

The Consequences of Child Soldiering*

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Abstract:

Reintegration of Africa's millions of young ex-combatants is one of the continent's most crucial development and peace-building concerns. Any impacts of war on the skills and productivity of so many young people could impede growth for decades. Moreover, any impact on aggression and social exclusion could imperil peace. This paper presents new survey data collected by the author in northern Uganda, where near-random variation in rebel recruitment practices allows important questions to be answered, namely: who is affected by combat, in what ways, and how much? Ugandan rebels have abducted thousands of youth, mostly adolescents, to serve in their insurgency. This paper identifies eight rural areas where abduction was large-scale, indiscriminate, and exogenous to a youth's individual traits. After demonstrating this unconfoundedness of abduction, the paper shows that the most pervasive impact of combat is upon their education and productivity. Contrary to popular belief, social and political exclusion appear no greater among ex-combatants. Moreover, psychological health and aggression are adversely affected by combat experiences, but predominantly in a minority. The paper takes an instrumental variables approach to show that extreme war violence, not abduction, can account for the concentrated psychological distress we observe in both child and adult ex-fighters. Finally, by all of measures the paper shows child soldiers to be at least as resilient as young adult ones. Implications for post-conflict reintegration, development, and peace-building in northern Uganda are discussed.

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1. Introduction

Death, destruction of capital, and political instability are the most obvious (and studied) costs of war. The purpose of this paper is to move to a more micro-level assessment of the consequences of conflict, demonstrating that the long-term impact of combat and violence on surviving youth is at least as critical a development and peace-building concern. Moreover, the paper aims to show that, in the near absence of micro-data, the impacts of war on youth may have been misconstrued—the economic consequences greatly underestimated and the vulnerability of child combatants overestimated.

Child and adult ex-combatants number in the millions. Two thirds of African nations have suffered civil warfare since the end of the Cold War, with most of these conflicts lasting for seven years or longer (Marshall and Gurr, 2005). These civil wars are typically brutal and all-consuming affairs, pulling up to a third of male youth into combat, including children. Children under 18 were believed to be serving in 72 government or rebel armed forces in about 20 countries as of 2002 (Achverina and Reich, 2006; CSUCS, 2005). The millions of other ex-combatants—114 armed conflicts have been recorded since 1990—are of course little older than children themselves.¹

Almost nothing is known, however, about the lasting impacts of military recruitment and war violence on youth. While the impacts are undoubtedly harmful, we cannot say with any certainty who is affected, in what ways, or how much? As a consequence, post-conflict reintegration programs for young ex-combatants are designed and targeted in a largely *ad hoc* manner—a state of affairs government and aid workers on the ground are the first to lament. Yet improving reintegration success is a crucial development and peace-building concern. Human capital takes decades to accumulate, and with so many young ex-combatants, any damage to health or interruption of education could hinder the performance and productivity of the entire nation for many years to come. Moreover, any impact of war on aggression or social exclusion could threaten the long-term political stability of the nation. Which impacts of combat are most salient and in need of attention, however, is essentially unknown.

One reason that the effects of combat are so poorly understood is the paucity of data in war zones. Another is the challenge of causal inference in the presence of sample selection; ex-fighters are usually a select segment of the population, including those who chose to join, those screened by the armed group, and those that remain to tell the tale. A final challenge is the heterogeneity of the

¹ Calculated from the 2005 UCDP/PRIO Armed Conflict Dataset (UCDP/PRIO, 2006; Gleditsch et al., 2002)

experience itself—the length and intensity of combat varies dramatically across youth, possibly in ways that are influenced by the youth’s unobserved traits. This paper tackles all three challenges.

First, the paper uses an original survey of youth embroiled in civil war to overcome the usual lack of data in conflict situations. More than 1000 households and 741 male youth (including 462 ex-combatants) were interviewed by the author, a psychologist, and a team of local assistants in the midst of an ongoing conflict in northern Uganda.² There an unpopular rebel group has abducted more than 60,000 children and young adults to serve in their insurgency over the past 19 years.

Second, the paper uses exogenous variation in rebel recruitment practices to overcome the usual problems of selection into armed groups. The “ideal” research design would be one where military participation was assigned randomly, at different ages, with outcomes observed at a later date for all youth at risk. Northern Uganda closely resembles such a terrible case. The survey identifies eight rural areas where recruitment was large-scale, indiscriminate, and entirely forcible. With no popular support, small parties of roving rebels raided rural homesteads for supplies and recruits, abducting all adolescent boys and young men they encountered. Using data on pre-war traits, as well as testimony from dozens of interviews with rebel commanders and fighters, the paper demonstrates that abduction was independent of all of a youth’s observable traits other than age and location.

Under this assumption of unconfounded abduction, average “treatment effects” can be estimated using non-combatants of the same age and location as a counterfactual. The results contrast sharply with current beliefs and practice in Uganda. There the principal focus has been upon addressing psychological distress and social dislocation, especially among former child soldiers. The results instead imply that the most pervasive impacts are upon education and productivity, and are largely explained by time spent outside of the normal process of skills and capital accumulation. Contrary to popular belief, the average psychological consequences are relatively moderate, and the impacts on aggression and social exclusion are weak or non-existent. Psychological traumatization is a clear consequence of combat, but among a minority of ex-combatants rather than all. These estimates are robust to alternative specifications, to the inclusion or exclusion of pre-treatment covariates, and to corrections for sample attrition and selective survival. Two forms of sensitivity analysis also demonstrate that reasonable amounts of unobserved selection cannot account for such large treatment effects.

² This paper focuses on data from a survey of males only. A similar study of women and girls is currently underway.

The paper also demonstrates that, again contrary to popular wisdom in Uganda, all of these impacts are at least as great for adults aged 18 to 30 as they are for children under 18. If anything, former child soldiers appear *more* resilient than adult ones.

Finally, the paper uses exogenous variation in exposure to violence to assess the relationship between the intensity of the war experience and reintegration success. Like Angrist's (1990, 1998) studies of the labor market consequences of military service in Vietnam, the treatment discussed so far has been binary: combatant or not. Yet war experiences are far from uniform—less than a sixth of Americans deployed to Vietnam, for instance, served in combat (Carey, 2006). Humphreys and Weinstein (2005) show in Sierra Leone that the abusiveness of one's military unit is a major determinant of reintegration success, suggesting that variation in violence is a key source of heterogeneity.

What we would prefer to estimate is a 'treatment response function' that relates the intensity of war to outcomes. The challenge is that simple ordinary least squares estimates will likely over- or under-estimate the causal impact of abduction length and violence on later life outcomes. For example, violence could be associated with greater psychological distress because pre-existing mental conditions lead to violent behavior rather than because violence leads to later distress.

The paper takes an instrumental variable approach to overcome this problem of endogenous treatment intensity. The abusiveness of a respondent's military unit, as reported by others in the same unit, is a strong predictor of the respondent's exposure to violence and appears to affect potential outcomes through violence alone. Instrumenting for violence with unit abusiveness suggests that exposure to violence is the primary determinant of psychological distress. Thus it is the concentration of violence that accounts for the concentration of distress in a minority of ex-fighters. Violence also has an adverse impact on productivity, however, suggesting that factors other than time away from school and the workforce account for the poor economic performance among ex-combatants.

Overall these results imply a mismatch between current reintegration programming and current needs in Uganda. Reintegration programs for abductees are overwhelmingly psychological and social in their focus ('psychosocial' in aid lingo), and almost entirely directed at children—an approach common to youth post-conflict programs worldwide.³ A tentative truce between the rebels and the government was reached in August 2006, just a few months after the completion of the survey. Inter-

³ See, for instance, CSUCS (2005), ILO (2003), Barnitz (1999), Machel (1996), and Cohn and Goodwin-Gill (1994).

national donors are now preparing to spend hundreds of millions of dollars on post-conflict aid in the north, much of it directed at the reintegration of abductees. The findings in this paper suggest that funds should be directed towards broad-based labor market and educational support for both children and adults, alongside targeted psychosocial assistance to the most traumatized youth.

2. Background

In 1988, a spiritual leader named Joseph Kony assembled the extremist remnants of several failed insurgent groups in northern Uganda into a new guerrilla force, the Lord's Resistance Army, or LRA.⁴ The rebels were unpopular among their ethnically Acholi brethren from the very beginning, and so from its earliest days Kony's LRA looted homes and abducted youth to maintain supplies and recruits. Rebel activity was concentrated in the three northern Acholi districts: Gulu, Kitgum, and Pader (see Figure 1). The Acholi populace, after three years of abductions and looting, began to organize a local defense militia in 1991. To punish them for this betrayal, and to dissuade them from further collaboration with the national government and army, Kony ordered the widespread killing and mutilation of civilians. Thus from 1991 onwards, Kony's war was waged not only against the government but against the populace at large.

The northern conflict has both spiritual and political roots. Locally Kony is believed to possess great spiritual powers, and he claims to seek a spiritual cleansing of the nation. His movement is also rooted in a longstanding political grievance. In 1986 rebels from the south of the country overthrew an Acholi-dominated government. Several guerrilla forces in the north initially resisted the takeover, but for the most part settled for peace or were defeated by 1988. Those that would not settle for peace gathered under Kony to continue the fight. The war would not likely have lasted these two decades, however, were it not for interference from neighboring Sudan. In 1994 Sudan's government began supplying Kony with weapons and territory upon which to build bases. Sudan's support enlarged and invigorated a weakening LRA. Rebel attacks and abductions escalated dramatically after 1995.

Kony never sought nor received political legitimacy, and in the absence of any popular support the abduction of Acholi youth became the LRA's sole means of recruitment. Abduction has been large-scale and indiscriminate. It is estimated that more than 60,000 youth have been taken, most

⁴ This history is based on Allen (2005), Behrend (1999), Doom and Vlassenroot (1999), Finnstrom (2003), Lamwaka (2002), and Omara-Otunnu (1994), as well as the author's interviews with leaders and military commanders on all sides of the conflict.

since 1996 (Annan et al., 2006). The majority are adolescent males, though men and women of all ages are taken. 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 rebels.

Lengths of abduction ranged from a day to ten years, with half taken for at least four months. The vast majority of those taken are tied, beaten, and tortured. Youth who fail to escape are trained as fighters and, after a few months, are given a gun for raiding and abducting. Roughly a quarter of abductees eventually become 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.

The war and the widespread abduction of children has resulted in massive influx of international aid agencies striving to assist those displaced by the conflict and reintegrate children escaping from rebel abduction. With so many abductions, and with more than a million displaced persons, these agencies have been somewhat overwhelmed. Resources have thus been concentrated on what agencies see as the most crucial issue: psychosocial trauma among former child soldiers. This focus seems to be driven largely by abundant anecdotal evidence of traumatization. For instance, one youth interviewed continues to be 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. When I came back home, 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.

Sadness and depression also seem associated with combat experiences. From one interviewee,

Before, if someone was quarrelling with me, however much you say something, I could still answer you back in a good way. But now if some one says something bad or quarrels I can only keep quiet or start to cry. So I find it has changed my life, and the reasons as to why I cry I even do not know.

While obviously salient, dozens of interviews conducted by the author suggest that the psychological consequences of combat are secondary in the minds of many youth. Rather, the interruption of education appeared to be a more prevalent concern. From one of the in-depth interviewees,

My stay in the bush has really wasted my time, and if they had not abducted me, maybe I would have completed my education now.

Those coming back from longer abductions may also feel uncomfortable returning to school with youth much younger than them. Speaking to a primary school teacher:

Q. What makes the difference between those who adjust well when they return and those who don't?

A. I think one of them is the duration in the bush. Some of them 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] feel inferior and take long to adjust.

Finally, anecdotal evidence suggests that abducted youth may be trapped in lower-productivity employment. Local labor markets can be characterized as an occupational ladder increasing in skill and resource requirements, which a youth gradually ascends through the slow accumulation of education and capital. One young man's experience is representative of that relayed by many youth:

[I began my business] when I was still at home. I was making charcoal. But when people came to the camps, I started riding a boda boda [bicycle taxi]... I used to ride as a boda boda on all the roads but when the rebels started killing boda boda cyclists along the way. I left this job and then started studying. ... [Later] the money that I had saved helped me to start this business.

By many accounts, abduction interrupts a youth's ascent up this ladder, or pulls him off one rung and places him on a lower one. This paper will look to survey data to show that this is indeed the case.

3. Existing evidence

What is known about child soldiers comes largely from interview-based, ethnographic case studies of children in conflict (e.g. Honwana, 2006; Shepler, 2005; Machel, 1996; Cohn and Goodwin-Gill, 1994). Just two reports, by Allen and Schomerus (2006) and ILO (2003), collect structured data on a large sample of former child combatants. The conclusion that emerges most commonly and clearly from this literature is the prevalence of psychological trauma and social dislocation among former child soldiers and the importance of addressing such traumatization through post-conflict programs. In the absence of representative, quantitative data on both combatants and non-combatants, however, the nature and magnitude of combat's impact on children is still unclear.

Economists and political scientists have meanwhile produced little theory and evidence on the micro-economic and micro-political consequences of violent conflict. Political scientists have concentrated on the macro-level consequences of war for state formation (e.g. Herbst, 2000; Tilly, 1992), for political redevelopment (e.g. Zartman, 1975), for health (Ghobarah et al., 2003), and for the decision to go to war in the first place (e.g. Fearon 1995; de Mesquita, 1981). Economists have likewise focused on the country-level impacts for physical capital (Collier, 1999) as well as institutions and social capital (Bellows and Miguel, 2006; Azam et al., 1994).

Only a handful of studies have tackled micro-level impacts. A small economics literature has estimated the earnings impact of military service on US 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 15 percent reduction in their long-term earnings as a result of involuntary recruitment—an impact due, he argues, to work experience lost. Similar empirical strategies have indicated higher levels of mortality and drug use among Vietnam veterans (Hearst et al., 1990; Hearst et al., 1986). It remains unclear, however, how these rich-country studies on war impacts generalize to child combatants in African civil wars.

Humphreys and Weinstein (2005) conduct the first large-scale, representative survey of ex-combatants in a civil war torn nation—Sierra Leone. They identify exogenous variation in the abusiveness of military units and show that it strongly predicts success in post-conflict reintegration. Their evidence suggests that exposure to violence is one of the most important determinants of social and economic reintegration after war. This empirical strategy holds combatants in low-abuse units as the counterfactual for combatants in high-abuse ones. To assess the overall impact of combat, however, a non-combatant counterfactual is required. Moreover, to test the violence and reintegration hypothesis directly, more detailed data on exposure to violence and outcomes are also needed.

The sole literature to attempt to measure violence and its direct effect on well-being is the psychological one. Such studies survey youth exposed to war violence and test them for psychological disorders, especially post-traumatic stress disorder (PTSD), aggression, and depression.⁵ 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. While insightful, these stud-

⁵ e.g. Dyregrove et al. (2002); Margolis & Gordis (2000); Mollica et al. (1997); Ajdukovic and Ajdukovic (1998); Husain et al. (1998); Kinzie et al. (1986).

ies 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.⁶ As a consequence, the proposed link between mental trauma and war violence is vulnerable to numerous forms of bias.

4. Data and Measurement

The population of interest is males currently aged 14 to 30 that were born in rural areas of the Ugandan districts of Kitgum and Pader (see Figure 1). The two districts were chosen for the exogeneity of their rural abduction patterns, discussed in the following section. Youth below 14 were considered too young to interview, while those over 30 were less likely to be targets of abduction during the war. Surveys were administered in eight clusters—four sub-counties in each of the two districts.

The survey faced three potential forms of sample attrition: youth that migrated away during the war, youth that perished, and youth that never returned from abduction. Three steps were taken to minimize any bias from such attrition. First, the study selected its respondents from a sample frame of youth living in the region before the war. 1100 households were sampled from lists compiled by the United Nations in 2002, and 1018 of these households (92.5%) were found. Household heads developed rosters of all youth living in the household in both 2005 and 1996 with the assistance of an enumerator. The study chose the year 1996 because it was easily recalled (as the date of the first election since 1980) and it pre-dates 85% of the abductions in Kitgum and Pader. 870 surviving male youth were randomly sampled from the retrospective rosters. Abducted youth were over-sampled.

Second, migrants were tracked across the country to their current location. Half of the surviving youth had migrated since 1996. 741 of the 870 youth (or 84 percent) were located, including all non-migrants and 70 percent of migrants. An absentee questionnaire was conducted with the families of all 129 unfound youth, collecting extensive education, employment, family, and abduction data.

Third, basic demographic data were collected on the 349 youth that had died since 1996 or had not returned from abduction. The latter can unfortunately be presumed perished, since few abductees remain with the LRA at this time (Annan et al., 2006). The following section describes how the data on unfound and non-surviving youth is used to account for potential bias from attrition.

⁶ Such a practice is especially inappropriate because baseline rates of psychological illness are not zero, and may reach up to 10 percent for some conditions (American Psychiatric Association, 2000).

The 741 youth that completed the questionnaire provided data on war experiences, educational economic outcomes, and social and political outcomes.⁷ Key variables are described in Table 1.

War and abduction experiences are self-reported and retrospective. While in some contexts we might be concerned that ex-fighters fail to report armed service, in Uganda this does not appear to be the case: there is little stigma associated with abduction, and some youth have been known to misrepresent themselves as abductees in order to receive aid. Several measures were taken to guard against misrepresentation. In community meetings and the interview, the absence of a link between the study and aid was made clear. Moreover, abductions were independently confirmed with the household head and inconsistencies 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.

Also note that the measures of violence, social exclusion, hostility and psychological distress are unweighted sums of the responses to survey questions, and proxy for latent characteristics. The survey questions were based on survey instruments developed for assessing violent trauma and psychosocial disorders in the context of war: the Harvard Trauma Questionnaire and the Hopkins Symptom Checklist (HPTR, 2005). These instruments were previously adapted to northern Uganda by MacMullin and Loughry (2002), and were further adapted and refined for this project by a psychologist with seven years experience working with northern Ugandan youth (see Annan, 2006).⁸

5. Empirical Methodology

Generally speaking, one would expect that any post-conflict differences between ex-combatants and civilians not only reflect the impact of war experiences, but also any pre-conflict differences that led some youth to self-select into the armed group, or an armed group to select those youth over others. Furthermore, mortality and migration are high in civil war, and so selection can occur in that we only observe those that survived or stayed behind. If combatants and civilians survive or migrate at

⁷ English versions of the questionnaires are available for download at www.SWAY-Uganda.org.

⁸ 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. Deciding which questions to include or exclude from a particular index only further increases the number of possible indices. The results reported in this paper are extremely robust to the method of index construction, including the addition or exclusion of questions, or the use of principal components or factor analytic weighting schemes. To avoid the cherry-picking of indices for their results, this paper employed a simple rule of thumb for the construction of 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 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.

different rates, or for different reasons, then any post-conflict differences may be further conflated with such differential attrition. In essence, each of these is a problem of potential sample selection.

The fundamental empirical problem we face is that it is impossible to observe the outcomes an ex-combatant would have realized had he not served in combat. The standard solution to this ‘missing data’ problem is known as the counterfactual approach, whereby a control group is identified and the average treatment effect (henceforth ATE) is estimated by taking the difference in the outcomes of the treated and the 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. Unfortunately, each of the potential selection problems discussed above constitutes a possible violation of this assumption of unconditional independence.

To deal with such violations, one can look for situations where treatment is based on observed variables, and is otherwise exogenous. Under this *unconfoundedness assumption*, treatment assignment is independent of the potential outcomes conditional on a set of observed pre-treatment variables, and the ATE can be consistently estimated (Imbens, 2006; Rosenbaum and Rubin, 1983a; Rubin, 1978). The following section will argue the unconfoundedness of abduction into the LRA after accounting for age and location, and thereby consistently estimate the impact of combat on youth.

While there are several alternative method for estimating the ATE under unconfoundedness, weighted least squares (WLS) regression with weighting on the inverse of a nonparametric estimate of the propensity score offers the most efficient and consistent estimates (Hirano et al., 2000). In this case the semi-parametric regression function for outcome Y is:

$$Y_i = \beta_0 + \tau \cdot T_i + X_i^S \cdot \beta_l + \varepsilon_i, \quad (1)$$

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.⁹ The weights used are:

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

where the ρ_i are sampling weights, the π_i are weights adjusting for survey attrition, and $\hat{e}(v_i)$ is a non-parametric estimate of the propensity score.¹⁰

⁹ 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 (i.e. 1.00) are included in X^S .

WLS estimation imposes strong parametric assumptions on the data, assumptions that are especially difficult to justify when the conditional outcome is non-normally distributed. One can alternatively estimate the ATE non-parametrically using a matching estimator (Abadie and Imbens, 2006; Abadie et al., 2001). As in regression (1), we can improve the efficiency of this estimator by re-weighting the observations by the inverse of their selection probabilities. The matching estimator is:

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

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

Testing the unconfoundedness assumption

Note that the crucial assumption above is that, within subpopulations characterized by covariate values, rebel recruitment is independent of potential outcomes. In most armed groups, such an assumption would be contentious. LRA abduction in the eight rural Ugandan areas surveyed, however, appears to offer an exception. Several partial tests are available to examine the credibility of this claim, including: (i) comparison of the means of pre-treatment traits; (ii) assessment of the predictive power of pre-treatment traits for abduction; (iii) comparison of the distribution of abduction probabilities (i.e. treatment assignment propensity) across treatment groups; and (iv) analysis of the sensitivity of the estimated treatment effect to the inclusion and exclusion of pre-treatment traits. The results of these tests, presented in the following section, suggest that observed pre-treatment variables are indeed largely independent of treatment assignment (i.e. abduction).

The greatest remaining concern is the remaining potential for selection on unobserved traits. Several possible sources of selection exist, including smarter youth “self-selecting” out of the armed group via a better ability to hide, or survival of only the physically strongest. While we cannot test the unconfoundedness assumption against unobserved selection directly, we can use sensitivity analysis to assess the robustness of the estimated ATE to violations of the assumption.

¹⁰ Sampling weights correct for the stratification of the sample. The weights for survey attrition are discussed below. Finally, the inverse propensity score weights are normalized so that the 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)

The exogeneity assumption can be relaxed by explicitly modeling a moderate degree of correlation between treatment and unobserved components of the outcomes (Imbens, 2003; Rosenbaum and Rubin, 1983b). The starting point of the sensitivity analysis is to weaken the unconfoundedness assumption by requiring independence of treatment assignment and potential outcomes conditional on the original vector of pre-treatment variables, \mathbf{X} , as well as a single unobserved covariate, U , that induces selection. By definition, such a U is correlated with both treatment assignment and the outcome. To model its impact, one must make parametric assumptions about the marginal distribution of U and the conditional distributions of treatment and outcomes given U and \mathbf{X} (see Appendix A). By examining a large set of hypothetical correlations with treatment and outcomes, one can thus bound the treatment effect. Moreover, one can thereby judge whether an unobserved covariate could conceivably eliminate (or materially reduce) the estimated ATE by comparing the impact of the hypothetical covariate with the actual impact of the observed pre-treatment variables. Influential pre-treatment covariates are thus held as a benchmark for comparison.

Dealing with selective attrition: re-weighting and bounding of treatment effects

The unconfoundedness assumption is also violated when the outcome and the likelihood of observing the outcome (i.e. sample attrition) are both associated with abduction. In this study, there are two main types of ‘attritors’: non-survivors and unfound migrants. Studies of survey attrition in the developing world have concluded that attrition due to death or migration in panel studies has surprisingly little impact on coefficient estimates, even with attrition rates as high as 50% (e.g. Fitzgerald et al, 1998, Falaris 2003). The tracking success rate of this study, 84%, far exceeds that in most US-based studies, and 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 raise some concern. For one, mortality rates among abducted males are twice that of the non-abducted (22% versus 11%). Meanwhile, the number of unfound migrants was greater among the non-abducted than the abducted (19 versus 9%). This paper thus takes several measures to guard against selection on observed and unobserved covariates. First, for all unfound youth, data on past and current outcomes were collected from family members. Following Fitzgerald et al. (1998), estimates are weighted by inverse attrition probabilities

to reduce bias from attrition on observable traits.¹¹ In the next section the paper shows that the more successful youth tend to out-migrate, and that these youth are disproportionately non-abducted. This finding suggests that attrition leads to an *underestimation* of ATEs, and so is less worrisome.

Second, particularly in the case of non-survivors, there remains a risk of bias arising from unobserved traits that influence survival, treatment assignment, and potential outcomes. For example, if stronger or smarter individuals are more likely to survive, then we will underestimate the average treatment effect (while we overestimate it if they are more likely to die). To see if the estimates are robust to such potential bias, the paper bounds the ATE with best- and worst-case scenarios. Lee (2005) suggests a trimming procedure, whereby the worst case scenario bound is calculated by dropping the best-performing non-abducted (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 procedure is implemented in the following section.

6. Results: The average impact of abduction on later-life outcomes

6.1 Testing the unconfoundedness of abduction

Northern Uganda appears to offer a special case where LRA abductions were conducted in what amounts to a quasi-experiment. Accounts of abduction practices from rebel leaders and abductees, as well as tests of survey data, strongly indicate that abduction in the eight areas under study is unconfounded with the individual’s pre-war characteristics and potential outcomes.

Two main factors account for the unconfoundedness of abduction. First, participation in the LRA was entirely involuntary in the areas surveyed, and so bias from self-selection is not a concern. The LRA was established by a handful of rebels that refused to settle for peace in 1988. A small number of initial volunteers were drawn largely from the neighboring district of Gulu. Murders and mutilations of civilians in 1991 destroyed what little support had ever existed for the LRA, however, and the supply of voluntary recruits virtually disappeared. Driven by powerful ideologies, Kony and his commanders refused to let a lack of public support stand in their way. By the early 1990s—the time

¹¹ Letting A_i be an indicator variable that equals one for attriters and zero otherwise, and W_i a vector of observed covariates that influence both the outcomes and also the likelihood of attrition. Sample attrition weights π_i are estimated as follows:

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

These weights are normalized to sum to one. If there is no selection on observables π_i will equal one.

that Kony's forces expanded operations in the surveyed sub-counties—abduction had become the sole means of acquiring recruits. During extensive field work in over a dozen sub-counties, it proved nearly impossible to find youth who voluntarily joined after 1991, even with the assistance of community leaders and former rebel commanders. Since the LRA's original volunteers readily self-identify, and since in many cases former recruits readily admit that they eventually enjoyed being rebels and even became loyal to Kony, it seems unlikely that post-1991 volunteers exist in significant numbers and are simply concealing their status.

Second, abduction tactics were large-scale and essentially indiscriminate, and interviews with the leaders of raiding parties suggest that by neither design nor accident did the rebels abduct a select group of youth. From their bases in the Sudan, rebel parties ventured into Uganda for weeks at a time, usually in groups of 15 to 25 fighters. These raiding parties had two principal aims: ambushing government forces; and raiding homesteads for food, supplies, and new recruits. Acholi households were especially vulnerable to such attacks. Rather than living in villages, roughly ninety percent live in relatively isolated homesteads in the countryside—a situation typical of East Africa. Rebels usually invaded homesteads at night, abducting all able-bodied members of the household to carry looted goods. Targets were generally unplanned, and raids *ad hoc*. After capture and the carrying of goods, the leaders of the abduction parties were under instruction to release any household members other than adolescent and young adult males. Seventy percent of abductions occur in this manner. The remaining abductees were set upon by passing rebels while in the fields or traveling on the road.

The survey data support these accounts of near-random abduction. The means of all pre-war covariates for non-abducted and abducted youth are listed in Columns 1 and 2 of Table 2. With the exception of year of birth, none of these differences are statistically significant at even a 10 percent level, and nearly all of the differences in means are close to zero (Column 3). The absence of any difference is especially remarkable when one considers that, in other African conflicts, these pre-war covariates have been identified as primary determinants of abduction by armed groups.¹²

Abduction into the LRA can be contrasted to participation in Local Defense Units, or LDU—a largely voluntary local 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 suggests that militia mem-

¹² Wealth, landholdings, education, and orphaning in particular have been found to be associated with voluntary and involuntary military participation (e.g. Honwana, 2006; Humphreys and Weinstein, 2006; Singer, 2005; and Cohn and Goodwin-Gill, 1994).

bers are significantly younger and wealthier than non-militia members, and their mothers are more likely to have only a small amount of primary education (Table 2, Columns 4 to 6).

The relevance of pre-war traits in predicting LDU membership, and the near irrelevance of the same traits in predicting abduction, are further illustrated using logit regressions in Table 3. Pre-war data on abduction, assets, and father's occupation are available for all youth, including the 478 non-surviving and unfound youth. With the exception of household size in 1996, the incidence of abduction appears unrelated to these covariates when entered linearly (Column 1) and with higher-order terms are used (Column 2). The coefficients on the higher-order terms are omitted from Column 2, but their joint significance is tested (and rejected) in F-tests reported at the base of the table.¹³ Indicators for year of birth and location are also jointly significant, but are omitted from the table.

A larger number of pre-war covariates are available from the 741 youth interviewed, including parental education and orphaning before the war (Columns 3 and 4). Only household size, year of birth, and location are significant and consistent predictors of abduction. The household asset variables are jointly rejected, with or without higher order terms, as are all other pre-war covariates.

In striking contrast, landholdings, livestock, and parent's education and occupation are significant predictors of joining the local militia (Column 5). Moreover, the coefficients in the militia regressions are generally much larger than in the case of abduction, even when not statistically significant. Forces of self-selection into (and selection by) armed groups are evident in the case of the militia, but not LRA abduction.

Of the observed traits, only age, location, and household size appear to predict abduction. Age matters largely because a youth's probability of ever being abducted depended on how many years of the conflict he fell within the target age range. Males younger than 11 and older than roughly 25 tended to be released or never abducted in the first place. Location matters as rebel activity varied across sub-counties. The significance of household size, however, is more puzzling. It may be that rebels, who traveled in small bands, were less likely to raid large households.

The distribution of predicted probabilities of abduction from this logit regression provides further evidence of unconfoundedness, as depicted in Figure 2. The overlap in the predicted probabilities of abduction between abductees (right hand panel) and non-abductees (left hand panel) is substantial.

¹³ Location and year of birth (i.e. age) dummy variables were also included in the regression, but are omitted from the table. Observations are weighted by their inverse sampling probability.

The addition of other pre-treatment covariates leaves the distribution of the propensity score almost undisturbed, whether abducted or not. Any difference in the distributions of abducted versus non-abducted is driven almost exclusively by the respondent's age and location of birth.

Below we shall see further evidence of unconfoundedness: first, the ATE of abduction will be unaffected by the addition of the pre-war covariates to the regression; and second, sensitivity analysis will show that the ATEs are sufficiently large, and the influence of covariates so small, that a reasonable amount of unobserved selection would not materially affect the results.

6.2 The impact of abduction

The survey data suggest that the most pervasive impact of abduction is upon education and productivity. Harmful psychological consequences, while evident, appear serious in only a minority, and social and political exclusion are not adversely affected. Table 4 displays the regression and matching estimates of the ATE of abduction for ten key outcomes. Each entry in the table represents a separate regression (Column 1) or matching estimate (Column 2) for the dependent variable listed at left. The WLS estimates control for age, location, and pre-war traits, while matching is performed on age and location. Estimates are weighted by inverse selection, sampling, and attrition probabilities.¹⁴

Educational and labor market impacts

The average education lost from abduction is not only substantial, but also equal in duration to the average length of stay. According to the WLS estimates, the abducted have 0.70 years less schooling, a figure that corresponds closely to the average length the abduction—8.5 months, or 0.71 of a year. The matching estimate is similar at 0.67 years. With average levels of education only seven years, abduction implies a 10 percent average reduction in education (see Table 1 for all means).¹⁵ As a consequence, both the WLS and matching results suggest that the abducted are 15 to 17 percentage points less likely to report that they are functionally literate—that is, able to read a book or newspaper. This figure suggests that abductees are roughly 1.75 times more likely to be functionally illiter-

¹⁴ For maximum model flexibility, age and location (and their interaction) enter as dummy variables. The coefficients on all controls are not displayed. The WLS standard errors are heteroskedastic-robust and clustered by survey stratum (location and abduction status). Matching is one-to-one of treatments and controls, based on exact matching of age group and location, followed by matching on age. Similar results obtain from increasing the number of matches. Matching is performed with replacement, and estimates are bias-adjusted (Abadie and Imbens, 2006). Standard errors are robust. Selection weights were predicted from a logit regression of abduction on household size and age and location dummies.

¹⁵ Abductees are also 9 percentage points less likely to be currently enrolled in school or vocational training.

ate than non-abductees.¹⁶ The magnitude of the literacy gap is easier to understand once we consider that, in Ugandan schools, functional literacy is usually reached after six to seven years of schooling. Falling below the average level of schooling by a year thus increases the likelihood of poor literacy. For instance, among youth in the sample, moving from six to seven years is associated with a 22 percentage point increase in functional literacy.

Labor market performance also suffers due to abduction, but in terms of the *quality* of work rather than the *quantity*. The WLS and matching estimates of the ATE of abduction on the probability of employment are substantively and statistically insignificant. Thus the abducted are no more or less likely to find work. Work found by abductees, however, tends to be of a lower skill and capital-intensity. Nine percent of youth in the sample are engaged in a profession, a vocation, or own their own small business. The WLS and matching estimates suggest that an abducted youth is 5 percentage points less likely as a non-abducted youth (or roughly half as likely) to be engaged in skilled work.

The ATE on wages is also negative, suggesting that the formerly abducted are less productive than their non-abducted peers. In WLS estimates not displayed in the table, wage levels are \$0.78 lower among abductees (significant at the 5% level). With average wages of \$1.91 per day among the non-abducted, this ATE is substantial. The distribution of wages is highly skewed, however, and so these level differences may be misleading. Table 4 displays the ATE using a log transformation of wages. The coefficient can be interpreted as the approximate percentage change in wages due to the treatment. The WLS result suggests that wages are 20 percent lower for abducted youth, although the result is not statistically significant (owing in part to the continued skew in residuals). The matching estimator, however, makes no parametric assumptions about the distribution of the residual, and is therefore more accurate. It suggests that wages are 28 percent lower for the formerly abducted.

One 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, bias from such ‘sample selection’ changes the interpretation of the ATE on wages, conflating the direct impact of abduction on wages with the indirect effects on the type of people that would be employed (Lee, 2005; Heckman, 1979). However, since abduction does not appear to affect the probability of employment (nor the

¹⁶ Abductees are also 6 percentage points (or 1.75 times) more likely to be fully illiterate (unable to read or write their name).

likelihood of earning zero wages) such sample selection bias is likely nonexistent or immaterial. Still, the wage ATE must be interpreted as the ATE for the subset of those youth that find employment, and do not necessarily represent the latent impact on the productivity and earnings of all youth.

One interpretation of the above results is that abduction interrupts the accumulation of skills and capital, reducing a youth’s earning potential and productivity. Two thirds of youth are engaged in casual labor and small entrepreneurial activities as their main source of income. Some of these activities require little capital or skill (such as quarrying stone or collecting firewood), while others require minor capital (hawking clothes or making charcoal), moderate capital (operating a bicycle taxi or brick-making), or substantial capital and skills (tailoring or construction, for instance). Employment is far from static, however, and could be characterized as an occupational ladder increasing in skills and capital, which a youth gradually ascends through the accumulation of schooling and funds.

Such an explanation is consistent with Angrist’s (1990) evidence that white U.S. veterans suffered lifetime earnings declines because of work experience foregone. Like Angrist, one can estimate a Mincerian human capital function, where wages are a function of education and experience. Within the subset of the sample with recorded wage data, the results of this regression are as follows:¹⁷

$$\ln(Wage)_i = \delta_0 + \delta_1 \cdot Education_i + \delta_2 \cdot Experience_i + \mu_i$$

0.16	0.05
[0.02]***	[0.01]***

The average abduction is 0.71 years, which by these estimates would be associated with 11 percent lower wages if abduction led to lower education, and 4 percent lower wages if it led to less experience.¹⁸ Both are less than the 28 percent decline in wages associated with abduction (Table 4, Column 2), suggesting that this simple human capital explanation is an influential but partial one.

Psychosocial and political impacts

Any impacts of combat on distress, aggression, and exclusion are important not just for their own sake, but also because, if present, they may impede peace-building. In particular, one risk of combat is that participants are socialized into violence. Aggression was measured by two indicators, one indicating whether the youth had been in a physical fight in the previous six months (7 percent of youth

¹⁷ See Mincer (1974) for the theoretical justification of this function. *Experience* is calculated as $(Age - 6 - Education)$. A linear *Experience* term enters much more significant than a non-linear specification. Standard errors are clustered by survey stratum.

¹⁸ $0.71 \times 0.16 = 0.11$ and $0.71 \times 0.05 = 0.04$.

in total) and another indicating self-reported aggressive behaviors such as being quarrelsome, threatening others, disrespecting other's property, and using abusive language (6 percent of youth in total).

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—or 1.7 times more likely than non-abducted youth. In-depth psychological assessments of a sub-sample of respondents by Annan (2006) suggest that while these results may indicate greater hostility among abductees, other possible interpretations exist. For instance, aggression is relatively taboo in Acholi society, and higher self-reported hostility could reflect a greater willingness to admit to these behaviors. More conclusive evidence of aggression will be sought in future follow-up surveys. Nonetheless, some socialization into violence, or at least its admission, seems to be a consequence of abduction.

Abductees also exhibit little evidence of social and political exclusion. Social exclusion was measured by an additive index of 14 forms of social support received in the previous four weeks (such as someone lending you items, comforting you when troubled, or helping you find work). While the abducted have a slightly lower level of social support, the ATE in Table 4 is significant only in the WLS case, and only at the 10 percent level. The ATE is also substantively small—abduction results in a decrease of just 0.23 on a scale that ranges from 0 to 14. In results not displayed, the abducted are also no more or less likely to report membership in a church or a school or community group. Abduction thus appears to have extremely little impact on social exclusion.

In contrast, however, the formerly abducted were much more likely to have participated recently in political life. Just a week before the survey a national referendum was held to amend the Constitution to allow for multi-party politics. Half of eligible youth voted, with former abductees 13 percentage points (or about 1.3 times) more likely to have voted. Evidence from a single referendum is not easily interpreted, but the disproportionate participation of abductees is certainly no evidence of exclusion from community and political life. It is consistent with a theory of expressive voting, whereby abductees turn out in large numbers because their experiences increase their desire to express their preferences via voting (e.g. Riker and Ordeshook, 1968; Downs, 1957). The evidence for this theory, however, is far from conclusive. The relationship between violence, abduction, and voting will be explored in more depth in a future paper.

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 for their intensity. The average youth exhibits 4.3 of these symptoms; the highest number of reported symptoms is 16. The WLS and matching estimates of the ATE indicate that abducted youth report half of one symptom more on average than non-abducted youth—an increase of roughly one sixth.

Yet while the average psychological impact is not large, the most traumatized youth do appear to be disproportionately former abductees. For instance, the formerly abducted are nearly three times as likely to have a measure of psychological distress above the 75th percentile (i.e. 6 or more reported symptoms). This top tail generally represents severe symptoms of depression and what might be considered a Ugandan form of PTSD. Nightmares and reliving of traumatic events figure in the index, and 20 to 25 percent of former abductees report such symptoms, compared to 10 to 15 percent of non-abducted youth. Furthermore, 10 percent of abductees report feeling always sad compared to 5 percent of their non-abducted peers. Issues pertaining to the construction and interpretation of the psychological measures and results are discussed in more detail in Annan (2006).

The results suggest that we should be interested in differences in the *distribution* of psychological outcomes, especially the top tail, rather than mean differences. If one compares the cumulative distribution functions of psychological distress for the abducted and non-abducted, as in Figure 3, we see that the formerly abducted have a much heavier top tail, implying increasingly greater severity of mental disorders than non-abducted youth.¹⁹ For instance, an abducted youth in the 90th percentile of his distribution reports two more symptoms of distress than a non-abducted youth in the same position. Meanwhile, at the median the difference between the two distributions is 0.62—nearly identical to the ATE.²⁰ Below we will see that these acute symptoms of distress are associated with exposure to extreme violence, and the disproportionate number of traumatized abductees is driven largely by their disproportionate exposure to such violence.

¹⁹ The line in Figure 3 is fit by running a least-absolute values regression of psychological distress on an abduction indicator and control variables (age and location indicators, as well as pre-treatment characteristics) at each quantile of the distribution.

²⁰ Under certain assumptions, these quantile differences could be interpreted as a quantile treatment effect (QTE). The QTE would represent the causal impact of abduction on psychological distress, where the size of the QTE varies depending on one's initial psychological health. Such an interpretation, however, is probably unsatisfactory. The increasing gap between the distributions of abducted and non-abducted youth's psychological distress is less likely a function of initial mental health than it is a function of the intensity of the war violence experienced. In essence this is a violation of the rank invariance assumption commonly required to identify the QTE (Firpo, 2006). What is at issue is the question of treatment effect heterogeneity versus a heterogeneous treatment. The latter case is explored in the following section.

Before moving on, we consider an alternative explanation for the patterns observe. Could the relatively moderate average psychological impact (and the absence of any social dislocation) be explained by the success of the aid and reintegration programs provided to former abductees? If so, this paper’s results would underestimate the true causal impact of war and violence on outcomes. This scenario seems unlikely, however. Unfortunately, reintegration programs have been modest in scale and reach (at least relative to the volume of need). Reception centers are the primary means of care for former abductees upon their return from the bush, and yet only half of abducted youth passed through one (Annan et al., 2006). Moreover, abductees who do pass through these centers receive only basic medical treatment, assistance with family reunification, and “counseling”—in essence advice-sharing from relatively untrained social workers. Only 1 in 10 youth had been followed up after leaving the reception center, at least at the time of the survey. The estimates presented in this paper thus reflect the long-term impact of combat conditional on half of abducted youth receiving only the most rudimentary reintegration services that a society could provide.

6.3 Robustness and sensitivity of the results

The above results are robust to changes in both controls and specification. Focusing on five outcomes—education, literacy, log wages, hostility, and psychological distress—we can compare the WLS estimates already discussed (reproduced in Column 1 of Table 5) to regressions that drop pre-treatment characteristics (Column 2), further drop the age and location dummy variables (Column 3), and further still eliminate the weights correcting for attrition (Column 4). The coefficients on each outcome vary little as a consequence of the changes.

The relative constancy of the estimated ATEs as controls and weights are removed is strong evidence of the balance on observable characteristics between abducted and non-abducted. The estimated ATEs are virtually identical to the original WLS results, save for education and wages when we examine the weighted difference in treatment group means (Columns 5 and 6). The drop in the education and wage coefficients suggests that there may be some selection on observable traits (even though statistically-speaking we cannot reject equality of the coefficients). If there is selection on observable traits, however, it is almost completely selection on year and location of birth alone, as expected. Adding age and location controls back into the estimation equation (Columns 7 and 8) re-

turns all coefficient estimates back to the levels and significance of the original WLS estimates. Together, these results are strongly supportive of the exogenous abduction hypothesis.

Sensitivity to unobserved selection

Of course, one may reasonably remain concerned that the above treatment effects still reflect unobserved pre-war differences in the abducted and non-abducted. It is possible, for instance, that more intelligent youth evaded abduction attempts, or that weaker abductees were more likely to perish. We are principally worried about bias that leads to overestimation of the ATEs, bias that would arise from the systematic selection of “low-types” into the pool of abductees, or from differentially greater death or attrition of “high-types” among abductees. The risk is non-ignorable. For instance, we can compare non-surviving abducted youth to survivors that received a serious injury from the LRA (i.e. “nearly” died) and see whether they differ along baseline characteristics. Those who did not return were slightly more likely to come from a wealthy household than those that returned with an injury.²¹ While inconclusive, this could be evidence of the selective death of “high-types” among abductees. Through various forms of sensitivity analysis, however, it can be shown that reasonable amounts of selection on unobserved traits could not account for the treatment effects observed.

For an observed covariate, X , to induce selection it must explain previously unexplained variation in both treatment assignment, T , and the outcome, Y . Figure 4 plots each of the pre-treatment controls according to the additional variation they explain (with the influence of X in explaining Y on the vertical axis, and the influence of X in explaining T on the horizontal axis). With the exception of age and location, the observed covariates explain little variation, a fact which accounts for the unresponsiveness of the ATE to their exclusion or inclusion in Table 5. Likewise, any unobserved trait, U , that is hypothesized to lead to selection into the armed group must explain previously unexplained variation in both T and Y . A simple parametric model from Imbens (2003) for analyzing the sensitivity of the ATE to a hypothetical U is developed in Appendix A, where the correlation between U and T is summarized by the parameter α , and that between U and Y by the parameter δ . The sensitivity analysis considers a range of plausible combinations of α and δ that could eliminate or materially reduce the predicted ATE. For example, for a binomially-distributed U that is independent of X , the downward sloping curve in Figure 4 represents all the combinations of α and δ that could reduce the ATE

²¹ These results come from a logit regression of an indicator for non-survival on baseline characteristics.

on education to its 95% confidence bound closest to zero: from -0.70 to -0.44. None of the observed covariates other than age or location come close to passing this threshold—a threshold that would still leave the sign and significance of the ATE intact.

A second method of sensitivity analysis can be examined, one more suited to accounting for possible bias from selective non-survival or migration. In a method proposed by Lee (2005), “best-case” and “worst-case” scenarios for differential attrition can be constructed by trimming the top or bottom of the distribution of the outcome in the treatment group with less attrition (Table 6). 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 the schooling and injury variables, since these data were collected on unbound migrants from their families. Lee’s method compares the untrimmed ATE (Column 3) to the trimmed means: the best and worst case scenarios (Columns 4 and 5).²² 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 smaller 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 education, labor market, and health outcomes.

6.4 Child versus adult combatants

As the sample includes youth abducted both as children and as adults, it is possible to examine how the ATEs varied by age of participation, and thereby identify what is special about being a child soldier. The evidence suggests that, if anything, child soldiers are more resilient than adult ones.

Graphically, the long run impacts on educational attainment and log wages appear most adverse for those taken as adults. Figure 5 displays the output from a locally-weighted regression of deviation from predicted education on age of abduction. The deviations from expected education are simply the predicted residuals from a regression of education on year and location of birth indicators and pre-war characteristics, using non-abducted youth only. The predicted residuals for abducted youth therefore measure the gap between that youth’s attainment and the average attainment of non-abducted youth of the same age and location. This benchmark is represented by the horizontal line at zero. The horizontal line at -0.70 represents the average gap between abducted and non-abducted youth (the

²² The untrimmed ATE is simply the unadjusted difference in the treatment and control means, weighted by inverse sampling and selection probabilities. It is not a regression technique, so does not employ controls or clustered and robust standard errors.

ATE from Table 4). Those abducted as young children and adolescents appear to have an educational deficit of roughly 0.5 years relative to their peers, while the deficit for those abducted as young adults can exceed a full year. Figure 6 takes a similar approach to assessing abduction's impact on log wages by age of abduction. The adverse impact of abduction on wages appears to be roughly 20 percent for adolescents, but falls to more than 35 percent for those abducted as young adults.

This relationship between age of abduction and wages (but not the relationship with education) proves to be statistically significant in a simple linear regression (Table 7). The table displays regressions of each outcome on a linear age of abduction term, including only abductees in the regression.²³ The signs on the age of abduction terms indicate that, with the exception of political participation, outcomes almost universally worsen as age of abduction increases. Only two of these estimates are statistically significant, however: those for wages and voting behavior. Overall, however, the signs on the coefficients imply that the belief that child soldiers are less resilient than adult ones is an unfounded one. If anything, child abductees seem to do better, particularly in terms of productivity.

7. Treatment heterogeneity: Estimating the causal impact of war violence

7.1 The heterogeneous treatment problem

So far this paper has handled military recruitment as though it were a binary treatment. This approach, however, obscures the heterogeneity of most military experiences. As noted in the introduction, Angrist's (1998, 1990) finding that white Vietnam veterans saw their long-term earnings decline is in fact an average impact over youth that saw combat and youth that did not (with the weight likely heaviest on the latter). Likewise, in northern Uganda, not all abductees became fighters and not all were abused. Abduction length ranged from a day to ten years, and violence varied as well—while 74 percent witnessed killings and 60 percent were severely beaten, only a quarter were ever forced to kill. The treatment considered by both Angrist and this paper is inherently multi-valued.

How can we interpret the binary treatment ATEs when treatment itself is heterogeneous? When treatment is multi-valued it is more appropriate to think of the response to abduction as a function of units of exposure (i.e. length and violence). Angrist and Imbens (1995) have shown that in this in-

²³ The regression is weighted by inverse sampling, attrition and selection weights, and in addition to the usual control variables (year and location of birth indicators and pre-war covariates), each regression include dummy variables for abduction length.

stance, the binary ATE can be interpreted as the average per-unit effect along a response function mapping treatment intensity to outcomes.

Ideally we would like to estimate the entire response function. One way to approximate this function is to examine the association between outcomes and measures of the intensity of abduction. Two measures of the intensity of abduction include an index of violence (a scale of 12 traumatic events) and the number of years abducted. Regression estimates in Table 8 suggest that the heterogeneity of treatment matters—longer abductions are associated with poorer educational outcomes, while violence is associated with lower wages and greater aggression and psychological distress.²⁴

For starters, schooling is strongly negatively correlated with the length of abduction, but exhibits little association with violence. A year of abduction is associated with 0.54 fewer years schooling and a 6 percentage point reduction in the probability of being functionally literate. This relationship is depicted in Figure 7, which displays a locally-weighted regression of the deviation from predicted education (i.e. the deviation from the education level of non-abducted youth of the same age and location) on abduction length. The education gap for the abducted appears to be small for abductions less than six months long and sharply increasing thereafter.

In contrast, lower wages are associated with more violent experiences but not longer abductions. The reasons for this relationship are not immediately clear. Violence may affect productivity and wages via the channel of mental traumatization, except that, after accounting for education and experience in a Mincerian wage determination equation, psychological distress has no substantively or statistically significant explanatory power for wages (regression not displayed). Moreover, the absence of an inverse relationship between abduction length and violence runs counter to the argument developed above, where the adverse impact on abduction was hypothesized to arise from time out of the workforce. It seems that the education channel, which is strongly associated with abduction length, may be more important. Nevertheless, this remains a puzzle to explore in future research.

Less surprisingly, aggression and psychological distress display a strong relationship with violence (although not abduction length). An additional incident of war trauma is associated with a one percentage point increase in the probability of having been in a fight in the past six months. With only 7 percent of youth reporting they were involved in a fight, this impact is substantial. Moreover,

²⁴ The regressions include year and location of birth indicators and pre-war traits. Sampling and attrition weights are used. Also used are indicators for the age and year of abduction to reduce unexplained variation. The estimates are robust to their exclusion.

an additional incident of violent trauma is associated with 0.38 more reported symptoms of distress on average. This relationship between violence and distress is depicted in Figure 8, a locally-weighted regression of the index of psychological distress (also adjusted for age and location) on the index of violence. The relationship is nearly linear, with a slope of roughly 0.38. This finding suggests that the concentration of psychological distress in a minority of formerly abducted youth is a consequence of selective exposure to violence rather than the abduction in general.

If assignment to different treatment values were unconfounded with potential outcomes, the coefficients from Table 8 would provide linear approximations of the (causal) treatment response function. When examining the length and intensity of combat, however, this unconfoundedness assumption is unlikely to hold. While abduction itself is exogenous, how long one stays with the rebel group, or what violence one experiences may not be. Much of the variation is likely random—escape after all seems to be a matter of the right opportunity, and violence may be a function of being in the wrong place at the wrong time. Yet the length or violence of one’s stay is quite possibly associated with intelligence, strength, emotional stability, and other pre-existing traits that influence outcomes.

How does this potentially endogenous exposure affect our interpretation of the ATEs? In this case, the ATE consistently estimates the *ex-ante expectation* of the average per-unit effect along a response function *on a randomly-selected abductee*—still a useful and informative statistic. If we are interested in estimating the entire causal response function, however, endogeneity will lead WLS estimates (like those in Table 8) to overstate or understate the true causal relation. Take, for example, the relationship between violence and psychological distress that was illustrated in Figure 8. Underestimation of the impact could arise if, as the psychological literature suggests, a pre-existing psychological condition (e.g. a psychopathic disorder) results in the both commission of greater violence and an absence of remorse or distress later on.²⁵ Overestimation could occur if youth exposed to violence prior to the war (e.g. from domestic abuse) leads to both greater measured violence and to later psychological distress. Finally, errors in measurement of violence will tend to bias the coefficients on the dependent variables towards zero, also understating the causal effect (i.e. attenuation bias).

²⁵ Prevalence of disorders in US males is estimated at 3 percent of the population (American Psychiatric Association, 2000)

7.2 Instrumental variables estimation of the treatment response function

One solution to the concerns of endogeneity and attenuation bias is the use of instrumental variables (IV). In this analysis the outcome of interest is determined by observed characteristics and two potentially endogenous regressors: violence experienced and length of exposure. The endogeneity concerns discussed above imply that violence and length may be correlated with unobserved factors in the error term, thereby introducing bias. An IV, if it exists, identifies exogenous variation in at least one of the endogenous variables (i.e. variation that is uncorrelated with the error term) and uses that variation to identify the causal relationship between the outcome and endogenous regressor.

This section proposes to instrument for violence using a measure of the abusiveness of the respondent's military unit, *as reported by other youth in the same unit*. The endogeneity concerns discussed above generally arise from unobserved individual traits that influence the degree of violence experienced. By using a group-level measure to predict violence, these individual sources of bias are avoided, especially since the group-level measure comes from reports by others. As we will see below, not only does unit abusiveness predict violence experienced, but it likely affects outcomes through the channel of violence alone—a key property of a valid IV. This empirical strategy follows Humphreys and Weinstein (2005), who examine the reduced form relationship between reintegration success and unit abusiveness in Sierra Leone. By measuring the channel of impact (violence) directly, this paper is able to use abusiveness as an IV and thereby identify the direct causal relation.

As in Sierra Leone, interviews with Ugandan abductees reveal the wide variation in abuse by military unit, typically due to the temperament of the rebel commander. From one interview:

Q. Can you tell me about the leaders in your group? How were they?

A. Some people were cruel while others were good... For example, [name of commander] was bad and he used to say he does not make friends with anyone. Every time he wants to beat people he says, to be a soldier, you must be one who is sad and cruel. He therefore beats us even if we have not done anything bad. He says that his purpose is to make people angry.

Other commanders attempted to promote loyalty and discipline through non-violent means:

[My commander] was not arrogant. He used to advise his fellow commanders that they should not treat their abductees cruelly because if you have taken someone to help you then why do you torture the person? He had a sympathetic heart...

Often abductees report a mix of positive and negative reinforcement:

One of [the leader's] escorts was really cruel. Once you did something... this escort would report you to the leader and the leader normally responded by warning you with beatings—the least being ten strokes, and that is if you are pitied. But in all, the leader was still friendly. He could interact with us freely, unlike leaders of other groups...

To measure such abusiveness, the survey asked former abductees five questions about the general treatment of abductees and fighters in their unit, in particular some of the more upsetting and sadistic practices. A standardized index of abusiveness was created from these five questions.²⁶

As demonstrated by Angrist et al. (1996), to be valid an IV must meet four criteria: independence, excludability, relevance, and monotonicity. Since an abductee's responses to the abusiveness questions are undoubtedly colored by his length of stay or his particular experiences, his recorded index is unlikely to satisfy the independence criterion. An exogenous measure of the unit's abusiveness, however, would be the average response from other youth in the same unit.

To implement this strategy, abductees were traced to a particular unit based on their time and place of abduction. LRA commanders tended to remain in a particular locale for months or years at a time. Certain areas were preferred over others because they offered cover (e.g. forest or tall grasses), a particular hideout, or access to a supply network. Thus youth abducted in the same subcounty and year generally had a high likelihood of being abducted by the same military unit and commander. 54 military units were identified in this fashion, with an average of 10 respondents per unit.²⁷ To calculate a measure of unit abusiveness for each of the 462 abductees, a regression of abusiveness on 54

²⁶ The questions asked of unit members were: (i) how frequently members were forced to beat or kill new abductees; (iii) how frequently members of the unit mutilated civilians; (iv) how frequently abductees were punished for no reason at all; (v) how often abductees were forced to desecrate dead bodies; and (v) whether fighters were beaten or killed for disobeying orders. A weighted index of these five questions was created using principal components analysis (i.e. using the first eigenvector of the variance matrix as the weights). This principal components-weighted index performs better as an instrument for violence than a simple additive index. While the additive index yields very similar results, the first stage relationship between abusiveness and violence is weaker, threatening the consistency and efficiency of the results from potentially weak instruments.

²⁷ The survey covered 8 subcounties with abductions over 11 years, for 88 possible "units". Two pairs of subcounties were combined due to their close proximity, and because it is common knowledge that the rebel unit plaguing one also plagued the other. Finally, 12 year/location combinations had no recorded abductions. Thus we arrive at 54 units.

unit dummy variables was run 462 times, each time dropping a single respondent from the regression. The predicted value for the omitted individual—i.e. the average abusiveness according to other members of the unit—is used that individual’s exogenous measure of unit abusiveness.

While it is not possible to test them explicitly, both the monotonicity and excludability of the unit abusiveness proxy appear sound. In terms of monotonicity, increases in the cruelty of a military commander seem unlikely to increase violence for some and decrease violence for others. In terms of excludability, the key assumption is that unit abusiveness matters for our outcomes of interest only because of its impact through violence. Although the unit abusiveness proxy could plausibly impact length of abduction (for instance, by changing the incentives and disincentives associated with escape) even this mechanism arguably operates through the channel of violence, and so the impact on length (and ultimately outcomes) is indirect. Moreover, in practice unit abusiveness is neither substantively nor statistically significantly associated with length of abduction.

Finally, the proxy for unit abusiveness is also relevant. A first-stage regression of violence on unit abusiveness (including the second-stage controls) yields a coefficient of 0.50 with an F-statistic of 12. This test suggests that the IV meets reasonable standards of relevance.²⁸ The F-statistic falls to 9.3, however, if unit-specific error terms included (i.e. the standard errors are clustered by unit).

Finally, note that the coefficient on abduction length cannot be given a causal interpretation in the absence of an IV for length of exposure. The study explored several instruments for length of abduction, including: lengths of abduction of others in the unit, the number of youth abducted at the same time, and the incidence of rescue operations by the Ugandan army. While each had the expected relationship with violence, all were relatively weak (had F-statistics below 3). Since length can be observed and included in the regression, its potential endogeneity will not bias the IV coefficient on violence directly. The presence of an endogenous regressor in any regression, however, can lead to slightly inconsistent estimation of *all* coefficients. Accordingly, in the IV results reported below, a causal interpretation of the IV coefficient on violence requires the assumption of conditional exogeneity of abduction length. This assumption will be made throughout the discussion below.

²⁸ To guard against the problem of weak instruments, an F-statistic of at least 10 is the common rule of thumb for a single instrument (Stock et al., 2002; Staiger and Stock, 1997).

7.3 Results

The IV results suggest that, as with the WLS estimates, the main impact of war violence is upon productivity (i.e. wages) and psychological distress, although the result on aggression is no longer statistically significant. These results are displayed in Table 9, and include the WLS results from Table 8 for comparative purposes.²⁹ In general the causal IV estimates appear much larger than the WLS ones. The IV coefficient on psychological distress suggests that distress has a virtually one-to-one association with violent trauma. Again this result bolsters the conclusion that concentrated traumatization is a direct result of concentrated and extreme violence, and not a general phenomenon.

Likewise, the impact of violence on wages is stronger under IV, rising from a 10 percent decrease under WLS to 41 percent under IV. This impact is extremely large, and adds to the puzzle of the wage results observed throughout this analysis. Future versions of this paper will seek to tackle and explain these wage and productivity puzzles.

Several factors could account for the difference between IV and WLS estimates. First, it should be noted that the width of the confidence intervals imply that, from a statistical standpoint, we cannot reject the hypothesis that the WLS and IV results are equal. Yet the divergence is sufficiently large that the IV estimates could be telling us something new. This is especially true in the case of psychological distress. One possibility is endogeneity bias in the WLS estimates, but another lies in noting that the IV estimator in effect estimates a *local* average treatment effect (Imbens and Angrist, 1994). The particular IV employed captures variation in the abusiveness and sadism of military unit commanders. While the unadjusted index of violence captures mild experiences and traumatic ones, the variation in the unit abusiveness measure is driven by acts of intense cruelty and trauma alone. Thus the variation in the violence index isolated by the IV is likely the most extreme form of violence, and the coefficients capture a localized effect on the treatment response function, rather than recovering the entire function. If true, this interpretation makes the size of the IV estimates much more plausible.

²⁹ Control variables include year and location of birth indicators and pre-war traits, as well as indicators for the age and year of abduction. Levels of violence varied over both the stage of the conflict and by the age of the abductee, and so controlling for these characteristics increases the precision of the IV estimates. Finally, abduction length is entered not as a linear term but as a series of indicator variables for the number of months abducted, for maximum flexibility. Inverse sampling weights are used.

8. Conclusions

That forcible recruitment into a rebel group is harmful comes as no surprise. The real questions of interest are rather in what ways, how much, and for whom? Under this paper's assumption of unconfounded abduction, the long term impact on education and productivity seems to be large and broad-based—results consistent with the view that abduction's impact on skills and earnings is mainly a consequence of time away rather than war trauma. Time away appears most devastating for skills accumulation. Acute psychological trauma, meanwhile, appears to be limited to those that have experienced the most violence, and on average formerly abducted youth appear nearly as psychologically healthy as their non-combatant peers. Aggression is also only weakly associated with abduction and violence, and social exclusion not at all. Most unexpectedly, by almost all measures child soldiers appear little different than adult ones, and if anything are more resilient than abducted adults.

These findings can be juxtaposed against current policy in Uganda, where most of the limited programming and funding are directed towards the psychosocial rehabilitation of child soldiers. These programs are sufficiently limited, and have reached few enough youth, that the psychosocial resiliency exhibited by the sample is probably weakly related to the interventions. The results suggest that a more appropriate program response would be broad-based funding for age-appropriate education and economic programs, programs described in Annan, Blattman and Horton (2006)

In what sense these results can be generalized beyond northern Uganda? While the 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. Popular discourse in Uganda holds that the abducted should not be held accountable for their actions. Parents of non-abducted children, for instance, frequently noted in interviews that it could just as easily have been their child that was taken, and all recognize the importance for their community of welcoming back the two fifths of young males that were taken over the years. The remarkable community response observed in Uganda directly diminishes the social exclusion of abducted children, and indirectly may mute 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 not receive as warm a welcome after war, the treatment effects estimated in this paper might be regarded as a minimum impact.

Concerns of external validity may be greatest with respect to the economic and educational findings in particular, as they seem difficult to separate from the local context—mass displacement and complete economic disruption. Therefore the results are most easily generalized to situations of ongoing insecurity. Unfortunately northern Uganda has a great deal of company in this respect; large-scale and long-term displacement of the population and destruction of the local economy are becoming commonplace. Such complex and long-term emergencies include Liberia, Sierra Leone, Sudan, Mozambique, the DRC, southern Uganda, and Angola, to name but a few.

Evidence from other regions, however, suggests that this paper's economic and educational results will be of broader relevance as well. The relationship between violence and long-term reintegration success coincides with that found by Humphreys and Weinstein (2005) in Sierra Leone years after the war ended. The results in this paper even coincide with those from studies of US soldiers. Angrist (1990) finds similar impacts of war on Vietnam veterans—a 15 percent wage decrease that he blames on lost work experience. Levels of income, schooling, and economic activity may change, but what may remain constant is the relative gap between ex-combatants and civilians, and the relative salience of the economic and educational impacts of combat versus psychological, social, and political ones.

Ultimately external validity is so difficult to assess in large part because of the paucity of micro-level data on war and war's consequences. Very simply, there is a need for more research in more zones of conflict worldwide. 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 and programming in post-conflict reintegration, redevelopment, and peace-building.

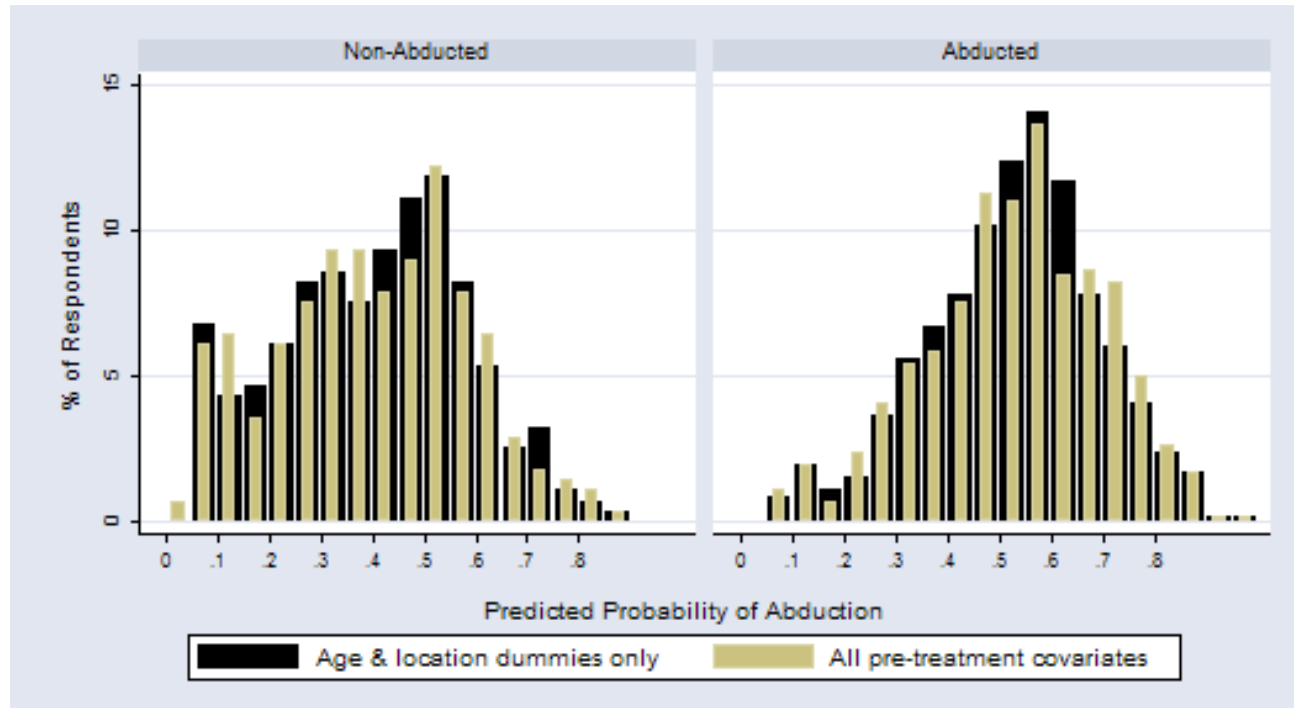
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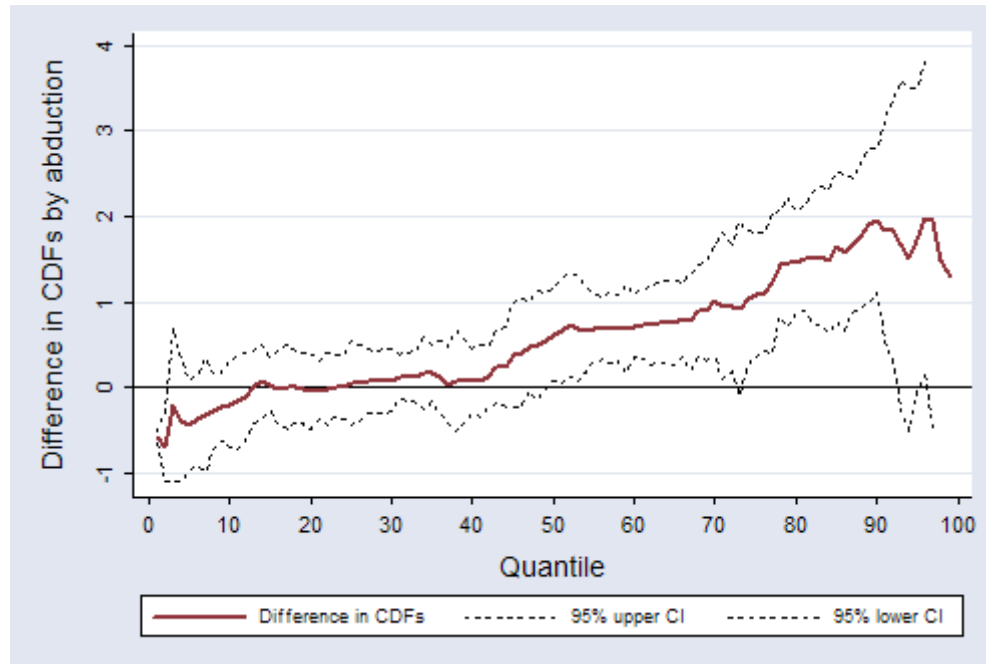
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Figure 2: Distributions of the predicted probability of abduction based on age and location alone versus all pre-treatment covariates (by abduction status)



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, religion, and father's main occupation.

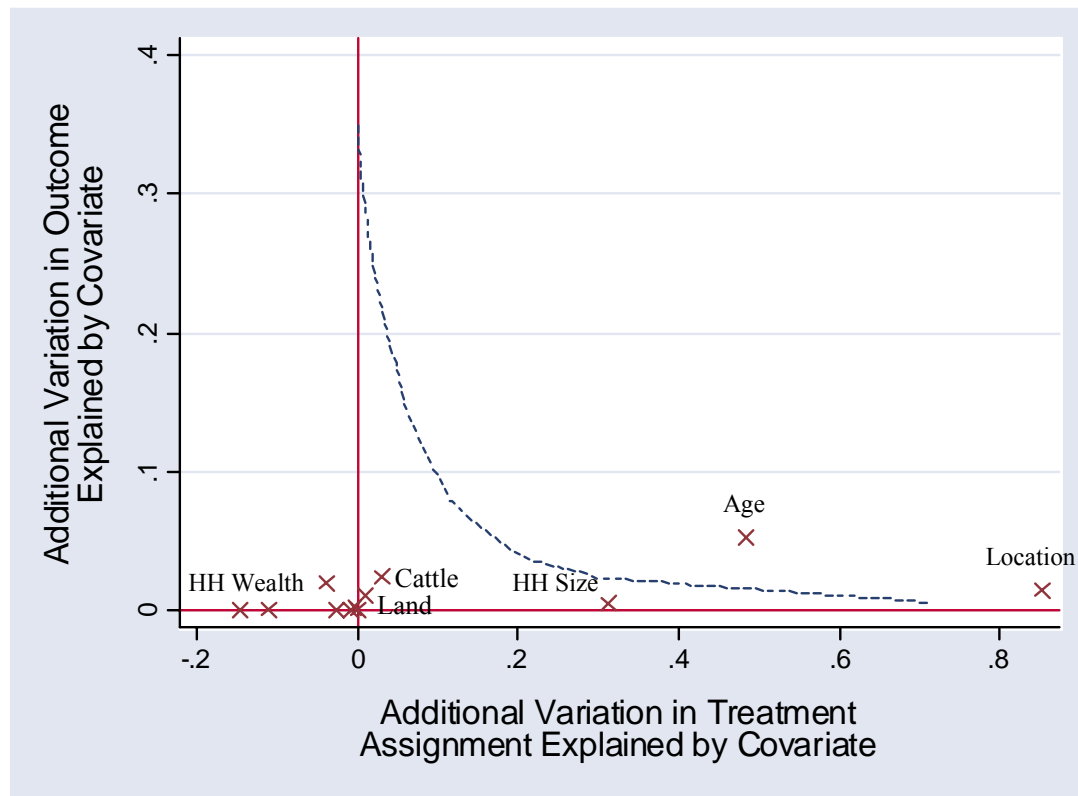
Figure 3: *Difference in the cumulative distribution of psychological distress of abducted versus non-abducted youth, by quantile*



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 and location dummies and interactions, as well as pre-treatment covariates.

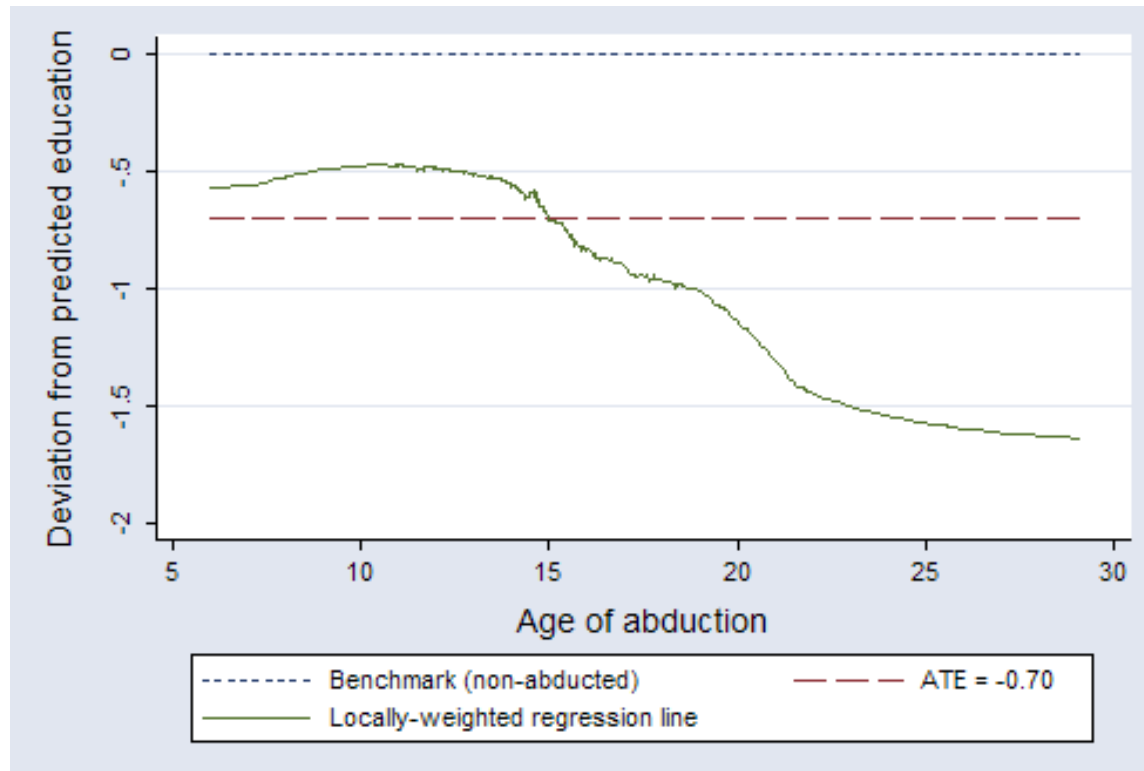
Figure 4: Sensitivity analysis on the ATE of education

The threshold an unobserved covariate must have to induce sufficient selection that the ATE on education is reduced from the ATE of -0.70 to its lower confidence bound of -0.44



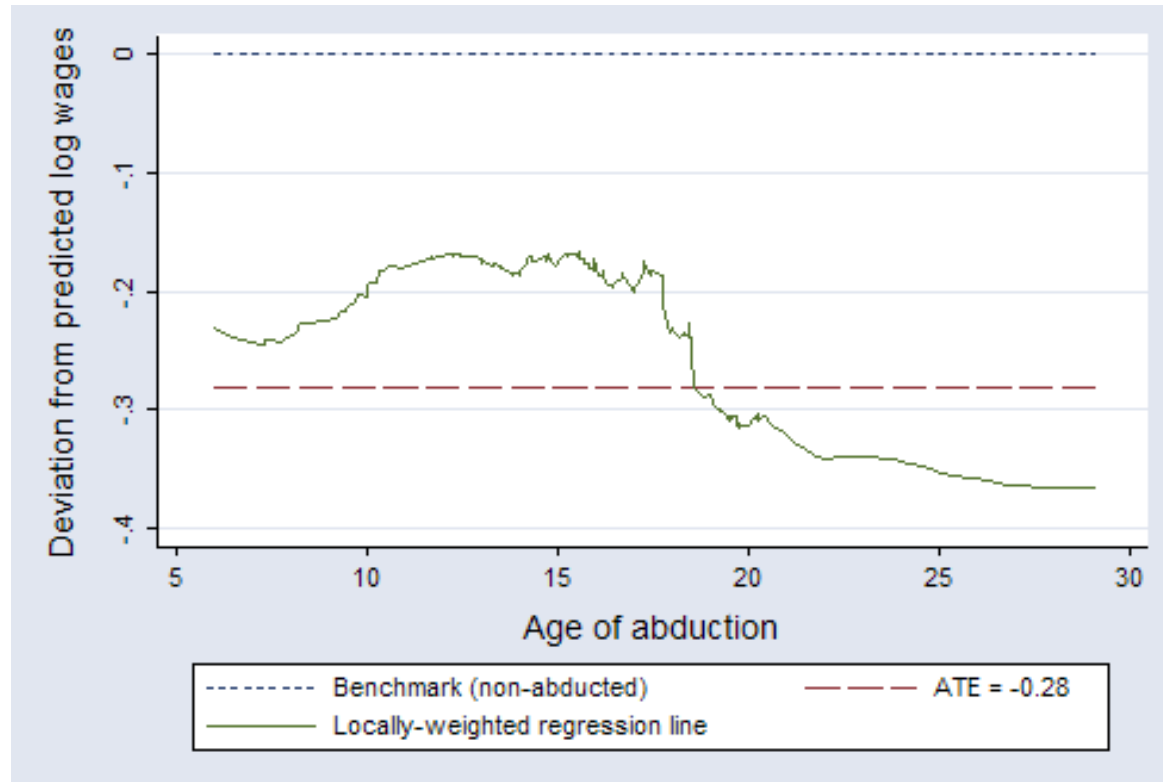
Notes: The figure presents the results of the sensitivity analysis following Imbens (2003). Each \times represents a pre-treatment covariate, plotted according to its additional explanatory power for treatment assignment (on the horizontal axis) and its explanatory power for the outcome (vertical axis), 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 covariate (observed or unobserved) would have sufficient association with both treatment and the outcome (educational attainment) to reduce the treatment effect on education to its lower confidence bound.

Figure 5: Locally-weighted regression of abduction age on the deviation from predicted years of education



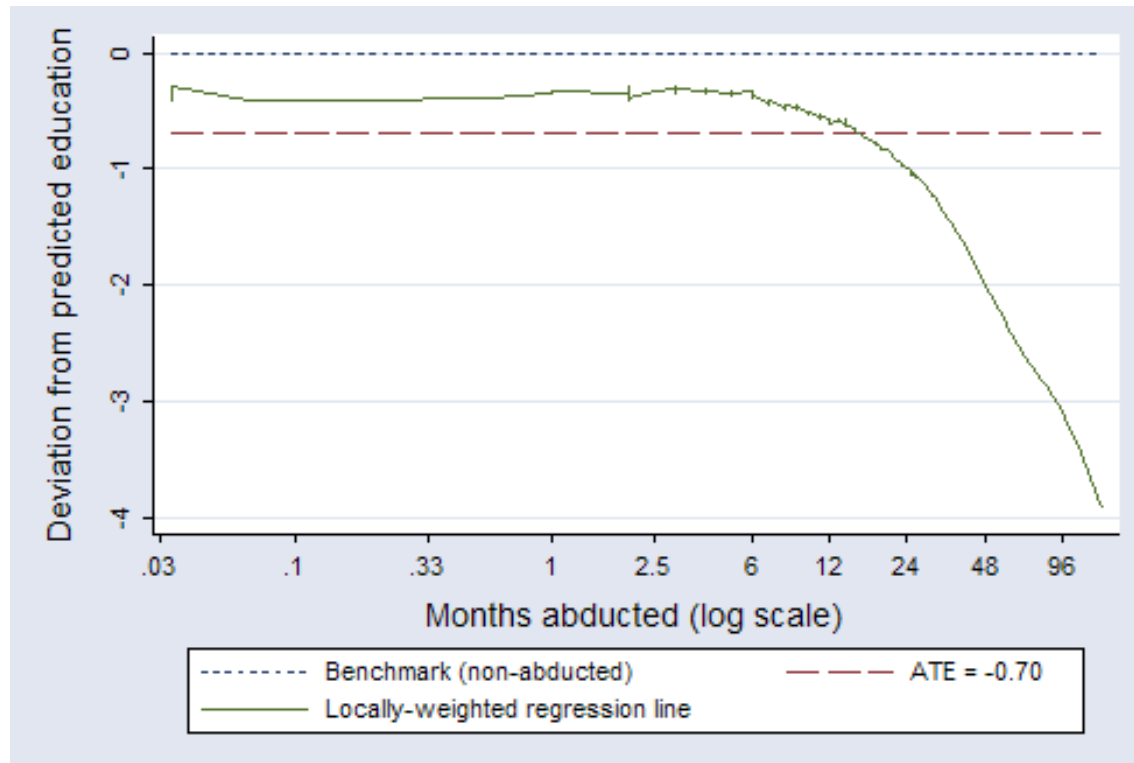
Notes: Predicted years of education are calculated from a regression of education on year and location of birth indicators and pre-treatment covariates, using non-abducted youth only. The predicted residuals for abducted youth therefore measure the gap between that youth's attainment and the average attainment of non-abducted youth of the same age and location. This benchmark is represented by the horizontal line at zero. The predicted residuals are regressed on age of abduction in a locally-weighted regression with a bandwidth of 0.8 with mean smoothing. N = 462 abductees.

Figure 6: Locally-weighted regression of abduction age on the deviation from predicted log wages



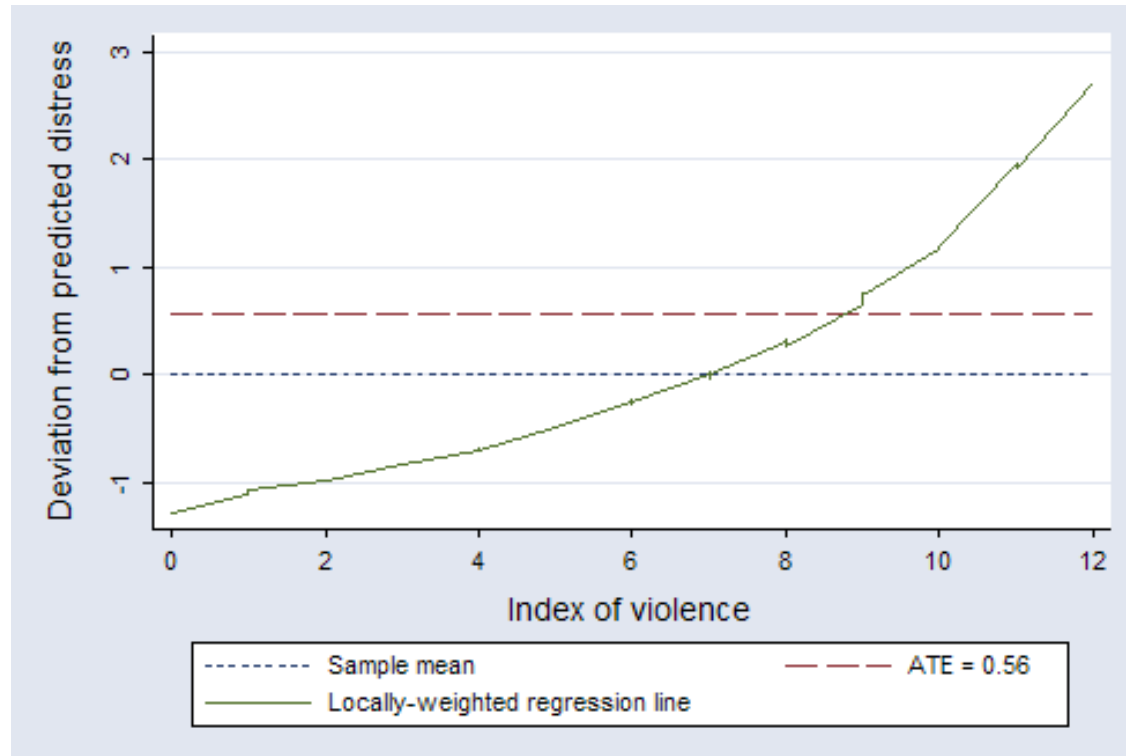
Notes: Predicted log wages are calculated from a regression of log wages on year and location of birth indicators and pre-treatment covariates, using non-abducted youth only. The predicted residuals for abducted youth therefore measure the gap between that youth's attainment and the average attainment of non-abducted youth of the same age and location. This benchmark is represented by the horizontal line at zero. The predicted residuals are regressed on age of abduction in a locally-weighted regression with a bandwidth of 0.8 with mean smoothing. N = 462 abductees.

Figure 7: *Locally-weighted regression of age and location-adjusted education on the index of violence*



Notes: Predicted years of education are calculated from a regression of education on year and location of birth indicators and pre-treatment covariates, using non-abducted youth only. The predicted residuals for abducted youth therefore measure the gap between that youth's attainment and the average attainment of non-abducted youth of the same age and location. This benchmark is represented by the horizontal line at zero. The predicted residuals are regressed on the length of abduction in a locally-weighted regression with a bandwidth of 0.8 with mean smoothing. N = 462 abductees.

Figure 8: Locally-weighted regression of age and location-adjusted psychological distress on the index of violence



Notes: Predicted distress is calculated from a regression of the index of distress on year and location of birth indicators and pre-treatment covariates, using non-abducted youth only. The predicted residuals for abducted youth therefore measure the gap between that youth's distress and the average distress of non-abducted youth of the same age and location. This benchmark is represented by the horizontal line at zero. The predicted residuals are regressed on the length of abduction in a locally-weighted regression with a bandwidth of 0.8 with mean smoothing. N = 462 abductees.

Table 1: Description of key variables

Variable	Description	Sample mean	Standard deviation	# of Obs.
<i>War Experiences</i>				
Months abducted (Abductees only)	Months from the date of abduction to the date of return (for the respondent's longest abduction).	8.5	[16.0]	462
Age of abduction (Abductees only)	Age (in years) at the time of the respondent's longest abduction.	15.2	[4.8]	462
Index of violence	Sum of 12 indicators of violence. Three of the 741 respondents declined to respond.	4.6	[3.5]	738
<i>Educational & Labor Market Outcomes</i>				
Educational attainment (in years)	Number of years of education (including tertiary and vocational training)	7.2	[3.0]	741
Indicator for functional literacy	Indicator equaling one if a respondent is unable to read a book or a newspaper in any language.	0.72	[0.45]	741
Indicator for any work in past month	Indicator equaling one if days employed were greater than zero.	0.69	[0.46]	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]	741
Daily wage (in USD)	Gross cash earnings in the past month divided by days employed. 237 respondents were unemployed.	1.54	[3.76]	504
<i>Psychosocial & Political Outcomes</i>				
Indicator for a physical fight	Indicator equaling one if the respondent reports being a physical fight in the past 6 months.	0.07	[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]	741
Index of social support	Sum of 14 questions on concrete social support received	5.60	[2.4]	741
Index of psychological distress	Sum of 19 survey questions on symptoms of depression and traumatic stress	4.30	[2.6]	741
Indicator for voting in 2005 referendum	Indicator equaling 1 if the respondent voted in the 2005 referendum. Youth under 18 excluded.	0.50	[0.5]	331

Note: Sample means weighted by inverse sampling probabilities

Table 2: Comparison of treatment group means

Pre-treatment Covariate	(1)	(2)	(3)	(4)	(5)	(6)
	Abducted versus non-abducted youth			Militia versus non-militia members		
	Sample means		Difference	Sample means		Difference
	Non-Abd	Abducted		Non-Militia	Militia	
Year of birth [†]	21.54 [0.44]	20.47 [0.29]	1.08 [0.44]**	23.12 [0.62]	19.69 [0.39]	3.43 [0.76]***
=1 if Father a farmer [†]	0.90 [0.01]	0.90 [0.03]	0.01 [0.02]	0.95 [0.04]	0.89 [0.03]	0.06 [0.05]
Household size in 1996 [†]	8.48 [0.33]	8.81 [0.55]	-0.33 [0.41]	9.30 [0.79]	8.27 [0.49]	1.04 [0.86]
Landholdings in 1996 [†]	26.78 [1.48]	26.36 [2.44]	0.42 [2.10]	14.50 [2.75]	22.82 [1.67]	-8.33 [2.99]**
Cattle in 1996 [†]	17.73 [7.68]	12.66 [4.89]	5.07 [4.12]	3.38 [2.07]	14.60 [7.78]	-11.22 [7.76]
Other livestock in 1996 [†]	14.18 [2.11]	13.23 [3.09]	0.94 [2.72]	6.37 [1.85]	11.60 [2.67]	-5.23 [2.55]**
=1 if owned a plow in 1996 [†]	0.23 [0.03]	0.19 [0.04]	0.04 [0.04]	0.10 [0.04]	0.19 [0.04]	-0.09 [0.06]
=1 if father has no schooling	0.12 [0.01]	0.13 [0.02]	-0.02 [0.02]	0.08 [0.05]	0.13 [0.01]	-0.04 [0.05]
=1 if father has some primary school	0.35 [0.02]	0.38 [0.02]	-0.03 [0.03]	0.40 [0.08]	0.37 [0.01]	0.03 [0.08]
=1 if father completed primary school	0.24 [0.02]	0.24 [0.01]	0.00 [0.03]	0.27 [0.08]	0.24 [0.01]	0.03 [0.08]
=1 if father completed some high school	0.29 [0.03]	0.24 [0.03]	0.04 [0.05]	0.24 [0.07]	0.26 [0.02]	-0.02 [0.07]
=1 if mother has no schooling	0.53 [0.04]	0.51 [0.02]	0.02 [0.04]	0.64 [0.11]	0.52 [0.02]	0.13 [0.11]
=1 if mother has some primary school	0.36 [0.03]	0.35 [0.02]	0.01 [0.04]	0.17 [0.05]	0.36 [0.01]	-0.19 0.0541***
=1 if mother completed primary school	0.06 [0.01]	0.08 [0.02]	-0.02 [0.02]	0.16 [0.08]	0.07 [0.01]	0.09 [0.07]
=1 if mother completed some high school	0.05 [0.01]	0.05 [0.01]	-0.01 [0.02]	0.03 [0.03]	0.05 [0.01]	-0.03 [0.03]
=1 if father died before 1996	0.36 [0.03]	0.34 [0.03]	0.02 [0.04]	0.43 [0.10]	0.34 [0.02]	0.09 [0.11]
=1 if mother died before 1996	0.13 [0.02]	0.12 [0.02]	0.01 [0.02]	0.06 [0.05]	0.12 [0.01]	-0.06 [0.05]
=1 if parents died before 1996	0.08 [0.01]	0.07 [0.02]	0.01 [0.03]	0.03 [0.03]	0.08 [0.01]	-0.05 [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%

[†] Columns 1 to 3 include data from unfound and non-surviving youth, and therefore omit inverse attrition weights

Table 3: Predictive power of pre-treatment covariates for treatment assignment

	(1)	(2)	(3)	(4)	(5)
	All Youth		Interviewed Youth[‡]		Interviewed Youth[‡]
	Pr(Ever abducted)		Pr(Ever abducted)		Pr(Join militia)
	Linear	Higher order	Linear	Higher order	Higher order
=1 if Father a farmer	0.17 [0.21]	0.20 [0.20]	-0.18 [0.38]	-0.13 [0.37]	1.40 [0.62]**
Household size in 1996	-0.04 [0.01]***	†	-0.06 [0.02]*	†	†
Landholdings in 1996	0.00 [0.00]	†	0.00 [0.00]	†	†
Cattle in 1996	0.00 [0.00]	†	0.00 [0.00]*	†	†
Other livestock in 1996	0.00 [0.00]	†	0.00 [0.00]	†	†
=1 if owned a plow in 1996	0.18 [0.18]	0.24 [0.20]	0.14 [0.32]	0.21 [0.34]	-1.00 [0.60]
=1 if father has some primary school			-0.03 [0.15]	0.00 [0.17]	1.17 [0.63]*
=1 if father completed P7			0.10 [0.21]	0.16 [0.20]	1.69 [0.78]**
=1 if father attended secondary school			0.22 [0.30]	0.29 [0.35]	1.00 [0.60]
=1 if mother has some primary school			-0.02 [0.19]	-0.01 [0.19]	-1.32 [0.73]*
=1 if mother completed P7			-0.38 [0.33]	-0.41 [0.34]	0.95 [0.70]
=1 if mother attended secondary school			-0.37 [0.64]	-0.31 [0.65]	-1.54 [1.19]
=1 if father died before 1997			0.14 [0.24]	0.12 [0.24]	0.40 [0.38]
=1 if mother died before 1997			0.19 [0.55]	0.20 [0.52]	-1.05 [1.21]
=1 if parents died before 1997			-0.33 [0.82]	-0.41 [0.80]	-0.16 [1.52]
Constant	0.73 [0.93]	-0.37 [1.33]	0.40 [1.21]	-1.16 [1.63]	-9.73 [2.45]***
Observations	1219	1219	741	741	741
† Tests of joint significance (p-values)					
Location and YOB indicators	0.10*	0.10*	0.61	0.62	0.05**
Household size in 1996	0.00***	0.01**	0.00***	0.01**	0.00***
Landholdings in 1996		0.61		0.88	0.00***
Cattle in 1996		0.67		0.90	0.33
Other livestock in 1996		0.22		0.68	0.01**
All pre-treatment covariates	0.10	0.10*	0.12	0.50	0.02**
Age and location dummies included	×	×	×	×	×
Inverse sampling weights used	×	×	×	×	×
Inverse attrition weights used			×	×	×
Higher order terms included		×		×	×
Includes non-surviving and unfound youth	×	×			

Notes:

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

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

† For household size, land, cattle, and livestock, a linear term and three higher-order terms are employed but omitted from the table. The p-values from F-tests of joint significance of the higher order terms are displayed at the base of the table.

‡ Data on parental schooling and death, as well as militia service, is only available for the 741 interviewed youth.

Table 4: Estimates of the average treatment effect

Dependent variable	(1) WLS estimate of ATE [†]	(2) Matching estimate of ATE [‡]
<i>Educational and labor market outcomes</i>		
Educational attainment (in years)	-0.70 [0.13]***	-0.67 [0.19]***
Indicator for functional literacy	-0.15 [0.04]***	-0.17 [0.04]***
Indicator for any work in past month	0.00 [0.03]	-0.02 [0.04]
Indicator for capital- or skill-intensive work	-0.05 [0.01]***	-0.05 [0.02]**
Log (Daily wage)	-0.20 [0.14]	-0.28 [0.12]**
<i>Psychosocial and political outcomes</i>		
Indicator for physical fights	-0.01 [0.02]	0.00 [0.02]
Indicator for hostility	0.03 [0.01]***	0.03 [0.02]*
Index of social support	-0.23 [0.12]*	-0.25 [0.21]
Index of psychological distress	0.56 [0.22]**	0.58 [0.20]***
Indicator for voting	0.13 [0.02]***	0.12 [0.05]***

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

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

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

Table 5: Robustness of the Semi-parametric treatment effects to alternative assumptions

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
WLS estimates of ATE under alternative specifications								
Dependent variable	Full controls, all weights	No pre-treatment traits, all weights	No controls, all weights	No controls, no attrition weights	No controls, no selection weights	Simple Difference of Means	No pre-trmt traits, no selection weights	No pre-trmt traits, no selection or attrition weights
Educational attainment (in years)	-0.70 [0.13]***	-0.68 [0.13]***	-0.69 [0.35]**	-0.69 [0.38]*	-0.37 [0.38]	-0.45 [0.39]	-0.74 [0.14]***	-0.83 [0.17]***
Indicator for functional literacy	-0.15 [0.04]***	-0.14 [0.04]***	-0.14 [0.05]**	-0.13 [0.05]**	-0.11 [0.04]***	-0.11 [0.04]***	-0.13 [0.03]***	-0.13 [0.04]***
Log (Daily wage)	-0.20 [0.14]	-0.17 [0.16]	-0.17 [0.16]	-0.23 [0.15]	-0.04 [0.13]	-0.13 [0.13]	-0.21 [0.16]	-0.29 [0.15]*
Indicator for hostility	0.03 [0.01]***	0.02 [0.01]*	0.02 [0.02]	0.03 [0.02]	0.03 [0.02]*	0.03 [0.02]	0.02 [0.01]	0.03 [0.01]*
Index of psychological distress	0.56 [0.23]**	0.51 [0.25]**	0.49 [0.29]*	0.52 [0.24]**	0.69 [0.25]***	0.67 [0.21]***	0.57 [0.20]***	0.58 [0.17]***
<i>Controls and weights employed in estimation:</i>								
Control for pre-treatment characteristics	×							
Control for year and location of birth	×	×					×	×
Weights on inverse selection probability	×	×	×	×				
Weights on inverse attrition probability	×	×	×		×		×	
Weights on inverse sampling probability	×	×	×	×	×	×	×	×

Notes:

Each coefficient represents a separate WLS regression, each row represents a different dependent variable, and each column represents an alternative specification

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

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

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

Table 6: Treatment effect bounding: Best and worst case scenarios

Dependent variable	(1)	(2)	(3)	(4)	(5)
	Percent missing [†]		Treatment effect bounds [§]		
	Non-Abd	Abd	Untrimmed ATE [‡]	"Best case" attrition bound	"Worst case" attrition bound
Educational attainment (in years)	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 work 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 voting	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 attriters and non-survivors. 31% of non-abducted youth and 30% of abducted youth are missing. Data collected from families on the education, employment status, and major injuries of migrant youth reduce these missing percentages to 14% and 23%. In the case of wages, additional observations are missing due to unemployed youth.

‡ The untrimmed ATE is the difference in the 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: Variation in the average treatment effect by age of abduction
(Linear age term, former abductees only)*

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Educational attainment	Functional literacy	Employment indicator	Skill-intensive work	Log (Daily wage)	Fight indicator	Hostility indicator	Social support	Psychological distress	Voting indicator
Age of abduction	-0.07 [0.05]	0.00 [0.01]	-0.01 [0.01]	0.00 [0.00]	-0.06 [0.02]***	0.00 [0.01]	0.00 [0.00]	-0.07 [0.04]	0.00 [0.03]	0.02 [0.01]**
Observations	462	462	462	462	288	462	462	462	462	331
R-squared	0.34	0.3	0.29	0.37	0.49	0.23	0.22	0.33	0.34	0.35

Notes:

Regression only includes formerly abducted respondents

Controls (not displayed) include age and location dummy variables (and interactions), pre-treatment characteristics, and indicators for abduction length and year

Robust standard errors in brackets, clustered by location

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

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

Table 8: WLS estimates of the relationship between abduction length, violence, and outcomes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Educational attainment	Functional literacy	Employment indicator	Skill-intensive work	Log (Daily wage)	Fight indicator	Hostility indicator	Social support	Psychological distress	Voting indicator
Index of violence	0.06 [0.05]	0.01 [0.01]	-0.02 [0.01]*	0.00 [0.00]	-0.10 [0.02]***	0.01 [0.00]**	0.00 [0.01]	-0.01 [0.05]	0.38 [0.04]***	0.00 [0.01]
Years abducted	-0.54 [0.06]***	-0.06 [0.02]***	0.03 [0.01]*	-0.01 [0.01]	0.03 [0.05]	-0.01 [0.01]	0.00 [0.01]	-0.08 [0.09]	-0.06 [0.11]	-0.03 [0.02]
Observations	459	459	459	459	286	459	459	459	459	330
R-squared	0.50	0.41	0.40	0.51	0.57	0.26	0.29	0.40	0.39	0.52

Notes:

Robust standard errors in brackets, clustered by location

Controls include age and location dummy variables (and interactions) and pre-treatment characteristics

All estimates are weighted by inverse sampling and attrition probabilities

Non-abducted youth are omitted from the regression

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

Table 9: Instrumental variables estimates of the relationship between violence and outcomes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Coefficient on Index of Violence	First-stage F-statistic	Educational attainment	Functional literacy	Employment indicator	Skill-intensive work	Log (Daily wage)	Fight indicator	Hostility indicator	Social support	Psychological distress	Voting indicator
WLS (from Table 8) [†]		0.01 [0.05]	0.00 [0.01]	-0.01 [0.01]	-0.01 [0.00]	-0.09 [0.03]***	0.01 [0.00]**	0.00 [0.01]	0.00 [0.02]	0.11 [0.01]***	0.01 [0.01]
IV estimate [‡]	9.3	-0.63 [0.44]	-0.14 [0.09]	-0.08 [0.07]	-0.01 [0.03]	-0.41 [0.20]**	0.02 [0.04]	-0.08 [0.05]	0.12 [0.36]	0.96 [0.46]**	0.02 [0.05]
Observations		459	459	459	459	286	459	459	459	459	330

Notes:

Robust, clustered standard errors in brackets

Controls include age and location dummy variables (and interactions), pre-treatment characteristics, as well as dummy variables for age, year, age/year interactions, and length of abduction

All estimates are weighted by inverse sampling probabilities and attrition probabilities

Non-abducted youth are omitted from the regression

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

[†] Standard errors clustered by location

[‡] Violence is instrumented using a proxy for the level of abusiveness of the military unit as described by other members traced to the same unit by age and location of abduction. Standard errors clustered by the proxy for military unit

Appendix A: Sensitivity of treatment effects 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, \mathbf{X} , and finally a normal conditional distribution of the outcome, Y , given U and \mathbf{X} :

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

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

$$Y(T) | \mathbf{X}, U \sim N(\tau \cdot T + \beta \cdot \mathbf{X} + \delta U, \sigma^2)$$

A more general model might allow for covariation between U and \mathbf{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 simple model is that the correlations between U and T and between U and Y are completely summarized by the parameter set (α, δ) . For a fixed parameter set, we can estimate $\hat{\tau}(\alpha, \delta)$ via maximum likelihood. Specifically, we denote $L(\tau, \beta, \sigma^2, \gamma, \alpha, \delta)$ the logarithm of the likelihood function:

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

The sensitivity parameters α and δ do not have an easy interpretation, but they can be transformed into two more easily interpretable quantities.

First, the proportion of the previously unexplained variation in Y that is explained by the unobserved covariate U can be represented by $\tilde{R}_Y^2(\alpha, \delta)$ —the partial R^2 -statistic obtained from adding the hypothetical covariate with fixed (α, δ) to the outcome regression:

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

This figure 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, $Pr(T = 1 | \mathbf{X}, U)$, that is explained by the unobserved covariate U can be represented by the term $\tilde{R}_T^2(\alpha, \delta)$ —the partial R^2 -statistic obtained from adding the hypothetical covariate with fixed (α, δ) to the outcome regression:

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

where $\hat{\psi}(\alpha, \delta)$ represents the unexplained sum of squares in the latent index regression.³²

³² This is a slight simplification for exposition. As detailed in Imbens (2003), there is in fact no natural R^2 or partial R^2 -statistic for the treatment indicator regression, and in fact he uses the explained variation in the latent index in a latent index representation.