An Empirical Exploration of the Near-Term and Persistent Effects of Conflict on Risk Preferences [1]

Marc Rockmore[2], Christopher B. Barrett[3] and Jeannie Annan[4]

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Abstract:

A burgeoning empirical literature on the effects of conflict on various economic behavioral parameters exhibits mixed results, with respect to both the magnitude and the direction of the effects. By estimating the distribution of estimated effects of violence on risk preferences, rather than just the average effect, we reconcile the discordant results of the prior literature. The distribution also reveals substantial and previously overlooked variation in the effects of exposure. This raises questions about the widespread use of aggregated measures of exposure to violence in conflict literature. We use panel data from northern Uganda, the latest round collected seven years after the violence ended, to explore the heterogeneous effects of different experiences of violence – personal suffering, perpetration, witness, or indirect experience through family members' suffering – on different measures of ambiguity and risk aversion, correcting for many of the methodological shortcomings of previous studies. We find that violence has an adverse near-term effect on mental health, but with heterogeneous effects on risk aversion depending on the nature of one's experience of violence. We also find that the risk preference effects persist even after a recovery in mental health.

I.   Introduction

Despite a decline in conflict worldwide after the Cold War (World Bank 2011), there were at least 25 distinct civil conflicts in Sub-Saharan Africa between 2000 and 2014.[5,6] Moreover, during the 1980's and 1990's, close to 70 percent of Sub-Saharan African countries suffered civil conflicts resulting in large sub-populations exposed to traumatic experiences (Miguel *et al.* 2004). Since the mental health literature finds that traumatic experiences can lead to symptoms that last decades (Hubbard *et al.* 1995; Kulka *et al.* 1990; McSharry and Kinny 1992; Schnurr *et al.* 2004), the legacies of conflict may persist despite post-conflict economic recovery. In the case of war-affected children, these behavioral changes are consistent with shifts in economic behavioral parameters (risk aversion, time preferences, and trust) that are believed to explain much heterogeneity in economic outcomes (Cunha and Heckman 2009; Macksoud *et al.* 1993).

An emerging economics literature confirms this potential by causally linking exposure to violence to changes in risk aversion (Callen *et al.* 2015; Moya 2014; Voors *et al.* 2012), time preferences (Imas *et al.* 2015; Voors *et al.* 2012), or trust/social preferences (Cassar *et al.* 2013; Luca and Verpooten 2015; Rohner *et al.* 2013; Voors *et al.* 2012). The empirical results in this recent literature are strikingly mixed, however, with varying magnitudes and even some studies finding that exposure to violence leads to greater risk aversion while others finding the opposite effect.

We reconcile these seemingly contrasting effects on risk attitudes. Using highly disaggregated measures of direct and indirect exposure to violence in Northern Uganda, we estimate individual-specific changes in risk aversion based on the respondent's particular pattern of exposure. The resulting distribution of estimated effects reveals considerable and previously unidentified heterogeneity, both in terms of magnitude and sign (risk aversion vs. risk seeking), arising from varied within-population variation in the type and intensity of individuals' exposure to violence. This contrasts with the existing literature, which estimates a single, uniform "average" effect of exposure for all individuals within a given region, and offers a possible reconciliation of the mixed findings in previous published studies.

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[5] Authors' calculation is based on the UCDP/PRIO Armed Conflict Dataset Version 4 - 2015. We define a civil conflict as an internal conflict with at least 25 battle related-deaths per annum.

[6] In other areas, such as Central America, the decline in civil conflicts has been replaced by high levels of violence arising from gangs and drug traffickers (World Bank 2011).

The varying and apparently contrasting results in the literature likely arise for two reasons. First, prior studies rely primarily on aggregate geographic measures of exposure, such as community or regional levels of violence. Consequently, the estimated effect is a weighted average of the treated (or more violence-affected) population and the untreated population (Moya 2014). By decomposing the overall distribution into the separate underlying distributions for abducted (i.e highly exposed) and non-abducted respondents (i.e. less exposed), we reveal the distinct distributions and average effects of each cohort. As the relative proportions of the groups shift, so does the estimate effect of geographical exposure. More broadly, this raises questions about the widespread use of aggregated measure of violence in the conflict literature,

Second, the few studies which use individual-level measures of exposure to violence use a single binary variable for (direct) exposure. Similar to the aggregate geographic measures, the estimated effect depends on the distribution of underlying types of exposure. In our context, certain types of exposure lead to increased risk aversion while other types lead to increased risk seeking. Consequently, depending on the predominant type of exposure, violence may have opposite effects on risk attitudes. Moreover, by only measuring direct exposure to violence, these studies do not capture indirect exposure to violence (and therefore the possibilities of externalities from exposure).

Using a unique data set collected by the authors, we overcome four important challenges inherent to this line of research. While other authors address these more or less successfully, we believe that we are the first to address all of these.

First, with the exception of Moya (2014), mental health is never directly measured. The economics literature explains the casual link between exposure to violence and behavioral changes by invoking the pre-existing mental health literature; in the absence of measure of mental health, this cannot be confirmed. Moya (2014) explicitly measures mental health and shows that changes in phobic anxiety explain the changes in risk aversion (over the gains and ambiguity domains). We extend Moya's work by using an index of distress which has been built and validated for the specific population we study. Moreover, whereas Moya (2014) uses a cross-section, we observe the evolution of mental health over a roughly seven year period following exposure to conflict, enabling us to explore the persistence of such effects.

Second, the measurement of exposure to violence poses another key challenge. Many studies rely exclusively or primarily on geographically defined indicators of violence. While useful in highlighting the population-level effects of violence, these measures impose a strong assumption of homogeneity of "treatment" and obscure the potential importance of the timing, severity, and duration of individual exposure.[7,8] Since the mental health literature emphasizes the importance of individual exposure and the differential impact one's experience has, it is important to correctly identify exposure and its intensity (Macksoud and Aber, 1996; Mollica *et al.* 1998). The few papers with individual-level data on exposure typically aggregate these in a binary variable. Using our unique data, we are the first to isolate the effects on risk aversion of several distinct types of exposure to violence: violence witnessed, violence personally suffered, violence experienced by family members, and violence perpetrated.

Third, since pre- and post- violence samples and estimates of behavioral parameters do not exist, researchers rely on the supposed "exogeneity" of violence (after controlling for observables) identify the effect of exposure.[9] This standard method is to estimate non-random selection into violence using a 'post-exposure to violence' sample. Survival bias is an important and unavoidable problem with this approach, however, as samples only include those individuals who survived the violence. Consequently, analyses based on post-exposure data cannot fully establish the exogeneity of (selection into) violence if survival is non-random. Our data are unique in that they are largely representative of the pre-exposure sample. As described below, the sampling frame was drawn from retrospective household rosters for 1996 based on World Food Programme lists (Blattman and Annan 2010). This time period pre-dates 90% of local abductions and represents the best estimate of pre-war characteristics in the literature since it includes information on both individuals in the sample and those who are abducted and never return. Analysis of the first round

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[7] The preceding discussion draws heavily on Moya (2014).
[8] Rockmore (forthcoming) similarly distinguishes between direct and indirect exposure to violence. Whereas we consider indirect exposure to violent events, Rockmore (forthcoming) considers the impact of exposure to the risk of violence (relative to actual exposure to violence) on per capita consumption.
[9] Nasir *et al.* (2016) and Brown *et al.* (2015) rely on panel data which brackets the large increase in drug violence in Mexico. As noted earlier, their sample is largely not directly affected by exposure to violence. Similarly, Jakiela and Ozier (2015) use a panel data set from Kenya, one round of which was collected during the outbreak of political violence. Again, their sample was largely spared direction exposure to violence. Consequently, the paper measures exposure to insecurity rather than the experience of violence itself.

of the panel data established the exogeneity of abductions (Blattman and Annan 2010)[10]; we re-confirm the exogeneity of abductions in the second round data.

Fourth, the literature uses data collected during periods of conflict/insecurity or immediately post-conflict. Consequently, while such studies can help explain behavior during and right after such periods, the crucial question of the persistence of any effects and hence any need for ongoing policy interventions remain largely unexplored. Exceptions are, Cassar *et al.* (2013) who finds that the effect of the Tajikistan civil war on trust persists after a decade and Voors *et al.* (2012) who measures the effect of violence between 1993 and 2003 on behavioral parameters

in 2009.[11,12] Although Voors *et al.* (2012) use violence until 2003, the violence did persist until 2005 although at lower levels. Lastly, Kim and Lee (2014) find that geographical exposure to violence in the 1950-53 Korean war affects risk aversion levels in 2007. The study is particularly notable for the persistence of the effect although the level and type of exposure is unclear.

We find that exposure to violence has lasting effects on risk aversion over the gains, losses and ambiguity domains. These effects are still visible seven years after the end of violence. The results strongly support the hypothesis pervasive in the psychology literature that the specific type of exposure to violence matters. Each type of exposure to violence is significant although the effect on risk attitudes varies by type: violence suffered (personally or to family members) lead to greater risk seeking while violence witnessed or perpetrated leads to greater risk aversion. This difference according to the type of violence seems to reconcile studies finding seemingly contradictory consequences of exposure to violence.

With respect to mental health, higher levels of psychological distress in 2005/6 lead to increased risk aversion in 2012. In contrast, current levels of psychological distress are statistically unrelated to risk aversion. This suggests that once mental distress induces behavioral effects, the behavioral

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[10] The only statistically significant factors were the age of the individual and household size. The former reflects the rebels targeting of particular age groups (i.e. babies and older adults do not make good recruits) while the latter is driven by households with more than 25 members. This is extremely rare. The 2002 Uganda Census finds less than 0.1 percent of households with 25 or more members in the relevant geographical area. Consequently, conditional on age, abductions are exogenous to household and individual characteristics.

[11] While some of the violence in Moya (2014) does occur a decade earlier, the conflict is still ongoing at the time of his sample. Many of his sample members were currently displaced and still at risk of further violence.

[12] In related research, Bauer *et al.* (2015) examine distrust exists between former rebels and the general population in Northern Uganda.

manifestations persist across time even if mental health improves. This highlights the importance of mental health interventions during and very early in the aftermath of conflict.

The remainder of the paper is organized as follows: section II describes the context in Northern Uganda, while section III describes the data and sample attrition; section IV explains the methodology and section V presents the results before concluding in section VI.

II.   Conflict and Trauma in Northern Uganda

Beginning with the emergence of the Lord's Resistance Army (LRA) in 1986, Northern Uganda experienced sustained violence until the LRA was pushed out of the country in 2005/2006. While the conflict was primarily located in the Acholi sub-region of Northern Uganda, it gradually spread especially after 2002. Anecdotal evidence and representative data suggest that the attacks were fairly widespread as the percent of communities suffering an attack in 1992, 1999 and 2004 was 16, 25 and 25 percent respectively (Ssewanyana *et al*. 2007).

Unable to obtain support from the local population, the LRA sustained itself by raiding villages, schools and internally displaced persons' (IDP) camps to abduct youth, primarily boys aged 12 to 16 (Blattman and Annan, 2010). These abductions were widespread; estimates of the total number of children abducted during the conflict range from 20,000-25,000 with more recent estimates placing the number as high as 60,000-80,000 (Blattman and Annan, 2009; Lomo and Hovil 2004; Pham *et al.* 2007). Using the first round of the panel data we analyze, Blattman and Annan (2010) and Berber and Blattman (2010) find that roughly 25% of males aged 14 to 30 were abducted for at least two weeks and 40% for at least one day. Abducted youths typically serve a variety of roles with boys typically serving as soldiers, and porters and girls as forced "wives" and cooks.[13] Data from reception centers suggest that abductions were also heavily concentrated in the three Acholi districts (Pham *et al.* 2005).

High levels of post-traumatic stress disorder (PTSD) symptoms are often found in post-conflict countries, including among children and adolescents (Attanayake *et al.* 2009). In Northern

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[13] Using a non-representative sample of 330 former child abductees, Klasen *et al.* (2010, p. 19) find the following tasks assignments: "41.8% were assigned primarily front-line tasks, e.g. fighting, looting, abducting civilians (55% for boys and 26.8% for girls), 28% performed mainly logistic tasks, e.g. carrying loads, spying, escorting commanders (34.3% for boys and 20.8% for girls), and 27.7% were assigned domestic chores, e.g. cooking, caring for younger children (10.1% for boys and 47.7% for girls)."

Uganda, studies of child soldiers and abductees report rates ranging from 30 to 90 percent for PTSD in addition to high rates of depression and anxiety (Bayer *et al.*, 2007; Okello *et al.* 2007; Amone-P'Olak *et al.*, 2007; Derluyn *et al.*, 2004; Klasen *et al.* 2010). While these studies typically use non-representative "convenience" samples (i.e. only abducted children), similar findings emerge from representative data where one fourth of the 14-30 year male olds have moderate to high levels of emotional distress (Blattman and Annan 2010).

Several formal rehabilitation centers and a variety of NGOs were created to assist returning abductees. Although the exact numbers are unknown, it is estimated that less than half of the abducted youths passed through the official reintegration centers and, of these, fewer than ten percent received follow-up care in their community (Annan *et al.* 2006; Allen and Schomerus 2006). Our field interviews suggest that participation in rehabilitation was largely determined by the length of abduction[14], how they escpated and who received the abductees upon their return. Individuals captured by the Ugandan government forces often went to rehabilitation centers while individuals who escaped on their own often did not. The important implication is that many abductees never received psychological support or mental health treatment.

III.   Conflict Intensity, Timing of Surveys and the Data

The conflict with the LRA began in 1986 and ended in 2006 when the LRA was pushed out of the country. During this period, the conflict went through several phases. Widespread abductions and violence against citizens began to escalate in 1991 with the arming of local village groups by the Ugandan army (UPDF) and again in 1994 as peace talks failed and the government of Sudan increased its support for the LRA. LRA activity in Northern Uganda peaked from 2002 to 2004 after the UPDF drove the LRA from its camps in Sudan. After 2004, the LRA was substantially weaker and then was finally pushed from Uganda in 2006. The number of attacks and abductions fell in 2005 and ceased in Uganda after 2006.[15, 16]

This paper relies on data from a household panel survey. The first round of the survey was fielded in 2005, while the LRA was still active in northern Uganda although the frequency of attacks and

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[14] Those abducted less than several months typically did not receive counseling.
[15] This section draws on Annan *et al.* (2006) and Beber and Blattman (2013).
[16] The LRA continues to be active in part of the Central African Republic, the Democratic Republic of Congo, and Sudan.

abductions were decreasing. Consequently, the data from the first round captures the conditions near the end of the conflict. The survey covers eight sub-countries in the Districts of Kitgum and Pader, two of the most violence-affected districts in northern Uganda. Due to the insecurity, the sample sub-counties were not randomly selected. The counties were chosen to represent the diversity of the area (e.g. LRA activity, size, presence of IDP camps, distance from towns, etc) (SWAY Codebook 2013).

The sampling frame was created based on World Food Programme food distribution lists for 2002 and 2003. By this time, most of the population had taken up residence in IDP camps and relied on food aid (as did many non-displaced households). The massive displacement was partially voluntary and, after 2002, part of a deliberate government strategy to deprive the LRA of targets (Fiala, 2009). It is estimated that over 90 percent of population of 2 of the 3 Acholi districts (Gulu and Kitgum) migrated from their districts (Pham *et al.* 2005). A preliminary survey was used to establish complete household rosters for 1996. This particular year was chosen for two reasons. Most importantly, this was the year of the first presidential election since 1980 and is therefore well remembered in this population. Second, this date pre-dated more than 90 percent of the abductions in the area and therefore could be used as a reasonable baseline for pre-abduction household rosters. The final sample is representative of young men residing in these 8 sub-counties in 1998 and born between 1976 and 1992 (i.e. aged between 13 and 29 years at the time of the first survey and thus prime age abduction targets).

We fielded the second round of the survey in 2013 and attempted to follow up with all of the first round respondents. During the first round survey, more than 60% of the respondents were in IDP camps and another 10% were out of the original districts. With expulsion of the LRA and the ensuing peace, individuals were once again able to freely move around the country and the IDP camps were closed. To track the respondents, the enumeration teams used information on the names of family members and the native villages as recorded in the first survey round. The village of every respondent was visited and enumerators attempted to locate the respondents. Respondents who were present were surveyed. Respondents who were not present at the time or did not show up received a second visit from the enumeration teams. Insofar as a possible, we attempted to track individuals who had moved from their native village based on information obtained in the village. But we were unable to obtain sufficient information to track some individuals further. We prioritized individuals in the Acholi districts, who were near other respondents and in major cities

such as the capital, Kampala. 442 of the 741 youths in the first survey round were successfully re-interviewed (59.4%),[17] implying an attrition rate of roughly 4.5% per year.

As with any panel survey, one needs to be concerned about the possibility of selective attrition. This is perhaps particularly true when the phenomenon we wish to study, violence, could well be a cause of non-random attrition. In order to test for non-random attrition, we estimate the following weight least squares logit using the weights and data from the 1st survey round:

$$(1) Attrition_i = \alpha_i + \beta_1 Exposure_i + \beta_2 Distress_i + \beta_3 Location_i + \beta_4 X_i + \varepsilon_i$$

where $Attrition_i$ is a binary variable equal to one if respondent i was in round 1 but not in round 2, and zero otherwise.[18] *Exposure* is a vector that contains a binary variable for being abducted and four separate indices for exposure to violence (personally suffered, witnessed, suffered by the family of the respondent, and perpetrated by the respondent). *Distress* is an index of psychosocial distress experienced by the respondent. We describe these variables in greater detail below. The variables in *Exposure* and *Distress* are the primary variables of interest in the analysis. *Location* is a vector that contains binary variables for being an IDP camp, and for being outside of the original districts. *X* is a vector of control variables.[19]

Attrition analysis confirms that attrition between the rounds was not random.[20] Within the primary variables of interest, the coefficient estimate for having been abducted is weakly significant and the coefficient estimate on the index for violence which was personally suffered is also significant. Former abductees are half as likely to appear in the second survey round. This is similar to the first survey round where they were also roughly 50 percent less likely to be traced (Blattman and Annan

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[17] Of the 299 who were not re-interviewed, 32 were either dead or mentally incapable of participating in the survey, 16 were unwilling to participate or the family refused to provide the information for them to be tracked, and 14 were out of the country. Consequently, we were able to track another 8.3% of the sample but they were not willing or able to participate in the survey. The remaining 239 were not in the original villages and there was insufficient information to track them further or they were unknown in the villages.

[18] Note we include every respondent who was interviewed in the attrition analysis. We subsequently drop two individuals who are in prison from the analysis as their circumstances are so different from the remainder of the sample. They are counted as being 'found' since we did interview them.

[19] The control variables include variables for literacy, education level, skill type of employment, a health index, any serious injury, being married, the respondent have lost his mother, father or both parents, binary variables for age group, a scale of comfort living with the family, and a set of interactions between specific camps and age groups.

[20] Detailed results from the attrition analysis are available upon request.

2010). The coefficient estimate implies that for each additional each of violence suffered implies that each additional act suffered raised the likelihood of not being interviewed by 27 percent. The coefficient for psychological distress was not significant. Among the other variables, we see that respondents who did not reside in the original districts were much less likely to be found (-78 percent). Consequently, the tracking was very effective for non-migrants in the first round but much less successful for the migrants.

Following Blattman and Annan (2010), we create weights to correct for selective attrition (and to account for the sampling weights). In particular, we estimate weighted least squares (WLS) regressions using the inverse of the nonparametric estimate of the propensity score (Hirano *et al.* 2003). The weights (w_i) are defined as:

$$w_i = w(T_i, v_i, P_i) = p_i \cdot \pi_{i1} \cdot \pi_{i2} \cdot \left(\frac{T_i}{\hat{e}(v_i)} + \frac{1 - T_i}{1 - \hat{e}(v_i)} \right)$$

where p_i, π_{i1}, π_{i2} are the sample and attrition weights for round 1 and 2, respectively. T is the treatment indicator (here abduction=1), and $\hat{e}(v_i)$ is a non-parametric estimate of the propensity score based on information (v_i) from the respondents or from the families of absentees in the first round. The difference between the weights developed by Blattman and Annan (2010) for round 1 and the weights we develop for round 2 arise due to updating based on the attrition rate between the first and second round, π_{i2}.

Measurement

The variables for exposure to violence and abduction are drawn from the first survey round. As noted earlier, the conflict ended soon after the survey. Consequently, there was minimal exposure to violence after the first round was collected. Only 13 individuals (2.86% of the sample) in the second survey round reported "spending any time with the LRA" since the first round of the survey. And this number may overestimate the frequency of abductions and violence since some Ugandans visited abducted relatives during the 2006 truce, so that they might have spent time with LRA members, not as conscripts, but rather as voluntary visitors.

We define exposure using Blattman and Annan's (2010) violence indices which sum binary indicators for whether or not an individual reports experiencing/witnessing/etc a particular act of violence. The 'violence received' index is the sum of six violent acts personally suffered[21]; the 'violence witnessed' index is the sum of six violent acts witnessed[22]; the 'violence perpetrated' index is the sum of eight violent acts perpetrated[23]; and 'the violence on the family' index is the sum of five violent acts inflicted on family members (not necessarily perpetrated nor witnessed by the respondent)[24]. Each individual has an index of each type, reflecting that individuals can have experienced violence in any of several ways. While measurement error concerns may exist, especially if one doubts the truthfulness of respondents, particularly regarding the violence perpetrated, there was little direct incentive for respondents to lie. Not only were the answers confidential, but it is widely understood and accepted in local communities that abducted individuals had no choice but to commit these acts, so social opprobrium is (perhaps surprisingly) minimal.[25]

Mental health is measured in both rounds using a series of questions from the Northern Ugandan Child and Youth Psychological Adjustment Scale (MacMullin and Loughry 2002), which was specially designed and validated for the context. The scale is created with an additive index of 17 questions where each symptom is scaled according to its intensity (often=1; sometimes=2/3; rarely=1/3). The second round used 16 questions since 1 question was no longer age appropriate. Consequently, the first round scale is limited to the same 16 questions for comparability across

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[21] The six categories are: Someone shot bullets at you or your home; You received a severe beating to the body by someone; Someone attacked you with a panga or other weapon; You were tied up or locked up as a prisoner; You received a serious physical injury in a battle or rebel attack; You were forced to carry heavy loads or do other forced labor.

[22] The six categories are: You heard gun fire regularly; You witnessed beatings or torture of other people; You witnessed a killing; You witnessed a massacre; You witnessed the setting of houses on fire with people inside; You witnessed the rape or sexual abuse of a woman.

[23] The eight categories: A family member received a serious physical injury from combat or landmine; You were forced to steal or destroy someone else's property or possessions; You were forced to kill an opposing soldier in battle; You were forced to beat or cut someone who was a family member or friend; You were forced to beat or cut a civilian who was not a family member or friend; You were forced to kill a family member or friend; You were forced to have sex with a woman; You were forced to step on or otherwise abuse the bodies of dead persons; You were forced to betray a family member or friend, placing them at risk of death or injury.

[24] The five categories are: A parent was murdered or died violently; A parent disappeared or was abducted; Another family member or friend was murdered or died violently; Another family member or friend disappeared or was abducted; A family member received a serious physical injury from combat or landmine.

[25] A quarter of the sample reported being forced to kill someone. Blattman and Annan (2008) report that 94% of the returning abductees reported feeling accepted by their communities at the time of their return. This is not to say that persecution does not happen but rather that communities are generally understanding and accepting of returning abductees, regardless of what they did while conscripted by the LRA.

waves. In the sample which was re-interview in round 2, the mean psychosocial index scores were 3.94 (range between 0 and 11) and 3.69 (range between 0 and 11.66) in rounds 1 and 2 respectively. To put these measures in perspective, a score of 4 from similar scales corresponds to mild to moderate symptoms of depression or anxiety and below a clinical cutoff. However, this measure was not clinically validated for this (second round) population (Blattman and Annan 2010; Beck *et al*. 1988).

Risk attitudes were solicited in a manner similar to Binswanger's (1981) lottery choice design and are therefore directly comparable to Moya (2014). Participants were asked to choose from a list of potential lotteries with real payoffs. Each lottery had a pair of outcomes: one outcome if a red chip was drawn and a separate outcome if a blue chip was drawn from an opaque bag. Different lotteries correspond with different levels of risk aversion. More risk averse respondents should selected less risky lotteries with lower expected payoffs. The opposite should be true of risk loving respondents. As noted earlier, the payoffs within a lottery are determined by the color of the chip which is drawn.

The risk attitudes of respondents were gauged over three domains: gains, losses and ambiguity. For the gains and losses domains, respondents knew that there was a 50 percent chance of drawing either color chip since they personally placed five red and five blue chips in the bag. The difference between the gains and losses domain was due to the payoff structure of the lotteries (see Appendix 1 for the wording of the games and the exact payoffs). In the gains domain version, all payoffs were non-negative and the expected value was positive for every lottery. In the losses domain, at least one payoff in each lottery was negative and the expected payoff of each lottery was negative.[26] When testing for the ambiguity domain, we used the same payoff structure as over gains but introduced uncertainty regarding the likelihood of drawing a red or blue chip. This was achieved by the respondent blindly adding 4 chips of unknown color to 3 blue and 3 red chips. These new chips were selected from a pile of four red and four blue chips. Consequently, the probability of drawing a chip of either color could vary between 30 and 70 percent.

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[26] To avoid respondents leaving the survey after losing money (and thereby being harmed by their participation), they were told that if they agreed to play the game, they would be given 5,000 shillings. Since the highest loss was -5,000 shillings, the worst monetary outcome was leaving the game with zero earnings. But since they were given the 5,000 shillings, the losses domain entailed real losses relative to their state *ex ante* of the lottery choice, but not real losses *ex-ante* to their agreement to participate in the game.

Each respondent played the games individually with an enumerator. Respondents were required to play at least one practice round (and could play as many as they wanted) for each risk domain. Additionally, they were encouraged to ask questions to make certain that they understood the odds and, more generally, the 'game'. The potential (net) payoffs ranged from 0 to 10,800 Ugandan Shillings (Sh) or roughly \$0 to \$4.15.[27] To put this into perspective, roughly 70 percent of respondents reported 10,000 or fewer shillings of income for themselves in a typical week. Consequently, respondents had a strong incentive to make certain that they understood the game and to choose carefully.

IV.   Empirical Strategy

This paper investigates the impact of violence on various measures of risk aversion in the medium run. Using WLS, we estimate the following regression function:

$$(2) Risk_{i,2} = \beta_0 + \beta_1 Exposure_{i,1} + \beta_2 Distress_{i,t} + \beta_3 X_{i,1} + e_i$$

where $Risk_{i,2}$ is an elicited risk preferences measure for person i during the second survey round, $Exposure_{i,1}$ is a vector of variables for person i's exposure to violence in the first survey round, $Distress_{i,t}$ is a vector with the measure of psychosocial distress for rounds 1 and 2, and $X_{i,1}$ is a vector of covariates for person i from the first round, and e_i is an error term which is assumed to be normally distributed, mean zero and iid. β_1 are the coefficient estimates of interest describing the effect of exposure to violence on risk preferences. As written, equation (2) relies on the assumption that there is no endogenous selection into exposure to violence. If certain types of individuals are selected into violence, this could bias the estimates of β_1.[28] We start the analysis by carefully interrogating that assumption.

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[27] Note that a variety of different behavioral parameters were elicited in different games. Since the project budget did not permit payoffs for every game, participants were told that one of the games would be chosen at random after the survey and their choices from the survey and their draw after the survey in that game determine the payoff. Consequently, they should have treated each game as if their answers determined the payoff.

[28] More broadly, some omitted variable may be correlated with both first round exposure (and distress) and risk preferences. Ideally, pre-violence (and pre-round 1) measures of risk preferences would be included as controls. However, these measures do not exist. As we discuss in the following paragraphs and formally show in Table 1,

Two samples are used in the analysis. The first is the full sample. Anecdotally, there are reasons to believe that there was little or no endogenous selection into violence in northern Uganda. The LRA viewed itself as different from the rest of Northern Uganda and had an ideology which aimed to "purify" northern Uganda of corruption and witchcraft through violence (Allen and Schomerus 2006; Branch 2010; Finnström 2003; and Titeca 2010). Consequently, once the LRA attacked an area, everyone was at risk of exposure to violence. Moreover, the LRA typically did not target particular locations. According to interviews of junior officers from the LRA by Blattman and Annan (2010, p. 887): "targets were generally unplanned and arbitrary; they raided whatever homesteads they encountered, regardless of wealth or other traits." Rather than operating a conventional army, the LRA was often just small roaming bands looting households and abducting citizens to carry loot or to serve as conscripts.

The second sample restricts the analysis to those abducted before the first survey. Using retrospective pre-violence indicators for household wealth, parents' occupation and education, and indicators for whether their father, mother, or neither were alive, Blattman and Annan (2010) examine whether these indicators predict abduction by Round 1. Their results are replicated in column (1) of Table 1. Each entry in the column is the coefficient estimate for abduction after controlling for the pre-violence indicators.

As can be seen, only two pre-violence characteristics are correlated with violence. The first is the age of birth. This reflects both variation in the abduction levels over time and the LRA's preferences for certain age groups. For instance, infants are not useful to carry loot nor as forced soldiers. While adults are useful for the former, they are less useful in the latter role as they are more difficult to indoctrinate and manipulate (Berber and Blattman 2013). Young men were therefore targeting for abduction and conscription.

The second significant factor is the size of the household in 1996. This is driven by households with more than 25 members. These types of households are extremely rare. In the 2002 Uganda Census, households of more than 25 members make up less than 0.1 percent of northern Uganda households. Consequently, after controlling for the age of the individual, abduction appears essentially random.

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substantial qualitative and quantitative evidence supports the apparent randomness of selection into violence and abduction. Consequently, any omitted variable should be post-exposure.

Column (2) of Table 1 shows that, despite the smaller sample, these results hold in the Round 2 data as well. The only difference is that estimated coefficient for having a father who is a farmer is also significant. The coefficient, however, is small suggesting that it has very little practical effect.

Not only does this demonstrate that abductions are exogenous, it also suggests that exposure to violence more generally – directly experiencing or witnessing violence, having family members suffer violence or even perpetrating violence – is also largely exogenous in the full sample. Abductions and exposure to LRA violence are necessarily correlated. While we favor the more precise coefficient estimates of the full sample, the smaller sample offers an important robustness check to address prospective concerns about endogenous selection into exposure to violence. Once abductees were under the control of the LRA, their likelihood to experience, witness and perpetrate violence dramatically increases because the LRA used violence as a means of controlling and indoctrinating abductees. But to a large extent, this exposure was random. If a captive was with an LRA group that raided further villages, encountered the Ugandan army or in which another captive tried to escape (none of which are not under the control of any abductee), they would be exposed to additional violence. Similarly, commanders had differing reputations for the violence to which they subjected their abductees.

Abductees therefore differ from the general population in that they are exposed to substantially higher levels of exogenously determined violence. These levels can be seen in Table 2. In column (1), each entry is the estimated coefficient relating abduction to a specific form of violence or mental distress after controlling for pre-treatment covariates.[29] Since abduction is exogenous, we can interpret this as the causal estimates of the "average treatment effect" (ATE) of abduction on the particular dependent variable. Abduction has a statistically significant positive effect on all types of exposure to violence. Unsurprisingly, abduction also has significant positive effect on both the levels of psychological distress and the likelihood of being in the top quartile of the distribution of psychological distress in the full first survey round sample.

Interestingly, abduction is not significantly correlated with current (round 2) levels of mental health. The psychosocial distress effects of violence appear to dissipate over a period of several

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[29] The covariates are age and location binary variables, age and location interactions, and pre-violence individual and household characteristics.

years.[30] Column (2) reports the unconditional mean of each dependent variable for the non-abducted population, while column (3) shows the ratio of the ATE to the non-abducted mean reflecting the relative magnitude of the estimated effects. Violence experience by family member had the mildest increase, 19%. Unsurprisingly, abductees committed significantly (3,547%) more acts of violence. The effect of abduction on round 1 mental health is more modest (16%) but abductees are disproportionately more likely to be in the highest quartile of psychological distress (62%).

Although individuals are randomly abducted and have little control over the violence experienced during their abduction, they may acquire some measure of control over time. In particular, some boys eventually were trained to be soldiers and given guns. At this point in time, exposure to violence (especially violence perpetrated) becomes endogenous. While we have information on who was given a gun, this choice is likely endogenous. Using the first round of the data, Beber and Blattman (2013) show that younger abductees (less than 12 years old) were the least likely to receive guns and waited the longest to receive them (roughly 10 months). Unfortunately, there are not sufficient observations to further limit the sample to the younger abductees.[31] Consequently, there remains potential for some endogenous exposure to violence, particularly with respect to the violence perpetrated. This concern is likely partially mitigated by the particular context. LRA soldiers who did not follow orders (whether to perpetrate a crime or otherwise) were typically killed.

In the analysis that follows, we explore the association of alternative plausibly-exogenous types of violence experienced – abduction, directly suffering violence, witnessing violence, perpetrating violence, or one's family members suffered violence – on various measures of respondents' risk preferences. We estimate the ATEs in the full sample and then repeat the analysis with the smaller subsample of abductees as a robustness check on our core results.

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[30] The lasting effects of exposure to traumatic events on behavior are complicated. Exposure to traumatic events, such as exposure to violence, does cause PTSD in some individuals although most do not develop index episodes. The majority of those with PTSD gradually recover irrespective of treatment (in US sample) over a period of years. That said, PTSD persists in a substantial minority (over one third) (Kessler *et al.* 2005). More broadly, exposure to traumatic events appears to lead to changes an individual's outlook on life (Carmil and Breznitz 1991; Punamäki, Qouta, and El Sarraj 1997; Tedeschi and Calhoun 2004).

[31] There are only 66 abductees who were 12 or younger at the time of their longest abduction. Of these, only 50 were abducted for less than 10 months, the average period which that age group waited to receive a gun.

We begin by establishing the effects of exposure to violence on psychological distress in Round 1. In particular, we estimate equation (3):

$$(3) Distress_i = \beta_0 + \beta_1 Exposure_i + \beta_2 X_i + e_i$$

where $Distress_i$ measures the psychosocial distress in round 1 for person i , $Exposure_{i,1}$ is a vector of variables for person i's exposure to violence in the first survey round. X_i are a set of covariates for person i from the first round. We estimate this regression using the two samples described earlier: the full and the abducted samples. In the latter estimation, we include further controls related to their abduction experience[32] and interventions after return[33]. e_i is an error term which is assumed to be normally distributed, mean zero and iid. The estimates for equation (3) are presented in Table 3. The results are extremely similar in magnitude and significance in both samples: exposure to violence, specifically violence which is witnessed or perpetrated, is associated with higher levels of distress. Consequently, using equation (2), we can investigate whether the effects of exposure to violence on risk attitudes occur mainly/only through induced psychological distress or whether there are additional channels.

To investigate the effects of exposure to violence on risk preferences, we estimate ordered logits for each risk domain (gains, losses and ambiguity) using equation (2).[34] In the exposure and distress vectors, we include a binary variable for abducted before round 1, the violence indices and the measures of psychological distress in rounds 1 and 2 respectively. In each estimation, we include two sets of control variables: pre-violence characteristics (from 1996) and first round survey characteristics from near the end of the conflict.[35] Similar to Moya (2014), we also control

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[32] We control for their age at the time of their longest abduction, the log of the length of their longest abduction, and the number of years since their return at the time of the interview in round 1.

[33] We include binary variables for whether or not an abductee went through a reception center, received counseling, participated in a cleansing ceremony, or whether they stepped upon an egg after returning. The latter two variables account for local ceremonies which help to cleanse the individual of any evil spirits and help them overcome their experiences. Although these ceremonies are likely endogenous, they are widely believed to be important steps by both the community and abductees. From our field interviews, certain abductees who continue to experience persistently high levels of distress attribute this to the absence of a ceremony.

[34] There are six categories of risk attitudes for the gains and ambiguity domain. Due to a mistake in the survey, a category was repeated in the losses domain resulting in only five distinct categories.

[35] The pre-violence characteristics are sub-county controls, educational attainment of each parent, household size, household land holdings, household cattle holdings, other livestock holdings, and whether the father and mother were alive in 1996. The first round characteristics are IDP camp residence, being located outside of the original districts, education levels, job skill category, literacy, a health index, a binary variable for any serious injury, a binary variable

for earnings from the last practice round. For the regressions that are limited to just the abductees sub-sample, we further include controls for the age at time of longest abduction, the log of the length of the longest abduction, and the number of years which have elapsed between the second round interview and the respondent's return from the most recent abduction.[36]

The estimates for the full sample are presented in Table 4. The columns report the estimates for the gains, losses and ambiguity domains respectively. Positive (negative) coefficient estimates indicate increasingly risk loving (averse) preferences. Seven years after the end of the conflict, exposure to violence continues to affect individual's risk attitudes. The end of the war and the ensuing post-conflict process, including a range of mitigation interventions, have not fully attenuated these effects. Such persistent effects of conflict on behavioral parameters can have a profound and lasting effects on lives and livelihoods.

Equally important, the specific type of exposure matters and these effects vary across risk domains. In particular, each type of violence only affects one type of risk attitude. Receiving violence (whether to the respondent or to his family) leads to risk loving attitudes, while witnessing or perpetrating violence or being abducted leads to risk aversion. Surprisingly, distress is generally statistically insignificant with one exception. Higher levels of current psychological distress are associated with more risk aversion over gains. This result is consistent with Moya (2014) although he finds a negative and significant effect on both the gain and ambiguity domains.

Table 5 presents the results for the same estimation using only the subsample of former abductees. The qualitative story for the violence indices is broadly similar. Receiving violence, both individually and to one's family, leads to risk loving attitudes. Witnessing or perpetrating violence leads to greater risk aversion.

In contrast to the earlier results, current psychological distress is no longer significantly related to risk aversion. Although this could be caused by the loss in precision due to the smaller sample, this is unlikely the main reason. The size of the estimated coefficient for psychological distress declines from -0.11 in Table 4 (over gains) to -0.05 in Table 5 (over gains). Moreover, the loss in

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for being married, an index for family connectiveness, binary variables for each birth year, whether the father and mother are alive in round one, and age-location interactions.

[36] Most abductees are only abducted once. The highest number of abductions in the sample is three.

precision due to the smaller sample size does not seem to have affected the significance of the other coefficients.

Round 1 psychological distress, however, leads to risk aversion over both gains and losses and increased risk seeking over the ambiguity domain. Since the exposure to violence in the subsample is more plausibly exogenous, we consider round 1 psychological distress to be more important. It is nonetheless clear, from Tables 4 and 5, it is clear that psychological distress is associated with changes to risk preferences.

Although there is some variation between Table 4 and 5, we consistently find that all of the exposure types are significant (over some domain) after controlling for psychological distress. This suggests that the effects of exposure to violence on risk aversion do not solely operate through increased psychological distress; other important channels remain.

Furthermore, the sign of the estimated coefficients varies predictably in both samples. Receiving violence directly or having one's family exposed led to greater risk seeking behavior. In contrast, perpetrating or witnessing violence leads to greater risk aversion. With the possible exception of violence perpetrated[37], the heterogeneity of experiences is likely not unique to Northern Uganda. This suggests that the use of spatially aggregated indicators of violence misrepresents populations that are intrinsically heterogeneous in their experience of violence, and in ways that matter to elicited risk preferences.

We examine this formally by estimating the overall effect of exposure to violence for each individual in the sample. Since most studies examine risk attitudes over gains, we focus on the estimates from column (1) of Table 4. We generate the individual-specific changes in risk attitudes using the respondent-specific value for each of the four exposure types and abduction status (since this represents a form of violence) and the estimated coefficients from column (1). The full distribution of the changes in risk attitudes is shown in Figure 1.[38]

Exposure to violence leads to a slight increase in average risk aversion (-0.17). This is analogous to the average effect calculated in the existing literature. There is, however, considerable

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[37] Similar rates likely exist in Liberia, Rwanda, and Sierra Leone. More broadly, this is likely more common in long civil wars.

[38] The confidence intervals are generated based on Hall (1992) and Horowitz (2001). In particular, it corrects for asymptotic bias by allowing for undersmoothing (Fiorio, 2004). The intervals are bootstrapped with 10,000 replications. This also true of the confidence intervals subsequently generated in Figure 2.

heterogeneity that has been overlooked in prior studies. For instance, despite the small increase in risk aversion on average, roughly 40 percent of the sample becomes more risk seeking. This suggests that calculations of the average effect can significantly misrepresent individual effects and not only in magnitudes but even in the sign of the estimated effect. Moreover, it suggests that even within a particular setting, individuals may respond very differently to the same opportunity or policy intervention based on their particular experience of violence. Thoughtful targeting of interventions then becomes especially important to program effectiveness.

Figure 2 decomposes Figure 1 into the distributions for respondents who are abducted and those who were never abducted.[39] While there is significant overlap between the two distributions, the distribution of the abducted respondents is clearly more risk averse. More precisely, using the Kolmogorov-Smirnov test (Kolmogorov, 1933; Smirnov 1933), the equality of the two distributions is rejected at the 1 percent level. The mean effects are, -0.39 and -0.02 for the abducted and non-abducted samples respectively. As with the overall sample (Figure 1), parts of each distribution exhibit positive estimated effects, implying that risk seeking predictably increases in a non-trivial part of each sub-population.

When an aggregate geographical measure of exposure to violence is used, the resulting coefficient estimate is a population weighted average of the affected and non-affected population. At some risk of oversimplification, the abducted and non-abducted samples can be considered as 'high' and 'low' exposure samples, respectively. The average effect is a linear combination of these samples. As the proportion of the 'low' exposure sample increases, the average overall effect approaches the average effect of the 'low' exposure sample. Since the average effect of the non-abducted sample is close to 0 (-0.02), it would appear that exposure to violence has no effect. This overlooks, however, the strong effect in the high exposure sample.

Lastly, we examine which factors might mitigate the effect of violence on risk attitudes. In particular, we focus on the abduction experience and interventions after their return. We present the regression results in Table 6. The leftmost column in each of the three column pairs replicates the results reported in Table 3. Unlike Moya (2014), we find no dissipation of the effect of violence (and displacement) over time. We find similar results with the inclusion of a quadratic term (results

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[39] To decompose the effects, we use the coefficients from Table 4. An alternative approach would be to use the coefficients from Table 5 to generate the distribution for the abducted sample. As can be seen in Appendix 2, these generate qualitatively similar distributions of changes to risk aversion for the abducted sample.

not presented here). Due to the duration of the conflict, many of the abductions occurred years before the first round. In our sample, abducted individuals had, on average, returned 12.4 years before the second interview with a high of 22.6 years earlier. Consequently, this suggests that time, by itself, does not attenuate the effects of exposure to violence on risk preferences. Similarly, age at exposure or the length of one's longest abduction do not appear to matter.

In even-numbered columns, we investigate the effects of interventions upon return from abduction. These interventions, however, are clearly endogenous so it is not possible disentangle causality from correlation.[40] Each of these interventions is associated with changes in risk attitudes. These results are consistent with interventions mitigating the effects of exposure to violence and, more generally, conflict. Further research is needed to both establish causality and to understand the relative effects of formal and informal/local interventions.

VI.   Conclusion

In this paper, we find that the effects of exposure to violence on risk preferences persist for years after the exposure and even after the end of the conflict. We extend the previous literature by disaggregating exposure to violence into four dimensions of direct and indirect exposure to violence: personal suffering, perpetration, witness, or indirect experience through family members' suffering. In doing so, we identify previously unexplored heterogeneity in the broader economics literature that can reconcile the divergent empirical results found in the prior literature. In particular, exposure to suffering (to the individual or the family) leads to higher levels of risk seeking behavior. In contrast, witnessing or perpetrating violence leads to greater risk aversion.

More broadly, our results raise concerns regarding both the prior literature's focus on 'average' effects and the widespread use of geographical measures of exposure to violence as a proxy for individual-level experiences. Because they ignore interpersonal variation in the experience of violence, these measures may not accurately represent the heterogeneous responses of subpopulations experiencing different types of exposure to violence. Consequently, these results

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[40] Abducted individuals who passed through the Ugandan army (UPDF) were three times more likely to report passing through a reception center (61.4 vs 20.1 percent respectively). Additionally, with the exception of one case, counseling only occurred among individuals in reception centers (64.4 percent).While we control for passing through the UPDF, participation in specific interventions is likely correlated with the mental health and general appearances of returning abductees – neither of which is observed in the data. Consequently, it is not possible to control for selection into specific interventions although the inclusion of the variable for passing through the UPDF may reduce this.

should be interpreted carefully especially in formulating policy and designing interventions aimed at mitigating the adverse effects of violence on mental health and risk preferences.

Our results also suggest that the effects of violence on risk aversion may be highly context-specific, and even person-specific. Individuals may experience violence both directly, to themselves or their family, or indirectly, as witnesses, or they might perpetrate violence, or some combination of these. Different experiences of violence may well have different psychological effects as well as differential effects on behavioral parameters, such as risk preferences, independent of any induced psychological distress. Depending on person-specific experiences, individuals in the same setting may have very different responses in terms of risk attitudes to violence. Consequently, there may be substantial heterogeneity in otherwise identical people in response to new opportunities or aid programs in post-conflict settings. At the more aggregate level, the relative distribution of direct versus indirect exposure to violence can lead to quite varied mean effects. It is therefore not surprising that the literature does not find a consistent result of violence on risk attitudes (or other outcomes).

The analysis also highlights the importance of mental health, particular psychosocial distress, in understanding the changes in risk attitudes in violence affected populations. Higher levels of mental distress lead to significant increases in risk aversion. While this has been found in an earlier study (Moya 2014), we extend this by looking at the evolution of distress in the post-conflict period. In our abductee sample, with the more plausibly exogenous exposure to violence, we find that the distress symptoms near the end of the conflict (in 2005/6) are significantly related to current risk aversion (2012); in contrast, current distress is not significantly related. This highlights the importance of mental health interventions during and very early in the aftermath of conflict for those with high levels of symptoms.

The expulsion of the LRA from Uganda and the end of nearly two decades of conflict have led to many new opportunities in northern Uganda. For many, their wartime experiences will shape their decision making and, ultimately, their response to these opportunities. Although the war is finished, its legacy continues.

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Table 1: Difference in Conditional Means for Abducted and Non-Abducted

Sample	Appear in Rd 1	Appear in Rd 2
Year of birth	1.27**	1.26**
Indicator for father a farmer	-0.01	-0.07**
Household size in 1996	-1.51***	-1.27***
Landholdings in 1996	-1.46	-2.15
Indicator for top 10% of landholdings	-0.02	-0.04
Cattle in 1996	6.21	4.78
Other livestock in 1996	2.07	-1.58
Indicator for plow ownership in 1996	-0.01	0.05
Indicator for uneducated father	0.02	0.05
Father's years of schooling	-0.06	-0.13
Indicator for uneducated mother	-0.01	-0.07
Mother's years of schooling	-0.12	0.25
Indicator for paternal death before 1996	0.03	0.03
Indicator for maternal death before 1996	0.02	0.02
Indicator for orphaning before 1996	-0.02	-0.01

Each estimate use pre-violence data on wealth, parents' occupation and education, and whether their parents are alive as controls. Clustered by location.

*,**,*** significant at the 10, 5, and 1 percent levels, respectively

Table 2: Average Treatment Effect of Abduction on Mental Health and Exposure

	Average Treatment Effect (ATE)	Non-Abducted Mean	ATE as % of mean
Round 1 Outcomes			
Psychosocial outcomes (Rd 1)			
Index of psychosocial distress	0.60***	3.65	16%
Indicator for top quartile of distress	0.11***	0.18	62%
Exposure to Violence (Rd 1)			
Index of violence received	2.68***	1.02	263%
Index of violence witnessed	1.70***	2.12	80%
Index of violence perpetrated	1.72***	0.05	3,547%
Index of violence experienced by family	0.38***	1.99	19%
Round 2 Outcomes			
Psychosocial outcomes (Rd 2)			
Index of psychosocial distress	0.27	3.68	7%
Indicator for top quartile of distress	0.00	0.22	1%

Control for: age and location dummies, age and location interactions, pre-treatment individual and household characteristics

*,**,*** significant at the 10, 5, and 1 percent levels, respectively

Table 3: Effect of Exposure to Violence on Mental Health (Rd1)

	Full Sample	Only Abducted
Index of violence received	-0.07	-0.17
	[0.09]	[0.16]
Index of violence witnessed	0.28**	0.30***
	[0.12]	[0.08]
Index of violence perpetrated	0.24**	0.29***
	[0.11]	[0.10]
Index of violence suffered by family	0.13	-0.19*
	[0.11]	[0.10]
R^2	0.43	0.56
Sample size	434	258

*,**,*** significant at the 10, 5, and 1 percent levels, respectively

Table 4: Effect of Exposure to Violence on Risk Attitudes (Full Sample)

Domain	Over gains	Over losses	Ambiguity
Index of violence received	0.30***	-0.01	0.05
	[0.09]	[0.18]	[0.20]
Index of violence witnessed	0.06	-0.15*	-0.02
	[0.14]	[0.08]	[0.16]
Index of violence perpetrated	-0.20	0.10	-0.23**
	[0.12]	[0.07]	[0.10]
Index of violence suffered by family	-0.22	0.38***	0.11
	[0.14]	[0.12]	[0.10]
Ever abducted (yes=1)	-0.86**	0.39	0.36
	[0.39]	[0.40]	[0.56]
Psychological distress, round 1	-0.05	0.01	0.03
	[0.07]	[0.12]	[0.07]
Psychological distress, round 2	-0.11*	0.01	0.02
	[0.06]	[0.09]	[0.06]
Sample size	427	421	419

Differences in sample size arise from missing answers for the winnings in the last practice round

The ordered logits control for pre-treatment and round 1 covariates.

*,**,*** significant at the 10, 5, and 1 percent levels, respectively

Table 5: Effect of Exposure to Violence on Risk Attitudes (Only Abductees)

Domain	Over gains	Over losses	Ambiguity
Index of violence received	0.40*	0.65**	0.19
	[0.21]	[0.28]	[0.23]
Index of violence witnessed	-0.34**	0.22	-0.23
	[0.16]	[0.15]	[0.23]
Index of violence perpetrated	-0.01	-0.02	-0.49***
	[0.18]	[0.12]	[0.17]
Index of violence suffered by family	-0.20	0.02	0.63***
	[0.25]	[0.32]	[0.20]
Psychological distress, round 1	-0.12*	-0.31**	0.23*
	[0.06]	[0.12]	[0.12]
Psychological distress, round 2	-0.05	-0.03	0.12
	[0.14]	[0.11]	[0.14]
Sample size	257	253	252

Differences in sample size arise from missing answers for the winnings in the last practice round

The ordered logits control for pre-treatment, round 1, and abduction covariates.

*,**,*** significant at the 10, 5, and 1 percent levels, respectively

Table 6: Recovery from Exposure in the Abducted Sample

Domain	Gains	Gains	Losses	Losses	Ambiguity	Ambiguity
Index of violence received	0.40*	0.49*	0.65**	0.65*	0.19	0.28
	[0.21]	[0.24]	[0.28]	[0.35]	[0.23]	[0.19]
Index of violence witnessed	-0.34**	-0.45**	0.22	0.29*	-0.23	-0.38
	[0.16]	[0.17]	[0.15]	[0.15]	[0.23]	[0.27]
Index of violence perpetrated	-0.01	0.03	-0.02	0.07	-0.49***	-0.49***
	[0.18]	[0.16]	[0.12]	[0.13]	[0.17]	[0.16]
Index of violence suffered by family	-0.20	-0.14	0.02	-0.10	0.63***	0.54**
	[0.25]	[0.31]	[0.32]	[0.31]	[0.20]	[0.26]
Psychological distress, round 1	-0.12*	-0.16**	-0.31**	-0.34***	0.23*	0.28**
	[0.06]	[0.07]	[0.12]	[0.12]	[0.12]	[0.13]
Psychological distress, round 2	-0.05	-0.04	-0.04	-0.02	0.12	0.08
	[0.14]	[0.16]	[0.11]	[0.13]	[0.14]	[0.16]
Age at longest abduction	-0.13	-0.13	0.09	0.02	0.00	0.11
	[0.15]	[0.18]	[0.16]	[0.17]	[0.16]	[0.14]
log of length of longest abduction	-0.15	-0.20	0.07	0.06	0.08	0.24
	[0.11]	[0.14]	[0.15]	[0.16]	[0.15]	[0.15]
Years since last abduction	-0.17	-3.31	0.06	-0.02	-0.11	0.27
	[0.15]	[2.57]	[0.16]	[0.82]	[0.19]	[0.60]
Years since last abduction squared		-0.01		0.00		-0.01
		[0.03]		[0.03]		[0.02]
Passed through reception center		0.08		3.30**		-1.07
		[0.77]		[1.45]		[0.78]
Receiving counseling		0.33		-2.11*		1.29**
		[0.64]		[1.22]		[0.51]
Ceremony: stepped on egg		0.44		-1.07*		-0.86
		[0.93]		[0.62]		[0.75]
Ceremony: cleansing		0.56		0.25		1.46**
		[0.59]		[0.80]		[0.57]
N	257	256	253	252	252	251

*,**,*** significant at the 10, 5, and 1 percent levels, respectively

Figure 1: Estimated Effect of Exposure to Violence on Risk Attitudes
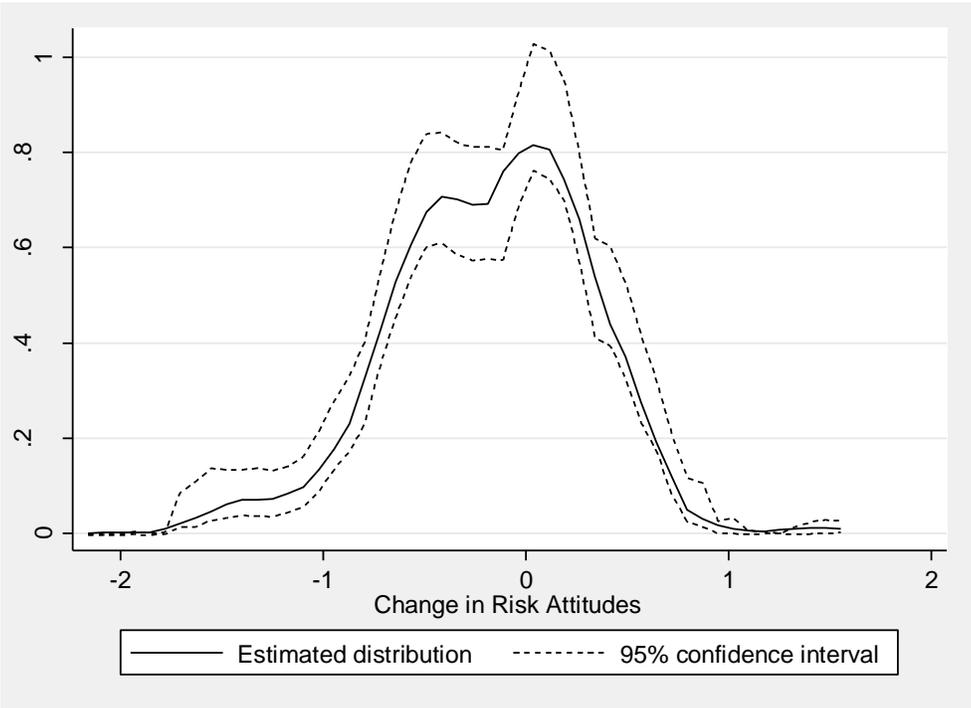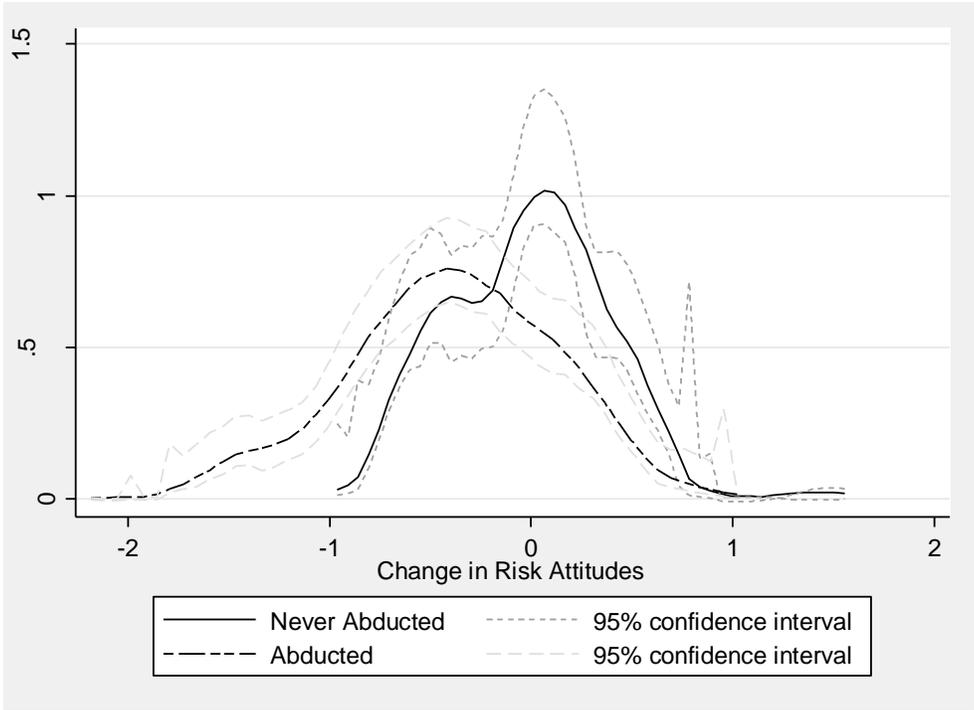
Figure 2: Decomposing the Estimated Effect of Exposure to Violence on Risk Attitudes

Appendix 1

Extract from questionnaire regarding risk attitudes:

Read: We are now going to play a different type of game. I will read you three groups of questions. In each group, you should choose the choice which you prefer. At the end, we will randomly choose which group of questions we will use for the game. Once we find out which game we will play, we will do the one option you had chosen. You will not be able to change your choice once we pick the game. Just like the last game, we will play this so that you can understand the game.

Read: In this first group of questions, you have the same chance to win either amount. If we play this game, we will put 5 blue chips and 5 red chips in a bag. If you choose the red chip, you will get the red amount. If you choose the blue chip, you will get the blue amount.

Choice	Blue	Red
A	2,000	2,000
B	600	5,000
C	0	5,800
D	1,600	3,000
E	500	5,500
F	1,000	3,500

Read: In this set of questions, some of the choices can result in you losing money. Since we do not want you to lose money from the survey, if we play this game I will give you 5,000 shillings to begin with. You will then have to pay out losses from this amount. You will get any remaining money including anything extra you win. Again, we will have 5 blue and 5 red chips in a bag. You will have an equal chance for either a red or blue chip.

Choice	Blue	Red
A	-5,000	4.500
B	-2,500	-100
C	3,200	1,200
D	-4,000	2,700
E	-4,500	4,000
F	-5,000	4,500

Read: In the previous groups of questions, you had an equal chance to get a red or blue chip since there were 5 blue and 5 red chips. While we will have 10 chips again in the bag, you will not know how many red or blue chips there are in the bag. We will begin with 3 blue and 3 red chips. We will then blindly and randomly add 4 chips to the bag. You will then draw the chip from the bag.

Win	Blue	Red
A	2,000	2,000
B	600	5,000
C	0	5,800
D	1,600	3,000
E	500	5,500
F	1,000	3,500

Appendix 2: Comparing the Distribution of Effects of Exposure to Violence in the Abducted
Sample Using Coefficients from Tables 4 and 5