

TRUSTING FORMER REBELS: AN EXPERIMENTAL APPROACH TO UNDERSTANDING REINTEGRATION AFTER CIVIL WAR

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Trusting Former Rebels: An Experimental Approach to Understanding Reintegration after Civil War*

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Abstract

The stability of many post-conflict societies rests on the successful reintegration of former soldiers. We use an experimental approach to study reintegration in Northern Uganda and examine behavior of former soldiers together with the behavior of receiving communities towards this group. We focus on trust-based interactions and find that individual trustworthiness increases with the length of time a person was with the Lord's Resistance Army, a rebel group which forcibly recruited a large fraction of young people in the area. The effect is strongest among former soldiers who were abducted during childhood and is mute among those who soldiered during adulthood. These results are consistent with predictions of recent theories that highlight the importance of cooperation during war. Furthermore, members of receiving communities with an abductee son, who thus have better knowledge of former soldiers are aware of the behavioral difference. They believe former soldiers are more trustworthy than their peers and trust them more. Last, we find no evidence of preference-based discrimination, suggesting that anger is attenuated when communities do not attribute responsibility for committed violence to returning soldiers.

Keywords: trust, cooperation, civil war, endogenous preferences, soldiers, reintegration

JEL classification: C93, D03, D74, O12

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Abstrakt

Stabilita zemí, které si projdou občanskou válkou, závisí do značné míry na tom, zda se podaří reintegrovat bývalé vojáky zpět do společnosti. V tomto článku studujeme reintegraci v severní Ugandě pomocí nástrojů experimentální ekonomie, které nám umožňují měřit jak chování bývalých vojáků tak chování komunit k bývalým vojákům. Zaměřili jsme se na interakce, které vyžadují důvěru. Hlavní zjištění jsou následující. Zaprvé, spolupráce roste s dobou, kterou jednotlivec musel bojovat pro Armádu božího odporu (Lord's Resistance Army), t.j. rebelskou skupinu, která unesla a násilně donutila bojovat velké procento mladých lidí ve válkou postižených oblastech. Tento efekt je silný zejména pro ty bývalé vojáky, kteří byli uneseni v nízkém věku. Tyto výsledky jsou v souladu s predikcemi teorií, které podtrhují důležitost spolupráce během válek. Zadruhé, lidé, kteří mají syna, jenž musel bojovat pro LRA, si uvědomují větší ochotu bývalých vojáků ke spolupráci. Očekávají, že bývalí vojáci jsou důvěryhodnější než jejich vrstevníci, a také jim více věří. Zatřetí, nenacházíme žádné známky diskriminace bývalých vojáků na základě preferencí (taste-based discrimination), což naznačuje, že hněv za násilí spáchané bývalými vojáky je potlačen, pokud jim za něj lidé nepřisuzují zodpovědnost.

1 Introduction

In conflicts around the world, forcible recruitment of soldiers, often children, is a widespread practice among many military, paramilitary and insurgent groups (Blattman and Miguel, 2010; Beber and Blattman, 2013).¹ After conflicts end, one of the central issues faced by these societies is the reintegration of soldiers in their home communities, in part because of the risk of falling into the “conflict trap” (Collier, 2007). If reintegration is unsuccessful, and former combatants become socially isolated or economically worse off, feelings of frustration and low opportunity costs make it more likely that they will join armed groups in the future (Collier and Hoeffler, 2004; Knight and Özerdem, 2004), and violence may re-emerge as a result. To assess the success of reintegration, the existing empirical literature has used surveys and responses to questions about labor-market outcomes, community participation and hostility towards those involved in conflict (Humphreys and Weinstein, 2007; Blattman, 2009; Restrepo and Muggah, 2009; Blattman and Annan, 2010; Annan et al., 2011). While these pioneering studies provide important insights into whether economic and social gaps between ex-soldiers and their peers exist, they are less informative about the underlying sources.

Since reintegration outcomes are determined by economic and social interactions between ex-combatants and the communities to which they return, a researcher would ideally have a way to separate the influence of soldiering on the behavior of returnees from differential behavior towards returnees by the receiving communities. Specifically, in order to understand sources of any observed gaps between ex-combatants and their peers, one would like to know (i) whether

¹Since 2001, the participation of child soldiers (under the age of 18) has been documented in 21 armed conflicts in almost every region of the world. In Africa, well-known examples include conflicts in Uganda, Sierra Leone, Sudan and Mali. Although there are no exact figures, hundreds of thousands of children under the age of 18 are estimated to serve in armed rebel groups and government forces every year (HRW, 2008).

former soldiers have different skills, preferences or beliefs about the behavior of others and (ii) whether community members treat the former soldiers differently compared to other individuals and in case they do, whether it is due to “taste-based discrimination” (Becker, 1971) or due to beliefs about future behavior of former soldiers, a type of discrimination commonly referred to as statistical discrimination (Phelps, 1972). Such distinctions are important, since each of these factors has different behavioral and policy implications.

This paper contributes to the existing literature by using an experimental approach, which allows us to study these detailed aspects underlying reintegration outcomes. We focus on key components of social capital—trust and cooperative behavior—because they are difficult to measure in surveys and because they play a central role in determining a range of important economic and social outcomes, including provision of credit, availability of jobs, and participation in informal saving or insurance arrangements.² This is especially true in settings where economic interactions are rarely governed by formal contracts, as in rural economies of developing countries. The setting is Northern Uganda, where an unpopular rebel group (the Lord’s Resistance Army or LRA) forcibly recruited a large fraction of youth (>25% in the most affected areas) during a war that lasted for 20 years. Most of these soldiers later returned to their communities and were found to have lower earnings compared to their peers and were less likely to have skilled employment (Blattman and Annan, 2010). We ran experiments on a representative sample of villagers between 35-55 years, who played a set of inter-locked games with younger, male partners, some of whom had been abducted by the LRA, for various lengths of time and at various ages. This design allows us to

²Societal trust has been found to be linked with a range of important aggregate outcomes, such as the self-governance of communities (Gächter and Herrmann, 2011; Cox et al., 2011), financial development and trade (Guiso et al., 2004) and the rate of economic growth (Knack and Keefer, 1997). Recent studies have also shown that cooperative preferences, such as altruism, inequality aversion and reciprocity, facilitate cooperation in large groups (Rustagi et al., 2010; Boyd and Richardson, 2005) and influence participation in public life and politics (Bowles and Gintis, 2002).

examine differences in cooperative behavior of abducted soldiers in comparison to their peers, whether soldiering during childhood or early adolescence leaves a deeper mark than soldiering later on in life and how members of the community differ in their behavior towards former soldiers.

While there are compelling reasons for conjecturing that soldiering may affect cooperative behavior,³ it is not clear a priori what the nature of these changes might be. On the one hand, experience of soldiering may have negative effects, as is frequently assumed by policy-makers who typically describe former child soldiers as “social pariahs” (New York Times, 2006) who remain alienated from the members of their original communities and “at war” in their minds (Richards, et al. 2003). Such negative changes in social behavior can, in principle, be due to trauma experienced (Catani et al., 2008), purposeful identity manipulation performed by rebels to create new social bonds and an expectation of rejection by home communities (Beber and Blattman, 2013), or due to grievances motivated by economic gaps.

On the other hand, several intriguing theories suggest that the experience of soldiering might, in fact, lower selfishness and intensify cooperative behavior. Evolutionary approaches to human prosociality have long emphasized the important role that lethal conflict between groups and other external threats have likely played in shaping human social psychology (Darwin, 1873; Choi and Bowles, 2007; Bowles, 2006). Since groups containing more individuals with altruistic motives towards in-group members are more likely to survive during fighting, the theory suggests that human social preferences may be sensitive to experiences of intergroup conflicts and survival threats, and such experience may activate or intensify preferences which facilitate within-group cooperation. In line with the

³For a large portion of our sample the violent experience we study took place during childhood and adolescence, a sensitive period during which social preferences have been found to develop (Harbaugh and Krause, 2000; Fehr et al., 2008) and during which social preferences are especially sensitive to environmental factors (D’Adda and Lively, 2012; Bauer et al., 2014b).

theory, recent behavioral experiments among victims of war-related violence in Israel, Burundi, Georgia and Sierra Leone have revealed that greater exposure to violence reduces selfishness and increases pro-social preferences (Voors et al., 2012; Gneezy and Fessler, 2012; Bauer et al., 2014a).⁴ However, these experiments analyze the cooperative behavior of recipients of violence and there is, to best of our knowledge, no comparable evidence using behavioral experiments with ex-soldiers, who were often perpetrators of violence.⁵ The most closely related evidence comes from a detailed survey work of Blattman (2009), which shows that forced recruitment by rebels in Northern Uganda leads to greater likelihood of reporting participation in voting.

Similarly, little is known about whether receiving communities discriminate against former soldiers. In principle, differential treatment of ex-combatants, as any other type of discrimination, may be an outcome of preferences or an outcome of beliefs about expected behavior (statistical discrimination).⁶ Preference-based discrimination reflects dislike or anger against certain groups; such discrimination against soldiers could arise if the receiving communities blame the ex-soldiers for their violent acts while with LRA. On the other hand, since in many civil wars soldiers take part against their will—as was the case in the LRA conflict—they may be seen by the receiving communities as victims who are in greater need than others, leading to more favorable treatment compared to peers.⁷ This distinction

⁴For related non-experimental evidence, see Bellows and Miguel (2009) who find positive link between exposure to violence and participation in local collective action in Sierra Leone and Rohner et al. (2013) who show a link between living in areas with more intense fighting, and less self-reported trust and stronger ethnic identity in Uganda.

⁵See also Cassar et al. (2011) who find a negative link between reporting involvement in fighting and social preferences and trust ten years after the civil war in Tajikistan. As the authors readily acknowledge, however, their sample of ex-combatants is small (10 individuals) due to challenges with identifying former soldiers in this context, making inferences about differences in behavior compared to non-combatants difficult.

⁶Although there are no studies measuring differential treatment of former child soldiers, there is a large literature that estimates the extent of discriminatory behavior based on ethnicity or gender (for surveys see Altonji and Blank (1999) and List (2004)). Studies designed to separate taste-based discrimination and statistical discrimination are still relatively rare. Important exceptions are Fershtman and Gneezy (2001), and Gneezy et al. (2012).

⁷For experimental studies on the role of need in social preferences see e.g., Eckel and Gross-

follows the logic of attribution theory, an influential concept in psychology (Heider 1958, Weiner 1995), which proposes that the controllability of an action or stigma affects the likelihood of helping or punishing behavior. Consistent with this argument, Gneezy et al. (2012) find that when a characteristic is considered to be an individual’s choice, which is under his or her power to change, such an individual is more likely to face taste-based discrimination, while this is not the case for characteristics perceived to be outside the control of the individual (e.g. race or sex).⁸

Our experimental design and main findings can be summarized as follows. First, we investigate cooperative behavior of former soldiers compared to their peers. We conducted a trust game (similar to Berg et al. (1995)) in which a member of the community—the “Sender”—was given a fixed endowment and was asked to decide whether and how much money she would like to transfer to an anonymous “Receiver.” The amount transferred was then tripled by the experimenter, after which the Receiver decided whether and how much money to transfer back to the Sender. In this game, the socially efficient outcome is obtainable through cooperation. We find that the longer the period the Receivers spent with LRA, the greater is the share they send back to Senders, i.e. that length of time spent with LRA is associated with higher trustworthiness. This result is robust to controlling for a large set of observable characteristics.

Second, we find that age of soldiering matters: the effect of soldiering on trustworthiness is strong for ex-soldiers who were abducted as children (below 14 years of age) and mute for participants who were abducted at a later age.

As a third step, we investigate whether former soldiers are less trusted. Prior to making their decisions, Senders received information about Receivers. In addition to other characteristics, three treatments varied whether they were told that

man (1996) and Cappelen et al. (2013).

⁸For further evidence from the psychology literature supporting the attribution theory see, for example, Teachman et al. (2003) and Haider-Markel and Joslyn (2008).

Receivers had been with the LRA for around one month, for around 1 year, or given no information on abduction history. On average, we find no statistically significant effect of Receivers' history with the LRA on trust. Interestingly, however, Senders who have had a son abducted by the LRA send significantly more in the trust game in the LRA treatments. It turns out that the difference that we observe in trust is statistical in nature. We directly elicited Senders' beliefs of the amount they expected to receive back and find that Senders with a son who had been abducted expect to receive more back from ex-soldiers, while other Senders (with no sons abducted) do not differ in their expectations of trustworthiness. These results reveal that Senders with an abducted son are aware of the more cooperative behavior of ex-soldiers compared to their peers and act based on this belief.

Fourth, we conducted a dictator game, in which the Sender decides how to allocate money between himself and the Receiver.⁹ The Receiver is passive in this game and thus any effect of the knowledge about the LRA history of the Receiver must be due to taste-based discrimination. We find no differences in the amount transferred, both for those who have and those who do not have an abducted son, indicating that former soldiers face neither taste-based discrimination nor favoritism.

The remainder of the paper is organized as follows. In Section 2 we briefly describe the background: the conflict in Northern Uganda, recruitment strategy of the Lord's Resistance Army and reintegration practices. In Section 3 we describe the sample selection and the experimental design. In Section 4 we present the empirical results about behavioral differences between former soldiers and their

⁹Our experimental design builds on Fershtman and Gneezy (2001) who study ethnic discrimination using the trust, dictator and ultimatum games among university students in Israel. In contrast to their study, we elicit beliefs about partners' behavior and use a within subject design instead of an across subject design. These extensions help us to decompose trust to the preference-based component and belief-based component at the individual level, as well as to measure expected discrimination.

peers. Section 5 presents results about differential treatment of former soldiers by their communities. Section 6 concludes.

2 A Short Background on Soldiering in Northern Uganda

The leader of the Lord's Resistance Army (LRA), Joseph Kony, led a group of fighters from the North of the country against the government from 1986 to 2006. At the height of the conflict with the LRA, exposure to violence in the districts in which this study was conducted—Kitgum and Gulu—was widespread, affecting virtually the entire population. In 2002, the government of Uganda, possibly in order to make fighting the rebels easier, forced the entire rural population of Northern Uganda, literally overnight in most cases, into Internally Displaced Persons (IDP) camps.¹⁰

The LRA has never enjoyed much support from the local population due to its brutality and few realistic goals. With this lack of civilian support, the LRA obtained supplies and new recruits by conducting raids on rural homesteads, carting off food and forcibly conscripting both children and adults for use as short-term laborers or to permanently join its ranks. Exposure to violence in Gulu and Kitgum districts peaked between 2002-2006. Vinck et al. (2007), who work with a representative sample of adults over 18 years of age in 2005, find that in Gulu and Kitgum districts 89.4% and 90.0%, respectively, of the population were displaced, 68.9% and 65.0% witnessed a child abduction and 46.9% and 46.4% witnessed a family member killed.

The violence with the LRA abated after a peace agreement was signed in 2006, and the LRA has since withdrawn into South Sudan, the Central African Republic and the Democratic Republic of Congo. At the time this study was carried out in 2011, the IDP camps had been closed, and the majority of the

¹⁰For more details about the conflict and the impact of displacement see, e.g., Allen and Vlassenroot (2010) and Fiala (2013).

population had returned to their home villages.

An estimated 24,000-38,000 child soldiers (and 28,00-37,000 adults) were forcibly recruited by the LRA between 1986–2006 (Vinck et al., 2007) and the LRA has demonstrated a preference for adolescent conscripts, particularly around 14 years of age. Besides age and gender, no individual or household characteristic was found to predict the likelihood of abduction (Blattman and Annan, 2010). There have been several explanations offered for targetting youth, including the possibility that younger combatants follow orders more readily and are more receptive to the LRA propaganda and misinformation directed at dissuading them from returning home after they were abducted. For example, abductees were told that their families and communities would not accept them or that they would be arrested or killed if they encountered government troops.

While with the LRA, abductees went through a period of training and indoctrination, after which they were selected for specialized tasks. While some fighters were given guns and sent into battle, other LRA members attended to domestic tasks for commanders or labored in the camps. Most abductees left the LRA by escaping, and a smaller percentage were rescued or released.

Former abductees report that the socialization within the LRA included an emphasis on maintaining group cohesion and avoiding tension with other group members (Vermij, 2011) and obeying rules and orders within one's unit (Mergelsberg, 2010).

To deal with the influx of returning former soldiers, reception centers were set up by government agencies and NGOs. Annan et al. (2006) estimate that around half of child soldiers who spent more than two weeks with the LRA passed through a reception center before returning home. Overall, they find that 95% of former abductees returned to their home communities.

A detailed survey of a representative sample of youth found that that more time spent with the LRA was associated with fewer years of schooling and a

lower likelihood of having formal employment (Blattman and Annan, 2010). In terms of social behavior, some authors—and the media—have emphasized the "damaged" nature of ex-LRA members and their difficulty re-assimilating into society after spending time under the vastly different normative environment of the LRA (Vermij, 2011). However, Blattman (2009) found ex-abductees to be surprisingly resilient. His main finding is that that ex-soldiers were more likely to vote in government elections. While this evidence is consistent with greater willingness to participate in local collective action and help others, it may also indicate different political interests than the average population.

3 Experimental Design

3.1 Sample selection

The experiments were conducted from July to September 2011 in Gulu and Kitgum districts in Northern Uganda. We focus on agrarian villages. We identified villages in which at least 20 ex-abductees were living, based on reports of village leaders, and randomly selected 33 villages (out of 52).¹¹ See Figure A1 for their exact location.

In each village we randomly selected 40 households from a village roster of all households and a member of each household was invited to participate in a pre-survey for which s/he was compensated with 1,000 UGX (around \$0.50 at the time). At this point, the prospect of participating in an experiment was not mentioned. The questionnaire included information on household assets, household members, their ages, conflict experience, whether they had been abducted and whether they had spent time in an IDP camp. Using this information, we compiled a list of individuals together with their characteristics, and identified those who fit the criteria for Senders and Receivers.

¹¹This initial list of villages was derived from a list of communities known to be affected by LRA abduction from Pham et al. (2007).

Since our experimental design models an economic interaction between older members of the community (who are more likely to control productive assets) and younger men, who may or may not have been abducted by the LRA, selection criteria were different for Senders and Receivers. In each village we randomly selected on average 15 individuals from the population of those between 35-55 years old to participate in the role of Senders. Receivers were randomly sampled from the pool of young men between 18-34 years old, the age range with highest proportion of former soldiers. We oversampled ex-abductees in order to have a large enough sample for the position of Receivers. Those invited to participate in the experiment were promised a show-up fee of 2,000 UGX, with the opportunity to earn more. Overall, the response rate was high for both Senders (96%) and Receivers (91% for former abductees and 87% for non-abductees). In all, we have valid experimental data from 378 Senders and 337 Receivers. However, due to incomplete survey data, most of our analysis includes only 360 and 328 individuals, respectively. Subjects were not made aware that they had been selected based on their conflict history, and at no point during interviews with local leaders, household pre-survey or subject invitations did we mention that the focus of the study was reintegration of former soldiers.

3.2 Experimental tasks

Senders

The individuals recruited as Senders were told that the task would be conducted in pairs and that they would be matched with another person from a different but nearby village. They were read a profile that described the person with whom they were matched, as detailed below, before making any decisions in the experiment. The first task consisted of the trust game. Senders were endowed with 2,000 UGX, which was equal to around \$1 US at the time of the experiment,

and is slightly less than average cash weekly income in our sample. Senders were told that Receivers would not be given any initial endowment¹² and were asked to decide between three options, by choosing an amount, $S \in \{0, 1000, 2000\}$, to transfer to their partner. They were informed that the amount transferred would be automatically tripled by the experimenter and that the Receiver would then be given the option of sending back a portion of the amount which he received. Specifically, Receivers could choose $R \in \{0, 1000, 2000, 3000, \dots, 3S\}$. Thus, Senders earned $2000 - S + R$, while Receivers earned $3S - R$. It was also explained to Senders that Receivers would be informed of all details pertaining to the task as well as the amount transferred by Sender. In addition to choosing how much to transfer, we also elicited beliefs about how much Senders expected to receive back. We used the strategy method, asking Senders two questions about the expected back-transfer from their partner, contingent on initially sending 1,000 UGX and 2,000 UGX, respectively. Accurate expectations—i.e. responses that matched the actual behavior of the Receiver with whom a Sender was paired—were rewarded with 500 UGX.

In the trust game, gains are obtainable through cooperation. The amount transferred by the Sender serves as an indication of his trust towards the Receiver or of the two players' ability to cooperate. The efficient outcome, which maximizes total welfare, requires the Sender to transfer the whole endowment to Receiver, since this amount is tripled. When Receivers decide to return an amount larger than that initially transferred by the Sender, both the Sender and Receiver are left better off than they were at the outset of the experiment. However, a purely self-interested Receiver would not be expected to return anything and a similarly self-interested Sender anticipating this would not be expected to send anything.

¹²Unlike the original Berg, Dickhaut and McCabe (1995) trust game, Receivers are not endowed in our experiment. This is to better represent a naturally occurring interaction, in which youth do not have the same access to productive resources as older individuals. Not providing endowment to Receivers in the trust game is also common, see e.g. Fershtman and Gneezy (2001).

This leads to an inefficient outcome which fails to exploit potential gains from sending a positive amount. Numerous studies have shown that individuals do, in fact, transfer and return positive amounts (Camerer, 2003).

The same subjects also participated in a triple dictator game. This task is designed to closely mirror the trust game and differs only in that Receivers do not have the option to send anything back. Senders were endowed with 2,000 UGX and decided how much to transfer to the (passive) Receiver. Upon deciding how much to allocate, the task is over. Thus, the Sender's earnings were $2000 - S$, while the Receiver's earnings were $3S$. Since the interaction is anonymous and the Receiver is passive in this task, purely selfish individuals would be expected to not transfer any money to the Receiver. However, if Senders positively value the welfare of others, care about overall efficiency or dislike inequality, they may transfer positive amounts.

In order to study differential treatment of former soldiers relative to their peers, we implemented three treatment conditions in which we varied information on the length of time one's partner spent with LRA that was given to Senders. Prior to making choices, the experimenter verbally provided each Sender with a profile including several pieces of information about the Receiver. We varied information on the Receiver's experiences during the conflict, implemented across subjects. In the LRA long condition, the Sender was told that the Receiver had been with the LRA for around a year, in the LRA short condition s/he was told that Receiver had been with LRA for around one month. There was no reference to LRA abduction in the control condition.¹³

There are several noteworthy features of the information we provided subjects. First, in addition to information related to the Receiver's abduction status, we

¹³In the control treatment we did not explicitly specify that Receivers had not been abducted. Thus, some Senders assigned to the control condition could have assumed that their partner had been abducted by the LRA, which would lower our estimates of differences in behavior across treatment conditions.

included several additional pieces of information in the Receiver profile, in order to make relevant information about LRA experience appear more natural and to mask the fact that this information was of primary interest. Specifically, Senders were told that the Receiver was between 18-35 years old, male, that he lived in a different village but in the same sub-county, whether he was married or single, and also that he had spent time in a camp for internally displaced persons (IDPs) during the conflict. Since 90% of people in the area we study spent time in IDP camps, this information should not convey anything meaningful about the anonymous partner (Pham et al., 2007). However, we included former IDP status in all treatments to avoid a potential confound that could arise if subjects in the LRA treatments were reminded of the conflict and those in the control treatment were not.¹⁴ Second, we matched Senders with Receivers so that they possessed the characteristics reported in these profiles, to avoid deception. Third, in order to verify that Senders were able to accurately recall information about their partner's conflict history, we included questions on the characteristics of their partner in the experiment at the end of the survey module (which took place after the experimental choices). Fourth, Senders were informed that Receivers would also receive a short profile of their characteristics, including: their gender, that they were between 35-55 years old and that they lived in the same sub-county but in a different village.

Since we used a within subject design in eliciting choices in the trust and dictator games, we varied the order in which Senders completed the two tasks. In estimations, we control for the order effects on Senders' choices and beliefs. Also, since the decision to trust is a risky decision, we elicited Senders' attitudes towards risk, which we use as a control variable in our estimates.¹⁵ Specifically,

¹⁴Specific wording was as follows: "Your partner is a man. He's between 18 and 34 years old. He's married/not married. During the conflict he was in an IDP camp [and was abducted by the LRA for around one month/one year]. After this he returned to his village where he lives now. This is in this sub-county but a different village than this one."

¹⁵For a similar approach to controlling for the attitudes to risk in trust decisions, see, for

Senders were given the choice between a lottery that paid 1,000 UGX with a 50% probability and nothing with a 50% probability, or to accept a fixed amount with certainty, which varied from 300, 400 and 500 UGX. One of the three choices was chosen at random for payment. The more an individual prefers the lotteries to the fixed amounts with certainty, the less risk averse she is.

Receivers

In the trust game, Receivers were asked to choose how much to return to the Sender. We used the strategy method, in which Receivers made two decisions, contingent on the two positive amounts they might receive: 3,000 UGX and 6,000 UGX (i.e. if the Sender had transferred 1,000 UGX and 2,000 UGX, respectively).¹⁶ It was explained that they would only be paid for the choice that corresponded to the actual amount sent by their partner. The existing literature considers three distinct types of social preferences, which can motivate people to be trustworthy even in one-shot interactions: reciprocity¹⁷ (Buchan and Croson, 1999; Fehr and Gächter, 2000), unconditional altruism (Andreoni and Miller, 2002) and inequality aversion (Fehr and Schmidt, 1999; Bolton and Ockenfels, 2000).

In addition to indicating how much they chose to send back, we also elicited beliefs about how much they expected to actually receive from Senders. Accurate responses were incentivized with 500 UGX. In the dictator game, Receivers were passive and did not make any choice. However, as in the trust game, we elicited beliefs about how much they expected the Sender to transfer.

example, Ashraf et al. (2006).

¹⁶The evidence shows no difference between the strategy method and the design, in which Receiver responds to the observed Sender's transfer (Brandts and Charness 2000). The advantage of strategy method is the increased number of observations.

¹⁷Reciprocity is defined as rewarding kind acts with kind acts and retaliating against hostile acts with hostile acts, and thus behavior is conditional on behavior or intentions of one's counterpart. For formalization see, e.g., Charness and Rabin (2002) or Falk and Fischbacher (2006).

Prior to making choices, Receivers were informed about a set of characteristics of the Sender with whom they were matched, as described above. We purposefully did not manipulate the Senders' profile. Receivers were also informed about which of their characteristics were reported to Senders. Thus, ex-soldiers knew that Senders knew that they had been with the LRA in the LRA treatments.¹⁸

3.3 Survey Data

A large part of the survey instrument was the same for Senders and Receivers, and measured a set of the individual and household-level characteristics such as age, sibling composition, parental education, marital status, education, literacy, income, wealth, and a set of questions on exposure to violence. For Senders, we included a specific module on abduction experiences of their family members, in particular their children. Surveys for Receivers included additional questions on exposure to violence, abduction by the LRA, individual community involvement and experience of hostilities. The wording of many questions in the survey instrument was modeled after questions included in the Survey of War Affected Youth (SWAY), in which economists and psychologists specifically tested how to ask sensitive questions about abduction-related experiences in a non-intrusive way (Annan et al., 2006). Key variables are described in Table 1.

3.4 Procedure and payments

In each session, participants first received preliminary verbal instructions as a group. Since many of the participants had little or no education, we adapted the explanation from the written experimental protocol developed by Barr (2003) and Henrich et al. (2006) for the specific purpose of conducting experiments in small scale societies. We also extensively used visual aids, to illustrate options

¹⁸Former soldiers were not, however, informed that the Sender had any information regarding the length of their soldiering, simply that they had been abducted.

and payoffs of Senders and Receivers, and sequence of choices (see Figure A2 for the set-up of the group explanation). All instructions were delivered in the local language (Acholi).¹⁹ During the group-explanation stage, subjects were not yet aware of any specific information about their partner, so that all participants received a similar view of the tasks.

After the group explanation, subjects were called individually, in a random order, to make decisions in a separate space. The task was then explained again to them individually in greater detail and participants were invited to ask questions. Subjects were read the profile of the player with whom they were matched. Before making choices, participants were asked a series of comprehension questions about payoff consequences of their actions as well as those of the other player. Those who failed to understand the instructions are excluded from the analysis (2% of Senders and 0.3% of Receivers). Immediately after they had made a decision, we elicited expectations about transfers of the other player they were matched with. Appendix B presents the complete instructions to the group and the individual game instructions.

In each village, we ran two experimental sessions—first with Senders and later during the same day with Receivers. The registration and group explanation for Receivers started before the end of the Sender’s session, and this overlap minimized the chance of communication between participants in each session.

We took several steps to increase the level of anonymity when making choices. Although subjects knew some information about their experimental partners, they could not identify with whom they were matched. Specifically, in order to minimize the role of strategic considerations due to potential impact of future (outside the lab) interactions, Senders knew they were not matched with Receivers from the same village, but with Receivers from a different (unknown)

¹⁹The script was translated into Acholi from the original English, then back-translated to English by a separate translator to check for consistency.

village from the same sub-county (and vice versa). Next, subjects made decisions behind cardboard dividers to keep their choices private from the experimenter who provided the one-on-one explanation. Further, decisions were tallied by a second person who did not know whose ID number corresponded to whom. Payouts were made in private, by a third person who distributed sealed envelopes with rewards from the experiment based on ID numbers. This procedure, explained to subjects prior to their choices, was effective in keeping decisions and payoffs anonymous, although subjects' perceptions of anonymity required them to trust the experimenters to keep decisions and identification information separate.

Subjects did not learn any results from trust game or dictator game during the experimental sessions. They were told that we would return in two days to make payments, during which time we would match their responses with participants from a different village. They were also informed that their partners would also be paid two days after making decisions. In practice, groups of Senders and Receivers were matched to form pairs for which actual individual characteristics correspond to characteristics reported in the partner profiles. Then, from the pool of potential pairs with corresponding characteristics we randomly matched Receivers to Senders. Subjects were paid according to either trust game or dictator game, based on flipping a coin in front of the village leader. The payment was made in private, one by one, at the same location as the experimental sessions. When collecting payments, subjects were told which task was chosen for payment and given money in closed envelopes. On average, Senders' total earnings were 4,012 UGX and Receivers' earnings were 5,832, including the show up fee (2,000 UGX).

4 Behavior and Beliefs of Former Soldiers

In this section we present results for Receivers and analyze differences in cooperative behavior of former child soldiers in comparison to their peers. Specifically, we focus on differences in trustworthiness and in expectations of trust and altruistic behavior from other villagers.

All of the results that follow are tested using the following regression model:

$$D_i = \alpha + \beta A_i + \gamma X_i + \epsilon_i \quad (1)$$

where D_i is individual i 's action in the experiments, A_i is length of abduction, X_i is a range of individual characteristics,²⁰ and ϵ_i is the error term. Standard errors are clustered at the village level.

4.1 Trustworthiness

We measure trustworthiness as the percentage returned in the trust game. Since we used the strategy method, participants made two conditional choices, deciding how much to return both in case a Sender transferred 1,000 UGX (and the Receiver would get 3,000 UGX) and when a Sender would transfer 2,000 UGX (and the Receiver would get 6,000 UGX). The percentage sent back by Receivers is very similar in both cases: 34% and 35% on average, respectively. In the main analysis we use the average of these two amounts. Given that the amount received is tripled, this number implies that participants returned a slightly higher amount than what was sent. The level of trustworthiness that we observe in our sample is similar to that observed in comparable studies.²¹

²⁰In the main estimations we control for both the pre-existing characteristics (e.g., parental education) as well as the characteristics, which may have been affected by soldiering (e.g., literacy or income). Note that the main results are not affected by using different sets of controls (available upon request).

²¹A recent survey by Johnson and Mislin (2011) finds that the average proportion sent back in trust game was 38% in Europe, 34% in North America, and 32% in Africa.

Does the cooperative behavior of abductees differ from their peers? In Column 1 of Table 2 we find a strong positive relationship between length of abduction by the LRA and the amount returned in the trust game (p -value <0.001). This link is robust to controlling for a large set of observable characteristics: age, marital status, sibling composition, parental education, wealth, household size, literacy, schooling and gender of the recipient. Greater wealth, number of household members, and literacy are also positively associated with returning more.

We next consider whether the link between abduction and trustworthiness is driven by all former soldiers, independently of the intensity of their experience, or whether the link is dose-dependent. To test for non-linearity in the relationship, we distinguish between four groups, based on abduction length: participants who were not abducted at all, those who were abducted for a relatively short period (up to 2 months), those who were abducted for a medium period (between 2 months and 1 year), and those who were with LRA for a long period of time (more than a year). As reported in Column 2, we find that the relationship is, in fact, dose-dependent: those who were abducted for a relatively short period return more compared to those who were not abducted, but the difference is not significant statistically (p -value=0.18). The coefficient is very similar for the medium-length group, and the difference, compared to the non-abducted group, further increases for those who spent the longest period with LRA, and it is significant at the 5% level. The difference is also economically significant: those who were with LRA for more than one year transfer back 7.7 percentage points more compared to the non-abducted group, which represents an increase of around 420 UGX (approximately twice the average daily cash income in our sample) in case 2,000 UGX is sent.

Observation 1: *The cooperative behavior of former soldiers differs from their peers. The longer a person was with the LRA, the higher is the amount sent back in the trust game.*

Next, we test whether the effect of soldiering on trustworthiness is more pronounced when experienced during an early age, compared to soldiering during late adolescence and adulthood. We exploit the fact that the age of abduction ranges in our sample from 6 to 30 (the median age of first abduction is 14). We find that age of abduction matters: the effect of abduction is strong for those who were abducted when younger than 14 years of age and mute for those abducted at later age. Column 3 of Table 2 demonstrates this by including an interaction between an indicator of first abduction at 14 years age or older and the total length of abduction. The coefficient for years of abduction, which shows the link with trustworthiness for those who were abducted as children (less than 14 years), is positive and larger than in the baseline regression. At the same time, we find a negative interaction effect between length of abduction and being abducted later than at 14 years of age. The two coefficients are the same size, indicating that the effect of time spent with the LRA on trustworthiness is specific for former soldiers who were abducted during childhood (younger than 14) and that there is no such link for those abducted during late adolescence and adulthood.

Furthermore, in Column 1 of Table A1 we regress the indicator of abduction after the age of 14 on the amount sent in the trust game for the sub-sample of abducted individuals, while controlling for indicator variables for the year of abduction (to control for variation in abduction patterns and experiences over time), in addition to other observable characteristics. The results reveal a marginally significant impact of abduction during early age compared to being abducted during adolescence and adulthood on higher trustworthiness (p-value=0.13). Note that the observed effect of abduction age on more cooperative behavior is unlikely to be driven by higher exposure to war-related violence among the younger group

compared to the older group, since we find little difference in specific war-related experiences across the two groups (Columns 2-8, Table A1).

***Observation 2:** The positive link between length of abduction and higher trustworthiness is driven by former soldiers who were abducted at an early age (younger than 14 years of age). It is not present for those who were abducted during late adolescence and adulthood (14 years and older).*

As noted, abduction by the LRA captures a host of war-related experiences. To understand which mechanisms could potentially drive the link between abduction and elevated cooperative behavior in the trust game, we study relationships between specific war-related experiences and cooperative behavior. To measure different types of exposure to violence on behavior, we created four indices based on survey data. The index of violence committed is a sum of positive responses to two questions: whether the individual was forced to do violent things to opposing soldiers in battle and whether he had been forced to do violent things to civilians. The index of violence received is a sum of positive responses to five questions related to violence received during the conflict (irrespective of the perpetrator): having property destroyed, experiencing bullets shot on self or own home, experiencing attack, being tied up, and being seriously injured. The index of violence witnessed is composed of four questions: whether the individual witnessed beatings, killing, rape and an LRA attack. Finally, the index of violence received by family members consists of two questions: whether any family member died violently and whether any family member disappeared during the conflict. We also consider an indicator variable of passing through a reception center for returning abductees, indicating exposure to formal reintegration programs, and an index of participation in informal cleansing ceremonies.²²

²²The index of participation in informal cleansing ceremonies is the sum of two indicator variables: whether subject “stepped on egg” and whether he took part in the Mato Oput ceremony. Stepping on egg refers to a traditional ceremony to welcome back people who have been gone for a long period of time. Mato Oput is a ceremony for creating peace among people who aggrieved another party, which has been adapted as a ceremony for forgiving and accepting

We find that none of these abduction-related experiences can be singled out as an explanation for the observed effect of abduction on trustworthiness. In Column 4 of Table 2 we show that individuals who reported committing more acts of violence returned more in the trust game, but the relationship is not statistically significant. Coefficients for violence witnessed and violence received are negative and not statistically significant, while the index of violence against family members is just slightly higher than zero. Coefficients for the index of participation in informal cleansing ceremonies and passing through a reception center are both positive, but also statistically insignificant.

In order to understand in greater detail how selfish and cooperative preferences change with abduction length, we compare the distributions of amounts sent back. We start by looking at the extensive margin, i.e. the prevalence of purely selfish individuals who did not send anything in either of the two choices. In Column 1 of Table A2 we find that the length of abduction somewhat increases the prevalence of returning a positive amount, and the effect is significant at 10% level. Further, the results in Column 2 demonstrate that increased trustworthiness with length of abduction is also driven by the intensive margin. After excluding Receivers who did not return anything in either of the two choices, we see the effect of abduction length continues to predict a higher percentage returned in the trust game.

We next consider which type of social preferences—inequality aversion, reciprocity or altruism—motivates the greater levels of trustworthiness we observe among former soldiers. We first identify individuals who preferred allocations leading to equal payoffs for themselves and Senders. Specifically, when Receivers were faced with the decision of how to allocate 6,000 UGX, they could achieve an equal distribution by sending back half of the amount (by sending 2,000 Senders did not keep any of their endowment). In Column 3 of Table A2 we find no link

abductees after they returned from the LRA (for more details see Allen (2010)).

between length of soldiering and prevalence of choosing the equal split. Similarly, when making decisions about how to allocate 3,000 UGX, a Receiver could achieve an equal distribution by sending back 1,000 UGX and keeping 2,000 UGX for himself (after sending 1,000 UGX Senders were left with 1,000 UGX). We find that length of abduction is not related to sending exactly 1,000 UGX in this decision (Column 4), but rather with increased likelihood of sending more than 1,000 UGX. Thus, we conclude that the increased trustworthiness is not due to greater adherence to norms of equality or a greater inequality aversion.

In order to determine whether differences in trustworthiness between ex-abductees and their peers are due to differences in reciprocity or unconditional altruism, we study whether the increase in the proportion sent back is related to behavior of the Sender, in particular, the amount transferred. Note that on average Receivers expected to receive 1,380 UGX (as discussed in greater detail in the next sub-section) and thus, it is likely that Receivers considered receiving 2,000 UGX a kind act from Senders, while receiving 1,000 UGX was considered a neutral (or perhaps slightly unkind) act. Therefore, if the greater amount returned by abductees is due to a higher degree of reciprocity, we should observe a greater difference in the proportion sent back when a Sender sends 2,000 UGX compared to when she sends 1,000 UGX. In Columns 5-6 we find that the link between abduction length and the proportion sent back is positive for both potential amounts that could have been sent. Although the size of the correlation between abduction and the proportion sent back is somewhat higher when 2,000 is sent than it is when 1,000 is sent, the difference in the proportion returned across the two possible amounts that could have been sent does not increase with abduction length (Column 7). Together, these results suggest that the observed differences in trustworthiness for abductees compared to their peers are driven by greater intensity of unconditional altruism and not by greater inequality aversion or reciprocity.

4.2 Expectation of Trust and Altruism

To measure expectations of trust and altruism of older community members towards participants, we elicited beliefs from each Receiver about the amount they expect to receive from the Sender in both the trust game and dictator game. On average, out of a possible 2,000 UGX, Receivers expect to receive 1,377 UGX in the trust game and 1,233 in the dictator game.

Do former soldiers expect to be less trusted than their peers? In Column 1 of Table 3 we find virtually no link between abduction length and the amount that was expected to be received in trust game, indicating little expected discrimination in terms of trust. We arrive at a similar conclusion when using a specification that tests for a non-linear relationship between abduction length and expected trust (Column 2). Note that Receivers were informed that their profile, which included whether they had been with LRA, had been provided to Senders prior to Senders' decisions, and thus a difference in expectations of trust could arise if abductees expect others to differentiate between abductees and their peers, or if abductees have different beliefs about behavior of others in general.

Do former soldiers expect others to be less kind to them compared to their non-abducted peers? In Column 3 we find a slight negative relationship between length of abduction and expected altruism of others, but the relationship is not statistically significant (p-value=0.12). In Column 4 we observe that it is driven by individuals who spent more than a year with LRA, but even for this group the relationship is not statistically significant (p-value=0.28).²³

Observation 3: *We do not find systematic evidence that former child soldiers expect different treatment by other people in their communities. That is, former child soldiers do not differ significantly in terms of the amount that they expect to receive in the trust game or in the dictator game.*

²³There is also no relationship between length of abduction and expected distribution of trust and altruism (results available upon request).

This (non) result is interesting in light of the purposeful effort of LRA to manipulate beliefs about how abductees would be treated by their communities. As a way to lower incentives for abductees to escape, the LRA often forced abducted soldiers to commit violent acts against their own communities and tried to convince them that upon return they would face hatred and rejection. Our results indicate that such psychological manipulation by the LRA has not left an enduring mark on the beliefs of abductees about the behavior of others. We arrive at similar conclusions when looking at the link between index of committed violence (rather than length of abduction) and expected trust and altruism (results available upon request). Alternatively, those who believe that members of their home communities would have negative attitudes towards them may have been less likely to return in the first place.

4.3 Robustness Checks

We now report a series of robustness analyses. First, since former soldiers were informed that Senders knew that they had been abducted—this was to mimic the real life interactions in the relatively small villages—we do not observe the behavior of ex-soldiers when they are not identified as such to their partner. Thus, it is possible that by identifying ex-LRA members in the experiment, we have made membership in this group more salient, and the increase of trustworthiness is a response to being identified as an ex-soldier rather than a more fundamental increase in social preferences. To address this question, we study the behavior of 28 subjects who reported being abducted by the LRA but were (due to mismatched reports of other household members during the pre-survey) assigned to the control treatment and thus their profile, which was revealed to the Senders as well to them, did not include information about their LRA history. We com-

pare these subjects with those who reported being abducted and who also knew that their abduction status was revealed to Senders. We find that the first group returns even more than the second (40% and 34%, respectively), indicating that differences in experience and not identification as abductees in our experiment drive the differences in cooperative behavior.

Second, we explore whether differences in experimental choices mimic systematic differences in behavior in the naturally occurring world. In Table A3 we replace our experimental variable with survey-based proxies for cooperative behavior. In Column 1 the dependent variable is an index based on responses about participation in local groups (such as farmers' cooperatives and community committees). We find a positive correlation between length of abduction and participation in local groups, consistent with our experimental results. In addition, in Column 2 the dependent variable is the likelihood of having a physical fight, and we find negative correlation with length of abduction.

Next, our measure of soldiering is based on self-reported information. Although former soldiers were willing to talk about their experiences, and the issue is not taboo in Northern Uganda, there is legitimate concern about systematic biases in truthful reporting. To assess the issue quantitatively, we use the information about abduction status gathered as a part of the household-level pre-survey (which served as a way to oversample former soldiers). First, we compare reports of abduction status as reported by (1) the participant and (2) another household member. The results reveal a strong correlation (abduction status: $r=0.82$, $p<0.0001$) — out of the total of 337 participants, only 6 individuals were identified by family members as ex-LRA soldiers but did not report being former LRA members during the post-experiment survey and 26 individuals were not identified as former child soldiers in the household survey, but did report being abducted by the LRA during the survey.²⁴ Second, as a robustness check,

²⁴A likely explanation is that some respondents to the household pre-survey may have recently

we perform the main estimations but exclude all participants, whose self-reports of abduction status did not match the reports of the other household members. Results are robust (Table A4).

4.4 Alternative explanations

In this sub-section we discuss alternative mechanisms which could explain the observed heightened cooperative preferences of former soldiers compared to their peers. The evidence documented in this section is consistent with the idea that preferences are malleable during early age and respond to extreme experiences. However, the nature of our data does not permit us to rule out alternative explanations of the observed correlation, in particular different types of selection. While the conflict in Northern Uganda presents a unique opportunity to study the effects of child soldiering without the conscious self-selection in the recruitment stage that is at work in many other civil wars (Blattman and Annan, 2010), there are still several ways in which personal characteristics, including trustworthiness, could have influenced the length of time spent with the LRA, surviving the conflict, returning and staying home, and finally deciding whether to participate in our study. We now consider which of these alternative selection mechanisms could explain the full set of findings.

The LRA forcibly conscripted soldiers by raiding rural villages and it is very unlikely that this practice would allow LRA soldiers to assess the character of potential recruits given the short interval between the attack and abduction, and therefore unlikely that ex-soldiers were directly selected for their level of trustworthiness at the outset. Nevertheless, since children were typically abducted during raids on homesteads in which materials were looted, household characteristics could have affected which children were targeted and these may be correlated with trustworthiness. In Table A5 we examine the effect of pre-abduction

married into the household and may not be aware of LRA history of each member.

household characteristics, such as mother's education, father's education, sibling composition, birth order and participant's age, on the length of abduction by the LRA as well as on age of first abduction and generally find little effect. Only birth order is negatively correlated with abduction length (the relationship is marginally significant) and participant's age positively correlates with age of first abduction.

Next, personal characteristics, including trustworthiness, might affect treatment by LRA commanders after forcible recruitment. The qualitative evidence seems to suggest that LRA commanders allocated duties, roles in combat, resources and punishment based, at least in part, on the logic of a principal-agent relationship (Beber and Blattman, 2013). Less trustworthy individuals may have been more severely punished or given more dangerous assignments, which could result in underrepresentation in our sample (Annan et al. (2006) estimated that 20% of ex-abductees did not return and can be presumed dead). While such deliberate selection of LRA commanders could explain the link between trustworthiness and abduction, it is harder to explain why the relationship is specific for individuals who were abducted during childhood. In particular, testimony from rebel leaders suggests that one of the reasons that the rebel group targeted youth, despite their lower physical strength, is because children are more malleable and obedient and hence easier to control (Beber and Blattman, 2013). Thus, one would expect that LRA commanders would be more selective when assigning roles and resources to older soldiers compared to younger ones, which would imply that the link between length of abduction and trustworthiness should be stronger among those who were abducted at later age, which is the opposite of what we find. Nevertheless, we cannot rule out that some kind of selection of LRA commanders based on abductees' trustworthiness drives our results.

Another concern is that less trustworthy ex-soldiers were less likely to return and be accepted by the home communities after they returned from the bush

and thus may have been forced to migrate to cities or villages outside of the regions we study. However, Annan et al. (2006), estimate that around 95% of ex-abductees stayed after returning to their home communities, which suggests that migration was quite rare. Also, it is not clear why selection, in terms of who was accepted back, that was based on individual's cooperative behavior (or some characteristic that would correlate with it) would be specific for youth who were abducted young.

The final potential selection issue that we consider is the sampling procedure. If former child soldiers expected higher psychological costs of answering questions relative to their peers (note however that we did not indicate the research would focus on LRA-related experiences), their level of altruism may have played a larger role in their decision of whether to accept our invitation compared to their peers, leading to under-representation of non-altruistic ex-abductees in our sample. However, such selection is unlikely because the response rate was generally very high and, in fact, was even higher among former soldiers compared to their peers (91% and 87%, respectively).

Alternatively, former child soldiers may behave more prosocially towards others in order to expiate guilt that they feel for acts which they have committed while with the LRA. Our data do not provide strong support for this hypothesis. We find only a relatively weak positive correlation between the amount sent in the trust game and the commission of violent acts against civilians when being with LRA, arguably the type of act which former soldiers may regret most. Also, 3% (18%) of former soldiers report that they were ever blamed by their family (by other people in their community) "for things they have done in the bush" and such experiences are likely to increase feeling of guilt. Nevertheless, we find no correlation between being blamed and the amount transferred (results available upon request).

This brings us to the possibility that preferences are malleable, especially

during childhood, and that soldiering during this sensitive period affects preferences. Given the survival threats and pressure for group cooperation when with the LRA, preferences of former child soldiers may have adapted to such an extreme environment. Such adaptation may have evolutionary underpinnings (in the spirit of the theory developed by Choi and Bowles (2007) or it may be an outcome of learning. Since cooperative preferences seem to be—like many other aspects of human psychology—disproportionately calibrated and set during childhood (Henrich, 2008), such a change in preferences may have long-term effects and persist into adulthood.

5 Behavior of Receiving Communities Towards Former Soldiers

In this section we explore whether Senders behave differently towards former soldiers and, if so, whether this is due to social preferences or beliefs about trustworthiness. The average age of the Senders is 43 years and 56% are female. We find no statistically significant differences in observable characteristics (gender, age, wealth, income, household size, likelihood of having son/daughter abducted, three indices of violence exposure) across the experimental manipulation of information about abduction status of the Receiver (no abduction, around one month, more than one year; see Table A6). Therefore, we are confident that any differences in Senders' behavior and beliefs in the experiments across these conditions can be attributed to differences in knowledge about length of Receiver's abduction.

5.1 Trust

On average, Senders transfer 1,120 UGX of their endowment of 2,000 UGX (56%) to the Receivers in the trust game.²⁵ As we used the strategy method in eliciting

²⁵This amount is close to average proportion found in other studies, which is around 50% of the endowment (Camerer, 2003; Johnson and Mislin, 2011).

beliefs about how much trustees would return (when 1,000 is sent and when 2,000 is sent), we have two data points about expected trustworthiness. The mean expected return on investment is 82% and the Senders expect a slightly higher return on investment when sending 2,000 UGX compared to sending 1,000 UGX. Thus, the Senders have inaccurately optimistic expectations, since the actual return on investment, based on the actual behavior of Receivers, is only 5.6%.²⁶

Do Senders differentiate trust based on how long Receivers spent with the LRA? In Table 4 the dependent variable is the amount sent to Receivers in the trust game. The exogenous explanatory variables of interest are two indicator variables for being informed that one's partner was with the LRA for around one month and that partner was with the LRA for more than a year. The control category (No LRA) is omitted and we control for Sender's observable characteristics (age, being female, attitude to risk, wealth, income, household size and an index of conflict exposure). On average, we find a positive but statistically insignificant effect of the LRA treatments, both in terms of means (Column 1 of Table 4) as well as distribution of choices (results available upon request).

Assuming that Senders are aware of behavioral differences and that they are, at least in part, motivated by self-interest, one would expect to see more trusting behavior in the two LRA treatments to reflect the higher proportion returned by ex-soldiers. Although we study relatively small villages, in which people generally know who was with the LRA and who was not, some Senders may not interact with former abductees on a regular basis. Since a lower social distance between Senders and ex-soldiers may improve the quality of information that they have, we examine one personal characteristic that is likely to increase accuracy of beliefs: whether Senders have at least one son who was abducted by the LRA during the

²⁶Such overly optimistic expectations of trustworthiness seem to be a common finding for high levels of trust—in all three countries they study, US, Russia and South Africa, Ashraf et al. (2006) find that Senders expect to receive around 50% return when sending the full endowment, while in practice they receive around zero return.

conflict (N=82).

Figure 1 and Columns 2-4 of Table 4 reveal a sharp difference in the effects of the LRA treatments on the sub-sample of those who have a formerly abducted son and those who do not. For the sub-sample of participants with no abductee sons, there is no significant difference between trust allocations in the three LRA treatments. In contrast, those who do have sons that were abducted send more when playing with an ex-soldier in both the LRA 1 month and LRA 1 year treatments. Compared to the control group, they sent 360 UGX (p-value=0.20) more to the LRA 1 month group and 530 UGX more to the LRA 1 year group (p-value=0.02). Put differently, while we find no difference in trust towards the non-abducted (control) group between those who had a son abducted and those who did not, we find positive interaction effects between having an abducted son and the LRA treatments on trust.²⁷

***Observation 4:** We do not find mistrust in former soldiers. Moreover, while we find no difference in the amount sent to former soldiers and to their peers among subjects who do not have a former soldier in their family, we find more trust in former child soldiers compared to peers among subjects who have an abducted son.*

In theory, the amount sent in the trust game reflects beliefs about trustworthiness combined with social preferences towards the Receiver (Ashraf et al., 2006; Fehr, 2009; Sapienza et al., 2013). In line with this intuition, we find that the amount sent in the trust game is positively related to the amount sent in the dictator game (p-value=0.00), which measures altruism, as well as to the amount expected to be transferred back by Receivers (p-value=0.02). Therefore, among those with an abducted son the effect of LRA treatment on higher trust may be due to more accurate beliefs about differences in trustworthiness, making it a profit-maximizing strategy to send more to ex-abductees, but it may also be

²⁷In the main estimations we use OLS. The results are robust to using alternative estimators, such as ordered probit, which takes into account the discrete nature of the dependent variable (Table A7).

driven by greater altruism towards ex-abductees, perhaps as a result of greater empathy or other positive emotions. Similarly, the failure to find an effect of the LRA treatments on trusting behavior among those who do not have an abducted son does not necessarily imply lack of taste-based discrimination or lack of differential treatment based on beliefs about behavior. These two motives could cancel each other out, if, for example, community members without abducted sons harbor anger towards former child-soldiers, motivating less favorable treatment compared to their peers, but at the same time are aware of ex-abductees' greater trustworthiness, which would motivate them to be more trusting. In the following sub-sections we aim to separate the role of belief-based and preference-based components of trust.

5.2 Altruism

In order to measure taste for discrimination against or favoritism towards former soldiers, the participants made choices in the dictator game as well. In this task Senders again allocated an amount of money between himself and a Receiver, but, in contrast to the trust game, Receivers are passive and do not have any strategic role. Thus, beliefs about expected behavior should not affect the decision of how much to transfer. If we found that the amounts transferred are higher in the LRA treatments compared to the control treatment, this would be a strong indication of favoritism of ex-soldiers, while lower amounts would indicate taste-based discrimination. Following Fershtman and Gneezy (2001) and Cox (2004) we made choices in the dictator game comparable with choices in the trust game, by tripling the amount transferred from Sender to Receiver.

On average, Senders transferred 860 UGX (43%) out of their 2000 UGX endowment, on average. We find no effect of LRA treatments on the mean amount sent in the dictator game (Column 5 Table 4). We also find little differences in terms of distribution of the amount sent (results available upon request). Further,

Figure 2 compares the mean amount sent across LRA treatments, separately for the participants with and without an abducted son. We observe virtually no effect of LRA treatments in either of these two groups. This is confirmed by the regression analysis in Column 6, where we find no interaction effect between having an abducted son and LRA treatments.

The finding of no statistically significant effect of LRA treatments on dictator game allocations is unlikely to be due to a low sample size. Given our sample size and the variation in dictator allocations, we have the statistical power to detect a treatment effect of 183 UGX at (9.2 percentage points) at the 5% level. This is equivalent to 0.25 standard deviations in our sample.²⁸ We thus conclude that there is no evidence of differences in kindness towards ex-soldiers and non-soldiers.

***Observation 5:** The results of the dictator game indicate that villagers do not differentiate their altruistic behavior based on whether they interact with former soldiers or their peers. Thus, we do not find evidence either for negative attitudes or for favoritism of former soldiers.*

5.3 Expectations of trustworthiness

In order to understand possible differences in expectations of trustworthiness, we use two different measures. First, we directly examine beliefs about how much Senders expect Receivers to transfer back. The variable of interest is the mean of the percentage expected by the Sender for both possible amounts she could have sent: 1,000 UGX and 2,000 UGX. Second, since the Sender may send money for purely altruistic reasons in the trust game, in addition to the expectation of return, we exploit the within subject design of our experiments and identify pure behavioral trust (i.e. the part of the transferred amount motivated by expected return) by taking the difference between what the Sender transferred in the trust game and what was voluntarily given in the triple dictator game,

²⁸Calculated using a power of 0.80 and a significance level of 0.05.

using an approach proposed in Cox (2004).²⁹ This difference can be thought of as the "investment portion" of the trust game allocation, or the strategic element of trusting behavior (Fehr, 2009).

On average, we find positive, but small and statistically insignificant effects of the LRA treatments on the expectation of trustworthiness. We obtain similar results both when analyzing the "investment portion" of the amount sent in the trust game (Table 5, Column 1) as well as the percent expected to be transferred back (Column 4).

Importantly, we do find a strong interaction effect between LRA treatments and having had a son abducted. For participants who have an abducted son, the difference in the amount sent in the trust game and in the dictator game increases by UGX 360 in the LRA 1 month treatment and by UGX 750 in the LRA 1 year treatment (Column 3). The magnitude of this increase is also economically significant (around 37% of Senders' average weekly cash income). In contrast, there is virtually no effect of LRA treatments in the sub-sample that do not have an abducted son (Column 2). The difference in the effects of the LRA treatments across the two sub-samples is statistically significant.

We observe a qualitatively similar pattern when analyzing expectations about the amount sent back. Among the sub-sample of Senders with a son who was abducted, expectations are higher when Senders are matched with an ex-abductee, though this is only (marginally) statistically significant for the longer LRA treatment (Column 6). Among those Senders with no ex-abductee sons, there was virtually no difference in expectations of how much Receivers would return across treatments (Column 5). These results suggest that having an ex-abductee son improves knowledge about their trustworthiness and how it differs from their peers.

²⁹This approach implies that 77% of the amount sent in the trust game is due to altruistic preferences and 23% is motivated by pure trust, i.e. expected return from Receivers. However, these numbers should be interpreted cautiously; see for example Fehr (2008) for why taking the difference in the amount sent in trust game and dictator game may understate the magnitude of behavioral trust.

Observation 6: *Participants who have an abducted son seem to be aware of the greater trustworthiness of former LRA soldiers compared to their peers and act based on this belief. We find that they expect that former soldiers will transfer a greater amount back. We also find that a greater portion of the amount transferred by these participants in trust game can be attributed to expected return rather than altruism. In contrast, we find no differences in expectations of trustworthiness or in trusting behavior among Senders with no ex-abductee sons.*

5.4 Robustness Checks and Further results

Our results for Senders strongly suggest that participants with abducted sons have more accurate knowledge of behavioral differences associated with soldiering compared to individuals without abducted sons. Nevertheless, in addition to better knowledge, it could be argued that having an ex-abductee son may correlate with other war-related experiences, and such shared experience of violence may drive differential treatment of ex-abductees. To test for this possibility, in Table A8 we study the interaction effects between different measures of exposure to violence and LRA treatments on the amount sent in the trust game. While we find a strong interaction effect of having a son abducted and LRA treatments (Column 1), as discussed above, we do not find similar interaction effects for other measures of LRA-related violence exposure: violence received, violence against family, violence witnessed or having a daughter abducted by the LRA (Columns 2-5). Further, the interaction effect of having an abductee son and the LRA treatments is robust to controlling for observable characteristics, measures of violence exposure, and the interaction terms of these variables with LRA treatments (Column 6).³⁰ These robustness checks indicate that the difference in the impact of LRA treatments among those with abductee sons is not due to differences in other types of war-related experiences or differences in observable characteristics.

³⁰Table A9 reports an analysis of which personal characteristics predict whether Senders have a formerly abducted son. Older respondents and females were more associated with a higher likelihood.

Also, we consider the possibility that the salience of Receiver's LRA history was greater for individuals with abductee sons, which, potentially, could explain the observed interaction effect on trust. Salience of this information was generally high: in the LRA treatments, 75% of individuals reported that the Receiver with whom they were matched was an abductee in an open-ended question at the beginning of the survey module (approx. 30 minutes after the experiments), which asked to recall the list of the Receiver's characteristics with which s/he had been provided. This is similar to the recall rate of other characteristics, which ranges between 55-70%. Also, we find no relationship between having an abductee son and recall of the Receiver's abduction.

Attribution theory implies that taste-based discrimination is unlikely to arise if the object of discrimination (group attribute) is not an outcome of individual choice, and thus not controllable. Given this, the fact that we do not find evidence of taste-based discrimination would suggest that communities do not consider ex-soldiers responsible for violent acts committed while with LRA. In order to test whether the lack of taste-based discrimination is consistent with attribution theory, we elicited perceptions about the degree to which LRA abduction was avoidable. We sampled a new group of 72 respondents from the same population several months after the main study. Each person received two fictitious profiles of a formerly abducted person. We randomly manipulated the information about length of abduction ("around 1 month" or "around 1 year") and also whether the person in the profile had returned to his village after being with LRA or whether he decided to move to a town or city. The second manipulation, not present in the experiments, was motivated by the questions whether perceptions of controllability of abduction experiences varies for abductees who returned to their home villages or migrated. Respondents were asked two related questions: To what extent do you think this person could have avoided being abducted (completely avoidable, somewhat avoidable and not avoidable)? How likely do

you think it is that this person had the chance to escape before they actually left the LRA (very likely; somewhat likely; not very likely)?

Overall, we find that 80% of respondents thought that abduction was completely unavoidable and 70% thought that such person would had no chance to escape from the LRA before they actually left. Further, we find that length of abduction reported in the profile and whether an ex-abductee stayed in his home village or migrated has little impact on answers to these questions (available upon request). These results reveal that in the setting we study, soldiering is not seen as an outcome of individual choice, which may help to explain why we do not find taste-based discrimination against former soldiers.

6 Conclusions

The literature on the consequences of civil wars and post-conflict reconstruction highlights the importance of reintegrating former soldiers back into communities. The common view is that reintegration is complicated by the negative effect of trauma and the normative environment of rebel groups on cooperative tendencies of ex-abductees and by anger and lack of acceptance by receiving communities. However, evidence from a recent survey has raised the surprising possibility that soldiering may not necessarily undermine the social capital of ex-abductees, by showing that former soldiers are more likely to vote (Blattman, 2009). Yet this is an indirect proxy of social capital, since it may also capture political opposition, and there has been no quantitative evidence testing for discrimination against ex-abductees. We aim to extend this earlier work by (1) separately observing behavior of former soldiers as well as treatment of former soldiers by receiving communities, (2) focusing on two important aspects of interpersonal relations, namely trust and willingness to cooperate, which are difficult to observe in the naturally occurring world, (3) using incentive-compatible field experiments, in

contrast to responses to survey questions.

This paper uses data from trust and dictator experiments implemented among 715 participants in Northern Uganda. We find that the length of abduction by the Lord's Resistance Army is robustly correlated with cooperative behavior. The longer period a person was with the LRA, the more trustworthy his behavior is in the trust game. The observed increase in cooperative behavior is driven by former soldiers who were abducted during early age (<14 yrs). In terms of treatment of former soldiers by others, we find neither systematic mistrust nor preference-based discrimination against former soldiers. Further, individuals with abductee sons, and thus with better knowledge of their behavior, trust abductees more compared to their peers, and this differentiation is statistical in nature—they expect ex-abductees to be more trustworthy and act based on this belief. In terms of expectations of ex-soldiers about how they would be treated by others, we find that former soldiers expected to face similar level of trust and altruism from others as their peers.

Our results are consistent with recent theories linking war and development of cooperative preferences, in the spirit of group-selection models such as Choi and Bowles (2007). Given the need for group cooperation during inter-group fighting, one mechanism behind the link between soldiering and increased cooperative preferences is an evolved psychological reaction to war and survival threat. The behavioral change may also be due to socialization and learning—ex-abductees who stayed longer with the LRA may have painfully learned the importance of group cooperation and being trustworthy—and may have internalized such behavior. It is intriguing that our finding among former soldiers is qualitatively similar to recent evidence from behavioral experiments among the victims of war-related violence, which consistently reveals that greater exposure to violence reduces selfishness and increases cooperative behavior towards one's in-group (Voors et al., 2012; Gneezy and Fessler, 2012; Bauer et al., 2014a). Taken together, the evi-

dence suggests that exposure to war-related experiences triggers similar behavioral response in victims of violence as well as in forcibly recruited perpetrators of violence. Alternatively, the results may indicate that armed groups treat less cooperative individuals more harshly, and thus the prevalence of individuals with cooperative preferences in the population of former soldiers increases. We hope these results will motivate future research to identify the exact mechanism linking soldiering during war and changes in cooperative preferences.

The finding that the effects of soldiering are more pronounced if experienced during childhood compared to later periods of the life cycle contributes to the literature that aims to identify critical periods in formation of non-cognitive skills (Cunha et al., 2006; Heckman, 2006). The existing research has demonstrated that social preferences, as well as other preferences and skills, develop substantially during early stages of the life cycle (Harbaugh and Krause, 2000; Fehr et al., 2008). Also, the effect of exposure to war-related violence has also been found to be greater or more enduring if experienced during childhood and adolescence compared to adulthood (Bauer et al., 2014a). Thus, the evidence from post-conflict societies suggests that people acquire and internalize much of their cooperative behavior during childhood and adolescence and it is, at least in part, shaped by environmental factors.

The finding that individual LRA history, which often involved committing violence against home communities, does not invite preference-based discrimination against former soldiers speaks to the theoretical debate about which underlying factors affect preference-based discrimination. Our results are consistent with attribution theory, which implies that this type of discrimination is less likely to arise if the object of discrimination is perceived not to be a choice and therefore uncontrollable (Heider, 1958; Gneezy et al., 2012). Given our evidence showing that individual LRA history is mostly perceived as completely unavoidable by the population we study, this theory may explain why former abducted soldiers

do not face taste-based discrimination in Uganda but, it may also caution against generalizing our findings to post-conflict societies in which the participation of soldiers in violent acts is seen as an outcome of their decisions. Such an interpretation may also help to explain the survey evidence of Humphreys and Weinstein (2007) from Sierra Leone, a setting where a large percentage of youth joined rebel groups voluntarily, who find that ex-soldiers who were more exposed to violence report lower community acceptance.

A main finding of this paper is that being a forcibly recruited soldier does not necessarily lead to long-term negative effects on social capital: the experiences of child soldiering with the LRA that occurred between 1990 and 2006 had a positive impact on cooperative tendencies in 2011 and have not created persistent negative attitudes towards former abducted soldiers in their home communities. Clearly, more research needs to be done to understand the generalizability of these findings, especially the role of forcible versus voluntary recruitment. Yet this behavioral experiment provides new evidence against automatically taking pessimistic views on some of these key factors that may undermine reintegration of former soldiers and thus peaceful development of post-conflict societies.

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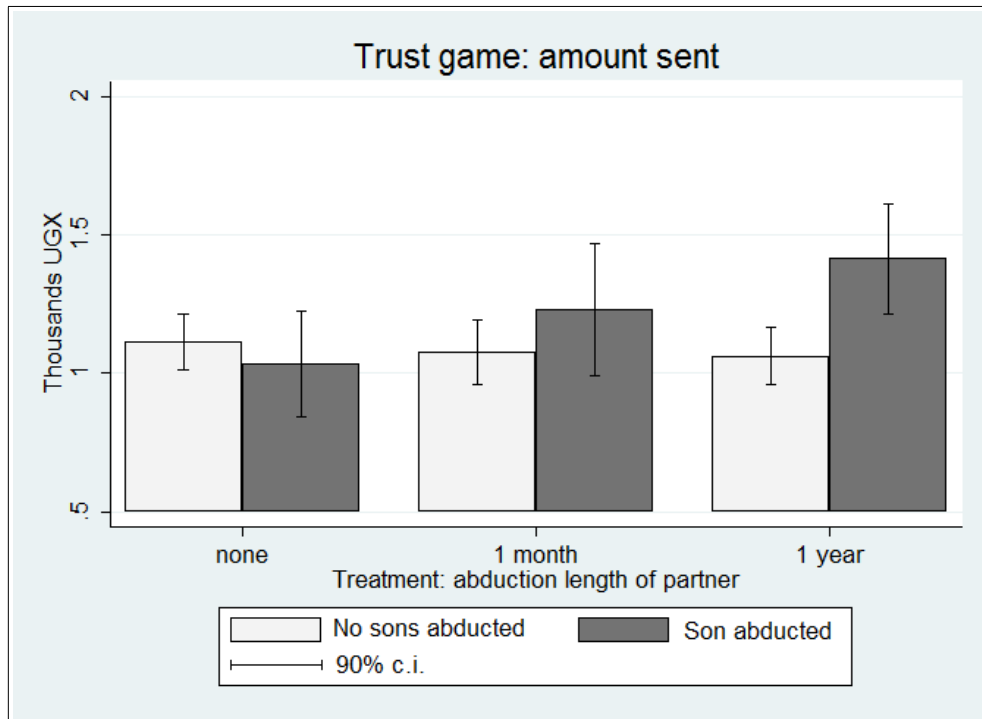


Figure 1: Amount sent in trust game, disaggregated by treatment and the abduction history of subjects' sons.

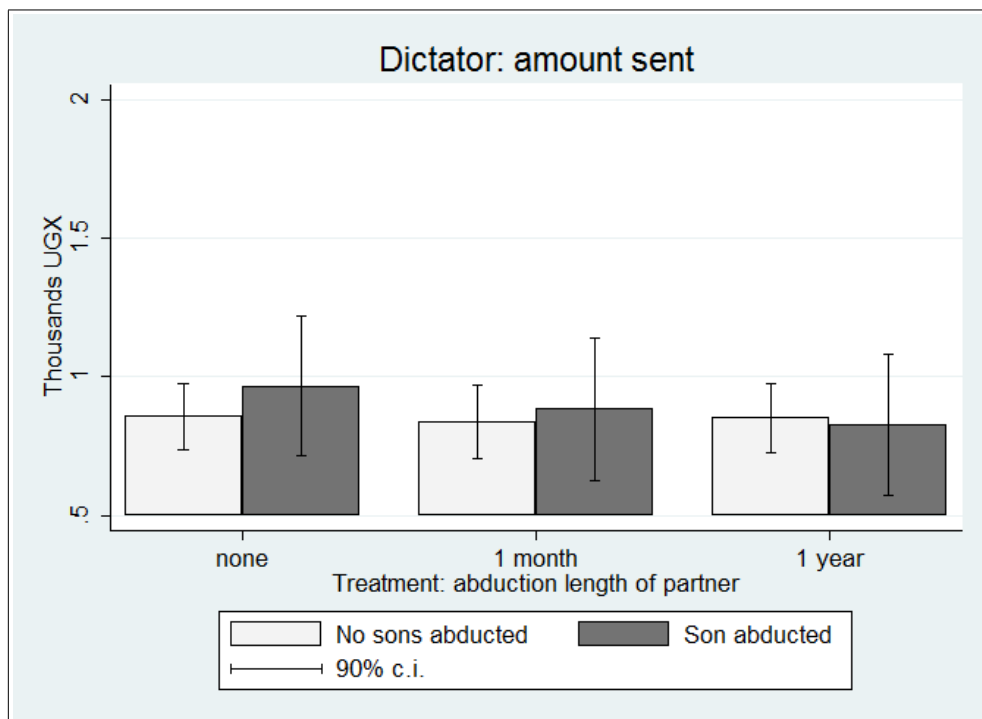


Figure 2: Amount sent in dictator game, disaggregated by treatment and the abduction history of subjects' sons.

Table 1: Summary Statistics: Mean (s.d.)

<i>Sample:</i>	<i>Receivers</i>	<i>Senders</i>
	(1)	(2)
<i>Panel A: Experimental outcomes:</i>		
Trustworthiness: % returned by Receiver if 1 ths UGX sent	34.72 (25.55)	
Trustworthiness: % returned by Receiver if 2 ths UGX sent	35.06 (25.77)	
Trustworthiness: average % returned ^a	34.89 (23.39)	
Expected trust: belief of Sender's allocation in trust game (ths UGX)	1.38 (0.61)	
Expected altruism: belief of Sender's allocation in dictator game (ths UGX)	1.23 (0.72)	
Partner in trust game male (treatment) (d)	0.48 (0.50)	
Trust: amount transferred in trust game (ths UGX)		1.12 (0.64)
Altruism: amount transferred in dictator game (ths UGX)		0.86 (0.75)
Expected trustworthiness: belief about average % returned		0.60 (0.30)
<i>Panel B: Conflict Experience</i>		
Ever abducted by LRA (d)	0.55 (0.50)	
Abduction length (years)	0.68 (1.72)	
Abduction length (years) ^b	1.25 (2.18)	
Son abducted (d) (at least one former abductee son)		0.22 (0.42)
Index of violence received (0-5) ^c	2.92 (1.82)	2.57 (1.84)
–bullets shot at home (d)	0.62 (0.49)	0.59 (0.49)
–received beating or attacked (d)	0.60 (0.49)	0.56 (0.50)
–tied up or taken prisoner (d)	0.56 (0.50)	0.43 (0.50)
–received serious physical injury (d)	0.55 (0.50)	0.48 (0.50)
–forced to carry heavy loads (d)	0.59 (0.49)	0.51 (0.50)
Index of violence against family(0-2) ^c	1.59 (0.72)	1.40 (0.81)
–family member or friend died (d)	0.78 (0.41)	0.68 (0.47)
–family member or friend disappeared or abducted (d)	0.81 (0.39)	0.72 (0.45)
Index of violence witnessed (0-4) ^c	2.41 (1.30)	1.92 (1.37)
–witnessed battle or attack (d)	0.75 (0.44)	0.56 (0.50)
–witnessed torture or beating (d)	0.81 (0.39)	0.72 (0.45)
–witnessed a killing (d)	0.62 (0.49)	0.46 (0.50)
–witnessed rape or sexual abuse (d)	0.22 (0.42)	0.19 (0.42)
Index of violence committed (0-2) ^c	0.65 (0.85)	
–forced to do violent things to soldier in battle (d)	0.28 (0.45)	
–forced to do violent things to a civilian (d)	0.36 (0.48)	
Reintegration ceremonies (index) ^c	0.43 (0.71)	
–participated in welcoming ceremony (stepped on egg) (d) ^b	0.52 (0.50)	
–participated in cleansing ceremony (mato oput) (d) ^b	0.31 (0.46)	
Passed through reception center (d) ^b	0.48 (0.50)	
<i>Panel C: Personal Characteristics</i>		
Age	24.45 (4.89)	43.08 (6.10)
Female (d)		0.56 (0.50)
Birth order	3.55 (2.33)	
Sisters	2.33 (1.65)	
Brothers	2.67 (1.95)	
Mother never attended school (d)	0.65 (0.48)	
Father never attended school (d)	0.27 (0.45)	
Married (d)	0.53 (0.50)	0.80 (0.40)

(Continued)

Number of household members	6.92 (4.83)	8.11 (3.56)
Cash earned in past 7 days (thousands UGX)	2.69 (1.02)	2.02 (5.42)
Wealth ^d	-0.04 (2.22)	-0.01 (2.19)
Literate (d)	0.75 (0.43)	0.28 (0.45)
Schooling (years)	7.07 (2.74)	3.27 (3.11)
Risk preference scale ^e		1.56 (1.09)
Observations	337	378

^aAverage percentage returned from two separate decisions made by Receivers, conditional on Senders' actions (strategy method). Senders could send 1 ths or 2 ths UGX, Receivers could return 0-3 ths and 0-6 ths UGX, in each decision respectively. ^bResults shown for sub-sample sample of ex-abductees. ^cIndex of violence-related dummy variables, elements of index listed below in italics. ^d1st principal component constructed from count of household assets, including: jerry cans, wash basins, bicycles, mattresses radios, plates, livestock, chairs, mobile phones and plows. ^eRisk scale is sum of instances when participant chose the safe option in lottery experiments (max. 3): 0 indicates low risk aversion, 3 indicates high risk aversion.

Table 2: Abduction by the LRA and Trustworthiness

Dependent variable: <i>Sample</i>	Trustworthiness: Average percentage returned in trust game			
	<i>All Receivers</i>			
	(1)	(2)	(3)	(4)
Abduction length (years)	1.36*** (0.34)		2.36*** (0.66)	
Abduction length over 1y (d)		7.67** (3.10)		
Abduction length 2m-1y (d)		3.41 (5.07)		
Abduction length <2m		5.29 (3.86)		
Age of first abduction ≥ 14 (d)			0.52 (4.03)	
Abduction length x age of abd ≥ 14			-2.36* (1.19)	
Violence committed (index)				2.37 (1.54)
Violence received (index)				-1.08 (1.39)
Violence against family (index)				0.38 (1.86)
Violence witnessed (index)				-0.35 (1.83)
Traditional Ceremonies (index)				2.14 (2.78)
Reception center (d)				4.09 (3.58)
Married (d)	-6.10 (3.92)	-7.38* (3.96)	-6.44 (4.12)	-4.82 (4.37)
Age	0.04 (0.41)	0.03 (0.41)	0.11 (0.43)	-0.03 (0.44)
Birth order	0.05 (0.59)	0.18 (0.60)	0.11 (0.60)	-0.29 (0.53)
Sisters	-1.01 (0.77)	-1.04 (0.80)	-1.02 (0.79)	-0.45 (0.74)
Brothers	0.12 (0.64)	0.03 (0.65)	0.00 (0.65)	0.06 (0.65)
Mother no school	1.28 (3.09)	1.91 (3.27)	1.58 (3.17)	2.44 (2.89)
Father no school	-1.64 (2.57)	-2.16 (2.62)	-1.56 (2.54)	-2.26 (2.88)
Wealth	1.20*** (0.38)	1.30*** (0.38)	1.23*** (0.37)	1.32*** (0.41)
Log of weekly income	-0.11 (0.28)	-0.04 (0.28)	-0.12 (0.29)	-0.20 (0.29)
Number of current HH members	0.36* (0.19)	0.37* (0.21)	0.33* (0.18)	0.29 (0.22)
Literate (d)	6.83* (3.45)	5.98 (3.56)	6.47* (3.33)	5.81* (3.30)

(Continued)

Schooling (years)	-0.33 (0.56)	-0.39 (0.57)	-0.34 (0.56)	-0.30 (0.55)
Partner in experiment male (d)	3.44 (2.54)	3.56 (2.56)	2.93 (2.50)	3.29 (2.23)
Constant	31.15*** (10.52)	30.36*** (10.18)	30.24*** (10.83)	33.41*** (11.07)
Observations	328	328	328	323
R-squared	0.06	0.07	0.07	0.07

Note: OLS. Robust standard errors in parentheses, clustered at village level.
* significant at 10%; ** significant at 5%; *** significant at 1%. The dependent variable is the average percentage returned from two decisions made by Receivers, who made two separate decisions, conditional on senders' actions (strategy method). Senders and 0-6 ths UGX, could send 1 ths or 2 ths UGX, receivers could return 0-3 ths respectively.

Table 3: Abduction by the LRA and Expected Trust and Altruism

Dependent variable:	Expected trust: belief of Sender's allocation in trust game		Expected altruism: belief of Sender's allocation in dictator game	
<i>Sample:</i>	<i>All Receivers</i>			
	(1)	(2)	(3)	(4)
Abduction length (years)	0.00 (0.01)		-0.03 (0.02)	
Abduction length over 1y (d)		0.06 (0.10)		-0.15 (0.13)
Abduction length 2m-1y (d)		-0.02 (0.13)		0.13 (0.12)
Abduction length <2m		0.12 (0.08)		0.02 (0.11)
Constant	1.36*** (0.29)	1.35*** (0.30)	1.06** (0.39)	1.09*** (0.38)
Observations	328	328	328	328
R-squared	0.04	0.04	0.03	0.04

Note: OLS. Robust standard errors in parentheses, clustered at village level. * significant at 10%; ** significant at 5%; *** significant at 1%. In Columns 1-2 the dep. var. is the amount Receivers expected to be transferred by Senders in trust game. In Columns 3-4 the dep. var. is the amount Receivers expected to be transferred by Senders in the dictator game. In all columns, we control for birth order, sisters, brothers, mother no school, father no school, log of weekly income (UGX), wealth, number of household members, married, literate, years of schooling and whether their partner in the experiment was male.

Table 4: Behavior Towards Former Soldiers: Trust and Altruism

<i>Sample</i>	Trust: amount transferred in trust game (ths UGX)				Altruism: amount transferred in dictator game (ths UGX)	
	<i>Senders</i>					
	<i>All</i>		<i>No sons Abducted</i>	<i>Son Abducted</i>	<i>All</i>	
	(1)	(2)	(3)	(4)	(5)	(6)
LRA-long treatment	0.09 (0.08)	-0.04 (0.10)	-0.03 (0.10)	0.53** (0.22)	-0.00 (0.11)	0.02 (0.12)
LRA-short treatment	0.07 (0.10)	0.01 (0.13)	0.01 (0.13)	0.36 (0.27)	0.01 (0.10)	0.02 (0.12)
LRA-long t. x Son abducted		0.50** (0.21)				-0.08 (0.29)
LRA-short t. x Son abducted		0.29 (0.24)				-0.04 (0.23)
Son abducted	0.12 (0.08)	-0.14 (0.12)			0.13 (0.10)	0.17 (0.17)
Constant	0.49* (0.28)	0.44 (0.27)	0.73** (0.32)	-0.70 (0.64)	0.87** (0.41)	0.87** (0.41)
Observations	360	360	278	82	360	360
R-squared	0.09	0.11	0.08	0.28	0.08	0.08

Note: OLS. Standard errors, clustered at village level in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%. LRA-long treatment and LRA-short treatments are indicator variables equal to one if Sender was informed that Receiver was with the LRA for around one year and around one month, respectively, and zero otherwise. The omitted group is the control condition, in which no reference to LRA abduction was made. In all regressions we control for order of the tasks, marital status of partner, indices of violence received and witnessed, index of violence against family, age, gender, marital status, results of risk experiment, wealth, log of income, and number of household members.

Table 5: Beliefs about Trustworthiness of Former Soldiers

<i>Sample</i>	Investment: Difference between trust and dictator allocations (Ths. UGX)			Expected back-transfer in trust game: directly elicited 1st-order beliefs (percent, pooled)		
	<i>Senders</i>					
	<i>All</i>	<i>No sons abducted</i>	<i>Sons abducted</i>	<i>All</i>	<i>No sons abducted</i>	<i>Sons abducted</i>
	(1)	(2)	(3)	(4)	(5)	(6)
LRA-long treatment	0.08 (0.10)	-0.05 (0.10)	0.75*** (0.27)	0.03 (0.04)	0.00 (0.04)	0.16* (0.09)
LRA-short treatment	0.07 (0.08)	-0.01 (0.10)	0.36* (0.21)	0.05 (0.04)	0.04 (0.04)	0.11 (0.11)
Son abducted	-0.02 (0.10)			0.00 (0.05)		
Constant	-0.46 (0.41)	-0.41 (0.48)	-0.81 (0.82)	0.65*** (0.14)	0.68*** (0.17)	0.58* (0.30)
Observations	359	277	82	360	278	82
R-squared	0.05	0.07	0.20	0.06	0.07	0.17

Notes: OLS. Standard errors, clustered at village level are shown in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%. The dep. var. in columns 1-3 is the differences b/w the amount transferred in trust game and dictator game. The dep. var. in Columns 4-6 is the expected trustworthiness of the Receiver: the percentage which the Sender expects to receive back from the Receiver. In all regressions we control for order of the tasks, marital status of partner, indices of violence received and witnessed, index of violence against family, age, gender, marital status, results of risk experiment, wealth, log of income, and number of household members.

Appendix: Additional Tables and Figures

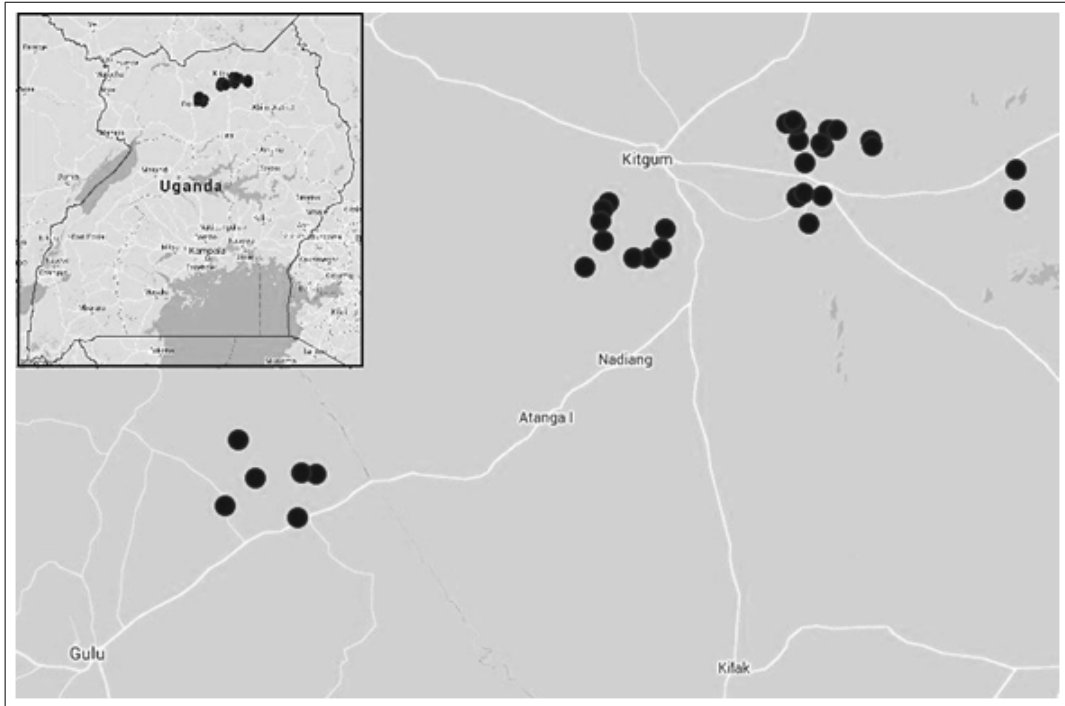


Figure A1: Location of villages in which experimental sessions were conducted in Gulu and Kitgum districts, northern Uganda.



Figure A2: Group explanation of experimental task.

Table A1: The Effect of Age of Abduction on Trustworthiness and War-Related Experiences among Former Soldiers

Estimator:	OLS						Probit	
<i>Sample</i>	<i>Receivers</i>							
Dependent variable:	Average % returned in trust game	Abduction length (years)	Index of violence committed	Index of violence received	Index of violence against family	Index of violence witnessed	Index of traditional cleansing ceremonies	Passed through reception center
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Age of first abduction ≥ 14 (d)	-9.09 (5.79)	0.12 (0.36)	-0.02 (0.18)	-0.06 (0.24)	-0.02 (0.19)	0.29 (0.23)	-0.06 (0.22)	0.01 (0.13)
Constant	23.64 (23.32)	3.32** (1.48)	-0.74 (0.70)	2.85*** (0.93)	1.12 (0.88)	4.30*** (1.04)	0.99 (0.62)	-3.05 (1.36)
Observations	182	177	182	182	182	182	172	182
(Pseudo) R-squared	0.21	0.45	0.24	0.23	0.26	0.20	0.26	0.16

Notes: Robust standard errors in parentheses, clustered at village/session level. * significant at 10%; ** significant at 5%; *** significant at 1%. All regressions include fixed effects for year of first abduction. Marginal effects reported in probit regression in Column 8. In each column we control for age, birth order, sisters, brothers, mother no school, father no school, log of weekly income (UGX), number of household members, married, literate, years of schooling and whether their partner in the experiment was male.

Table A2: Understanding Pro-Social Motivations Driving Higher Trustworthiness Among Former Soldiers

Estimator:	Probit	OLS	Probit		OLS		
Dependent variable:	Trustworthiness: Amount returned in trust game:						
	Return positive amount =1	Average % returned	Achieve equal allocation of payoffs when 2 ths sent	Achieve equal allocation of payoffs when 1 ths sent	% returned when 2 ths sent	% returned when 1 ths sent	Difference in % returned when 2 ths and 1 ths sent
<i>Sample:</i>	<i>Receivers</i>						
	<i>All</i>	<i>Returned positive amount</i>	<i>All</i>				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Abduction length (years)	0.10* (0.05)	0.83** (0.31)	0.08 (0.05)	-0.01 (0.04)	1.70** (0.75)	1.03 (0.62)	0.68 (1.19)
Constant	1.68*** (0.57)	35.17*** (10.28)	0.13 (0.40)	30.85*** (10.28)	31.45** (11.57)	-1.42** (0.56)	0.60 (6.08)
Observations	328	279	328	328	328	328	328
(Pseudo) R-squared	0.13	0.05	0.05	0.06	0.05	0.06	0.03

Notes: Robust standard errors in parentheses, clustered at village level. * significant at 10%; ** significant at 5%; *** significant at 1%. In columns 1,3,4 marginal effects are reported. In Column 1 the dependent variable is an indicator variable equal to one if Receiver returned any positive amount in either choice (choices conditional on 1 ths or 2 ths UGX sent using strategy method) and zero otherwise. In Column 2 the dependent variable is the average percentage returned (pooled across the two conditions) and we restrict the sample to individuals who returned any positive amount in either choice (to not purely selfish individuals). In Column 3 the dependent variable is an indicator variable equal to one if the Receiver returned 3 ths. UGX when 2 ths UGX is transferred (and thus achieved equal distribution of rewards b/w self and Sender) and zero otherwise. In Column 4 the dependent variable is an indicator variable equal to one if the Receiver returned 1 ths UGX when 1 ths is transferred (and thus achieved equal distribution of rewards b/w self and Sender), and zero otherwise. In Columns 5 and 6 the dependent variable is the average percentage returned when 2 ths UGS is transferred and 1 ths UGS is transferred, respectively. In Column 7 the dependent variable is the difference in the percentage returned when 2 ths UGX and 1 ths UGX is transferred. In all regressions we control for age, birth order, sisters, brothers, mother no school, father no school, log of weekly income (UGX), number of household members, married, literate and years of schooling and whether their partner in the experiment was male.

Table A3: Abduction by the LRA and Survey-Based Outcomes

Estimator:	OLS	Probit	OLS	
Dependent variable:	Index of group membership	Reported fight	Schooling (years)	Log of weekly income
<i>Sample:</i>	<i>All Receivers</i>			
	(1)	(2)	(3)	(4)
Length of abduction (years)	0.11* (0.06)	-0.09* (0.05)	-0.25*** (0.06)	0.16 (0.15)
Age	0.01 (0.01)	0.01 (0.02)	-0.15*** (0.03)	0.09 (0.06)
Birth order	0.03 (0.04)	0.11*** (0.04)	-0.06 (0.07)	0.25 (0.15)
Sisters	-0.01 (0.05)	-0.08* (0.04)	0.22** (0.09)	-0.06 (0.20)
Brothers	0.05 (0.04)	-0.03 (0.04)	0.05 (0.06)	0.11 (0.17)
Mother no school	-0.47*** (0.16)	0.18 (0.19)	0.05 (0.30)	-1.03 (0.74)
Father no school	-0.09 (0.16)	-0.00 (0.28)	-0.62* (0.31)	-0.19 (0.79)
Constant	1.28*** (0.36)	-1.60*** (0.47)	10.65*** (0.71)	-1.64 (1.43)
Observations	332	332	332	332
(Pseudo) R-squared	0.06	0.04	0.13	0.04

Note: Robust standard errors in parentheses, clustered at village level. * significant at 10%; ** significant at 5%; *** significant at 1%. In Column 2, marginal effects are reported. The dependent variable in Column 1 is an index of 10 survey questions on membership in community groups and civic participation (drama and music club, peace club, farmers group, church group, school committee, sports team, volunteer for an NGO, community mobilizer, member of other community group). The dependent variable in Column 2 is an indicator variable equal to one if the respondent reported having quarrels with family, neighbors or village elders that led to physical fights in the past six months.

Table A4: Robustness Check on Mis-reporting of Abduction Status

Dependent variable:	Trustworthiness: Average % returned in trust game	
<i>Sample</i>	<i>Receivers</i>	
	<i>All</i>	<i>Sub-sample of those whose abduction status matches reports of other hh member</i>
	(1)	(2)
Abduction length (years)	1.36*** (0.34)	1.50*** (0.47)
Constant	31.15*** (10.52)	26.50*** (9.21)
Observations	328	297
R-squared	0.06	0.08

Notes: OLS. Robust standard errors in parentheses, clustered at village/session level. * significant at 10%; ** significant at 5%; *** significant at 1%. In Column 1 we repeat the main estimation from Table 2, Column 1, for ease of comparison. In Column 2 we exclude individuals whose self-report of abduction status did not match with reports of abduction by other family members during the household survey conducted prior to the experiment. In both Columns we control for age, birth order, sisters, brothers, mother no school, father no school, log of weekly income (UGX), number of household members, married, literate and years of schooling.

**Table A5: Pre-existing Family
Characteristics and Abduction Length**

Dependent variable: <i>Sample</i>	Abduction length (years)	
	<i>Receivers</i>	
	<i>All</i>	<i>Ex-abductees</i>
	(1)	(2)
Age	0.03 (0.02)	0.74*** (0.06)
Birth order	-0.11* (0.06)	0.09 (0.14)
Sisters	0.09 (0.08)	0.01 (0.18)
Brothers	0.00 (0.05)	0.02 (0.14)
Mother no school	0.02 (0.20)	-0.02 (0.51)
Father no school	0.04 (0.23)	0.38 (0.42)
Constant	-0.02 (0.52)	-4.19*** (1.48)
Observations	332	182
R-squared	0.02	0.53

Notes: OLS. Robust standard errors in parentheses, clustered at village/session level. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table A6: Randomization Check

<i>Sample</i>	Mean (sd)						F-test
	Partner's length of abduction (treatment condition)						p-value
	<i>Senders</i>						
	<i>No-LRA t.</i>	<i>LRA-short t.</i>	<i>LRA-Long t.</i>				<i>All</i>
(1)	(2)	(3)				(4)	
Son abducted (d) (at least one former abductee son)	0.22	(0.42)	0.22	(0.41)	0.23	(0.43)	0.96
Index of violence received during LRA conflict: (0-5) ^a	2.78	(1.86)	2.43	(1.79)	2.49	(1.85)	0.27
<i>-bullets shot at home (d)</i>	0.61	(0.49)	0.59	(0.49)	0.55	(0.50)	0.57
<i>-received beating or attacked (d)</i>	0.57	(0.50)	0.53	(0.50)	0.57	(0.50)	0.77
<i>-tied up or taken prisoner (d)</i>	0.48	(0.50)	0.37	(0.49)	0.43	(0.50)	0.25
<i>-received serious physical injury (d)</i>	0.52	(0.50)	0.49	(0.50)	0.43	(0.50)	0.30
<i>-forced to carry heavy loads (d)</i>	0.59	(0.49)	0.45	(0.50)	0.49	(0.50)	0.08
Index of violence against family during LRA conflict: (0-2) ^a	1.38	(0.85)	1.43	(0.80)	1.41	(0.79)	0.91
<i>-family member or friend died (d)</i>	0.68	(0.47)	0.67	(0.47)	0.69	(0.46)	0.94
<i>-family member or friend disappeared or abducted (d)</i>	0.71	(0.46)	0.75	(0.43)	0.72	(0.45)	0.72
Index of violence witnessed during LRA conflict: (0-4) ^a	2.04	(1.40)	1.76	(1.32)	1.94	(1.37)	0.27
<i>-witnessed battle or attack (d)</i>	0.60	(0.49)	0.55	(0.50)	0.54	(0.50)	0.55
<i>-witnessed torture or beating (d)</i>	0.74	(0.44)	0.68	(0.47)	0.73	(0.45)	0.57
<i>-witnessed a killing (d)</i>	0.49	(0.50)	0.39	(0.49)	0.48	(0.50)	0.23
<i>-witnessed rape or sexual abuse (d)</i>	0.22	(0.41)	0.16	(0.43)	0.20	(0.40)	0.55
Married (d)	0.84	(0.36)	0.80	(0.40)	0.77	(0.43)	0.28
Partner in experiment married (d)	0.52	(0.50)	0.50	(0.50)	0.48	(0.50)	0.85
Age	42.91	(6.21)	43.33	(6.30)	43.02	(5.82)	0.86
Female (d)	0.57	(0.50)	0.58	(0.50)	0.54	(0.50)	0.84
Risk preference scale ^b	1.63	(1.12)	1.45	(1.09)	1.57	(1.05)	0.84
Wealth ^c	-0.07	(2.06)	0.05	(2.14)	0.01	(2.38)	0.91
Number of household members	8.11	(3.28)	8.27	(4.18)	7.97	(3.22)	0.81
Dictator game played first	0.14	(0.35)	0.13	(0.34)	0.15	(0.36)	0.91
Cash earned in past 7 days by respondent (thousands UGX)	1.89	(5.85)	2.44	(6.04)	1.78	(4.19)	0.60
School (years)	3.43	(3.18)	3.46	(3.13)	2.95	(3.00)	0.36
Literate	0.32	(0.47)	0.30	(0.46)	0.22	(0.41)	0.73

Notes: Means. Standard deviations in parentheses. Column 4 reports p-value for an F-test testing the null hypothesis that the means are equal across all three treatment conditions. (d) indicates dummy variable. ^aIndex of violence-related dummy variables, elements listed below. ^bRisk scale is the sum of instances when the participant chose the safe option in lottery experiments (max. 3): 0 indicates low risk aversion, 3 indicates high risk aversion. ^c1st principal component constructed from count of household assets, including: jerry cans, wash basins, bicycles, mattresses radios, plates, livestock, chairs, mobile phones and plows.

Table A7: Effects of Receiver's Abduction on Sender's Decisions—Ordered probit

<i>Sample</i>	Trust: the amount transferred in trust game (ths UGX)				Altruism: the amount transferred in dictator game (ths UGX)	
	<i>Senders</i>					
	<i>All</i>		<i>No sons abducted</i>	<i>Son abducted</i>	<i>All</i>	
	(1)	(2)	(3)	(4)	(5)	(6)
LRA-long treatment	0.14 (0.15)	-0.06 (0.18)	-0.06 (0.18)	1.08** (0.42)	0.01 (0.16)	0.03 (0.19)
LRA-short treatment	0.13 (0.19)	0.01 (0.23)	0.01 (0.23)	0.74 (0.53)	0.03 (0.16)	0.04 (0.19)
LRA-long t. x Son abducted		0.96** (0.41)				-0.12 (0.44)
LRA-short t. x Son abducted		0.55 (0.46)				-0.04 (0.35)
Son abducted	0.23 (0.15) (0.06)	-0.26 (0.23) (0.06)			0.21 (0.16) (0.05)	0.26 (0.25) (0.06)
Observations	360	360	278	82	360	360

Notes: Ordered probit. Standard errors, in parentheses, clustered at village level. * significant at 10%; ** significant at 5%; *** significant at 1%. Regressions include controls for order of experiment, age, gender, risk aversion, indices of violence received and witnessed, index of violence against family, wealth, log of income, and number of household members.

Table A8: The Interaction Effect of Having a Son Abducted and LRA Treatment: robustness check (adding control variables for war experiences)

<i>Sample</i>	Trust: the amount transferred in trust game (ths UGX)					
	<i>Senders</i>					
	(1)	(2)	(3)	(4)	(5)	(6)
LRA-long treatment.	-0.03 (0.10)	0.07 (0.08)	0.07 (0.17)	0.10 (0.21)	0.05 (0.18)	0.02 (0.25)
LRA-short treatment.	0.00 (0.13)	0.06 (0.10)	0.18 (0.13)	0.13 (0.21)	0.08 (0.13)	0.13 (0.20)
Son abducted	-0.13 (0.12)					-0.13 (0.14)
LRA-long t. x Son abducted	0.49** (0.21)					0.53** (0.23)
LRA-short t. x Son abducted	0.28 (0.24)					0.30 (0.28)
Daughter abducted		-0.12 (0.30)				-0.09 (0.31)
LRA-long t. x daughter abducted		-0.02 (0.51)				-0.04 (0.56)
LRA-short t. x daughter abducted		0.01 (0.50)				-0.08 (0.58)
Violence received (index)			0.01 (0.03)			0.02 (0.04)
LRA-long t. x violence received			-0.05 (0.04)			0.01 (0.05)
LRA-short t. x violence received			0.00 (0.05)			-0.08* (0.04)
Violence against family (index)				0.04 (0.07)		0.03 (0.07)
LRA-long t. x violence family				-0.05 (0.12)		-0.09 (0.10)
LRA-short t. x violence family				-0.02 (0.11)		-0.04 (0.12)
Violence witnessed (index)					0.01 (0.04)	-0.02 (0.05)
LRA-long t. x witnessed					0.01 (0.06)	0.03 (0.06)
LRA-short t. x witnessed					-0.01 (0.05)	0.07 (0.08)
Constant	0.47* (0.24)	0.42 (0.26)	0.42 (0.28)	0.39 (0.26)	0.42 (0.29)	0.41 (0.28)
Observations	360	360	360	360	360	360
R-squared	0.11	0.08	0.09	0.08	0.08	0.12

Notes: OLS. Standard errors, in parentheses, clustered at village level are shown in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%. Regressions include controls for order of experiment, age, gender, risk aversion, wealth, log of income, and number of household members.

Table A9: Predictors of Having a Son Abducted

Dependent variable: <i>Sample</i>	At least one son abducted	
	<i>Senders</i>	
	(1)	(2)
Violence received (index)		-0.01 (0.05)
Violence against family (index)		0.33* (0.17)
Violence witnessed (index)		0.10 (0.07)
Risk scale	0.01 (0.06)	0.01 (0.07)
Age	0.07*** (0.01)	0.08*** (0.01)
Female	0.58*** (0.14)	0.66*** (0.14)
Married	-0.26 (0.21)	-0.36 (0.19)
Wealth	0.07 (0.04)	0.08* (0.04)
Log of income	0.01 (0.02)	0.00 (0.02)
Number of hh members	0.00 (0.02)	-0.01 (0.02)
Constant	-4.09*** (0.68)	-4.80*** (0.76)
Observations	368	361
Pseudo R-squared	0.14	0.18

Notes: Probit. Marginal effects reported. Standard errors, in parentheses, clustered at village level. * significant at 10%; ** significant at 5%; *** significant at 1%.

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