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

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ABSTRACT

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The stability of many post-conflict societies rests on the successful reintegration of former soldiers. We examine social capital of former soldiers in Northern Uganda, where the Lord's Resistance Army forcibly recruited tens of thousands of youth during a recent brutal conflict. We use a set of interlocked experiments to study behavior of ex-soldiers jointly with the behavior of receiving communities towards this group. Consistent with theories that highlight the importance of cooperation during war, we find that individual cooperativeness robustly increases with the length of time a person was with the LRA, especially among those who were abducted during early age. Furthermore, parents of former-soldiers are aware of the behavioral difference: they trust ex-soldiers more because they expect them to be more trustworthy. 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. Together, the results reveal that the impact of child soldiering on social capital, in contrast to human capital, is not necessarily detrimental and, speculatively, may facilitate reintegration.

JEL Classification: C93, D03, D74, O12

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

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I. Introduction

In conflicts around the world, forcible recruitment of soldiers, often children, is a widespread practice among many military and insurgent groups (Beber and Blattman, 2013; Blattman and Miguel, 2010).[1] After conflicts end, the reintegration of former soldiers is a critical issue, in part because of the risk of falling into the conflict trap (Collier, 2007)—former combatants may become socially isolated or economically worse off, and feelings of frustration and low opportunity costs may increase the chances that they join armed groups in the future (Collier and Hoeffler, 2004; Knight and Özerdem, 2004), which may lead to the re-emergence of violence. To assess the success of reintegration, the existing empirical literature literature has used survey data on 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 former 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 of separating 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 former soldiers have different skills, preferences or beliefs about the behavior of others and (ii) whether community members treat the former soldiers differently

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[1] Civil wars have afflicted a third of all countries and two thirds of Africa since 1991 (Blattman and Miguel, 2010). Although there are no exact figures, several millions of children under the age 18 are estimated to have served in combat during these wars and since 2001, the participation of child soldiers has been documented in 21 armed conflicts in almost every region of the world (HRW, 2008).

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 determine access to jobs, credit and participation in informal saving and insurance arrangements,[2] especially in societies where economic interactions are rarely governed by formal contracts. The setting is Northern Uganda, where an unpopular rebel group (the Lord's Resistance Army or LRA) forcibly and indiscriminately recruited tens of thousands 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. We conducted a large-scale experimental data collection (N=688) 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 examine cooperative behavior of former soldiers in comparison to their peers, whether soldiering during early age leaves a deeper mark than soldiering later on in life and how members of the community differentiate their behavior towards former soldiers and why.

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

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[2]Societal 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 social preferences 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).

[3]For a large portion of our sample the violent experience we study took place during child-

might be, due to paucity of data. On the one hand, the experience of soldiering may have negative effects on cooperativeness due to trauma experienced (Catani et al., 2008) or purposeful identity manipulation performed by rebels (Beber and Blattman, 2013), 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).

On the other hand, several intriguing theories suggest that soldiering might, in fact, lower selfishness and intensify cooperative behavior. Evolutionary approaches to human cooperation have long emphasized the important role that lethal conflict between groups and other external threats have likely played in shaping human preferences (Darwin, 1873; Choi and Bowles, 2007; Bowles, 2006). Since more cooperative groups are more likely to survive during fighting, 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 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 no comparable evidence using behavioral experiments with ex-soldiers, who were often perpetrators of violence.[5] The

hood 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 Levely, 2012; Bauer et al., 2014b).

[4]For 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.

[5]See 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 challanges with identifying former soldiers in this context, making inferences about

most closely related evidence comes from the detailed survey work of Blattman (2009), which shows that forced recruitment by rebels in Northern Uganda leads to greater likelihood of voting. While this evidence is consistent with greater willingness to help and participate in local collective action, it may also indicate distinct policital interests, which may not benefit others.

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 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 receiving communities blame 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. This distinction 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 that one is subject to 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).

Our experimental design and main findings can be summarized as follows.

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differences in behavior compared to non-combatants difficult.

[6] Although there are no studies measuring differential treatment of former 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).

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 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

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belief.

Fourth, we conducted a dictator game, in which the Sender decides how to allocate money between himself and the Receiver.[7] 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 II we briefly describe the background: the conflict in Northern Uganda, recruitment strategy of the Lord's Resistance Army and reintegration practices. Section III describes the sample selection and the experimental design. In Section IV we present the empirical results about behavioral differences between former soldiers and their peers. Section V presents results about differential treatment of former soldiers by their communities. Section VI concludes.

II. 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

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[7]Our 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.

Northern Uganda into Internally Displace Persons (IDP) camps.[8]

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 of this study, in 2011, the IDP camps had been closed, and the majority of the 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.

While with the LRA, abductees went through a period of training and in-

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

doctrination, 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 soldiers report that 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) finds ex-abductees to be surprisingly resilient.

III. Experimental Design

III.A   Sample selection

The experiments were conducted from July to September 2011 in rural areas of Gulu and Kitgum districts in Northern Uganda. We identified villages in which at least 20 ex-abductees were living, based on reports of village leaders, and

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randomly selected 33 out of 52 villages (Appendix Figure A.1).[9]

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. Using this information from the pre-survey, we compiled a list of individuals together with their characteristics, and identified those who fit the criteria for Senders and Receivers.

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 soldiers and 87% for non-soldiers). 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.

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

III.B   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. 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 endowment[10] and were asked to decide between three options, by choosing an amount, $S \in \{0, 1000, 2000\}$, to transfer to their partner. The amount transferred was automatically tripled by the experimenter and the Receivers were given the option of sending back a portion of the received amount, $R \in \{0, 1000, 2000, \ldots, 3S\}$. Thus, Senders earned $2000 - S + R$, while Receivers earned $3S - R$.

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— 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

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[10]Unlike 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.

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, leading to an inefficient outcome which fails to exploit potential gains from sending a positive amount.

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 care about the welfare of Receivers, 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.

There are several noteworthy features of the information we provided subjects. First, in addition to information related to the Receiver's abduction status, we included several 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.[11] Second, we matched Senders with Receivers so that they possessed the characteristics reported in these profiles, to avoid deception. Third, Senders were informed that Receivers would also receive a short profile of their characteristics (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 and control for the order effects in estimations. Since the decision to trust is a risky one, we also elicited Senders' attitudes towards risk and use it as a control variable.[12] Specifically, 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. The more an individual prefers the lotteries to the fixed amounts with certainty, the less risk averse s/he is.

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[11]Specific 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."

[12]For a similar approach to controlling for the attitudes to risk in trust decisions, see, for example, Ashraf et al. (2006).

Receivers

In the trust game, Receivers chose 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.[13] The existing literature considers three distinct types of social preferences, which can motivate Receivers in a one-shot trust game to return positve amounts: reciprocity,[14] unconditional altruism (Andreoni and Miller, 2002) and inequality aversion (Fehr and Schmidt, 1999). In the dictator game, Receivers were passive and did not make any choice. We also elicited beliefs about how much they expected to actually receive from Senders in both the trust and the dictator games. Accurate responses were incentivized with 500 UGX.

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.[15]

III.C Survey Data

A large part of the survey instrument was the same for Senders and Receivers, and included questions about individual characteristics and exposure to violence during the conflict. For Senders, we included a specific module on abduction experiences of their family members, in particular their children. Surveys for

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[13]A recent review of experiments studying the effect of the strategy method finds no cases in which its use led to different treatment effects (Brandts and Charness, 2011). The advantage of strategy method is the increased number of observations.

[14]Reciprocity 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).

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

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Receivers included additional questions on exposure to violence, soldiering for 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 I.

III.D Procedure and payments

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, delivered all instructions in the local language (Acholi),[16] and extensively used visual aids, to illustrate options and payoffs of Senders and Receivers, and sequence of choices (see Appendix Figure A.2).

After a group explanation stage, 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. Comprehension was generally high, and only 2% of Senders and 0.3% of Receivers answered one or more of these questions incorrectly. (Complete instructions available upon request.)

In each village, we ran two experimental sessions—first with Senders and later during the same day with Receivers, with sessions overlapping in order to

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

minimize the chance of communication between participants.

We took several steps to increase the level of anonymity when making choices. 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 (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.[17]

Subjects were paid for either the trust 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 two days after the experiments, during which time we would match their responses with participants from a different village. When collecting payments, subjects were informed 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).

IV. Behavior and Beliefs of Former Soldiers

In this section we present results for Receivers and analyze the link between soldiering for the LRA and cooperative behavior. We use the following regression model:

$$D_i = \alpha + \beta A_i + \gamma X_i + \epsilon_i \tag{1}$$

where D_i is individual i's action in the experiments, A_i is length of soldiering, X_i is a range of individual characteristics,[18] and ϵ_i is the error term. Standard

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[17]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.

[18]In 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.,

errors are clustered at the village level.

IV.A   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 transfered 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.[19]

Does the cooperative behavior of former soldiers differ from their peers? The average length of abduction by LRA in our sample is 0.68 years (conditional on being abducted, the average length is 1.25 years). In column (1) of Table II we find a strong positive relationship between length of soldiering 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. We distinguish between four groups, based on abduction length: participants who were not abducted at all, those who were

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literacy or income). Note that the main results are not affected by using different sets of controls (available upon request).

[19]The level of trustworthiness in our sample is similar to that observed in comparable studies. A 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.

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). We find that the relationship is, in fact, dose-dependent (column 2): 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 that of 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) and 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 II 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 at early age (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 younger than 14 and that there is no such link for those abducted during late adolescence and adulthood.

Furthermore, in Appendix Table A.1 we regress the indicator of abduction after the age of 14 on the amount sent in the trust game for the sub-sample of former soldiers, 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 (Appendix Table A.1).

Observation 2: *The positive link between length of soldiering 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, we created four indices based on survey questions: violence received, violence witnessed, violence against family and violence committed (Table I details questions based on which each of these indices was constructed). We also consider an indicator variable of passing through a re-

ception center for returning abductees, indicating exposure to formal reintegration programs, and an index of participation in informal reintegration ceremonies.[20]

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 II 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.

We next consider which social preferences—inequality aversion, reciprocity or altruism—motivate 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). We find no link between length of soldiering and prevalence of choosing the equal split (Table A.2), suggesting that the increased trustworthiness is not due to greater adherence to norms of equality or a greater inequality aversion.

In order to distinguish between greater reciprocity and 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 Re-

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[20]The index of participation in informal reintegration ceremonies is the sum of two indicator variables: whether the subject took part in a traditional welcoming ceremony and cleansing ceremony. The welcoming ceremony involves stepping on an egg as a way of welcoming back people who have been gone for a long period of time. The cleansing ceremony, *Mato Oput*, is a ceremony for creating peace among people who aggrieved another party, which has been adapted as means of forgiving and accepting abductees after they return from the LRA (for more details see Allen (2010)).

ceivers 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 s/he sends 1,000 UGX. We find that the link between abduction length and the proportion sent back is positive for both potential amounts that could have been sent and also the difference in the proportion returned across the two possible amounts does not increase with abduction length (Table A.2). Together, these results suggest that the greater trustworthiness of former soldiers is motivated by greater unconditional altruism and not by greater inequality aversion or reciprocity.

IV.B   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? 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 ex-soldiers expect others to differentiate between ex-soldiers and their peers, or if abductees have different beliefs about behavior of others in general.

In column (1) of Table III we find virtually no link between abduction length and the amount that was expected to be received in trust game. We arrive at

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a similar conclusion when using a specification that tests for a non-linear relationship between abduction length and expected trust (column 2). In column (3) we find a small negative relationship between length of abduction and expected altruism of others, but the relationship is not statistically significant. 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.[21]

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

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 manipulation by the LRA has not left an enduring mark on the beliefs of abductees about the trust and the kindness of others. Alternatively, those who believed that members of their home communities would have negative attitudes towards them may have been less likely to return in the first place.

IV.C Robustness Checks

We now report a series of robustness analyses. First, since former soldiers were informed that Senders knew that they had been with the LRA, it is possible that by identifying ex-LRA soldiers 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

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

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 find that these subjects return even more than those subjects who reported being abducted and who also knew that their abduction status was revealed to Senders (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 Appendix Table A.3 we replace our experimental variable with survey-based proxies for cooperative behavior. Consistent with the experimental results, we find a positive correlation between length of abduction and an index of participation in local community groups (column 1) and a negative correlation between abduction lenght and the likelihood of having a physical fight in the past six months (column 2).

Next, our measure of soldiering is based on self-reported information and there is legitimate concern about systematic biases in truthful reporting. To assess the issue, we first compare reports of participant's abduction status as reported by (i) the participant during the post-experiment survey and (ii) another household member during the pre-survey (which served as a way to oversample former soldiers). 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 themselves, and 26 individuals were not identified as a former soldier in the household survey, but did report being abducted by the LRA during the survey.[22] As a robustness check, we perform the main estimations but exclude

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[22]A likely explanation is that some respondents to the household pre-survey may have recently married into the household and may not be aware of LRA history of each member.

all participants, whose self-reports of abduction status did not match the reports of the other household members. Results are robust (see Appendix Table A.4).

IV.D   Alternative explanations

Here 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. While the conflict in Northern Uganda presents a unique opportunity to study the effects of 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, and finally returning and staying home. We now consider which mechanisms could explain the full set of findings.

The LRA forcibly conscripted soldiers by raiding rural villages and it is 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 selected for their level of trustworthiness at the outset. Nevertheless, household characteristics could have affected which children were targeted and these may be correlated with trustworthiness. In Appendix Table A.5 we examine the effect of pre-abduction 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. In line with the previous work of Blattman and Annan (2010) we generally find little effect of household characteristics on soldiering. Only birth order is negatively correlated with abduction length (the relationship

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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. Non-cooperative 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 the possibility that the LRA has affected the distribution of behavioral types in the population of surviving soldiers.

Another concern is that less cooperative returning soldiers were less likely to be accepted by the home communities 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 such selection would be specific only for youth who were abducted at early age.

Alternatively, former child soldiers may behave more prosocially towards oth-

ers 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 feelings of guilt, but we find no correlation between being blamed and the amount transferred (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.

V. 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. The randomization was successful; we find no statistically significant differences in observable characteristics across the experimental manipulation of information about LRA history of the Receiver (Appendix Table A.6).

V.A   Trust

On average, Senders transfer 56% of their 2,000 UGX endowment to the Receivers in the trust game.[23] Do Senders differentiate trust based on how long Receivers spent with the LRA? 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 IV) as well as distribution of choices (available upon request).

Assuming that Senders are aware of behavioral differences and 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 soldiers on a regular basis. 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 conflict (N=82).

Figure I and columns (2)-(4) of Table IV 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)

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

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.[24]

Observation 4: *We do not find mistrust in former soldiers. However, 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 that Sender's believed would be returned by Receivers (p-value=0.02). Therefore the effect of the LRA treatments on higher trust among those with an abducted son 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 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, but at the same time are aware of ex-abductees' greater trustworthiness. In the following sub-sections we separate the

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[24]In 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 (Appendix Table A.7).

role of belief-based and preference-based components of trust.

V.B   Altruism

Results from the dictator game allow us to measure taste for discrimination against or favoritism towards former soldiers. In this task Senders again allocated an amount of money between themselves and a Receiver, but, in contrast to the trust game, Receivers are passive and thus beliefs about expected behavior should not affect the decision of how much to transfer. Higher amounts transferred in the LRA treatments compared to the control treatment would indicate favoritism towards 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 43% out of their 2000 UGX endowment. We find no effect of LRA treatments on the mean amount sent in the dictator game (column (5) of Table IV). We also find little differences in terms of distribution of the amount sent (available upon request). Further, Figure II 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.[25] We thus conclude that

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[25]Calculated using a power of 0.80 and a significance level of 0.05.

there is no evidence of differences in kindness towards ex-soldiers and non-soldiers.

Observation 5: *The results of the dictator game reveal 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.*

V.C 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.[26] Second, 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 transfered in the trust game and what was voluntarily given in the triple dictator game, using an approach proposed in Cox (2004).[27] 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 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 V, column

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[26]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%. Such 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.

[27]This 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.

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 a former soldier (column 6). Among those Senders with no ex-abductee sons, there was virtually no difference in expectations of how much Receivers would return across the LRA treatments (column 5). These results suggest that having an ex-abductee son improves knowledge about their trustworthiness and how it differs from their peers.

Observation 6: *Participants who have an abducted son are aware of the greater trustworthiness of former LRA soldiers compared to their peers and act based on this belief by trusting them more. In contrast, we find no differences in expectations of trustworthiness or in trusting behavior among Senders with no ex-abductee sons.*

V.D Robustness Checks and Further results

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-soldiers. To test for this possi-

bility, we study the interaction effects between different measures of exposure to violence (violence received, violence against family, violence witnessed or having a daughter abducted by the LRA) and LRA treatments on the amount sent in the trust game. The coefficients are small and statistically insignificant (Appendix Table A.8). 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).[28] This analysis indicates 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.

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. The 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.

In order to test whether the lack of taste-based discrimination is consistent with attribution theory, we elicited the perceptions of the degree to which LRA soldiering was avoidable from members of receiving communities. 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

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[28]Appendix Table A.9 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.

("around 1 month" or "around 1 year"). 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, 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. 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.

VI. 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-soldiers 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). We aim to extend this earlier work by (i) separately observing behavior of former soldiers as well as treatment of former soldiers by receiving communities, (ii) focusing on two important aspects of interpersonal relations, namely trust and willingness to cooperate, which are difficult to measure in surveys (iii) using incentive-compatible field experiments, in contrast to responses to survey questions.

In this paper we study the impact of soldiering on social capital and use data from a series of economic experiments implemented on a randomly-selected sam-

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ple of 688 participants from 33 villages in Northern Uganda. We find that the longer period a person was with the Lord's Resistance Army, the more cooperative 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). We find neither systematic mistrust nor preference-based discrimination against former soldiers. Moreover, individuals with abductee sons, and thus with better knowledge of their behavior, trust ex-soldiers more compared to their peers, because they expect ex-soldiers to be more trustworthy.

Our results are consistent with recent theories linking war and development of cooperative preferences (Choi and Bowles, 2007). Given the need for group cooperation during inter-group fighting, one mechanism linking soldiering and cooperative preferences is that armed groups treat uncooperative individuals extremely harshly, increasing the prevalence of cooperative types in the population of former soldiers. Perhaps more interestingly for theory, the preferences of former child soldiers may have adapted to the war environment. Such preference adaptation may have evolutionary underpinning or be due to socialization—former soldiers may have painfully learned the importance of being cooperative and internalized such behavior. Our results also nicely complement recent evidence among the victims of war-related violence, which shows that greater exposure to violence increases cooperative behavior towards one's in-group (Voors et al., 2012; Gneezy and Fessler, 2012; Bauer et al., 2014a), suggesting there is a similar mechanism underlying behavioral response in victims of violence as well as in forcibly recruited perpetrators of violence.

The finding that the effects of soldiering are more pronounced if experienced during early age compared to late adolescence and adulthood 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 substan-

tially during early stages of the life cycle (Harbaugh and Krause, 2000; Fehr et al., 2008). Our results are consistent with the idea that social preferences are malleable mainly during childhood and thus environmental factors during this period may have lasting impacts on this type of preferences.

The finding that soldiering for a brutal rebel group has not invited preference-based discrimination against former soldiers is consistent with attribution theory (Heider, 1958; Gneezy et al., 2012), given that individual LRA history is mostly perceived as completely unavoidable by the population we study. Such interpretation cautions againts generalizing our findings to post-conflict societies in which the participation of soldiers in violent acts is seen as an outcome of their decisions and may help to explain the survey evidence of Humphreys and Weinstein (2007) from Sierra Leone, a setting where youth joined rebel groups mostly voluntarily, who find that ex-soldiers who were more exposed to violence report lower community acceptance. In terms of policy, our results showing a limited awareness of the greater cooperativeness of former soldiers gives additional rationale for designing reintegration programs that provide training and services jointly with other people in war affected communities, instead of providing services to former soldiers separately.[29] Doing so may provide an additional benefit by facilitating the updating of beliefs, as we see with parents.

Although the psychological and human-capital costs of being a forcibly recruited soldier are enormous (see Blattman and Annan (2010) and Appendix Table A.3), the main finding of this paper is that it does not necessarily have negative effects on social capital. Clearly, more research needs to be done to understand the generalizability of this finding. Yet this behavioral experiment provides new evidence against automatically taking pessimistic views on one of the key factors that may undermine reintegration of former soldiers and thus peaceful development of post-conflict societies.

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[29]For a debate on this issue see, for example, Muggah (2009).

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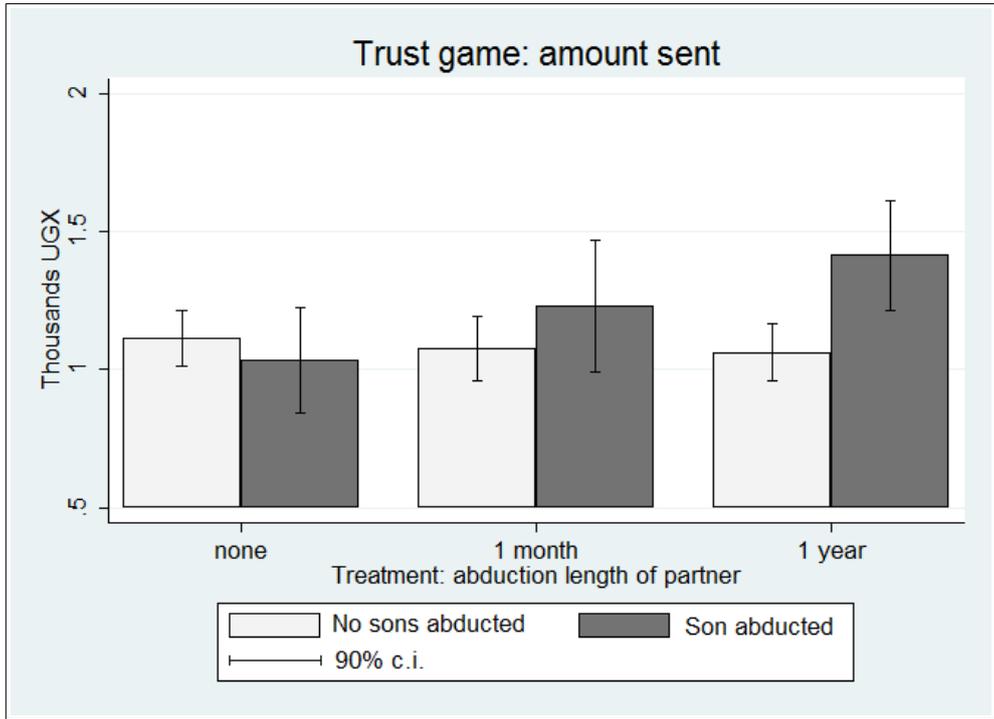
Figure I: Amount sent in trust game, disaggregated by treatment and the abduction history of subjects' sons.
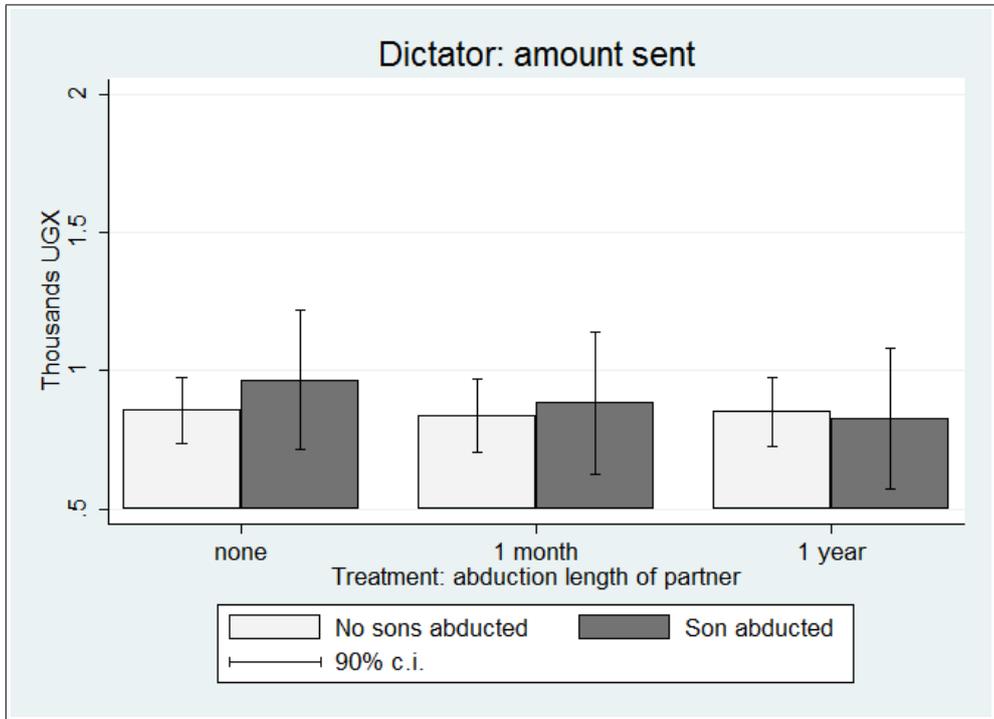
Figure II: Amount sent in dictator game, disaggregated by treatment and the abduction history of subjects' sons.

TABLE I

Summary Statistics: Mean (s.d.)

Sample:	Receivers	Senders
	(1)	(2)
Panel A: Experimental outcomes:		
Trustworthiness: average % returned[a]	34.89 (23.39)	
Expected trust: belief of Sender's transfer in trust game (ths UGX)	1.38 (0.61)	
Expected altruism: belief of Sender's transfer in dictator game (ths UGX)	1.23 (0.72)	
Trust: transfer in trust game (ths UGX)		1.12 (0.64)
Altruism: transfer in dictator game (ths UGX)		0.86 (0.75)
Expected trustworthiness: belief of average % returned		0.60 (0.30)
Panel B: Conflict Experience		
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)		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/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 a soldier (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 (d)[b]	0.52 (0.50)	
–participated in cleansing ceremony (d)[b]	0.31 (0.46)	
Passed through reception center (d)[b]	0.48 (0.50)	
Panel C: Personal Characteristics		
Age	24.45 (4.89)	43.08 (6.10)
Female (d)		0.56 (0.50)

(Continued)

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)
Number of household members	6.92 (4.83)	8.11 (3.56)
Cash earned in past 7 days (ths 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

[a]Average 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. [b]Results shown for sub-sample of ex-abductees. [c]Index of violence-related dummy variables, elements of index listed below in italics. [d]1st principal component constructed from count of household assets, including: jerry cans, wash basins, bicycles, mattresses radios, plates, livestock, chairs, mobile phones and plows. [e]Risk 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 II
Abduction by the LRA and Trustworthiness

Dependent variable:	Trustworthiness: Average percentage returned in trust game			
Sample	*All Receivers*			
	(1)	(2)	(3)	(4)
Abduction length (years)	1.36***		2.36***	
	(0.34)		(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 receieved (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	-7.38*	-6.44	-4.82
	(3.92)	(3.96)	(4.12)	(4.37)
Age	0.04	0.03	0.11	-0.03
	(0.41)	(0.41)	(0.43)	(0.44)
Birth order	0.05	0.18	0.11	-0.29
	(0.59)	(0.60)	(0.60)	(0.53)
Sisters	-1.01	-1.04	-1.02	-0.45
	(0.77)	(0.80)	(0.79)	(0.74)
Brothers	0.12	0.03	0.00	0.06
	(0.64)	(0.65)	(0.65)	(0.65)
Mother no school	1.28	1.91	1.58	2.44
	(3.09)	(3.27)	(3.17)	(2.89)
Father no school	-1.64	-2.16	-1.56	-2.26
	(2.57)	(2.62)	(2.54)	(2.88)

(Continued)

Wealth	1.20***	1.30***	1.23***	1.32***
	(0.38)	(0.38)	(0.37)	(0.41)
Log of weekly income	-0.11	-0.04	-0.12	-0.20
	(0.28)	(0.28)	(0.29)	(0.29)
Number of current HH members	0.36*	0.37*	0.33*	0.29
	(0.19)	(0.21)	(0.18)	(0.22)
Literate (d)	6.83*	5.98	6.47*	5.81*
	(3.45)	(3.56)	(3.33)	(3.30)
Schooling (years)	-0.33	-0.39	-0.34	-0.30
	(0.56)	(0.57)	(0.56)	(0.55)
Partner in experiment male (d)	3.44	3.56	2.93	3.29
	(2.54)	(2.56)	(2.50)	(2.23)
Constant	31.15***	30.36***	30.24***	33.41***
	(10.52)	(10.18)	(10.83)	(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 decesions 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, receviers could return 0-3 ths respectively.

TABLE III

Abduction by the LRA and Expected Trust and Altruism

Dependent variable:	Expected trust: belief of Sender's transfer in trust game		Expected altruism: belief of Sender's transfer in dictator game	
Sample:	All Receivers			
	(1)	(2)	(3)	(4)
Abduction length (years)	0.00		-0.03	
	(0.01)		(0.02)	
Abduction length over 1y (d)		0.06		-0.15
		(0.10)		(0.13)
Abduction length 2m-1y (d)		-0.02		0.13
		(0.13)		(0.12)
Abduction length <2m		0.12		0.02
		(0.08)		(0.11)
Constant	1.36***	1.35***	1.06**	1.09***
	(0.29)	(0.30)	(0.39)	(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 (ths UGX). In Columns 3-4 the dep. var. is the amount Receivers expected to be transferred by Senders in the dictator game (ths UGX). 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 IV

Behavior Towards Former Soldiers: Trust and Altruism

Dependent variable:	Trust: transfer in trust game				Altruism: transfer in dictator game	
Sample	Senders					
	All		No sons Abducted	Son Abducted	All	
	(1)	(2)	(3)	(4)	(5)	(6)
LRA-long treatment	0.09	-0.04	-0.03	0.53**	-0.00	0.02
	(0.08)	(0.10)	(0.10)	(0.22)	(0.11)	(0.12)
LRA-short treatment	0.07	0.01	0.01	0.36	0.01	0.02
	(0.10)	(0.13)	(0.13)	(0.27)	(0.10)	(0.12)
LRA-long t. x Son abducted		0.50**				-0.08
		(0.21)				(0.29)
LRA-short t. x Son abducted		0.29				-0.04
		(0.24)				(0.23)
Son abducted	0.12	-0.14			0.13	0.17
	(0.08)	(0.12)			(0.10)	(0.17)
Constant	0.49*	0.44	0.73**	-0.70	0.87**	0.87**
	(0.28)	(0.27)	(0.32)	(0.64)	(0.41)	(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%. Dependent variables in ths UGX. 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 V

Beliefs about Trustworthiness of Former Soldiers

Dependent variable:	Investment: Difference between trust and dictator transfers (Ths. UGX)			Expected back-transfer in trust game: directly elicited 1st-order beliefs (percent, pooled)		
Sample	Senders					
	All	No sons abducted	Sons abducted	All	No sons abducted	Sons abducted
	(1)	(2)	(3)	(4)	(5)	(6)
LRA-long treatment	0.08	-0.05	0.75***	0.03	0.00	0.16*
	(0.10)	(0.10)	(0.27)	(0.04)	(0.04)	(0.09)
LRA-short treatment	0.07	-0.01	0.36*	0.05	0.04	0.11
	(0.08)	(0.10)	(0.21)	(0.04)	(0.04)	(0.11)
Son abducted	-0.02			0.00		
	(0.10)			(0.05)		
Constant	-0.46	-0.41	-0.81	0.65***	0.68***	0.58*
	(0.41)	(0.48)	(0.82)	(0.14)	(0.17)	(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 A: Additional Tables and Figures
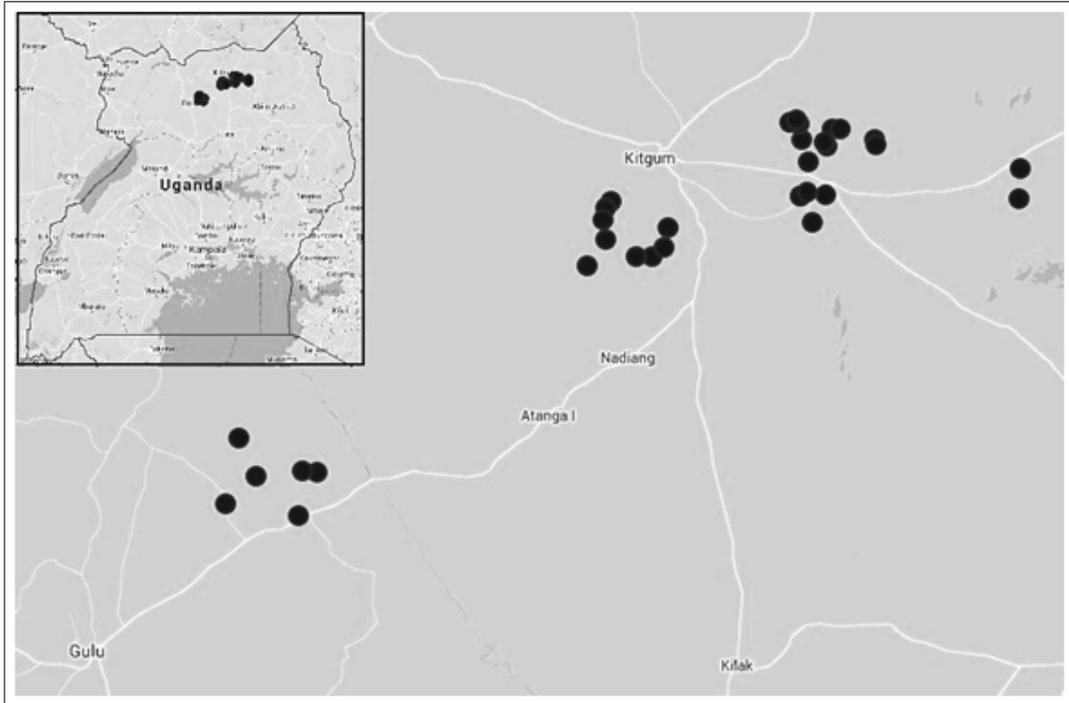
Figure A.1: Location of villages in which experimental sessions were conducted in Gulu and Kitgum districts, northern Uganda.

Figure A.2: Group explanation of experimental task.

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

Estimator:	OLS							Probit
Sample	*Receivers*							
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	0.12	-0.02	-0.06	-0.02	0.29	-0.06	0.02
	(5.79)	(0.36)	(0.18)	(0.24)	(0.19)	(0.23)	(0.22)	(0.15)
Constant	23.64	3.32**	-0.74	2.85***	1.12	4.30***	0.99	-3.06
	(23.32)	(1.48)	(0.70)	(0.93)	(0.88)	(1.04)	(0.62)	(1.36)
Observations	182	177	182	182	182	182	172	181
(Pseudo) R-squared	0.21	0.45	0.24	0.23	0.26	0.20	0.26	0.12

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 A.2

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 when 2 ths and 1 ths sent	
Sample:			*Receivers*					
	All	*Returned positive amount*	*All*					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	
Abduction length (years)	0.01*	0.83**	0.02	-0.00	1.70**	1.03	0.68	
	(0.01)	(0.31)	(0.01)	(0.02)	(0.75)	(0.62)	(1.19)	
Constant	1.68***	35.17***	-1.42**	0.13	31.45**	-1.42**	0.60	
	(0.57)	(0.40)	(0.56)	(0.40)	(11.57)	(0.56)	(6.08)	
Observations	328	279	328	328	328	328	328	
(Pseudo) R-squared	0.13	0.05	0.06	0.05	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 A.3

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
Sample:	*All Receivers*			
	(1)	(2)	(3)	(4)
Length of abduction (years)	0.11*	-0.02*	-0.25***	0.16
	(0.06)	(0.01)	(0.06)	(0.15)
Age	0.01	0.00	-0.15***	0.09
	(0.01)	(0.00)	(0.03)	(0.06)
Birth order	0.03	0.02***	-0.06	0.25
	(0.04)	(0.04)	(0.07)	(0.15)
Sisters	-0.01	-0.02*	0.22**	-0.06
	(0.05)	(0.01)	(0.09)	(0.20)
Brothers	0.05	-0.01	0.05	0.11
	(0.04)	(0.01)	(0.06)	(0.17)
Mother no school	-0.47***	0.04	0.05	-1.03
	(0.16)	(0.04)	(0.30)	(0.74)
Father no school	-0.09	-0.00	-0.62*	-0.19
	(0.16)	(0.06)	(0.31)	(0.79)
Constant	1.28***	-1.60***	10.65***	-1.64
	(0.36)	(0.47)	(0.71)	(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 A.4

Robustness Check on Mis-reporting of Abduction Status

Dependent variable:	Trustworthiness: Average % returned in trust game	
Sample	*Receivers*	
	All	*Sub-sample of those whose abduction status matches reports of other hh member*
	(1)	(2)
Abduction length (years)	1.36***	1.50***
	(0.34)	(0.47)
Constant	31.15***	26.50***
	(10.52)	(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 A.5

Pre-existing Family Characteristics
and Abduction Length

Dependent variable:	Abduction length (years)	
Sample	*Receivers*	
	All	*Ex-abductees*
	(1)	(2)
Age	0.03	0.74***
	(0.02)	(0.06)
Birth order	-0.11*	0.09
	(0.06)	(0.14)
Sisters	0.09	0.01
	(0.08)	(0.18)
Brothers	0.00	0.02
	(0.05)	(0.14)
Mother no school	0.02	-0.02
	(0.20)	(0.51)
Father no school	0.04	0.38
	(0.23)	(0.42)
Constant	-0.02	-4.19***
	(0.52)	(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 A.6

Randomization Check

Sample	Mean (sd)						F-test p-value
	Partner's length of abduction (treatment condition)						
	Senders						
	No-LRA t.		LRA-short t.		LRA-Long t.		All
	(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
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
Index of violence witnessed during LRA conflict: (0-4)[a]	2.04	(1.40)	1.76	(1.32)	1.94	(1.37)	0.27
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. [a]Index of violence-related dummy variables, for elements, see Table I. [b]Risk 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. [c]1st principal component constructed from count of household assets, including: jerry cans, wash basins, bicycles, mattresses radios, plates, livestock, chairs, mobile phones and plows.

TABLE A.7

Effects of Receiver's Abduction on Sender's Decisions–Ordered probit

Dependent variable:					Altruism: the amount transferred in dictator game (ths UGX)	
	Trust: the amount transferred in trust game (ths UGX)					
Sample	Senders					
	All		No sons abducted	Son abducted	All	
	(1)	(2)	(3)	(4)	(5)	(6)
LRA-long treatment	0.14	-0.06	-0.06	1.08**	0.01	0.03
	(0.15)	(0.18)	(0.18)	(0.42)	(0.16)	(0.19)
LRA-short treatment	0.13	0.01	0.01	0.74	0.03	0.04
	(0.19)	(0.23)	(0.23)	(0.53)	(0.16)	(0.19)
LRA-long t. x Son abducted		0.96**				-0.12
		(0.41)				(0.44)
LRA-short t. x Son abducted		0.55				-0.04
		(0.46)				(0.35)
Son abducted	0.23	-0.26			0.21	0.26
	(0.15)	(0.23)			(0.16)	(0.25)
	(0.06)	(0.06)	(0.06)	(0.18)	(0.05)	(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 A.8

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

Dependent variable:	Trust: the amount transferred in trust game (ths UGX)					
Sample	Senders					
	(1)	(2)	(3)	(4)	(5)	(6)
LRA-long treatment.	-0.03	0.07	0.07	0.10	0.05	0.02
	(0.10)	(0.08)	(0.17)	(0.21)	(0.18)	(0.25)
LRA-short treatment.	0.00	0.06	0.18	0.13	0.08	0.13
	(0.13)	(0.10)	(0.13)	(0.21)	(0.13)	(0.20)
Son abducted	-0.13					-0.13
	(0.12)					(0.14)
LRA-long t. x Son abducted	0.49**					0.53**
	(0.21)					(0.23)
LRA-short t. x Son abducted	0.28					0.30
	(0.24)					(0.28)
Daughter abducted		-0.12				-0.09
		(0.30)				(0.31)
LRA-long t. x daughter abducted		-0.02				-0.04
		(0.51)				(0.56)
LRA-short t. x daughter abducted		0.01				-0.08
		(0.50)				(0.58)
Violence received (index)			0.01			0.02
			(0.03)			(0.04)
LRA-long t. x violence received			-0.05			0.01
			(0.04)			(0.05)
LRA-short t. x violence received			0.00			-0.08*
			(0.05)			(0.04)
Violence against family (index)				0.04		0.03
				(0.07)		(0.07)
LRA-long t. x violence family				-0.05		-0.09
				(0.12)		(0.10)
LRA-short t. x violence family				-0.02		-0.04
				(0.11)		(0.12)
Violence witnessed (index)					0.01	-0.02
					(0.04)	(0.05)
LRA-long t. x witnessed					0.01	0.03
					(0.06)	(0.06)
LRA-short t. x witnessed					-0.01	0.07
					(0.05)	(0.08)
Constant	0.47*	0.42	0.42	0.39	0.42	0.41
	(0.24)	(0.26)	(0.28)	(0.26)	(0.29)	(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 A.9

Predictors of Having a Son Abducted

Dependent variable:	At least one son abducted	
Sample	*Senders*	
	(1)	(2)
Violence received (index)		-0.01
		(0.05)
Violence against family (index)		0.09*
		(0.04)
Violence witnessed (index)		0.02
		(0.02)
Risk scale	0.00	0.00
	(0.02)	(0.02)
Age	0.02***	0.02***
	(0.00)	(0.00)
Female	0.15***	0.17***
	(0.03)	(0.04)
Married	-0.08	-0.36*
	(0.06)	(0.06)
Wealth	0.02*	0.02**
	(0.02)	(0.04)
Log of income	0.00	-0.00
	(0.00)	(0.00)
Number of hh members	0.00	-0.00
	(0.01)	(0.01)
Constant	-4.09***	-4.80***
	(0.68)	(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%.