance of former abductees is high, and they re-

port similar levels of social support as non-abductees. Abductees also exhibit little difference in aggression. Finally, we highlight evidence that formerly abducted youth do report 15% more symptoms of emotional distress, in large part because of exposure to more extreme violence (Annan and Blattman 2009). Considering the high incidence of violence received and perpetrated, this result speaks to a remarkable resilience among youth, even among those youth reporting the highest levels of distress—a conclusion is bolstered by in-depth interview evidence (Annan et al. 2008).

These results challenge the conventional assumptions about ex-combatants. Unfortunately our counterfactual—non-abducted youth in the war zone—does not help to identify the impact of war on non-combatants. Rather, our approach assesses the added impact of military service on youth already in a war zone. This incremental effect is important in order to address post-war gaps in reintegration, especially when so many policy-makers and aid agencies appear to be ignoring the largest gaps. We conclude with policy lessons for ex-combatant reintegration and post-conflict recovery.

2. Background

A. War and abduction in northern Uganda

Historically Uganda's economic power rested in the south, while political and military power came from the north (Omara-Otunnu 1994). In 1986, however, southern rebels overthrew a government and army dominated by a northern ethnic group, the Acholi. Several Acholi guerrilla forces resisted the takeover, but settled for peace or were defeated by 1988. A handful of these fighters refused to settle, however, and gathered under a spiritual leader named Joseph Kony to form the Lord's Resistance Army, or LRA (Doom and Vlassenroot 1999; Allen 2005). Like many armed group leaders in Africa, Kony is widely believed to possess spiritual powers. He claims to seek a spiritual cleansing of Uganda and theocratic rule.

The decision to continue fighting was an unpopular one, however, and the LRA received little public support. With few recruits and no material resources, the LRA immediately took to looting homes and abducting youth to maintain supplies and force. The Acholi populace, after three years of

such abductions and looting, began to join a government-sponsored defense militia in 1990. To punish this betrayal and dissuade further collaboration, in 1991 Kony ordered the widespread killing and mutilation of Acholi civilians, further alienating the population (Behrend 1999; Branch 2005).

LRA activity was initially low-scale, but in 1994 and 1995, in response to Uganda's support for Sudanese rebels, the Government of Sudan began supplying Kony with weapons and territory upon which to build bases. Sudan's support invigorated the LRA, and rebel attacks and abductions escalated dramatically after 1996.

Abduction was large-scale and seemingly indiscriminate; 60,000 to 80,000 youth are estimated to have been abducted (Annan et al. 2006; Pham et al. 2007) and more than a quarter of males currently aged 14 to 30 in our study region were abducted for at least two weeks (Table 1). Most were abducted after 1996 and from one of the Acholi districts of Gulu, Kitgum, and Pader (Figure 1).

Youth were typically taken by roving groups of 10 to 20 rebels during night raids on rural homes. Adolescent males appear to have been the most pliable, reliable and effective forced recruits, and so were disproportionately targeted by the LRA (Beber and Blattman 2008). Youth under age 11 and over 24 tended to be avoided, as seen in Figure 2, and had a high probability of immediate release. Lengths of abduction ranged from a day to ten years, averaging 8.9 months in our sample (Table 1). Youth who failed to escape were trained as fighters and, after a few months, received a gun. Two thirds of abductees were forced to perpetrate a crime or violence. A third eventually became fighters, and a fifth were forced to murder soldiers, civilians, or even family members in order to bind them to the group, to reduce their fear of killing, and to discourage disobedience.

Eighty-four percent of abductees eventually escaped, usually in an unsupervised moment such as the heat of battle. The remainder perished, as no more than 1000 abducted youth (about 1% of all abductees) are thought to remain with the LRA at this time. An Amnesty has been granted to all "returnees" and community acceptance rates are high; fewer than 2% report insults or fear from their community and family.

B. Current practice and evidence

The focus on trauma in the wake of war and disaster is pervasive, and youth post-conflict programs concentrate heavily on psychosocial care (e.g. Cohn and Goodwin-Gill 1994; Machel 1996; ILO 2003; CSUCS 2005; Wessells 2006). In Uganda, foreign aid has concentrated on psychosocial programs for former abductees—a focus driven by abundant anecdotal evidence of social rejection and trauma. For example, one youth we interviewed is haunted by being forced to kill his brother: "I started dreaming of him a week after the incident, and at times I would see him during the day. How I beat him would all re-surface." Frequently reliving such events through nightmares or flashbacks is a common symptom following traumatic events, and nightmares are one of the most commonly reported symptom of distress in the sample (Annan and Blattman 2009).

Some psychological studies emphasize high levels of traumatic stress among child soldiers. One Ugandan study compared abducted to non-abducted youth in the war zone and concluded that abducted youth were more anxious and depressed, more hostile, less pro-socially active, and less confident (MacMullin and Loughry 2002). A second study identified clinical post-traumatic stress in 97% of abductees (Derluyn et al. 2004). Both studies, however, used non-random 'convenience' samples and did not discuss potential selection effects. The validity of their conclusions is thus questionable.

In contrast, other literature on child and refugee mental health emphasizes the resiliency of victims of traumatic events, as well as the concentration of disabling trauma in a minority (Miller and Rasco 2004; Wessells 2006). Conclusions remain uncertain, however, because few studies consider endogeneity problems and measuring trauma and distress across cultures is challenging at best. Hollifield et al. (2002) review 394 studies of war trauma and conclude that most "are either descriptive or include quantitative data from instruments that have limited or untested validity and reliability."

Pre-survey qualitative field work suggested that disabling distress was the exception. Forty youth were selected for in-depth interviews and assessments, including multiple interviews of the youth, his family, friends, teachers, and co-workers. These interviews suggested that aggression was low and that social reintegration and psychological resilience were widespread (Annan et al. 2008). Symptoms

of distress that interfered with daily functioning—a defining element of psychological disorder—seemed to be concentrated in a minority, and were not limited to former abductees.

Rather, in the minds of many of abducted youth and their families, the interruption of education and employment was of greater concern. Youth complained of difficulty re-entering the school system, creating an education gap that in turn limited their options in the labor market. According to one 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."

Youth earn income mainly through small entrepreneurial activities. Some require little capital or skill (e.g. collecting firewood), while others require a little capital (hawking goods), moderate capital (a bicycle taxi), or substantial capital and skills (tailoring). This labor market is dynamic, and as youth accumulate skills and funds they shift to more productive work. One youth began making charcoal from discarded wood. With his profits he purchased a bicycle and began a taxi service, and with these profits in turn he educated himself and later opened a small store. Qualitatively, abduction appears to interrupt this accumulation of skills and capital, and thus retards productive employment.

3. Data and Measurement

In 2005 and 2006 we conducted Phase 1 of the Survey of War Affected Youth (SWAY)—a survey of 741 males born between 1975 and 1991 in one of eight rural sub-counties in the Districts of Kitgum and Pader.[3] To minimize attrition from migration and mortality, we tried to identify a representative sample of youth living in the eight sub-counties *before* the conflict. We randomly sampled 1,100 households from U.N. World Food Programme lists compiled in 2002. 93% of these households were found, and Acholi enumerators worked with household heads to develop a roster of

[3] Sub-counties include 25 to 100 villages and ranges from 10,000 to 40,000 people. The eight sub-counties represent roughly 10% of each District and 5% of all Acholi. These clusters were not selected randomly since insecurity limited our team to sub-counties that (i) could be reached within a 90-minute drive from Kitgum, and (ii) were visited regularly by our military escorts. We selected sub-counties of varying size that seemed representative of the region as a whole.

household members in 1996.[4] We chose the year 1996 because it was easily recalled as the date of the first election since 1980, and because it pre-dates 85% of local abductions. A sample of 870 surviving male youth was drawn from this retrospective roster. Abductees were over-sampled.

Of surviving males, 41% had moved since 1996, and enumerators attempted to track all migrants. 741 (or 84 percent) were located, including all non-migrants and 70% of migrants. We interviewed the families of all 129 unfound youth for data on abduction experiences and current outcomes, and collected demographic data on the 349 youth that had died or not returned from abduction.

The 741 youth that completed the survey provided data on their war experiences as well as current well-being and outcomes. Key variables are described in Table 1. Two aspects of these data are noteworthy. First, war experiences are self-reported and retrospective.[5] Second, the measures of violence, social integration, hostility and distress are additive indices of questions commonly used for measuring psychosocial well-being in conflict zones. Each is described in more detail below.

4. Empirical Strategy

We develop a comparison group of non-abducted youth in the war zone who for largely exogenous reasons were never abducted. As discussed below, we can interpret the difference in outcomes as the incremental effect of conscription in communities subjected to abduction. This counterfactual is a crucial one if we are interested in addressing reintegration gaps—that is, closing any inequality

[4] These lists are an approximate census of the population since the entire population in each sub-county was displaced and receiving food aid. Based on interviews with local leaders, we believe that most households left family members in the camp, in part to receive the food aid. Hence these household's youth would be included in the sample frame. We estimate 5% of households left entirely and so do not enter the sample frame, introducing unknown selection.

[5] We took several measures to guard against youth misrepresenting themselves as abductees (in the hopes of aid). We emphasized the absence of any link between the study and aid. Abduction data were also collected separately from the household head and irregularities were investigated. Finally, the survey asked more than 200 detailed questions on abduction. Only 5% of abductees raised suspicion and reclassifying these has no material impact on our conclusions.

between combatants and non-combatants, or providing reparations beyond that received by other war-affected populations. We discuss the effects of war on non-abducted youth in the conclusion.

A. Dealing with endogenous selection into the armed group

The fundamental empirical problem we face is that we cannot observe an ex-combatant's well-being in the absence of abduction. The standard solution is the counterfactual approach, where 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 (Rubin 1974; Imbens and Wooldridge 2008). The estimated ATE is only as reliable as the counterfactual, of course, and it will be unbiased only when abduction and potential outcomes are independent.

In the case of ex-combatants, we are concerned that current differences are the result of pre-war traits that led to selection into the armed group. To deal with such potential endogeneity, we look for situations where participation in the armed group is independent of outcomes conditional on observed pre-recruitment variables. LRA abduction presents just such an unlikely case.

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

Second, LRA officers explained that by neither design nor accident did they abduct a select group. Typical of East Africa, rural Acholi households live in relative isolation, in the midst of their fields rather than villages. This custom made them particularly vulnerable to the roving raiding parties. Rebels usually invaded homesteads at night, abducting all able-bodied civilians to carry loot. We interviewed roughly two dozen junior officers from the LRA who had returned under the Amnesty. According to these raiding party leaders, targets were generally unplanned and arbitrary; they raided whatever homesteads they encountered, regardless of wealth or other traits. In essence, their strategy

was to abduct first and sort out later. Junior officers were instructed to release young children and older adults, but to keep all adolescent and young adult males. Indeed, fewer than 5% of males abducted between the ages of 10 and 24 were released.

The survey data support these claims. We gathered retrospective data on pre-war levels of household wealth (land, livestock, and plows), and parent's education, occupation, and death—measures which predict abduction in armed groups elsewhere in Africa (e.g. Cohn and Goodwin-Gill 1994; Honwana 2005; Humphreys and Weinstein 2006). We observe little difference in pre-war traits between the abducted and non-abducted, as seen by the unconditional and conditional mean differences in Columns 3 and 4 of Table 2. None of the unconditional differences in means except year of birth are significant at even a 10% level, and nearly all differences are close to zero. Conditional mean differences, which control for all other pre-treatment covariates, are generally small as well.

Abducted and non-abducted youth differ only in mean year of birth and mean pre-war household size. This relationship between year of birth 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. The significance of household size, meanwhile, is driven by households greater than 25 in number. We believe that rebel raiders, who traveled in small bands, were less likely to raid large, difficult-to-control households.

The distribution of predicted probabilities of abduction based on pre-treatment data, in Figure 3, offer more evidence of unconfoundedness. Probabilities are predicted from a logit regression of abduction on indicators for year and sub-county of birth, as well as all pre-war household covariates. The predicted probabilities for abductees (the right-hand panel) and non-abductees (the left-hand panel) overlap substantially. The difference in the abducted and non-abducted distributions is driven exclusively by year and sub-county of birth, and adding other pre-war covariates leaves the distributions virtually undisturbed (an F-test of their joint significance yields a p-value of 0.40).

We can contrast LRA abduction to participation in Local Defense Units, or LDU—a militia under the Ugandan army command. Six percent of the sample is a current or past LDU member. Un-

like abduction, prewar traits predict LDU participation; militia members come from poorer and more agricultural households (Table 2, Columns 3 and 4), and an F-test of all household characteristics yields a p-value of 0.05. The coefficients in the LDU regressions are also much larger than when predicting abduction. Collectively, the results support the assumption of unconfounded abduction.

B. Dealing with selective attrition and survival

The tracking success rate of this study meets or exceeds the rates achieved by several 'gold-standard' panel surveys in poor countries (Thomas et al. 2001; Hamory and Miguel 2006). Nevertheless, there are three main types of 'attritors': 12.4% of the 1996 sample migrated and were not found; 7.5% died; 9.2% did not return from abduction (virtually all of whom can be presumed perished). Attrition patterns vary by treatment status: abductees are half as likely to be unfound migrants, twice as likely to have perished, and comprise all of those who did not return from abduction.

Attrition in large developing country panel surveys typically has little impact on coefficient estimates, even with attrition rates of 50% (Fitzgerald et al. 1998; Falaris 2003). Even so, we worry that attrition due to war deaths and non-return might be particularly selective. To correct for observable attrition we used the data collected from surviving and found household members to predict attrition probabilities (Fitzgerald et al. 1998).[6] All regression estimates are weighted by the inverse of these probabilities to eliminate bias from observed determinants of attrition.

Not all attrition determinants are observed. For instance, if abductees who died tended to be the weak or less clever, then the estimated ATEs will be biased towards zero. Moreover, those who remain with the armed group are likely missing more education and work experience than the average returnee, also contributing to potential under-estimation of our economic and educational ATEs. Accordingly, we employ a method of sensitivity analysis proposed by Lee (2005) whereby "best-

[6] Data on attritors usually come from a prior round of survey data, while in this case the data are provided by families. The third-party, retrospective nature of these data reduces the reliability of predicted attrition. The availability of data on current activity, however, is a distinct advantage over attrition-correction methods that rely on data from prior surveys.

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).

C. Estimation

Assuming conditional unconfoundedness, the most efficient and consistent approach to ATE estimation is a weighted least squares (WLS) regression with weighting on the inverse of a nonparametric estimate of the propensity score (Hirano et al. 2003). Under WLS the regression function for outcome Y is:

$$Y_i = \beta_0 + \tau \cdot T_i + X^S_i \cdot \beta_1 + \varepsilon_i, \tag{1}$$

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:

$$\omega_i = \omega(T_i, v_i, \rho_i) = \rho_i \cdot \pi_i \cdot \left(\frac{T_i}{\hat{e}(v_i)} + \frac{1 - T_i}{1 - \hat{e}(v_i)} \right). \tag{2}$$

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

Note that the ATE estimated in equation (1) will be biased if abduction has positive or negative externalities on non-abducted youth. We address this concern after seeing the results.

5. The Impacts of Abduction

A. Average treatment effects

Abduction ATEs for ten outcomes are listed in Table 3. Each entry in Column 1 represents the coefficient on an abduction indicator, Column 2 lists the mean level of each outcome among non-abducted youth, and Column 3 calculates the proportional impact of the ATE relative to the non-abducted mean.[7] All results are robust to alternative specifications and controls.[8]

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[7] We control for year and location (sub-county) of birth with dummy variables for year and location as well as year/location interactions. Also included are quartic terms for each pre-treatment household characteristic. Matching estimates match one-for-one exactly on location and four-year age intervals, followed by matching on specific age.

Educational and labor market impacts

Abducted male youth attain 0.75 fewer years of education, a 10% reduction relative to the average non-abducted youth's 7.6 years of education. This schooling loss corresponds closely to the average length of abduction—8.9 months, or 0.74 of a year. The abducted are also 15 percentage points less likely to report being functionally literate (i.e. able to read a book or newspaper), implying that abductees are nearly twice as likely to be illiterate than non-abductees.[9]

Labor market performance also suffers due to abduction, but in the quality of work rather than the quantity. Abducted and non-abducted youth display little difference in the probability of having any work. Work found by abductees, however, is of a lower skill and capital-intensity. Eight percent of all youth are engaged in a profession, a vocation, or own their own small business. Abducted youth, however, are five percentage points (or 43 percent) less likely than non-abducted youth to be engaged in such skilled work. The wage ATE also implies that the abducted are less productive. Wages are proxied by the total gross earnings reported in the previous four weeks, divided by days employed in the month. Using the log of this wage proxy, the ATE can be interpreted as the approximate percentage change in wages due to abduction. The results suggest that wages are 33 percent lower among abducted youth.[10]

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[8] Results are robust to: the removal of the selection-correction weights; the further exclusion of pre-war household characteristics; the removal of attrition-correction weights; and the exclusion of the age and location dummy variables (i.e. a simple difference of means between treatment and control groups). The results are also robust to the use of a non-parametric matching estimator

[9] This literacy ATE seems large given the ATE in schooling. In local primary schools, however, pupils learn to read only by their 6th or 7th year of school, and losing these crucial years dramatically increases illiteracy. For instance, looking at all youth in the sample, moving from 6 to 7 years of schooling is associated with a 22 percentage point increase in literacy.

[10] Wages are not observed for 237 unemployed youth (and log wages are undefined for 56 males with zero earnings). If abduction is associated with the propensity to be employed or earn nothing, we will conflate the direct impact of abduc-

Psychosocial outcomes

We adapt a measure of Social Support using an additive index of 14 concrete forms of support received from family and friends in the previous month (such as someone lending you things, praising you, giving you advice, or helping you find work), based on Barrera et al. (1981). The average youth reported 5.5 such forms of support. From Table 3, we see little substantive or statistically significant difference between abducted and non-abducted youth. Moreover, the abducted are just as likely to report membership in a church or community organization (results not displayed).

We also examine aggression. One risk of combat is that participants are socialized into violence. We measure aggression with two indicators, one for whether the youth reported being in a physical fight in the past six months (6.7 percent overall) and a second for self-reported aggressive behaviors such as being quarrelsome, threatening others, and using abusive language (7.2 percent). As seen in Table 3, abductees were slightly less likely to have been in a fight, although the result is not statistically significant. Abductees are, however, 3 percentage points more likely to report aggressive behaviors—40 percent greater than non-abducted youth. These results may indicate greater hostility among abductees. This result, however, is fragile and disappears in almost any other specification. Thus we conclude with great caution that moderate aggression results from abduction.

Not only is there little evidence that abductees are social pariahs, the opposite may be true. Blattman (2008) uses the same survey and empirical strategy to assess the political legacies of violent conflict. Former abductees are 28% more likely to have voted in a 2005 national referendum, are 106% more likely to be a community organizer (an elected position common among youth), and are 160% more likely to hold a position of political leadership.

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tion on wages with the indirect effects on the type of people employed (Heckman 1979; Lee 2005). Since abduction is uncorrelated with employment and the likelihood of zero earnings, such sample selection bias is likely immaterial.

Finally, we adapt an additive index of psychological distress using 19 self-reported symptoms of depression and anxiety (including traumatic stress).[11] The mental health measures and results, including risk and protective factors, are discussed in depth by Annan and Blattman (2009). We highlight key results here to provide a complete picture of abduction. The average youth has a score of 4 (on a potential scale of 19). The highest score in our sample is 15. No norms or scores to determine clinically significant levels of symptoms have been established in this population, and so the severity of the average index value is open to subjective interpretation. A score of 4 indicates a range of symptom profiles, from a youth experiencing approximately four symptoms (i.e., nightmares, difficulty concentrating) frequently to having thirteen symptoms of these symptoms on rare occasion.[12] On other standard scales, this number of symptoms falls below the clinical cut-off or in a mild to moderate range of depression or anxiety (Beck et al. 1988).

Abductees exhibit a 0.57 point increase in the distress index—an increase of roughly 15 percent relative to the non-abducted average of 3.8 (see Table 3). This ATE could imply an increase in the frequency of a single symptom of distress from 'rarely' to 'often', or the addition of two symptoms from 'rarely' to 'sometimes'. The effect size (mean difference divided by standard deviation) is 0.21, indicating that the mean of the abducted group is at approximately the 58th percentile of the non-abducted group and that there is a great deal of overlap between the two groups.

The youth that exhibit the most symptoms of distress, however, are disproportionately abductees. Abducted youth are 11 percentage points more likely to be in the top quartile of the distress

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[11] We adapt our scale from the Northern Ugandan Child and Youth Psychosocial Adjustment Scale (MacMullin and Loughry 2002). Each symptom is scaled between zero and one according to its reported intensity. For each symptom, "often" receives a value of 1, "sometimes" 0.66, "rarely" 0.33, and "never," 0. We construct the index of distress using factor analysis, and employ a standard procedure in the psychological literature: symptoms with factor loadings over 0.3 are included in the index additively (Fabrigar et al. 1999). Our results are robust to other index constructions.

[12] The six most commonly reported symptoms are "excessive worrying", "bad memories from the past", "difficulty concentrating", "finding life difficult", "crying when remembering bad memories", and "restless nights".

index—a 49 percent increase relative to the non-abducted. Nearly 37 percent of former abductees report re-experiencing traumatic events through nightmares, versus 25 percent of non-abducted youth. Furthermore, 16 percent of abductees report feeling "always sad", compared to 13 percent of their non-abducted peers.

These findings suggest that we should be interested in the distribution of psychological outcomes, especially the top tail. Figure 4 illustrates the difference in distress levels of abductees and non-abductees at each quantile of the conditional distribution of distress. This difference is equivalent to the horizontal distance between the age and location-adjusted distributions of distress for abducted and non-abducted youth at each percentile. The median abducted youth exhibits a distress score that is 0.57 points greater than the median non-abducted youth (who has a score of 3.3). An abducted youth at the 90th percentile of his distribution, however, reports 1.6 more symptoms of distress than a non-abducted youth in the same position.

B. Caveats

Several caveats are in order. First, abduction reflects a heterogeneous mix of experiences, and so it is not clear what is our "treatment", and whether these ATEs are generalizable. Below we unpack the treatment effect according to specific war experiences such as age, length and violence.

Second, outcomes may be mismeasured or misreported. One concern is that under-reporting of distress symptoms among the most distressed would lead us to underestimate treatment effects. Without established clinical norms in this population, we are unable to assess possible underreporting of distress symptoms (although under-reporting due to repression or avoidance is unlikely).[13] Nevertheless, even substantial underestimation is unlikely to change the basic conclusion: that at the

[13] Repression of traumatic memories tends to occur among younger children than our sample. Moreover, even if a youth actively avoids traumatic memories, this repression will often manifest itself in other symptoms. Finally, while distress may manifest itself long after the event, most people respond immediately (American Psychiatric Association 2000).

average (or median) increases in reported distress of abducted youth is moderate, and that the highest symptoms of distress are disproportionately concentrated in abductees.

Finally, the moderate average psychological impacts could be a consequence of the success of humanitarian programs provided to former abductees. If so, our results would underestimate the psychosocial impact of abduction. Unfortunately, this scenario is unlikely, as reintegration programs have been modest in scale and reach: only half of abductees passed through the formal care system, and those who did pass through the centers received only basic medical treatment, family reunification, and advice from social workers. Receipt of NGO services, moreover, is uncorrelated with levels of psychological distress. The estimates presented in this paper thus reflect the impact of combat conditional on half of abducted youth receiving the most rudimentary reintegration services.

C. Externalities from abduction

What do our treatment effects mean when the counterfactual group, non-abducted youth, is still affected by war? Non-abductees in raided areas could have worse schooling outcomes due to fewer schools being open, fewer teachers available, and general income shocks to the household. If they exist, such negative externalities will lead us to underestimate the true impact of abduction. On the other hand, abductions and displacement could lead to positive externalities as non-abducted populations are "urbanized" in displacement camps (closer to schools) or displaced to towns (closer to markets). Excessive displacement into towns could account for the higher levels of skilled work and wages we see among non-abducted youth.

To test for externalities, we calculate a measure of *Abduction intensity*, the proportion of the Parish population aged 7 to 40 ever abducted, in each of the 36 parishes that comprise our eight subcounties. Intensity, which ranges from 8 to 32 percent, is uncorrelated with well-being among non-abducted youth. Table 4 displays the coefficient on abduction intensity in regressions of each outcome on intensity, year and location of birth, and pre-war controls. Greater intensity correlates with lower education, higher employment, lower wages, higher distress, and lower aggression among non-abductees, but not statistically significantly so. Abduction intensity is strongly correlated with lower

social support, however—significant at the 5% level. Yet we see no difference in social support between abducted and non-abducted youth. It may be that abductions displace and disperse social networks, but for all war victims and abductees equally. We see no evidence of spillovers in the outcomes where abductees performed more poorly than non-abducted youth.

We also see no effect of abduction intensity on displacement to towns or other districts. The majority of the population was displaced to rural displacement camps, but some migrated in search of work. Table 4 displays the correlations between intensity and an indicator for migrating to a town, outside the district, or to a town outside the district (none being mutually exclusive). Non-abducted youth were more likely to migrate, and those that did earned higher wages than their rural counterparts. But this migration does not appear to have been a direct spillover of abduction intensity.

D. Child versus adult combatants

What's special about being a child soldier? To see, we compare impacts by age of abduction, which ranges from 5 to 29. We regress each outcome on age of abduction while controlling for location of birth indicators, as well as indicator variables for the year of abduction (to control for any possible changes in abduction patterns and experiences over time). The results, displayed in Table 5, suggest little significant difference in outcomes for children versus adult abductees.

The absence of any significant relationship between education, wages, and age of abduction is surprising, especially in light of the employment coefficient. Younger abductees are more easily influenced and indoctrinated by the LRA, and so are more likely to remain (the average abduction length is ten months for those under age eighteen, versus six months for those of age eighteen or older). Furthermore, younger abductees are more likely to be enrolled in school at the time of abduction, and so more likely to have their education interrupted. As a result, we would expect to see greater educational and labor market impacts for child soldiers than adult ones. One reason we see little variation in outcomes by age is that younger abductees are more likely to return to school upon return: two thirds of youth abducted before the age of 18 return to school, versus less than a third of those abducted over that age. Thus longer abductions may be offset by ease of reintegration.

E. Sensitivity Analysis

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

Sensitivity to violations of unconfoundedness

To assess the potential for unobserved selection, we explicitly model relaxations of unconfoundedness. An unobserved covariate, U, will induce a material degree of bias only if it is sufficiently associated with both treatment assignment, T, and the outcome, Y. We model a hypothetical U with a given distribution and calculate the combinations of correlation between U and T and between U and Y that would lead our ATE estimate to be biased by a fixed amount and benchmark this unobservable using observed covariates, X. (Rosenbaum and Rubin 1983; Imbens 2003). We employ a parametric model postulating an independent binomial distribution for U, a logistic conditional distribution for T, and a normal conditional distribution for Y. The model is illustrated in Appendix A.

Figure 5 plots each of the observed pre-war controls according to their ability to explain variation in both in T (abduction) and Y (in this case, education). The vertical axis charts 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 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.

The curve in Figure 5 represents all the combinations of correlation between U and T and between U and Y that would be sufficient to reduce the estimated education ATE by half. The curve is therefore a threshold, beyond which the hypothetical U is influential enough to materially reduce the education ATE. It is also a threshold, incidentally, that would leave the sign and significance of the ATE (and hence our general policy conclusion) intact. Only year and location of birth—the primary

determinants of selection by the armed group—meet or cross this hypothetical threshold.[14] Meanwhile, traits that normally influence military recruitment (such as household wealth or orphaning) lie well beneath the threshold.

Bounding the treatment effect for selective survival and non-return

Table 6 displays rates of attrition and attrition bounds for each outcome using the Lee (2005) method. The best case scenario bound is calculated by dropping non-abductees with the lowest values of the outcome and calculating the 'trimmed' ATE. The worst-case bound is likewise calculated by dropping the best-performing non-abducted youth (i.e. the 'worst case' is one where the untrimmed ATE erroneously leads us to conclude the existence of an ATE different from zero). Lee's method compares the untrimmed ATE (Column 3) to the trimmed means (Columns 4 and 5). The ATE's under the "worst-case" scenario are generally closer to zero and less than robust than the untrimmed ATEs. By the standards of this sensitivity analysis, this worst-case performance is actually quite strong, since not one of these lower bounds changes sign. The loss of statistical significance is not uncommon. The results imply that under austere and implausible dramatic selection, abduction still has the predicted effect on outcomes, albeit at a lower level of statistical significance.

6. Heterogeneous Treatments and the Channel of Impact

Abduction has so far been handled as a binary treatment. This approach, however, obscures the diversity of experiences and the true 'treatment' received. In particular, 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

[14] Year of birth also influences treatment assignment mechanically (as abduction levels changed year to year) as well as because rebel abduction parties targeted adolescents). Thus its distance from the origin in Figure 5 overstates the role of age as a selection criterion.

treatment exposure to outcomes (Angrist and Imbens 1995). One might prefer, however, to estimate the entire response function, considering abduction length or violence the "true" treatment.

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, even if abduction itself is not. Even so, estimating the (potentially biased) relationship between our measures of treatment exposure and outcomes are useful for understanding the underlying causal channels and are more easily generalized.

Psychosocial outcomes

The evidence suggests that youth who exhibit the most serious symptoms of psychosocial distress are generally those that experienced the greatest war violence, as discussed in detail by Annan and Blattman (2009). Table 7 displays regression of each outcome on three measures of abduction intensity: Years abducted (Column 1), an Index of 17 violent acts experienced (Column 2), and an Index of 8 violent acts perpetrated (Column 3). The indices of violence are linear and additive, based on self-reported indicators for six different acts witnessed by the youth (e.g. rape and killings of others); six acts inflicted upon the youth himself (e.g. beatings, imprisonment), and five acts upon his family (e.g. abduction, war injury, killing). Acts perpetrated include theft, beatings, killings, and rape.

After controlling for violence, longer abductions are not robustly associated with higher distress. Each additional incident of violence experienced, however, is associated with a 0.15 point increase in our distress index, while acts perpetrated are associated with a 0.27 point increase in distress. The average abductee reported 4 more violent acts experienced and 1.4 more perpetrated than non-abductees (Table 1), implying a distress impact of 0.98.

Such a link between increased exposure to violence and higher emotional distress has been identified among war-affected populations in settings as diverse as Iraq, Cambodia, Rwanda, and Croatia (Kinzie et al. 1986; Sack et al. 1986; Mollica et al. 1997; Ajdukovic and Ajdukovic 1998; Paardekooper et al. 1999; Dyregrov et al. 2000; Dyregrov et al. 2002). Violence rather than abduction is the underlying "treatment" resulting in distress.

Turning to social support, our index is decreasing in abduction length. An obvious possibility is that longer lengths of time away reduces social and family ties. Finally, there is no robust association between our two measures of aggression and abduction length or violence: the coefficients are close to zero and not statistically significant.

Educational and Labor Market Outcomes

Time away from human capital accumulation rather than violent trauma may account for the persistent educational effects of abduction. Long abductions are strongly correlated with losses in education and literacy: each year of abduction is associated with 0.54 years less education and a 9 percentage point reduction in literacy. The association between violence and education is fairly weak. One interpretation is that, when it comes to education, the relevant 'treatment' may not be violence but rather the interruption of schooling and work experience. An alternative is that more educated abductees are able to escape more quickly, leading to upward bias in the coefficient on abduction length. Both could be at work, but only the former could account for the average adverse impact of abduction on education we saw in Table 3.

The correlates of wages suggest that time-away from education could also be the main driver of the wage gap between abducted and non-abducted youth. Consider a human capital earnings function where wages are a log-linear function of education, experience, social capital, and health.[15]

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

We estimate this equation in Column 1 of Table 8 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 health are the strongest observed correlates of wages.[16]

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

[16] Potential measurement error and bias from omitted variables prevent a causal interpretation of these coefficients. International evidence on returns to schooling, however, suggests that the education coefficient is likely to be correct to a first order of approximation. For a discussion of estimation bias regarding schooling see Card (1999) and for health see

We can obtain a rough measure of the relative influence of each component of human capital in the abduction wage gap by multiplying the earnings function coefficients by the respective abduction ATEs (Table 8, Columns 2 and 3).[17] 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 55 percent of the reduced-form ATE for log wages (Column 4). 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.

Given the association between abduction length and education and between education and wages, we would expect a strong inverse relationship between abduction length and log wages. No such relationship appears to exist, however—the point estimate on abduction length is positive, close to zero and not significant. It is the violence experienced coefficient that is negative and significant: each violent act is associated with an 8 percentage point decrease in wages. Length of abduction is unassociated with skilled employment.

The absence of a relationship between wages and abduction length (and the positive correlation with violence experienced) is inconsistent with the human capital mechanism we hypothesize above. The relationship between wages and violence does not appear to be driven by injuries; controlling for an injury indicator or an indicator for being in the top quartile of distress (regressions not shown). One possibility is that the log wage-violence correlation is spurious; the same regression using wage levels (rather than log wages) yields the expected negative coefficient on years abducted, although it is not statistically significant. The coefficient on violence experienced remains positive, and is no longer significant. Our measure of wages, average gross daily earnings, is measured with

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Strauss and Thomas (1998). The literature suggests that ability and attenuation biases are moderate and tend to offset one another (Card 1999; Ashenfelter et al. 2000). While the coefficient on wages in Table 8 is high relative to developed-country estimates of the returns to schooling, Kruger and Lindahl (2001) suggest that the returns to education are higher in poor countries.

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

error, the distribution is skewed, and values are missing for non-workers. Moreover, the regressions in Table 8 are vulnerable to endogeneity. Even so, given the relationship between education and both abduction length and wages, we would expect a large and statistically significant relationship between abduction length and wages. This failure to observe a relationship between abduction length and wages is puzzling, and suggests that a reduction in education and experience may not be the only causal channel by which abduction affects labor market outcomes. The result presents a puzzle, and bears future investigation.

7. Conclusions

New data and a tragic natural experiment 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 work experience, and higher levels of psychological distress concentrated in those that experience the most violence.

We can juxtapose these findings against current policy in Uganda, where funding has been focused on broad-based programs for the psychosocial reintegration of former child soldiers. With distress and aggression concentrated, more targeted and specialized psychosocial services are probably needed for those who have experienced the most violence (abducted or not). The most broad-based impact of abduction, meanwhile, instead appears to be economic and educational, implying that increased attention ought to be paid to education and economic programming. Evidence on the effectiveness of different programs is needed to make firm policy conclusions, however.

We interpret our findings as the incremental effect of conscription in communities already subjected to the horrors of war and abduction—an important quantity of interest if we wish to target aid and development assistance to the people who need them most. We would also like to measure the impacts of war itself, but unfortunately we have neither the data nor the research design to do so. It remains a crucial area for future research. In the meantime, it is worth noting that the incremental impact of military service may be more important than the gap between war- and non-war

affected populations, at least in Uganda. National survey data suggest that while the impact of the war on wealth may have been large, the educational impact has been small—non-abducted youth in the war zone have levels of education and literacy that similar or even *greater* than that in other parts of Uganda. We discuss these comparisons in Appendix B. While selection undoubtedly biases any results, they do suggest that the gap between non-abducted youth in Acholiland and youth outside the region is not enormous. We do not have data to compare economic indicators, however, which we would assume to be significantly lower than the rest of Uganda.

Can we generalize these results? The psychosocial findings concur with academic literature on refugee mental health and an NGO literature on child protection—both emphasize the resilience to violent trauma among youth (as described in the introduction). Combat may be no different. In the economic realm, the parallels to the conscription of American soldiers are striking. For instance, Angrist (1990) 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.

While the Ugandan results are most easily generalized to other instances of forcible recruitment, we speculate that our results likely understate the consequences of voluntary participation in unpopular armed groups such as those in Sierra Leone, Liberia, and the Congo. Volunteer fighters in an unpopular war might see a greater loss in social capital and greater impact on mental health. Ugandan abductees have granted amnesty for any of their actions as soldiers, and have been largely welcomed home with open arms—a remarkable community response that could mute the economic and psychosocial effects. Globally a third of child soldiers are thought to be forcibly recruited (ILO 2003). For the other two thirds, who might experience more social exclusion upon return, the treatment effects estimated in this paper might be regarded as a minimum impact.

Finally, the 'time away' channel, if true, suggests that the Ugandan findings may be relevant for understanding other forms of child labor and interruptions of human capital accumulation more generally. Meng and Gregory (2007) find a sizable reduction in educational attainment (though little decrease in long-term earnings) among Chinese taken out of school during the Cultural Revolution. Causal estimates of the impact of agricultural child labor on education and earnings in both Vietnam

and Tanzania suggest that a doubling of hours employed reduces school enrolment by nearly a third and educational attainment by six percent (Beegle et al. 2004; Beegle et al. 2006). In the longer term, however, they 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 can offer the ability to acquire useful human and physical capital, muting the long term economic impact. 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 and the terrible experiences of violence, but also because of the dramatic decrease in lifetime earnings ability that comes from the lack of transferable skills gained from 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.

Appendices

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

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

$$U \sim B(1, \tfrac{1}{2})$$
$$\Pr(T = 1 \mid X, U) = \frac{\exp(\gamma' X + \alpha U)}{1 + \exp(\gamma' X + \alpha U)}$$
$$Y(T) \mid X, U \sim N(\tau T + \beta X + \delta U, \sigma^2)$$

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

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

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

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

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

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 $\widetilde{R}^2_T(\alpha,\delta)$—the partial R^2-statistic obtained from adding the hypothetical covariate with fixed (α,δ) to the outcome regression:

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

where $\hat{\psi}(\alpha,\delta)$ represents the unexplained sum of squares in the latent index regression.[18] This procedure is implemented in Figure 5 for the education outcome, and is described in the text.

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

The treatment effects estimated in Table 3 identified the incremental impact of conscription on already war-affected youth. The impact of war on non-abducted youth is not known, but could be estimated if we possessed a comparable sample of youth outside the war zone. Unfortunately, a valid counterfactual is not available. We can turn, however, to national survey data for a very rough assessment of the impact of war. The 2002/03 Uganda National Household Survey (e.g. Collier 1999; Ghobarah et al. 2003) collected data on more than 8000 youth, excluding the war-affected (Acholi) districts. Three education measures (years of schooling, enrolment, and illiteracy) and three household asset indicators (mobile phone, bicycle, and radio ownership) were measured by both the national survey and the survey conducted by the author.

Age-adjusted mean differences between non-abducted youth in the Acholi region (from the author's dataset) and youth in four other Uganda regions (other Northern districts, as well as Uganda's

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

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.

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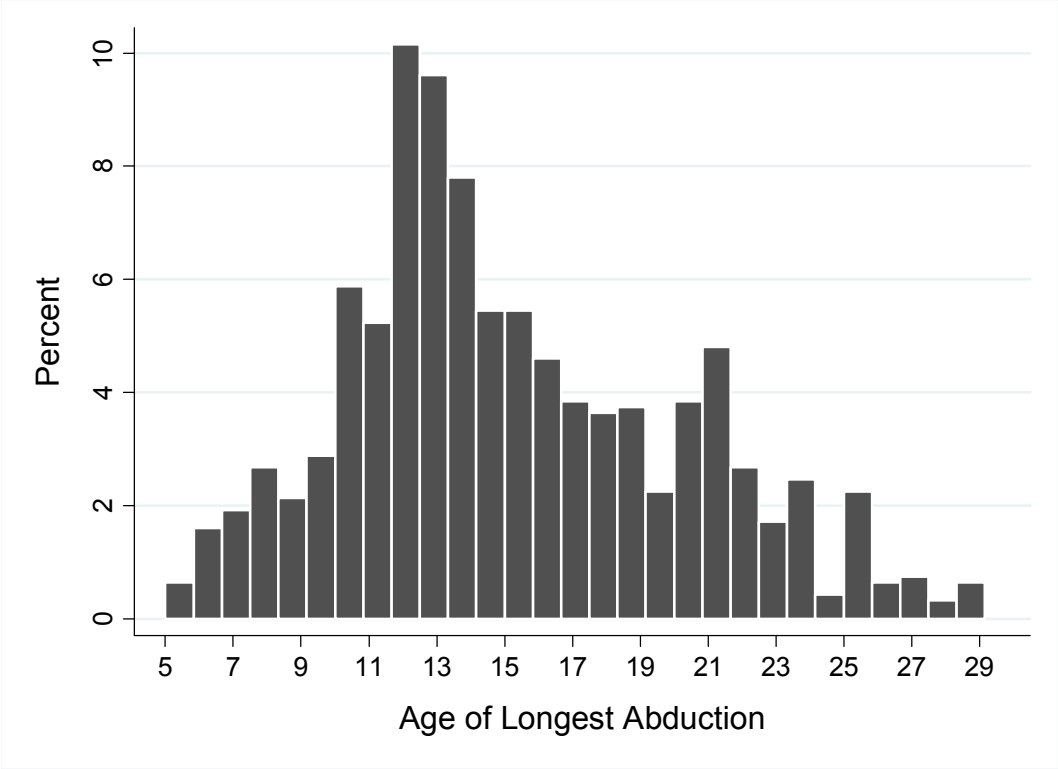
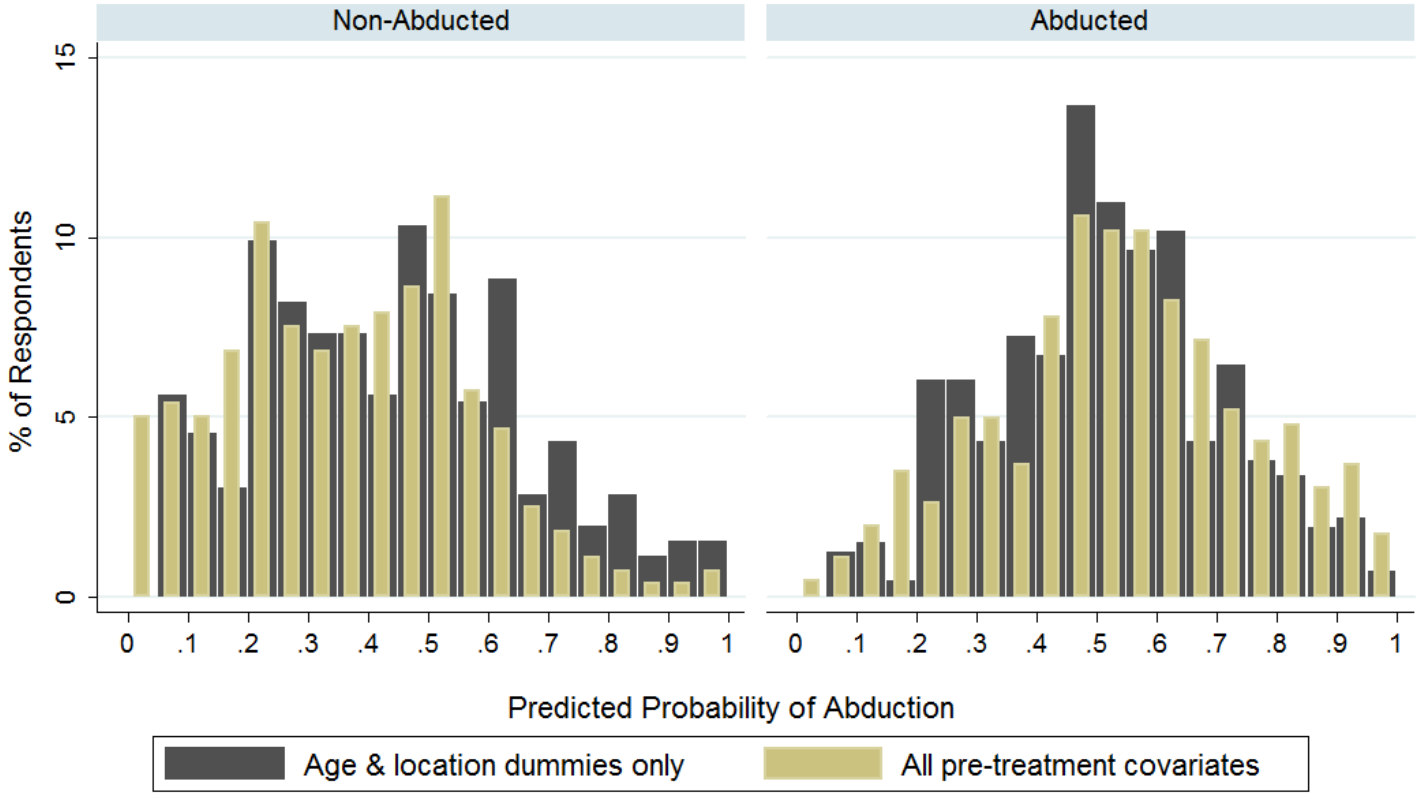
Figure 1: Map of Uganda and survey field site

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 longest 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).

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

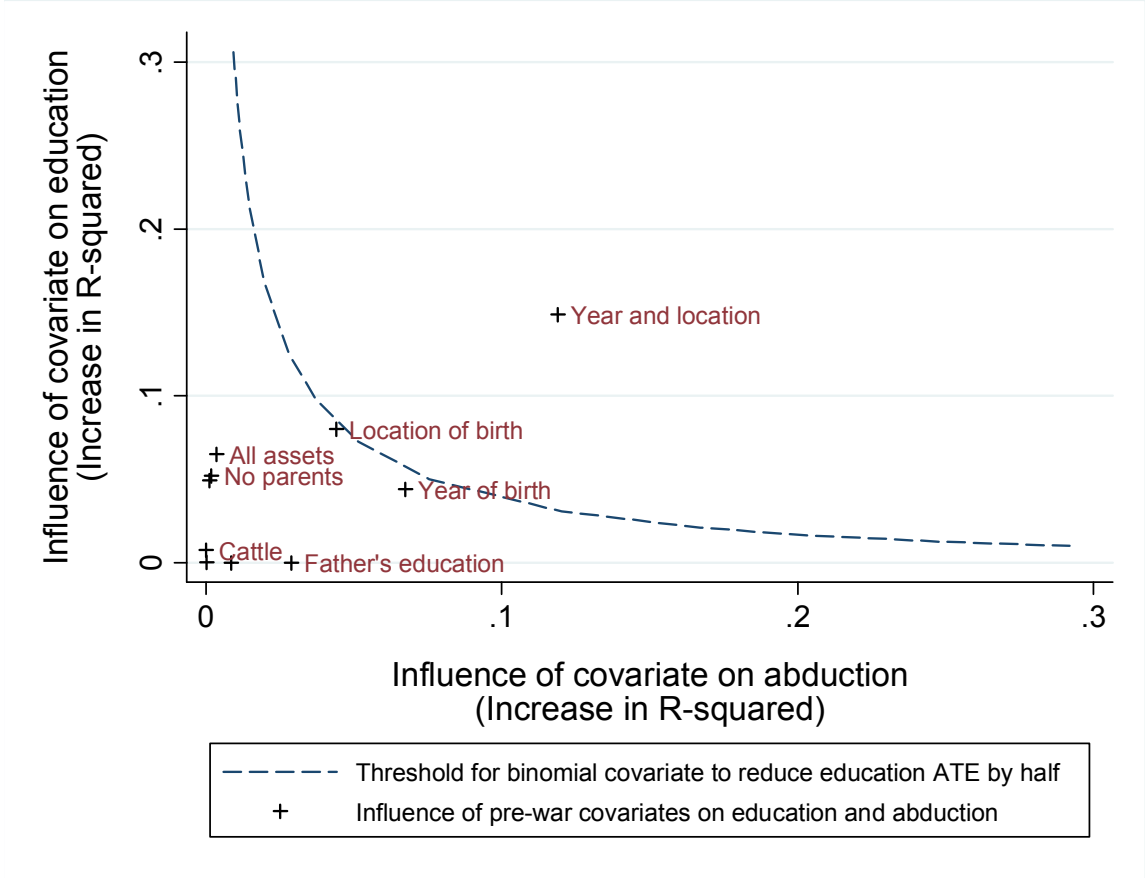


Notes: N = 741 males aged 14 to 30, weighted by the inverse sampling probability and the inverse attrition probability. Pre-treatment covariates other than age and location dummy variables include mother's and father's education, mother's and father's death in 1996, initial household landholdings and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

Figure 4: Quantile differences in the distribution of psychological distress between abducted and non-abducted youth

Notes: The solid line is calculated from the coefficients on the abduction indicators from least-absolute deviation (quantile) regressions of the index of psychological distress on an abduction indicator and controls, at every quantile of the distribution. Controls include age, location dummies and interactions, as well as pre-treatment covariates. Extreme values of the confidence interval (above 2 and below 1) are not displayed, for clarity.

Figure 5: Impact of relaxing the assumption of unconfoundedness



Notes: The figure presents the results of the sensitivity analysis following Imbens (2003). Each + represents a pre-war 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.

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.		8.9 [15.6]		462
Age of abduction	Age (in years) at the time of the respondent's longest abduction.		15.3 [4.7]		462
Index of violence experienced	Sum of 17 indicators of violence witnessed, received, or upon own family.	5.0 [3.1]	7.2 [2.7]	3.2 [2.1]	738
Index of violence perpetrated	Sum of 8 indicators of violence perpetrated by the respondent (self-reported)	0.7 [1.4]	1.5 [1.8]	0.1 [0.3]	738
Education & Labor Market Outcomes					
Educational attainment	Highest level of education obtained (including tertiary and vocational training)	7.4 [3.0]	7.1 [2.9]	7.6 [3.0]	741
Indicator for functional literacy	Indicator equaling one if a respondent reports being able to read a book or a newspaper in any language.	0.75 [0.43]	0.69 [0.46]	0.80 [0.40]	741
Indicator for any work in past month	Indicator equaling one if days employed were greater than zero.	0.64 [0.48]	0.69 [0.46]	0.61 [0.49]	741
Indicator for capital or skill-intensive work	Indicator equaling one if the main occupation is a profession, a vocation, or a small business.	0.10 [0.30]	0.08 [0.26]	0.12 [0.32]	741
Daily wage (in Uganda shillings)	Gross cash earnings in the past month divided by days employed. 237 observations are undefined.	3,221 8,621	2,498 4,941	3,915 11,018	504
Psychosocial Outcomes					
Index of psychological distress	Sum of 19 survey questions on symptoms of depression and traumatic stress.	4.0 [2.4]	4.2 [2.5]	3.8 [2.2]	741
Indicator for top 25% of distress index	Indicator equaling one if the psychological distress index exceeds a score of 5 (i.e. the top quartile).	0.27 [0.4]	0.32 [0.5]	0.23 [0.4]	741
Index of social support	Sum of 14 questions on concrete social support received from family and friends in past month.	5.5 [2.4]	5.5 [2.4]	5.5 [2.5]	741
Indicator for hostility	Indicator equaling one if reported being one of four histile behaviors.	0.07 [0.3]	0.07 [0.3]	0.07 [0.3]	741
Indicator for a physical fight	Indicator equaling one if the respondent reported being in a physical fight in the past 6 months.	0.07 [0.3]	0.07 [0.2]	0.07 [0.3]	741

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

Table 2: Comparison of means

Pre-treatment Covariate	(1)	(2)	(3)	(4)
	Abducted vs. Non-abducted		Militia versus non-militia members	
	Difference in means‡		Difference in means‡	
	Unconditional	Conditional	Unconditional	Conditional
Year of birth†	1.02	1.27	2.76	2.31
	[0.44]**	[0.51]**	[0.82]***	[0.65]***
Indicator for father a farmer†	0.01	-0.01	0.06	0.05
	[0.02]	[0.02]	[0.04]	[0.04]
Household size in 1996†	-0.33	-1.51	0.34	1.32
	[0.41]	[0.32]***	[1.01]	[0.54]**
Landholdings in 1996†	0.57	-1.46	-6.69	-7.12
	[2.09]	[2.72]	[4.13]	[4.28]
Indicator for top 10% of Landholdings†	0.00	-0.02	-0.10	-0.12
	[0.03]	[0.03]	[0.04]**	[0.05]**
Cattle in 1996†	5.12	6.21	-10.07	-4.51
	[4.14]	[4.98]	[6.18]	[3.51]
Other livestock in 1996†	0.96	2.07	-6.25	-1.94
	[2.72]	[1.66]	[2.60]**	[2.38]
Indicator for plow ownership in 1996†	0.03	-0.01	-0.18	-0.06
	[0.07]	[0.04]	[0.08]**	[0.04]
Indicator for uneducated father	0.01	0.02	-0.05	-0.12
	[0.02]	[0.02]	[0.03]	[0.04]***
Father's years of schooling	-0.05	-0.06	-0.04	0.41
	[0.28]	[0.30]	[0.44]	[0.43]
Indicator for uneducated mother	-0.01	-0.01	0.09	0.05
	[0.03]	[0.04]	[0.08]	[0.10]
Mother's years of schooling	-0.10	-0.12	-0.44	-0.14
	[0.26]	[0.34]	[0.41]	[0.65]
Indicator for paternal death before 1996	0.02	0.03	0.04	0.05
	[0.04]	[0.05]	[0.13]	[0.11]
Indicator for maternal death before 1996	0.01	0.02	-0.07	-0.03
	[0.02]	[0.02]	[0.04]*	[0.03]
Indicator for orphaning before 1996	0.00	-0.02	-0.05	-0.01
	[0.02]	[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: Estimates of the average treatment effect of abduction

	(1)	(2)	(3)
Dependent variable	**ATE**	**Non-abducted mean**	**%Δ**
Educational & Labor Market Outcomes			
Years of education	-0.75 [0.17]***	7.6	-10%
Indicator for functional literacy	-0.15 [0.04]***	0.80	-19%
Indicator for any employment in the past month	0.03 [0.04]	0.61	5%
Indicator for capital- or skill-intensive work	-0.05 [0.02]**	0.12	-43%
Log (Daily wage)	-0.33 [0.15]**	n.a	n.a
Psychosocial & Health Outcomes			
Index of psychological distress	0.57 [0.20]***	3.8	15%
Indicator for top quartile of distress	0.11 [0.04]***	0.23	49%
Index of social support	-0.16 [0.14]	5.5	-3%
Indicator for hostile attitudes	0.03 [0.01]**	0.07	40%
Indicator for physical fights	-0.02 [0.02]	0.07	-29%

Notes:

Each entry represents a separate WLS regression

All variables defined and described in Table 1

* significant at 10%; ** significant at 5%; *** significant at 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)

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

Table 4: Spillover effects of abduction intensity

	Correlation between outcome and Parish-level abduction intensity
Educational & Labor Market Outcomes	
Years of education	-0.02 [0.04]
Indicator for functional literacy	0.00 [0.01]
Indicator for any employment in the past month	0.01 [0.01]
Indicator for capital- or skill-intensive work	0.00 [0.01]
Log (Daily wage)	-0.04 [0.02]
Psychosocial & Health Outcomes	
Index of psychological distress	0.03 [0.03]
Indicator for top quartile of distress	0.01 [0.01]
Index of social support	-0.10 [0.04]**
Indicator for hostile attitudes	0.00 [0.00]
Indicator for physical fights	-0.01 [0.00]
Migration outcomes	
Migrated to a town	0.00 [0.00]
Migrated out of home district	0.00 [0.00]
Migrated to town outside home district	0.00 [0.00]

Notes:

Each entry represents a separate WLS regression of the outcome on abduction
rik, year and location of birth, and pre-war traits

Abducted youth are omitted from the analysis

Robust standard errors in brackets, clustered by sampling unit (location and abd

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

Table 5: Variation in the average treatment effect by age of abduction

Dependent variable	WLS coeffcient on age of abduction	Obs
Educational & Labor Market Outcomes		
Years of education	0.00 [0.04]	462
Indicator for functional literacy	0.00 [0.01]	462
Indicator for any employment in the past month	-0.01 [0.01]	462
Indicator for capital- or skill-intensive work	0.00 [0.01]	462
Log (Daily wage)	-0.03 [0.02]	288
Psychosocial & Health Outcomes		
Index of psychological distress	-0.02 [0.03]	462
Indicator for top quartile of distress	0.00 [0.01]	462
Index of social support	-0.05 [0.04]	462
Indicator for hostile attitudes	0.00 [0.00]	462
Indicator for physical fights	0.00 [0.00]	462

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

Table 6: Treatment effect bounding for selective attrition

Dependent variable	(1)	(2)	(3)	(4)	(5)
	% Missing Data†		Treatment effect bounds§		
	Non-Abd	Abd	Untrimmed ATE‡	"Best case" attrition bound	"Worst case" attrition bound
Educational & Labor Market Outcomes					
Years of education	11%	23%	-0.53 [0.20]**	-1.19 [0.24]***	0.23 [0.24]
Indicator for functional literacy	11%	23%	-0.12 [0.03]***	-0.23 [0.04]***	-0.09 [0.03]***
Indicator for any employment in the past month	11%	23%	0.11 [0.03]***	0.17 [0.04]***	0.03 [0.04]
Indicator for capital- or skill-intensive work	28%	30%	-0.04 [0.02]	-0.05 [0.02]	-0.02 [0.04]
Log (Daily wage)	59%	54%	-0.13 [0.12]	-0.38 [0.15]**	0.14 [0.15]
Psychosocial & Health Outcomes					
Index of psychological distress	28%	30%	0.56 [0.17]***	0.71 [0.30]**	0.49 [0.22]**
Indicator for top quartile of distress	28%	30%	0.13 [0.03]***	0.15 [0.05]***	0.12 [0.04]***
Index of social support	28%	30%	-0.10 [0.19]	-0.20 [0.27]	0.05 [0.28]
Indicator for hostile attitudes	28%	30%	0.03 [0.02]	0.05 [0.04]	0.03 [0.02]
Indicator for physical fights	28%	30%	0.00 [0.02]	0.02 [0.04]	-0.01 [0.02]

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 7: Estimates of the impact of abduction length and violence on outcomes

	(1)	(2)	(3)
Dependent variable	**Coeffcient on**		
	Years abducted	**Index of violence experienced**	**Index of violence perpetrated**
Educational & Labor Market Outcomes			
Years of education	-0.54 [0.090]**	-0.05 [0.043]	0.11 [0.089]
Indicator for functional literacy	-0.09 [0.021]**	-0.01 [0.007]	0.02 [0.012]
Indicator for any employment in the past month	0.00 [0.014]	-0.01 [0.013]	0.01 [0.015]
Indicator for capital- or skill-intensive work	-0.01 [0.010]	0.00 [0.004]	-0.01 [0.009]
Log (Daily wage)	0.02 [0.046]	-0.08 [0.023]**	0.04 [0.052]
Psychosocial & Health Outcomes			
Index of psychological distress	-0.03 [0.103]	0.15 [0.034]**	0.27 [0.080]**
Indicator for top quartile of distress	-0.01 [0.020]	0.03 [0.007]**	0.01 [0.014]
Index of social support	-0.11 [0.062]	0.11 [0.058]	0.04 [0.118]
Indicator for hostile attitudes	0.00 [0.007]	0.00 [0.004]	0.00 [0.008]
Indicator for physical fights	0.00 [0.009]	0.00 [0.005]	0.01 [0.007]

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

Table 8: 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]***	-0.75	-0.12	-55%
Years experience§	0.05 [0.01]***	-0.74§	-0.03	-15%
Index of social support	0.04 [0.03]	-0.16	-0.01	-3%
Indicator for serious injury	-0.26 [0.20]	0.10	-0.03	-12%
Index of psychological distress	-0.03 [0.02]	0.57	-0.02	-8%
Observations	448			
R-squared	0.142			

Notes:

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.

§ 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 (8.9 months, or 0.74 years).

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)	7.6	0.56	0.09	0.16	0.41	0.38
Age-adjusted mean in non-Acholi regions (from 2002/03 UNHS) relative to Acholi non-abducted sample mean:						
Non-Acholi north	-0.15	-0.01	0.13	-0.03	0.02	0.13
	[0.13]	[0.01]	[0.03]***	[0.01]**	[0.02]	[0.01]***
Central region	-0.45	-0.17	0.10	0.03	0.07	0.30
	[0.13]***	[0.01]***	[0.02]***	[0.02]**	[0.02]***	[0.01]***
Eastern region	0.28	-0.01	0.14	0.02	0.01	0.23
	[0.12]**	[0.01]	[0.02]***	[0.01]	[0.02]	[0.01]***
Western region	-0.45	-0.12	0.09	0.00	0.00	0.26
	[0.12]***	[0.01]***	[0.02]***	[0.01]	[0.02]	[0.01]***
Observations	8830	8839	5445	8840	8842	8842
R-squared	0.12	0.45	0.03	0.01	0.02	0.17

Notes:

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.