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

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Abstract

We use a set of experiments to study the effects of forced military service for a rebel group on social capital. We examine the case of Northern Uganda, where recruits did not self-select nor were systematically screened by rebels. We find that individual cooperativeness robustly increases with length of soldiering, especially among those who soldiered during early age. Parents of ex-soldiers are aware of the behavioral difference: they trust ex-soldiers more and expect them to be more trustworthy. These results suggest that the impact of child soldiering on social capital, in contrast to human capital, is not necessarily detrimental. (JEL C93, D03, D74, O12)

In conflicts around the world, the 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

[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 (Human Rights Watch 2008).

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 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). 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. To understand the motivations behind differences in behavior, one would like to know (i) the legacy of soldiering on individual preferences and beliefs about the behavior of others and (ii) whether community members treat former soldiers differently compared to their peers 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 policy as well as behavioral implications, for instance, for understanding whether individual preferences adapt in response to key experiences.

This paper contributes to the existing literature by combining survey methods with experimental tools, in order to study such detailed aspects underlying reintegration outcomes. The second distinguishing feature is our focus on social capital—trust and cooperative behavior—a crucial factor since it determines 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

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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, Ostrom and Walker 2011), financial development and trade (Guiso, Sapienza and Zingales 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, Engel and Kosfeld 2010) and influence participation in public life and politics (Bowles and Gintis 2002).

thousands of youth (> 25 percent in the most affected areas) during a war that lasted for 20 years. Most of these soldiers later returned to their communities. The advantage of choosing Uganda for studying the legacy of soldiering is that at the point of recruitment soldiers were not a select group, in comparison to their peers, in contrast to ex-fighters in most of the other post-conflict societies. In particular, we build on the previous of evidence of Blattman and Annan (2010) and argue that the LRA recruitment methods created exogenous variation in conscription, i.e. not affected by self-selection and screening by the armed group.

We conducted a large-scale experimental data collection (N=688) on two groups of individuals: a representative sample of villagers between 35-55 years, who were not abducted by the LRA and 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. The experiments were designed to examine (i) cooperative behavior of former soldiers in comparison to their peers, (ii) whether soldiering during early age leaves a deeper mark than soldiering later on in life, and (iii) how members of the community differentiate their behavior towards former soldiers and why.

A priori, it is not clear whether and how soldiering affects cooperative behavior, due to paucity of data for this difficult-to-access subject pool. 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, recent theories emphasize the important role that lethal conflict between groups and other external threats have likely played in the development of parochial altruism (Choi and Bowles 2007, Bowles 2008). 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.[3] The most closely related empirical evidence for a link between soldiering and pro-

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[3] In line with the theory, recent behavioral experiments implemented among victims of war-related violence have revealed that greater exposure to violence reduces selfishness and increases pro-social behavior (Voors, et al. 2012, Gneezy and Fessler 2012, Bauer, et al. 2014). 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

social behavior comes from the detailed survey work of Blattman (2009), who shows that forced recruitment by rebels in Northern Uganda leads to greater likelihood of voting.[4] While this evidence is consistent with greater willingness to help and participate in local collective action, it may also indicate distinct political interests, which may not benefit others.

Similarly, little is known about whether receiving communities discriminate against former soldiers.[5] 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 (Heider 1958, Weiner 1995, Gneezy, List and Price 2012), which proposes that the controllability of an action or stigma affects the likelihood that one is subject to helping or punishing behavior.

Our experimental design and main findings can be summarized as follows. First, we investigate cooperative behavior of former soldiers compared to their peers. We conducted a trust game (similar to Berg, Dickhaut and McCabe, 1995) in which a member of the receiving 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' who differed in terms of their LRA experience. 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. Age of soldiering matters: the effect of soldiering on trustworthiness is strong for ex-soldiers who were abducted at early age (below 14 years of age) and mute for participants who were abducted during late adolescence or adulthood.

Leone and Rohner, Thoenig and Zilibotti (2013), who show a link between living in areas with more intense fighting, and less self-reported trust and stronger ethnic identity in Uganda. However, these papers 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.

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

[5] Studies designed to separate taste-based discrimination and statistical discrimination are still relatively rare. Important exceptions are Fershtman and Gneezy (2001), List (2004) and Gneezy, List and Price (2012).

The observed increase in cooperative behavior of former child soldiers is economically important and it is robust to controlling for a large set of observable characteristics, village fixed effects or to replacing experimental measures of cooperative behavior with survey-based proxies. While the results are most consistent with the impact of soldiering on behavior, we also explicitly analyze whether the effect could be attributed to selection caused by higher mortality among uncooperative soldiers.

As a next step, we investigate trust towards former soldiers. 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 belief.

Fourth, we conducted a dictator game, in which the Sender decides how to allocate money between himself and the Receiver.[6] The Receiver is passive in this game and thus any effect of the knowledge about the LRA history of the Receiver can be attributed to taste-based discrimination or to social norms. We find no differences in the amount transferred, indicating that former soldiers face neither taste-based discrimination nor favoritism.

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

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[6] 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.

peers. Section IV presents results about differential treatment of former soldiers by their communities. Section V concludes.

I.   A Short Background on Soldiering in Northern Uganda

The leader of the Lord's Resistance Army (LRA), Joseph Kony, led a group of Acholi fighters from the North of the country against the government from 1987 to 2006. Kony claims to seek a spiritual cleansing of Uganda and overthrow of the government. The LRA has never, perhaps with the exception of an initial period from 1986-9, enjoyed support from the local Acholi population due to its brutality and few realistic goals (Allen 2010). 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 to join the group.

The LRA attacks and abductions escalated dramatically after 1996, when Sudan started to supply Kony with weapons and provided territory to build bases. Exposure to violence in Kitgum and Gulu districts (which we study) was widespread, affecting virtually the entire population.[7] In 2005, around 90 percent of the adult population in Gulu and Kitgum districts had been displaced, 67 percent had witnessed a child abduction and 48 percent had witnessed a family member killed (Vinck, et al. 2007). 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 camps for displaced people had been closed, and the majority of the population had returned to their home villages.

An estimated 24,000-38,000 child soldiers and 28,000-37,000 adults were forcibly recruited by the LRA (Vinck, et al. 2007). In the districts we study the large scale and seemingly indiscriminate abduction concerned around one quarter of youth aged 18-35 in 2011(the year of our study). Youth were taken by groups of ten to twenty rebels during night raids on rural homes (Beber and Blattman 2013). The LRA has demonstrated a preference for adolescent conscripts, and youth under age 11 and over 24 were rarely taken, with the highest abduction rate at around 14 years of age. Using a representative sample of youth who were born between 1975-1991,

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

(Blattman and Annan 2010) show that, except age, no individual or household characteristic predicted the likelihood of conscription. The strategy of targeting youth is typically explained by the fact that younger combatants follow orders more readily and are more receptive to the LRA propaganda.

While with the LRA, abductees went through a period of training and indoctrination. 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). Two thirds were forced to commit a crime or violence and a fifth were forced to murder soldiers, civilians and sometimes family members in order to dissuade them from escaping. 84 percent of abductees eventually left the LRA by escaping, and a smaller percentage was rescued or released (Blattman and Annan 2010). Around 1 percent of abductees were thought to be still with LRA in 2010. The remainder perished. To deal with the influx of returning former soldiers, reception centers were set up by government agencies and NGOs. Annan, Blattman and Horton (2006) estimate that 95 percent of former abductees returned to their home communities.

In terms of social behavior, some authors and the media have emphasized the "damaged" nature of ex-soldiers and their difficulty re-assimilating into society after spending time under the vastly different normative environment of the LRA (Vermij 2011). However, Blattman (2009) and Blattman and Annan (2010) find ex-abductees to be surprisingly resilient and they show the negative impacts of LRA soldiering on human capital and employment.

II. Experimental Design

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 randomly selected 33 out of 52 villages (Appendix Figure A.1).[8]

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[8] This initial list of villages was derived from a list of communities known to be affected by LRA abduction from Pham, Vinck and Stover (2007).

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 who were extremely unlikely to be soldiers in the LRA) 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-soldiers 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 percent) and Receivers (91 percent for former soldiers and 87 percent 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.

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[9] 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, $S \in \{0; 1000, 2000, ... 3S\}$. 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 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 or adhere to sharing norms[10], they may transfer positive amounts.

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[9] 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.

[10] See List (2007) or Lazear, Malmendier and Weber (2012) for thoughtful experiments on whether dictator game allocations are motivated by social preferences or social pressure.

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 percent of people in the area we study spent time in IDP camps, this information should not convey anything meaningful about the anonymous partner. 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

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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."

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 percent probability and nothing with a 50 percent 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.

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 positive 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]

C. Survey Data

A large part of the survey instrument was the same for Senders and Receivers, and included

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

[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.

[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.

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 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, in which economists and psychologists specifically tested how to ask sensitive questions about abduction-related experiences in a non-intrusive way (Annan, Blattman and Horton 2006). Key variables are described in Table 1.[16]

D. Procedure and payments

To ensure understanding of tasks, 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),[17] and extensively used visual aids, to illustrate options and payoffs (see Appendix Figure A.2). After a group explanation stage, subjects were called individually and 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 percent of Senders and 0.3 percent 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 minimize the chance of communication between participants.

We took several steps to increase the level of anonymity when making choices. Senders knew they were not matched with Receivers from the same village (and vice versa). This was to minimize the role of strategic considerations due to potential impact of future (outside the lab) interactions, including potential fear of reprisals from former soldiers. Next, subjects made

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[16] For a detailed description of variables on exposure to violence and abduction experience, see Table A.1.

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

decisions behind cardboard dividers to keep their choices private from the experimenter who provided the one-on-one explanation.[18]

Subjects were paid for either the trust or dictator game, based on flipping a coin. The payment was made in private, one by one, at the same location as the experimental sessions two days after the experiments. 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).

III.   Behavior and Beliefs of Former Soldiers

A. Predictors of abduction

Studying the impact of soldiering with cross-sectional data after the conflict is challenging due to several identification problems. In this section we discuss whether it is plausible to consider selection into the armed group as exogenous—without self-selection and the screening of recruits by rebels, arguably the two most common concerns in this type of study. In particular, a legitimate concern is that current differences in cooperative behavior are results of prewar traits that lead to selection into the rebel group. First, since the LRA's killings in 1991 destroyed the little remaining support which the group had and since after this recruitment was only in the form of forced abduction (Blattman and Annan 2010, Allen and Vlassenroot 2010), self-selection is unlikely. The median year of abduction in our sample is 2001 and there are only two ex-soldiers who were abducted prior to 1991 (excluding these subjects does not affect our findings). Furthermore, in the analysis we distinguish between those who were abducted at later age (14 and above) and those abducted earlier. In fact, the main result is driven by those abducted at early age, arguably the group for which self-selection is extremely unlikely.

Second, our motivation for choosing to study the Ugandan conflict is the existence of uniquely detailed evidence on LRA recruitment practices, which suggests the LRA has not

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[18] 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.

abducted, either deliberately or by chance, a select group. Most importantly, using a large and representative sample of youth who were born prior to the conflict (1975-1991), Blattman and Annan (2010) show that a pre-war level household characteristics do not predict the likelihood of abduction, in contrast to other civil wars in Africa (e.g., Humphreys and Weinstein, 2007). We arrive to similar conclusions in Table 2 using our data, where we study predictors of being abducted, age of abduction as well as abduction length. As in Blattman and Annan (2010), the only strong predictor is year of birth, which is intuitive, given that abduction rates varied in different phases of the conflict.[19]

The lack of systematic correlations between observable pre-existing family (family size, education of father and mother, parental death) and abduction also accords with qualitative descriptions. As described in greater detail in Blattman and Annan (2010) rebels typically invaded villages and homesteads at night, abducting all civilians who could carry loot. Officers were instructed to release young children and older adults, but to keep all adolescent and young adult males. Accordingly, the most common age of abduction is around 14 years. Given the short interval between the attack and abduction, LRA soldiers had little chance to assess the character of potential recruits, and therefore it is unlikely that ex-soldiers were selected for their level of trustworthiness *at the outset*. It should be noted, however, that this does not imply that that other forms selection, after abduction, did not play a role and we return to the issue in the robustness checks section.

B. *Trustworthiness*

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

$$D_i = \alpha + \beta A_i + \gamma X_i + \varepsilon_i \qquad (1)$$

where D_i is individual i's action in the experiments, A_i is a variable capturing a soldiering

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[19] The only other variable which is marginally significant predictor of being abducted is having a mother who had some schooling (p = 0.08). At the same time the variable predicts neither length of abduction nor age of abduction.

experience, X_i is a range of individual characteristics,[20] and ε_i is the error term. Standard errors are clustered at the village level.

We measure trustworthiness as the percentage returned in the trust game. Participants made two conditional choices (using strategy method), deciding how much to return both in case a Sender transferred 1,000 UGX (and the Receiver would get 3,000 UGX) and when a Sender would transfer 2,000 UGX (and the Receiver would get 6,000 UGX). The percentage sent back by Receivers is very similar in both cases: 34 percent and 35 percent on average, respectively. Thus, in the main analysis we use the average of these two amounts.[21]

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 3 we find a strong positive relationship between length of soldiering and the amount returned in the trust game (p-value<0.001). We arrive to similar conclusion when allowing for non-linearity in the relationship and comparing four groups, based on abduction length: participants who were not abducted at all, those who were abducted for up to two months, those who were abducted for 2-12 months period, and those who were with LRA for more than a year (column 2). Even those who were abducted for a relatively short period return more compared to those who were not abducted (p-value=0.18). The difference, compared to the non-abducted group, further increases for those who spent the longest period with LRA (p-value<0.05). Greater wealth, number of household members, and literacy are also positively associated with returning more.

Next, we test whether the effect of soldiering on trustworthiness is more pronounced when experienced during an early age, compared to soldiering during adolescence and adulthood. Previous experimental evidence among young children consistently shows that social preferences develop steeply during the age range between 3 to around 13 years (Fehr, Bernhard and Rockenbach 2008, Almås, et al. 2010, Fehr, Rutzler and Sutter 2011, Bauer, Chytilová and Pertold-Gebická 2014), suggesting this a sensitive period in development of social preferences.

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

[21] Given that the amount received is tripled, this number implies that participants returned a slightly higher amount than what was sent. The level of trustworthiness in our sample is similar to that observed in comparable studies. A recent survey finds that the average proportion sent back in trust game was 38% in Europe, 34% in North America, and 32% in Africa (Johnson and Mislin 2011).

The evidence is scarce and less conclusive about the period after 13 years—two studies that include both young children as well as adolescents suggest that development of fairness motivations plateaus after the age of 13-14 years (Almås, et al. 2010, Fehr, Rutzler and Sutter 2011).[22] We exploit the fact that the age of abduction ranges in our sample from 6 to 30 and the median age of first abduction is 14.

We find that age of abduction matters: the effect of abduction is strong for those who were abducted when younger than 14 years of age and mute for those abducted at a later age. Column 3 of Table 3 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.

In Column 4 we show the main pattern by simply comparing three groups of subjects: those who were abducted at early age (less than 14), those who were abducted at later age and those who were not abducted (omitted). We find that those who were abducted young transfer back 8.2 percentage points more compared to the non-abducted group. This represents an economically important increase—in case 2,000 UGX is sent, an increase of this size represents around 529 UGX, which is approximately twice the average daily cash income in our sample. At the same time, we find virtually no difference in behavior between those who soldiered during later age and the non-abducted group.[23]

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[22] There is also some related evidence from psychology literature. For example Kohlberg (1976) argues, based survey data from on how children reason about a series of hypothetical moral dilemmas, that there is an important shift around age 12, when children begin to move away from seeing moral behavior as a matter of obeying rigid rules and towards seeing it as being a matter of fulfilling obligations to others.

[23] Table A2 supports these findings by providing more detailed classification. We divide the ex-soldiers into 7 groups, based on age of abduction (<10, 10-11, 12-13, 14-15, 16-17, 18-19, >20). Although the results are less significant statistically given the low number of observation for each of these groups, we find that, when compared with the non-abducted group, there are large positive coefficients for each of the three groups with the lowest age of abduction (<10, 11-12, 12-13), while the coefficients turn out to be very small for all the groups with the higher age of abduction.

Observation 1: The longer a person was with the LRA, the higher is the amount sent back in the trust game. 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 mute for those who were abducted during later adolescence and adulthood.

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. For instance, when Receivers were faced with the decision of how to allocate 3,000 UGX, they could achieve an equal distribution by sending back 1,000 (by sending 1,000 Senders kept 1,000 of their endowment). We find no link between length of soldiering and prevalence of choosing the equal split (columns 1-4 of Appendix table A.3), 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 Receivers expected to receive 1,380 UGX (as discussed in greater detail in the next sub-section) and thus, it is likely that Receivers considered receiving 2,000 UGX a kind act from Senders, while receiving 1,000 UGX was considered a neutral (or perhaps slightly unkind) act. Therefore, if the greater amount returned by abductees is due to a higher degree of reciprocity, we should observe a greater difference in the proportion sent back when a Sender sends 2,000 UGX compared to when 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 (Appendix table A.3). 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.

LRA soldiering captures a host of experiences. We test whether those who soldiered during early age were more exposed in terms of various initiation practices (prayer ceremonies, beating others, receiving beating, forcing to kill), different types of violence (violence received, violence

witnessed, violence against family, violence committed) or reintegration programs and informal reintegration ceremonies. As expected, soldiering is positively related with indices of violence received, committed or witnessed by an individual. The exception is the index of violence against other family members (Panel A Table A.4). Interestingly in light of previous findings, LRA experiences also seem to be systematically related to age of abduction. We find that those who were abducted at earlier age report more exposure LRA prayer ceremonies, receiving more violence and they also were more likely to be forced to beat and kill others while with LRA, compared to those who were abducted at later age (Panel B of Table A.4). We also find that they were more likely to participate in informal reintegration ceremonies[24] after returning, but we find no difference in exposure to formal reintegration programs via reception centers.

Robustness checks

We now report a series of robustness analyses of the main finding: the link between soldiering in childhood and cooperative behavior in adulthood. First, the main results are not driven by the few ex-soldiers who were with LRA for many years, as can be also seen from column 4 of Table 3. Also, excluding subjects who were with the LRA for more than five years does not affect the main findings (columns 1-2, Appendix Table A.5). Second, the correlation between soldiering and cooperative behavior is robust to using different regression specifications and 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 (Table 3), excluding the control variables which may have been affected by soldiering, or including village fixed effects (Appendix Table A.6). Third, in Section III.A we argued that there was virtually no screening of recruits by LRA, especially those around the age of 14, which was LRA's "preferred" age for recruits, while there might have been some systematic screening of those below 10 years and adults. As a robustness check, we exclude all ex-soldiers who were younger than 11 years or older than 17 years at the time of their abduction. Columns 3-4 of Appendix Table A.5 show that the main findings hold when comparing this sub-sample of ex-soldiers to

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[24] 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)).

non-abducted peers, in line with the interpretation that LRA screening at the outset is unlikely to drive our results.

Next, we explore whether differences in experimental choices mimic systematic differences in behavior in the naturally occurring world. We replace the experimental measure with survey-based proxies for cooperative behavior and find strikingly similar patterns. We find a positive link between length of abduction and an index of participation in local community groups and a negative correlation between abduction length and the likelihood of having a physical fight in the past six months (Appendix Table A.7). Age of abduction also matters: the effects of soldiering on the survey-based proxies of cooperativeness are greater for those who soldiered during early age.

Last, 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 a 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. The results are robust to excluding the participants, whose self-reports of abduction status did not match the reports of the other household members (columns 5-6 in Appendix Table A.5).

C 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 columns 1-3 of Table 4 we find virtually no link between abduction length and the amount that was expected to be received in trust game. In column 4 we find a small negative relationship between length of abduction and expected allocation in dictator game, but the relationship is not statistically significant. Although the relationship is somewhat stronger for those abducted at early age (column 5), it seems to be driven solely by ex-soldiers with the longest abduction length, as is clear from column 6, which shows that the correlation between being abducted below 14 and expected kindness is small and insignificant statistically.[25]

Observation 2: 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.

D. Alternative explanations

Here we discuss alternative mechanisms which could explain the observed heightened cooperative preferences of former soldiers compared to their peers. The first possibility is a behavioral change caused by soldiering experience. The evidence documented in this section is consistent with the idea that social 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, Fehr, Bernhard and Rockenbach 2008, Cunha, et al. 2006), such a change in preferences may have long-term effects and persist into adulthood.

Alternatively, former child soldiers may be more pro-social towards others in order to

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

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, since 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 percent (18 percent) of former soldiers report that they were ever blamed by their family (by other people in their community) for things they have done while with LRA 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).

While the conflict in Northern Uganda represents a unique opportunity to study the effects of soldiering without the conscious self-selection and systematic screening in the recruitment stage that is at work in many other civil wars, as discussed in Section III.A, there are still several ways in which personal characteristics could have influenced the length of time spent with the LRA, surviving the conflict, and finally returning and staying home. We now consider whether these mechanisms could explain the full set of findings.

Personal characteristics—including trustworthiness—might have affected how LRA soldiers were treated by commanders after forcible recruitment. In particular, non-cooperative individuals may have been more severely punished or given more dangerous assignments, which could have resulted in higher mortality and thus underrepresentation in our sample. Annan, Blattman and Horton (2006) estimate that 15 percent of ex-abductees did not return and can be presumed dead. We use the sensitivity analysis proposed by Lee (2008) and calculate bounds of the effect of soldiering, taking into account selective survival. We trim the distribution of the outcome variable in the group with less attrition (the non-abducted) and we drop 15 percent of the most selfish individuals. The results imply that under such implausible dramatic selection, LRA soldiering during childhood still increases the cooperative behavior, but the effect is small and not statistically significant (Appendix Table A.8). Thus, we cannot rule out that the effect of LRA abduction on higher trustworthiness in the population is caused by higher mortality among uncooperative 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, Blattman and Horton (2006) estimate that around 95 percent 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.

IV.   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 trust-worthiness. The average age of the Senders is 43 years and 56 percent 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.9).

A. Trust

On average, Senders transfer 56 percent of their 2,000 UGX endowment to the Receivers in the trust game.[26] 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 5) 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).

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

Figure 1 and columns 2-4 of Table 5 reveal a sharp difference in the effects of the LRA treatments on the sub-sample of those who have a formerly abducted son and those who do not. For the sub-sample of participants with no abductee sons, there is no significant difference between trust allocations in the three LRA treatments. In contrast, those who do have sons that were abducted send more when playing with an ex-soldier in both the LRA 1 month and LRA 1 year treatments. Compared to the control group, they sent 360 UGX (p-value=0.20) more to the LRA 1 month group and 530 UGX more to the LRA 1 year group (p-value=0.02). Put differently, while we find no difference in trust towards the non-abducted (control) group between those who had a son abducted and those who did not, we find positive interaction effects between having an abducted son and the LRA treatments on trust.[27]

Observation 3: 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 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, Bohnet and Piankov 2006, Fehr 2009, Sapienza, Toldra-Simats and Zingales 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

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[27] 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.10).

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

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 affected 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 or different norms how to treat ex-soldiers and others. 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 percent 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 5). We also find little differences in terms of distribution of the amount sent (available upon request). Further, Figure 2 compares the mean amount sent across LRA treatments, separately for the participants with and without an abducted son. We observe virtually no effect of LRA treatments in either of these two groups. This is confirmed by the regression analysis in column 6, where we find no interaction effect between having an abducted son and LRA treatments.

The effect of LRA treatments on dictator game allocations is both very small in magnitude and statistically insignificant. This 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 percent level. This is equivalent to 0.25 standard deviations in our sample.[28] We thus conclude that there is no evidence of differences in kindness towards ex-soldiers and non-soldiers.

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[28] Calculated using a power of 0.80 and a significance level of 0.05. The intraclass correlation within villages for dictator allocations is 0.00871.

Observation 4: 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, the results do not suggest any negative attitudes or unfavorable social norms governing interactions with former soldiers.

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.[29] 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 transferred in the trust game and what was voluntarily given in the triple dictator game, using an approach proposed in Cox (2004).[30] 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—the amount transferred in the trust game minus the amount sent in the dictator game—(Table 6, column 1) as well as the percent expected to be transferred back (column 4). Importantly, we do find a strong interaction effect between LRA treatments and having had a son abducted. For participants who have an abducted son, the difference in the amount sent in the trust game and in the dictator game increases by UGX 360 in the LRA 1 month treatment and by UGX 750 in the LRA 1 year treatment (column 3). The magnitude of

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[29] 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 (Ashraf, Bohnet and Piankov 2006).

[30] 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 (2009) for why taking the difference in the amount sent in trust game and dictator game may understate the magnitude of behavioral trust.

this increase is also economically significant (around 37 percent 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 of 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).

Observation 5: 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.

D. Further Results

The set of results presented above strongly suggests that having an ex-soldier son improves knowledge about behavioral differences between ex-soldiers and their peers their peers, which in turn affects actions in trust-based interactions. Here we discuss possible alternative interpretations. First, 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 possibility, 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.11). 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). 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.

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Next, since Senders were sampled from the pool of older villagers, who were outside of the age range targeted by the LRA, the increased trust among parents of LRA soldiers is unlikely to represent social capital within the LRA, due to a common connection.[31]

Last, we consider the possibility that the salience of Receiver's LRA history during the experiments 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 percent of individuals reported that the Receiver with whom they were matched was an abductee in an open-ended question asked approx. 30 minutes after the experiments. 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 (Heider 1958), 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 ("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 percent of respondents thought that abduction was completely unavoidable and 70 percent 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.

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[31] An alternative interpretation is that people with more familiarity with the LRA are trained to fear ex-soldiers and cooperate more out fear. While this explanation is consistent with greater transfers in the trust game, it struggles to explain why the transfers are not higher in the dictator game as well and why Senders accurately expect increased trustworthiness of ex-soldiers.

V. Conclusions

The common view is that reintegration of soldiers after civil wars 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 forcibly recruited soldiers are more likely to vote (Blattman, 2009). We extend this earlier work by (i) separately observing the behavior of former soldiers as well as the treatment of former soldiers by receiving communities, (ii) focusing on two important aspects of interpersonal relations, namely trust and willingness to co-operate, 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 sample 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 at an 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 the 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. 2014), 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 develop substantially during early stages of the life cycle (e.g. Fehr, Bernhard and Rockenbach, 2008), while our results show that environmental factors during childhood may have lasting impacts.

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, List and Price 2012), given that individual LRA history is mostly perceived as completely unavoidable by the population we study. Such interpretation cautions against generalizing our findings to post-conflict societies in which the participation of soldiers in violent acts is seen as an outcome of their decisions 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.[32] 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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[32] For a debate on this issue see, for example, (Muggah 2009).

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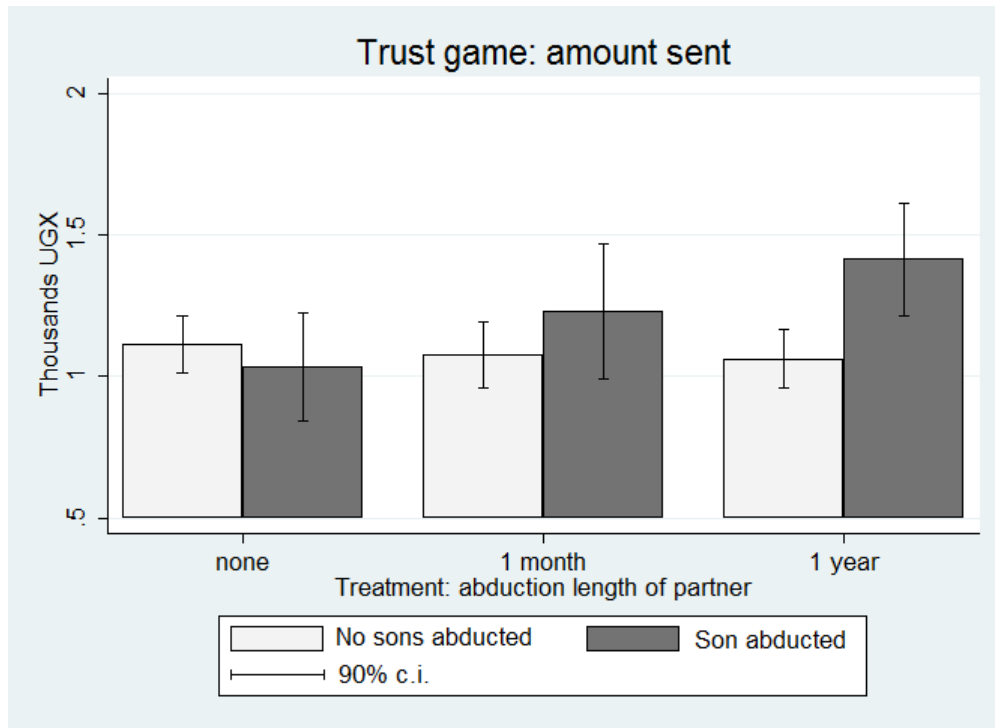**Weiner, Bernard.** 1995. *Judgments of Responsibility.* The Guilford Press.

FIGURE 1
AMOUNT SENT IN TRUST GAME, DISAGGREGATED BY TREATMENT AND THE ABDUCTION HISTORY OF SUBJECTS' SONS.
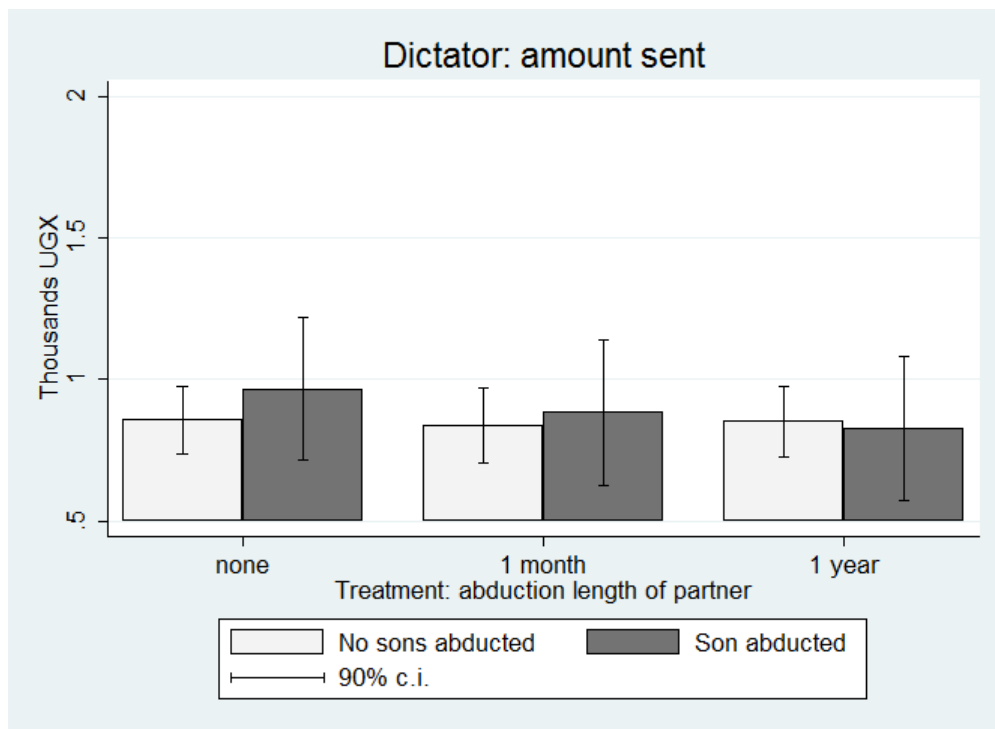

FIGURE 2
AMOUNT SENT IN DICTATOR GAME, DISAGGREGATED BY TREATMENT AND THE ABDUCTION HISTORY OF SUBJECTS'
SONS.

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: Personal Characteristics				
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)
Age	24.45	(4.89)	43.08	(6.10)
Birth order	3.55	(2.33)	0.56	(0.50)
Number of siblings	5.01	(2.74)		
Mother no school (d)	0.65	(0.48)	2.67	(1.95)
Father no school (d)	0.27	(0.45)	0.65	(0.48)
Father alive in '96 (d)	0.80	(0.40)		
Mother alive in '96 (d)	0.92	(0.27)		
Married (d)	0.53	(0.50)	0.80	(0.40)
Current household size	6.92	(4.83)	8.11	(3.56)
Cash earned in past 7 days (thousands UGX)	2.69	(10.23)	2.02	(5.42)
Wealth[c]	-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[d]			1.56	(1.09)
Observations	337		378	

Notes: (d) indicates dummy variable. Means. Standard deviations in parentheses. [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] 1st principal component constructed from count of household assets, including: jerry cans, wash basins, bicycles, mattresses radios, plates, livestock, chairs, mobile phones and plows. [d] 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 2: FAMILY CHARACTERISTICS AND ABDUCTION

Dependent Variable	Abducted by the LRA				
	Ever (d)	Before the age of 14 (d)	After the age of 14 (d)	Abduction length (years)	Age of fist abduction
Sample	*Receivers*				
	All				*Abductees*
	(1)	(2)	(3)	(4)	(5)
Year of birth	-0.02***	0.02***	-0.05***	-0.03	-0.76***
	(0.01)	(0.01)	(0.01)	(0.02)	(0.06)
Number of siblings	0.00	0.01	-0.01	-0.02	0.03
	(0.01)	(0.01)	(0.01)	(0.03)	(0.10)
Father no school (d)	0.10	0.05	0.05	0.09	0.52
	(0.08)	(0.05)	(0.09)	(0.20)	(0.42)
Mother no school (d)	-0.12*	-0.05	-0.07	0.03	-0.26
	(0.06)	(0.06)	(0.07)	(0.22)	(0.53)
Mother alive in '96 (d)	-0.05	-0.14	0.07	0.36	1.65*
	(0.12)	(0.12)	(0.10)	(0.25)	(0.96)
Father alive in '96 (d)	0.12	0.02	0.08	0.27	0.57
	(0.07)	(0.06)	(0.08)	(0.30)	(0.78)
Observations	328	328	328	328	175
(Pseudo) R-squared	0.06	0.09	0.21	0.05	0.55

Note: Marginal effects reported for probit regressions (columns 1-3). Columns 4-5, OLS. Robust, standard errors in parentheses, clustered at village level. * significant at 0.10%; ** significant at 5%; *** significant at 1%. (d) indicates dummy variable. All regressions include dummies for missing information on mother's/father's level of schooling.

Dependent Variable	Trustworthiness: Average percentage returned in trust game					
Sample	Receivers					
					Excluding	
			All		Those abducted adol./adult (≥14 yrs)	Those abducted young (<14yrs)
	(1)	(2)	(3)	(4)	(5)	(6)
Abduction length (years)	1.17***		2.18***			
	(0.32)		(0.69)			
Abducted young (< 14 years) (d)				8.82**		
				(3.91)		
Abducted as adolescent/adult (≥14) (d)			0.37	1.42		
			(4.05)	(3.89)		
Abd. length x abd. adol./adult (≥14)			-2.41*			
			(1.19)			
Abducted for over 1y (d)		7.02**			14.85***	-1.07
		(3.17)			(4.20)	(4.52)
Abducted for 2m-1y (d)		2.80			6.49	0.52
		(5.10)			(6.99)	(6.08)
Abducted for <2m (d)		4.98			7.65	3.34
		(3.86)			(6.88)	(4.71)
Age	0.01	0.00	0.09	0.20	0.54	0.33
	(0.41)	(0.41)	(0.43)	(0.42)	(0.46)	(0.43)
Number of siblings	-0.31	-0.32	-0.37	-0.36	-1.14*	-0.19
	(0.39)	(0.41)	(0.40)	(0.39)	(0.60)	(0.52)
Father no school (d)	-1.44	-1.84	-1.29	-2.10	-3.76	-5.27
	(2.62)	(2.66)	(2.59)	(2.54)	(3.06)	(3.65)
Mother no school (d)	0.87	1.45	1.15	1.59	-0.16	2.86
	(3.15)	(3.29)	(3.20)	(3.14)	(3.74)	(3.38)
Mother alive in '96 (d)	-3.47	-3.40	-3.08	-3.68	-4.13	2.14
	(6.27)	(6.13)	(6.15)	(5.76)	(7.53)	(6.36)
Father alive in '96 (d)	0.68	0.47	0.78	0.42	2.53	-0.36
	(2.93)	(2.98)	(2.83)	(2.73)	(3.79)	(3.13)
Log of weekly income	-0.09	-0.03	-0.11	-0.07	-0.04	-0.16
	(0.29)	(0.29)	(0.29)	(0.28)	(0.30)	(0.34)
Current household size	0.34*	0.35*	0.31*	0.37*	0.77	0.27*
	(0.18)	(0.20)	(0.17)	(0.20)	(0.47)	(0.16)
Married (d)	-6.16	-7.27*	-6.43	-6.55	-9.72*	-7.20*
	(3.99)	(3.98)	(4.20)	(4.00)	(5.18)	(3.75)
Literate (d)	6.94**	6.10*	6.57**	6.41*	1.57	5.63
	(3.24)	(3.41)	(3.15)	(3.27)	(3.71)	(3.67)
Schooling (years)	-0.45	-0.49	-0.45	-0.44	0.35	-0.32
	(0.56)	(0.58)	(0.57)	(0.59)	(0.68)	(0.60)

Continued…

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Wealth	1.18**	1.27***	1.21***	1.25***	1.44***	1.37**
	(0.44)	(0.46)	(0.43)	(0.45)	(0.38)	(0.54)
Partner in experiment male (d)	3.79	3.89	3.26	3.25	4.13	3.88
	(2.45)	(2.50)	(2.45)	(2.40)	(3.01)	(2.65)
Constant	35.39***	34.83***	33.92**	30.31**	22.50	21.70*
	(12.47)	(12.24)	(12.50)	(11.92)	(14.20)	(11.16)
Observations	328	328	328	333	222	257
R-squared	0.07	0.07	0.07	0.08	0.12	0.09

Note: OLS. Robust, standard errors in parentheses, clustered at village level. * significant at 10%; ** significant at 5%; *** significant at 1%. (d) indicates dummy variable. The dependent variable is the average percentage returned from two decisions made by Receivers. All regressions include dummies for missing information on mother's/father's level of schooling.