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DISSERTATION

Three Essays on Post-Conflict Reintegration

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Abstract

This dissertation consists of three essays which explore the effects of conflict and the post-conflict reintegration process, each using a different methodology to study a different facet of these issues, including an analysis of survey data, an artefactual economic experiment conducted in the field, and an laboratory experiment. The research presented here demonstrates how these methods complement one another in contributing to our understanding of how conflict affects individuals' well-being and behavior. In the first essay, I analyze an existing data set from a survey of ex-combatants in Liberia to estimate the effect of a reintegration program for former soldiers on participants' income and employment status, using propensity score matching to account for self-selection bias. Though the results indicate a higher employment rate for those who complete the program, there is consistently no effect on income. This has implications for evaluating the integrated approach to ex-combatant reintegration that the program embodied. The second chapter also deals with the reintegration of ex-soldiers, but focuses on social capital, using a set of experiments, including trust and dictator games, to study the effects of forced military service for a rebel group on social capital in northern Uganda. We find that individual cooperativeness robustly increases with soldiering, especially among those who soldiered during early age, and that 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. In the third chapter, we study cooperation within and between groups in the laboratory, by modeling conflict with an inter-group Tullock rent-seeking contest, and manipulating groups' conflict history to measure the effect of conflict on cooperation using a multi-level public good game. We demonstrate that conflict increases cooperation within groups, while decreasing cooperation between groups.

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Chapter 1:

Introduction: Empirical Approaches to Studying Post-Conflict Reintegration

A frequently cited passage in a 2003 World Bank report describes civil war as "development in reverse" (World Bank, 2003), and this reflects the far-reaching impact that conflict has on all facets of society. Violent conflict has an obvious detrimental effect on physical capital, but also lowers the level of human capital in the economy—both through loss of life, as well as by disrupting education—and recent research suggests that effects on social capital are significant as well. While the number of interstate conflicts has decreased since the end of colonialism, the number of intra-state conflicts has increased over the same period: civil war has occurred in more than a third of all nations between 1960-2010, and the situation in sub-Saharan Africa is worse, where a third of all nations suffered from civil wars in the 1990's alone (Blattman and Miguel, 2010).[1] Thus, understanding both the causes and effects of violent conflict is essential to understanding economic development as a whole.

Over the past decade and half, there has been an increased interest in empirical research on both the causes and effects of conflict, beginning with a number of studies that have examined the correlates of civil war using cross-country analyses. Seminal studies in this area include Fearon and Laitin (2003), who identify lower national income—which they argue is a proxy for state power—as positively correlating with civil war, and Collier and Hoeffler (2004), who identity primary resources as risk factors for conflict. While these studies, as well as a number of later authors,[2] come to different conclusion regarding which specific factors predict civil conflict, they agree on the importance in focusing on environmental, social and economic factors play an important role in violent conflict.

Given this, there are several mechanisms by which the effects of violent conflict might increase the chances of future conflict: by decreasing state capacity, creating a

[1] Blattman and Miguel's (2010) estimates are based on the UCDP/PRIO (Gleditsch et al. 2002; Harbom and Wallensteen, 2007).
[2] See Blattman and Miguel (2010) for a thorough review.

marginalized group of ex-combatants or by creating or exacerbating tensions between ethnic or political groups in post-conflict societies. This can result in a self-perpetuating cycle of violence or "conflict trap" (Collier, 2007), and understanding the effects of conflict is essential in breaking this cycle. The studies presented here are aimed at advancing this goal.

The essays in this dissertation focus on two themes related to post-conflict reintegration: the reintegration of former combatants and the effects of conflict on cooperative behavior. The latter theme covers both violent conflict as well as conflict in a broader sense, including any competition over fixed resources. Each chapter approaches these topics using a different empirical methodology, including analysis of survey data, an artefactual field experiment with former combatants, and a laboratory experiment which studies the effects of non-violent group conflict on subsequent cooperation, respectively.

Reintegrating former combatants

Both Chapters 1 and 2 focus on the reintegration of former combatants. In the former, "Measuring Intermediate Outcomes of Liberia's Disarmament, Demobilization, Rehabilitation, and Reintegration Program,"[3] I use an existing data set from Pugel (2007), based on a survey of ex-combatants in Liberia, to assess the effects of the United Nations-backed program designed to facilitate the disarmament and economic and social reintegration of rebel soldiers after the recent civil war. This class of policy interventions—Disarmament, Demobilization and Reintegration (DDR) programs—are designed to help former rebels build social ties and job skills, and this has become the standard approach for addressing the problem (Muggah, 2005). Assuming that DDR programs are successful in these regards, this should increase the opportunity costs of engaging in peace-spoiling activities—such as criminal activity or continued fighting—by providing an alternative set of skills on which these individuals can rely and by developing social capital with receiving communities.

After partially accounting for self-selection and attrition (based on observables) through the use of propensity-score matching, I find a modest increase in employment for those who complete the program, but no effect on income. This implies that previously

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observed increases in economic performance attributed to the intervention are primarily due to self-selection into the program. This research builds on several previous surveys that have been conducted on ex-combatants, including evaluations of reintegration programs (Pugel 2009; Muggah 2007). Notably, Humphreys and Weinsten (2007) come to similar conclusions in evaluating a DDR program in Sierra Leone.

The lack of a more significant impact on economic outcomes for former soldiers in Chapter 2, likely due to very limited economic opportunities in the country as a whole, indicates that perceived success of the program (including the absence of further conflict) may be the result of the social, rather than strictly economic factors. These results have implications for both evaluating the outcomes in Liberia as well as assessing the integrated approach to ex-combatant reintegration that the program embodied.

In a survey of war-affected youth in northern Uganda, Blattman (2009) finds that while ex-combatants fall behind economically, they are surprisingly resilient in social dimensions, and were more likely to vote than non-abducted peers. Using the same data, Blattman and Annan (2010) find that in northern Uganda the Lord's Resistance Army (LRA) forcibly recruitment of a large number of soldiers from rural areas, the majority of whom were children, was nearly at random. This exogeneity in recruitment makes northern Uganda a unique setting for studying the causal effects of experience as a combatant, due to the minimal role that self-selection played in determining who was with the rebels.

We exploit this in order to further explore the social dimension of reintegrating former combatants in Chapter 3. We use an artefactual field experiment run with two groups of individuals: former soldiers who were abducted (many as children) into the LRA and members of receiving communities. Using this methodology allows us to measure a specific aspect of social capital—trust and trustworthiness—which is difficult to measure through surveys, and is involved in virtual all economic transactions (Arrow, 1972).

The experiment consists of two behavioral games, the trust and dictator games, in a design similar to Fershman and Gneezy (2001) and Cox (2004) that allows us separate behavioral trust from altruism. The experiment simulates an interaction in which members of the community control productive assets, which can be used by returning soldiers to the benefit of both parties, but only if community members trust former combatants and the latter are sufficiently trustworthy. We sampled both male and female members of the community between the ages of 35-55 to fill the role of senders (investors) and sampled males between the ages of 18-35, to fill the role of receivers (trustees) in the trust game. We over-sampled former abducted soldiers from the population such that receivers were roughly half of receivers had been soldiers with the LRA. The senders were informed if they were anonymously matched with a non-soldier or an ex-soldier, and if so, whether he had been abducted for a shorter period (around 1month) or longer period (around 1 year). This design allows us to examine a behavioral aspect of reintegration, and to do so from both the perspective of the ex-soldiers themselves as well as from the perspective of receiving communities.

Contrary to the popular conception of former child soldiers as "damaged goods" (Vermij, 2011), we find that individuals abducted by the LRA at a young age (13 years of age or younger) are actually *more* trustworthy than their (non-abducted) peers. Interestingly, there is no effect for ex-soldiers who were abducted by the LRA at a later age.[4] We hypothesize that this difference in behavior is the result of experiences while with the rebels, either exposure to violence or efforts by the LRA at promoting cohesion within units.

Moreover, we do not find evidence that members of receiving communities discriminate against former combatants. Our design allows us to separate "statistical" discrimination (Phelps, 1972) from taste-based discrimination (Becker, 1971) by comparing the results across treatments. Overall, members of the community do not discriminate against former soldiers in either the trust or dictator games. However, the sub-set of individuals who reported having sons who had been abducted by the LRA, and who arguably have better knowledge of former LRA soldiers behavior, sent more money in the trust game. Interestingly, they did not send more in the dictator game, which indicates that the greater trust-game allocations are driven by behavioral trust, rather than altruism. This supports the interpretation that parents of former soldiers are aware of their greater trustworthiness, and act accordingly.

These results suggest that experience as a child soldier may actually increase certain types of cooperative behavior. Moreover, it may inform the policy debate over how to successfully reintegrate former soldiers―although we note that our results are specific to child soldiers who were forcibly conscripted. As we demonstrate that a particular type of social capital―trustworthiness―is positively affected by soldiering, former child soldiers

[4] This is in line with previous research on social preferences which demonstrates that there are periods of sensitively during which social preferences develop and are more responsive to environmental factors (Harbaugh and Krause, 2000; Fehr et al., 2008; D'Adda and Levely, 2012; Bauer et al., 2014b).

may be more resilient than typically assumed, and thus reintegration programs might be more effective if they increase contact between ex-soldiers and community members.

Conflict and cooperation and group identity

While the study presented in Chapter 3 is unique in that it focuses on perpetrators of violence, our finding that child soldiers are more trustworthy fits into a broader literature on conflict and preferences for pro-social behavior. Bellows and Miguel (2009) find that households in Sierra Leone that were exposed to more violence during the civil war demonstrated higher levels of cooperation and political participation. Using economic experiments, Voors et al (2012) find that exposure to violence in Burundi let to more cooperation, and Gneezy and Fessler (2012) observe an increase in cooperative behavior in Israel during the 2006 war with Hezbollah.

There is evidence that these changes in social preferences are sensitive to social identity and group membership. Bauer et al. (2014) observe a difference in behavior in Georgian children exposed to violence during the 2008 conflict with Russia, finding that those with higher exposure to violence are more averse to inequality, but only towards in-group members (specifically, towards members of their school class). These results are linked to theoretical work by Bowles and Gintis (2011) and Choi and Bowles (2007), who argue that other-regarding preferences developed in humans as a result of inter-group conflict, which favors altruism towards in-group members, but selfish behavior towards individuals who are not part of one's social group.

If conflict creates or strengthens group identities, this can play a role in post-conflict reintegration. In many contexts, former parties to a conflict fail to cooperate with one another, even when the efficiency gains are high. To give an extreme example: two decades after the wars in the Balkans, Muslims and Christians in Bosnia have established separate schools and even separate fire departments (Brunwasser, 2011). However, separating the causal effect of conflict on group identity is difficult in the real world, as the root causes of conflict may influence both the initial conflict as well as post-conflict behavior.

Group identity—including but not limited to ethnic identity—plays a large role in causing conflict (Hirshleifer, 1995), which is apparent from the number civil wars observed around the world that break out along ethnic lines. This phenomenon could result from antipathy towards members of other groups, or alternatively because within group cooperation is easier in ethnically homogenous groups (Habyarimana et al., 2007).[5]

An emerging literature examines related issues of group identity and conflict in the economic experiments. While both the costs and benefits of conflict over fixed resources in such experiments (small amounts of money paid by the experimenters) pale in comparison to conflict in the real world—especially violent conflict—the advantage of this methodology is the ability to exogenously create groups and manipulate incentives, which provides the opportunity to examine how group identity is causally related to conflict.[6]

The rent-seeking model developed by Tullock (1980) is perhaps the most popular game used to model conflict in laboratory experiments (Abbink, 2012). The set-up of the Tullock game is simple: two agents compete over a fixed resource by investing a portion of their endowment into a competition, in which the probability of winning is given by the agent's investment divided by the sum of all agents' investments. In this model, investment into the competition is socially wasteful, but has a positive and well-defined equilibrium. In practice, however, investments in the competition are higher than the equilibrium level (Sheremeta, 2013).

Abbink et al. (2010) extend the basic Tullock game to study conflict in groups, and find that investment in the competition is even further in excess of the equilibrium level than for individuals. We adapt this set-up in Chapter 4 to study how conflict in groups affects subsequent cooperation, both within and between groups, in a public-goods environment.

Increased competition, or decreased cooperation, between groups might result from increased altruism towards in-group members, or alternatively, from animosity towards out-group members. Halevy et al. (2012) address this in an experiment using a modified prisoner's dilemma, in which individuals can choose to cooperate with their in-group while simultaneously harming the out-group, and after a set number of rounds some individuals are given the option to cooperate with their in-group without harming the out-

[5] The cross-country studies by Collier and Hoeffler (2004) finds that ethnic diversity can both increase and decrease the chances of civil war. When there is a dominant ethnic group, this increases the chances of conflict, but greater ethnic diversity limits the size of insurgent groups and thus decreases chances of conflict.

[6] Our study uses university students, predominantly from a business and economics background, which may call into question the generalizability of the study. However, Cleave, Nikiforakis and Slonim (2012) find preferences of subjects recruited through the standard procedures that we use matched those of the general population.

group. They conclude that inter-group conflict, at least in this set-up, is driven by "in-group love," rather than "out-group hate." In other words, subjects were willing to harm the other group to benefit their own group, but chose not to harm the other group when it was possible.

Ke, Konrad and Morath (2013) also use the Tullock game to model conflict in the lab, pairing individuals to compete against a third subject. They find that the experience of competing together as teammates—albeit teammates who were aware that they would subsequently compete against one another— did not decrease subsequent conflict between former group-members, which raises the question of whether the competitive aspect of conflict is in and of itself sufficient in forming in-group solidarity.

Our design in Chapter 4 addresses a similar research question as these two studies, but focuses on the role of conflict in forming group identity and how this affects subsequent cooperation, through a laboratory experiment run with university students in the Czech Republic. Our design models a post-conflict setting in which two groups have previously competed against one another, then, with different incentive structures, are able to mutually benefit from inter-cooperation after the conflict. However, if group identities are strengthened as a result of the conflict, cooperation between groups might be more difficult. This, in turn, could result from greater in-group solidarity or negative feelings towards the other group.

Our experiment is in two stages. In the first, we randomly and anonymously match subjects into groups, who then play a version of the Tullock game. In the second stage, subjects remain in the same group and play the multi-level public goods game (MLPG) (Blackwell and McKee, 2003; Fellner and Lünser, 2008; Buchan et al., 2009). In the MLPG game, each group is again paired with another group. Each individual is given an endowment, which she can keep or contribute to two public goods: one "local" public good which accrues her group, and a second, more efficient "global" public good that accrues to all members her group, plus all other group. The socially efficient outcome is for all individuals of both groups to contribute their entire endowments to the global public good, whereas a self-interested player will keep her entire endowment and contribute nothing to either public good (as in a standard public goods game). Players will contribute to the less efficient, local public good if they have other-regarding preferences such that they care sufficiently more about the members of their own group than those of the other group.

By varying the conflict history of the groups, we uncover the motivations at play. Groups may be matched in the same pairings in both the Tullock game as well as the MLPG game, or rematched with a different group for each game. We find that competition in the Tullock game strengthens within-group cooperation, but weakens between-group cooperation. This happens for two reasons: conflict increases in-group cooperation, which may displace inter-group cooperation. This occurs even when the two groups were re-matched (i.e. when groups are not cooperating with former enemies). Secondly, inter-group cooperation decreases when groups have previously competed directly against one another.

This indicates that conflict, even the weak form that we study in the lab, increases the salience of group identity and impedes inter-group cooperation. Moreover, this happens both because conflict increases within-group solidarity, as well as because conflict decreases pro-social preferences towards former enemies.

Empirical methodologies for studying conflict

In this dissertation, my co-authors and I employ three distinct methodologies to explore the effects of conflict. Chapter 1 uses the more conventional methodology of analyzing survey data. This is the approach taken by the majority of studies on ex-combatants (Humphreys and Weinstein, 2009; Pugel 2007; Restrepo and Muggah, 2009). Such surveys are effect tools in measuring observable economic and social outcomes and are essential in evaluating the obstacles to the successful reintegration of former combatants. Surveys have also proven to be an effect tool in assessing physical mental health outcomes, as demonstrated by several studies from the psychology and medical literature on war affected populations, including former combatants (Vinck et al., 2007; Betancourt et al. 2010; Klassen et al. 2010).

Surveying populations after conflict raises certain practical obstacles that can lead to biased results. Working in conflict zones, especially soon after or during fighting, makes obtaining a large, representative sample difficult and in some settings perpetrators of violence may be reluctant to identify themselves as such. Nonetheless, the existence of several large-scale studies demonstrates that high-quality, representative survey data is attainable in post-conflict settings. The most impressive example to date is the work by Survey of War Affected Youth conducted by Blattman, Annan and Horton (2006), who surveyed a representative sample of youth who lived in northern Uganda before the height of the conflict with the LRA in northern Uganda, which involved tracking respondents who had moved out of their home villages and even out of the region.

Studies such Blattman (2009) and Bellows and Miguel (2009) demonstrate that survey data can also be used to study certain elements of social capital, such as political participation and collective action. However, as economic experiments are playing an increasing role in understanding development (Cardenas and Carpenter, 2008), and previous economic experiments on other-regarding preferences and conflict (Voors et al., 2012; Gneezy and Fessler Bauer et al. 2014) demonstrate that this methodology can uncover effects of conflict that survey questions cannot. The design and findings in Chapter 3 serve as further evidence of this.

The third methodology employed in this dissertation is a laboratory experiment. Theoretical predictions about behavior in conflict, particularly in the Tullock rent-seeking game, have been studied in detail by experimental economists (Abbink, 2012), and there are also a number of experimental studies that examine issues related to group identity and cooperation. Chapter 4 contributes to both streams of literature, exploring how group identity is formed through participation in conflict and how this affects subsequent cooperation. While laboratory experiments are limited by low stakes and a short time frame, when compared to naturally occurring conflict, they have the advantage of affording researchers full control of the environment and allow for testing more precise, causal hypothesis about conflict, cooperation and group identity.

Conflict plays a large role in all societies, and violent conflict in particular remains prevalent in many parts of the world, especially in developing countries. Understanding both the causes and effects of conflict should therefore be a priority for economic researchers. While the topic has received increasing attention in the past decade, there remain many unanswered questions, and this dissertation attempts to answer a selected subset of these, and demonstrates that a variety of empirical approaches can contribute to our understanding of this issue.

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Chapter 2

Measuring Intermediate Outcomes of Liberia's Disarmament Demobilization and Rehabilitation and Reintegration Program

Abstract

This paper re-analyzes data from a survey of ex-combatants in Liberia conducted in 2006, to estimate the effect of Liberia's Demobilization, Disarmament, Rehabilitation and Reintegration program on participants' income and employment status. As program completion was not random, these estimates are biased. I use propensity score matching to obtain a more precise estimate. The results indicate a higher employment rate for those who complete the program, although there is consistently no effect on income. These results have implications for both evaluating the outcomes in Liberia and the integrated approach to ex-combatant reintegration that the program embodied.

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2.1 Introduction

It has now become standard practice for the post-conflict reconstruction process to include Demobilization, Disarmament and Reintegration (DDR) programs as a way of reintegrating combatants. The programs typically take a holistic approach, and concentrate on decreasing the likelihood that ex-combatants will return to violence or engage in other peace-spoiling activities by facilitating economic, social and political reintegration. This paper analyzes outcomes from one such program: the Demobilization, Disarmament, Rehabilitation and Reintegration (DDRR) program in Liberia. Specifically, this paper examines how the DDRR program affected employment rates and income.

While most previous empirical studies have concentrated on the overall effectiveness of DDR to facilitate reintegration and to contribute to peace in the region, this paper focuses specifically on the job training component of the program. Ultimately, reintegration as a means of rebuilding a society and preventing conflict should not be judged by any one factor. However, as economic reintegration is a vital part of DDR, the ability of reintegration programs to improve the economic situation of participants, usually through job-training programs, is necessary in determining the overall impact of these programs in post-conflict reintegration.

This paper re-analyzes survey data on ex-combatants in Liberia, collected in 2006 by Pugel (2006; 2007). The goal of this exercise is to measure the effect that DDRR training had on participants' daily wages and employment status, including effects on specific educational and geographic cohorts and accounting for selection bias. The results indicate that the program, on the whole, unlikely had any significant effect on income and a small impact on employment status. This is true for all educational cohorts. The effect of training did, however, have a significant impact in certain regions of Liberia. This may be attributable to differences in the quality of training provided across regions, to availability of training or to heterogeneity in employment opportunities.

This suggests that any success in reintegration achieved by the Liberian DDRR program may not have hinged on job training. In post-war Liberia and similar settings, where employment opportunities are extremely limited, job training may be of little value to participants—even when it is of high quality, which is often not the case.

Measuring the effect of job training is challenging because unobserved personal characteristics can play a large role in determining both whether an individual will participate in a training program and that individual's income level independent of any active labor market programs. This problem is particularly difficult when unemployment is high and when many individuals earn income in the informal sector. This analysis attempts to correct for this selection bias using propensity-score matching. However this technique is far from perfect. If the effect of future programs is to be properly measured and understood, more careful study is needed. While DDR programs are poor candidates for randomized control trials, more resources should be dedicated to measuring the effects of job training programs for ex-combatants to ensure that the quality of the programs is adequate. In the absence of higher wages and employment level, it may make more sense to spend money on other types of reintegration and employment generating programs.

While these conclusions are not at odds with previous analyses of this and other reintegration programs, this exercise contributes to the debate by showing that, at least in this instance, the evaluation of the general approach taken towards reintegrating ex-combatants should take into account that the program, as a whole, failed to significantly improve the participants' chances of being employed or of having a higher income.

This paper begins by introducing the DDR concept, reviewing relevant literature and discussing the effectiveness of the approach. Sections 2.2 and 2.3, describe the data used and the methodology employed, respectively. Section 2.4 presents outcomes and section 2.5 concludes.

2.2 Background: The Liberian Conflict and Post-Conflict Reintegration

The DDRR program in Liberia began in 2003, following the end of over a decade of civil war. The program followed from the Comprehensive Peace Agreement that was brokered by international and regional actors in 2003. Since this time, there has been relative peace in the country, although several violent incidents involving ex-combatants have occurred and poverty and a lack of infrastructure continue to pose serious problems. The Comprehensive Peace Agreement included a provision requesting that the UN send troops to support the transitional government. This included a mandate to aid in the reconstruction of the country, including the reintegration of former combatants. To this end, the United Nations Mission in Liberia (UNMIL) was established in 2003 under UN Security Council resolution 1509. UNMIL's mandate was to demobilize, disarm and provide training and reintegration support to former soldiers for the two main factions, the Liberians United for Reconciliation and Democracy (LURD) and the Movement for

Democracy in Liberia (MODEL), as well as the Armed Forces of Liberia (AFL). This included women and children.

The issue of training and reintegrating former combatants into the workforce has been a goal of most post-conflict development programs in recent history. Both feedback from practitioners in the field and academic work has established that there is a connection between the economic conditions of combatants and their willingness to fight or lay down arms. Berdal and Ucko (2009) link the development of DDR to the Angolan conflict, in which a failure to successfully reintegrate combatants resulted in ongoing conflict.

DDR programs have since become more or less standard practice in UN-negotiated peace agreements since 1989 (Humphreys and Weinstein 2007) and are “part of the United Nations (UN) system's multidimensional approach to post-conflict peace-building and reconstruction” (UNDDRC). To date, there have been around sixty DDR programs in Asia, Africa the Caribbean and South America (Muggah, 2009). These programs represent a holistic approach to reintegrating combatants into civil life, based on a multi-dimensional understanding of reintegration. In other words, successful reintegration depends on an individual’s simultaneous progress in all relevant aspects of transition to civilian life, including economic and social.

DDR might serve only certain groups of former combatants—for example only those from the non-government factions—or they might be available to soldiers from government forces as well. This paper concentrates on aspects of DDR intended for adults, specifically job training, although DDR can include minors and even the families of former combatants (UNDDRRC, 2006).

As an integrated approach to peacekeeping and peace-building, DDR serves several purposes. Special attention might be given to the most vulnerable groups, such as female and child combatants because those individuals are in need of the most assistance in reintegrating. On the other hand, the ultimate goal in post-conflict settings is to reduce the chances that violence will reoccur, and to this end, training programs are devised to help combatants who may turn to peace-spoiling activities if they fail to reintegrate.

After units are demobilized, combatants typically enter a program by turning in a weapon—the “disarmament” phase of the program—and receive an ID card that allows the individual to participate in other aspects of the program.

Reintegration usually consists of financial and social support offered to demobilized combatants, with the goal of increasing the chances that ex-combatants will transition into the peacetime economy and become economically self-sufficient. In Liberia, eligible participants were allowed to register for a job training program of their choice (UNDDRC). These programs were divided into four main categories: agricultural training programs, civil works programs, vocational training and formal education.

Although DDR programs are necessarily tailored to specific conditions of a given country, the United Nations Integrated Disarmament, Demobilization and Reintegration Standards (UNIDDRS) were developed to promote coordination between United Nations agencies and other stakeholders. The UNIDDRS also defines a reinsertion phase, which may entail post-discharge orientation, food assistance or cash payments (Özerdam and Podder, 2008).

The Liberian DDRR program was divided into two main stages: the first phase, or "DD" phase, consisted of disarming and demobilizing combatants. Initially, combatants were required to turn in serviceable weapons (or ammunition of a certain amount) in order to register for the program, although this restriction was relaxed in later phases of demobilization. Upon doing so, each individual was given US $300 and an ID card that allowed him or her to register for further benefits, including training.

The program got off to a rocky start as there were some misconceptions over the distribution of payments and unease among combatants. Initially, the United Nations Mission in Liberia paid individuals half of their payments as a first installment, with the other half to be administered at the end of a three week demobilization program (Alusula, 2008). This raised tensions among ex-combatants at demobilization centers, who were often ill-informed on the procedures to be employed. In addition, problems ensued when the UN staff decided to begin disarming and distributing payments among government soldiers first (Jaye, 2009). Further rounds of disarmament and demobilization went more smoothly, however, and by the end of the program, 101,495 combatants had been demobilized, of whom 60, 28 and 12 percent had fought for the AFL, LURD and MODEL, respectively (UNDDRRC).

The Rehabilitation and Reintegration portion of the program mainly consisted of job training. Participants were given the choice to enter formal education programs (for which funding was provided to cover school fees, related costs and a stipend for up to three years) and vocational training programs that were provided by contracted partners. As part of the reintegration package, each student was given a stipend of $30 a month plus in-kind support from the contracted training institutions (Alusula, 2008). The vocational programs prepared participants for careers in masonry, tailoring, agriculture and other fields. Most of those who demobilized—around 90%—registered for training benefits, although not all of those individuals completed training programs (UNDDRRC 2011).

Although some have considered Liberia's DDRR program to be a general success—a view supported by sustained peace—the program has many faults that have been noted both by researchers studying the process and by officials in the field (Alusula, 2008).

Recently, events in the region have highlighted the threat to peace and stability that ex-combatants who have not successfully reintegrated may pose. Credible reports indicate that Liberian mercenaries have played a part the violence in Ivory Coast following Laurent Gbagbo's refusal to give up power after losing the 2010 presidential elections. Harrison S. Kamwea Sr., Liberia's interior minister linked the ex-combatants' involvement in the conflict with employment opportunities: "when people have been used to living on violence, they have got no profession to earn their living on" (Akam, 2011).

In general, employment opportunities in post-war Liberia have been very poor and this may be the most serious restraint to the ability of training programs targeted at ex-combatants to produce results. According to the World Bank, only 65.7% of the Liberian population over 15-years old was employed in 2006 (World Bank). This is reflected in a very low level of development (the 2007/2008 Human Development index ranked Liberia 169th of the 182 countries ranked).

2.3 Review of Literature

This section summarizes literature on dealing with DDR and the reintegration of ex-combatants in Liberia.

Much work has been analyzing DDR as a component of broader security-sector reform and practical issues surrounding implementation. Berdal (1996) examines post-cold war DDR programs and emphasizes the political nature of the process, while discouraging a compartmentalized view of program components. Knight and Özerdem (2002) examine the role of cash transfers and in the DDR process, concluding that the process should be designed to promote long-term economic integration rather than to placate potentially dangerous individuals during the period of transition. In a similar vein, Muggah (2005) remarks that DDR can be interpreted narrowly as a "means of eliminating spoilers," or more broadly, "as an opportunity for longer-term development," in which "combatants and their dependents are potentially cast as *prima facie* vessels of human capital." Verkoren et al. (2010) question whether it is realistic for DDR programs to have such ambitious goals, and suggest that "DDR can only contribute to long-term security if it is part of a wider set of security promotion strategies."

This puts the training aspects of the Liberian DDRR program into context, and suggests that the program's success should not be determined by any one outcome, but rather on the long-term prospect for stability and peace in the country. However, understanding whether programs succeed in achieving more parochial outcomes is necessary in evaluating the merits of DDR as a general approach.

Despite a recent increase in the study of economic elements of civil war (Blattman and Miguel, 2010), there are relatively few of these studies are quantitative studies that clearly demonstrate the precise effectiveness of the programs in terms of measurable outcomes (Muggah 2009).

A notable exception is Restrepo and Muggah (2009), who study the effects of a DDR program in Columbia by comparing levels of violent incidents in time periods and areas in which the program was active and in which it was not. They conclude that the number of violent incidents in a given area in a given period did in fact decline as a result of the program. Additionally, Willibald (2006) reviews theoretical and empirical evaluations of the effectiveness of cash transfers to demobilized combatants.

A larger number of qualitative evaluations have been conducted. Sedra (2008) and Özerdem (2002), for example, discusses DDR in Afghanistan as a pillar of the demilitarization process, Torjesen and MacFarelane (2009) look at DDR in Tajikistan, and Porto and Parsons (2003) analyze DDR in Angola.

There have also been several surveys of ex-combatants in post-conflict countries that measure economic and social outcomes and include analysis of DDR programs. This paper deals with data from one such survey, by Pugel (2007, 2009). This project was based on methodology employed in a previous survey of ex-combatants by Humphreys and Weinstein in 2004 in Sierra Leone (Humphreys and Weinstein, 2007).

Blattman (2009) finds that former combatants in northern Uganda—the majority of whom were abducted by the Lord's Resistance Army—were actually *more* likely take on leadership positions and be politically active.

Humphreys and Weinstein (2009) are generally skeptical that the DDR program in Sierra Leone was effective. They use propensity matching to condition those who entered and didn't enter Sierra Leone's DDR program (Humphreys and Weinstein 2007). The economic dimension was measured in binary outcomes, for example whether the individual was employed. They come to the conclusion that there is no effect in any dimension of reintegration measured. Interestingly, they indicate that combatants from a higher socio-economic background had a harder time reintegrating, including finding employment (Humphreys and Weinstein 2007, 2009).

Pugel (2009) comes to similar conclusions about data from his study Liberia, upon which this analysis is based, although he notes that those who completed the DDRR program displayed some indications that they were economically better off than those who did not, and that those who did not enroll were overall in worse economic condition. However, this analysis does not account for potential selection bias, as the program was voluntary, nor for bias due to attrition, as a fairly large number of those who joined did not complete the program.

Humphreys and Weinstein (2009) point out that DDR may be important despite its effectiveness ultimate effectiveness in facilitating reintegration as the program “enabled faction leaders to sell their soldiers on a peace deal.” However, the failure of this and other programs at the micro level might also suggest that demobilization and disarmament be delinked from economic development. One potential explanation for the failures of these programs to increase employment is that there is generally a lack of jobs available and the economic situation is difficult for all in post-conflict economies. The lack of any significant effect in this regard would therefore be the result of the general economic situation and not a specific failure of the program. If this is the case, it may be better for effort and aid to be spent on job creation and development in general, and specifically linked to the reintegration of ex-combatants.

The situation may also be that the job training portions of DDR have been extremely ineffective and have not led to positive results for even those who managed to find employment. In this case, it may be too early to do away with the integrated approach. The solution may be to simply improve the quality of job training associated with DDR programs.

There are a number of studies, from several academic fields, specifically devoted to the Liberian conflict and its aftermath, including some that focus on issues facing ex-combatants and the success of the DDRR program. Jennings (2007), for example, uses qualitative fieldwork with ex-combatants to identify issues that continue to face this population. She concludes that the process (both in Liberia and generally) should do more to account for local conditions, argues that demobilization and disarmament should not necessarily be linked with rehabilitation and reintegration and suggests that post-conflict job training might be targeted towards ex-combatants while being open to the general population (Jennings 2007).

Ansorge and Ansorge (2011) are more supportive of the Liberian DDRR program and note that many non-combatants did in fact participated by turning in weapons or ammunition that did not belong to them, which had the unintended positive effect of lessening the stigma attached to DDRR training. They argue that concentrating on short-term outcomes of the Liberian DDRR program, such as employment, may be counterproductive, as the principle desired outcomes are "improved capacity and legitimacy of the Liberian state."

Bøås and Hatloy (2008) describe the results of a survey-based study conducted in Monrovia, which included a high percentage of participants who had been through the DDRR program who were unable to find work. Overall, however, they find that ex-combatants in did not seem to be "more idle, marginalized and alienated than any other group of young men in Liberia." Bøås and Bjørkhaug (2010) question whether DDRR training fully utilized the linkage and skills that ex-combatants developed during the conflict.

Klein and Civic (2011) note that Liberia's DDRR program was implemented under chaotic settings and before "all of the 'pieces' were in place—before sufficient numbers of troops were deployed or the DDRR infrastructure was ready." Given this, they claim that the "promise of money was the bottom-line, most compelling tool for inducing ex-combatants to participate in DDRR" along with the promise of "training in a livelihood other than pillaging in war."

This paper contributes to the literature by examining the effects of the DDRR program in Liberia simply in regards to returns to income—in other words as an ordinary job training program would be analyzed. While the goal of DDR programs is not simply to help ex-combatants attain higher incomes, but to facilitate reintegration in order to prevent peace-spoiling activities and further violence. However, understanding how effective the programs were in terms of this one dimension should contribute to the broader debate on the effectiveness of DDR.

2.4 Data

The data for this analysis come from a United Nations Development Program-supported country-wide survey of 590 ex-combatants in 2006 by Pugel (2007). The questionnaire was administered to ex-combatants who had participated in some way in the conflict in that country. The study included questions on basic biographical information, the individuals experience in the war (which faction they fought in, locations of fighting and rank for example).

The respondents were asked about their participation in the DDRR program, which consisted of several phases. Participation in the program components forms the key variables for this analysis. Firstly, units were demobilized combatants entered the DDRR program by turning in serviceable weapon and registering with the DDRR program. The individuals were then given an identification card that allowed them to participate in other portions of the program. Of the sample of ex-combatants in this analysis, 88.3% of respondents demobilized. 50% or respondents registered for training benefits, while around 43% of respondents enrolled in DDRR-sponsored training programs. Around 15% of the sample had completed DDRR training at the time of the survey.

Around 16 percent of respondents both entered and completed a DDRR training program. Table 2.1 reports characteristics of those who participated in the various phases of the program, dropping observations for those who are over 65 years of age, those who reported an income of 700 LRD a day or more (i.e. more than three standard deviations from the mean), disabled individuals and students. This brings the total number of observations to 565.

The individuals entering DDRR job training programs could choose between programs in civil works, agricultural training, vocational skills/apprenticeships and formal education. Slightly more than half of those who enrolled in a job training program chose vocational skills.

While around 14% of survey respondents registered for the formal education program, only 1.3% had completed the program. This means that a large percentage of the respondents may have been currently attending classes connected to the DDRR program. Overall, around seventeen percent of respondents considered their primary occupation to be “student.”

One interesting feature of the data set is that employment was actually lower for those who had completed junior high than it was for those with no school or for those who had

Table 2.1 – Summary Statistics

	Mean (Standard deviation)							
Sample	*All*	*No DDRR*	*Demobilized*	*Registered for DDRR benefits*	*Enrolled in training program*	*Enrolled, did not finish*	*Still in training*	*DDRR training finished*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Age	26.47	28.14	26.25	25.8	25.47	27.14	24.02	27.12
	(6.17)	(7.31)	(5.98)	(5.49)	(5.04)	(4.94)	(4.78)	(4.83)
Education (years)	6.98	7.95	6.85	7.92	8.18	7.86	8.04	8.49
	(3.89)	(3.70)	(3.90)	(3.22)	(2.98)	(2.71)	(2.89)	(3.21)
Daily income (LRD)	168.65	200.23	164.48	173.02	172.82	148.88	169.73	185.96
	(120.53)	(133.74)	(118.20)	(119.86)	(117.98)	(97.04)	(121.83)	(118.20)
Employed (d)	0.57	0.58	0.57	0.57	0.58	0.45	0.56	0.65
Student (d)	0.15	0.14	0.15	0.18	0.19	0.17	0.24	0.11
Single (d)	0.55	0.58	0.55	0.64	0.67	0.55	0.75	0.59
Male (d)	0.79	0.85	0.79	0.82	0.82	0.9	0.77	0.87
Monrovia (d)	0.42	0.53	0.4	0.6	0.62	0.59	0.65	0.59
Officer at end of conflict (d)	0.33	0.36	0.32	0.3	0.28	0.38	0.24	0.31
County[a]								
Bomi (d)	0.07	0.18	0.05	0.04	0.04	0	0.06	0.02
Bong (d)	0.1	0.11	0.1	0.04	0.03	0.1	0	0.06
Grand Bassa (d)	0.04	0	0.04	0.05	0.05	0.07	0.08	0.01
Grand Cape Mt. (d)	0.04	0.02	0.04	0.01	0.01	0	0.01	0.02
Grand Gedeh (d)	0.1	0	0.12	0.13	0.13	0.14	0.09	0.19
Lofa (d)	0.07	0.02	0.07	0.02	0	0.03	0	0
Montserrado (d)	0.49	0.68	0.46	0.66	0.7	0.62	0.74	0.65
Nimba (d)	0.07	0	0.07	0.03	0.03	0.03	0.02	0.04
Sinoe (d)	0.03	0	0.04	0.03	0	0	0.01	0
Number of Observations	565	66	499	280	240	29	128	83

(d) indicates dummy variable. [a] county of residence at time of survey.

completed only elementary school. This is consistent with findings reported by the author of this survey (Pugel 2007b, 2009) and findings from Sierra Leone data (Humphreys and Weinstein 2009) that ex-combatants with higher socio-economic status had a harder time reintegrating. Those ex-combatants who finished DDRR training had slightly higher levels of education than those who did not.

The capital city, Monrovia, was a destination for many combatants after the conflict (Pugel 2007). Although mean daily income for those residing in Monrovia, which is in Montserrado county, was higher than the country-wide average the employment rate was lower. Pugel (2007) notes in a report on the survey results that those who had not returned to where they had lived before the war had a harder time reintegrating. Table 2.1 shows the regional composition of program participants by county. Only 33 percent of the sample left their respective faction as officers. Surprisingly, these individuals do not seem to have a higher income on average. Around half (48.0 percent) of the respondents were members of the Taylor (government) faction when the conflict ended in 2003, with 25.2 percent and 16.1 percent belonging to the LURD and MODEL respectively, and the remaining 10.7 percent of respondents belonging to no faction at the end of the conflict, usually indicating that they were not active during this time. The vast majority of the respondents were active in combat (84.1 percent).

2.5 Methodology and Estimation Results

I model the results of the DDRR program components in terms of two dependent variables: natural log of daily income and employment. Daily income results from individuals' responses to the question "how much money do you get in a day?" and employment as determined by five questions on the UNDP survey. I count an individual as employed if the survey classified him or her as "employed," an "employer," or "self-employed." This definition covers participation in any income-generating activity, including small-scale enterprises and informal-sector employment.

The key variables of interest are those designating whether an individual has participated in a given phase of the DDRR program, namely "reintegration registered," which is equal to one if an individual registered for reintegration benefits and DDRR. There are not enough observations for the other training programs (civil works, agriculture and formal education) to draw meaningful conclusions, which somewhat limits the overall analysis. All specifications exclude those currently in training.

I use ordinary least squares as a baseline for estimating the effects of the program components on daily income, and probit analysis to estimate the effect on one's chances of being employed at the time of the survey. Both of these approaches, however, do not account for self-selection into training programs. I use propensity-score matching to account for self-selection on observable characteristics, which produces a more accurate estimate.

2.5.1 Ordinary Least Squares and Probit

A basic OLS model serves as an orientation point in understanding the effects of the DDRR program components on daily income. The model is a basic Mincer equation:

$$ln\ (dailyincome)_{ij} = \delta_0 + \delta_1 X_i + \delta_2 Ei + \delta_3 C_j + \delta_4 DDRR_i + \varepsilon_{ij}\ , \qquad (2.1)$$

where natural log of individual i in county j is given by X, a vector of personal characteristics that consists of age, age squared and dummy variables that equal one if the person is single, female, faction without being an officer, and from the Kpelle tribe, Ei is individual i's highest level of education completed and C_j is The DDRR term includes a dummy for finishing the program as well as a variable that indicates registering for training benefits and education is measured by dummy variables that indicate the highest level of education achieved and ε is an error term.

Results from the basic OLS model are given in Table 2.2. Education, unsurprisingly, affects earnings significantly, although age does not. This may indicate that age is a poor proxy for experience for this group of ex-combatants.

DDRR was not significant at the 90% level in any of the specifications. Model 4 restricts the sample to employed individuals only. This accounts for the fact that daily income is a misleading measure of earnings for those who are not employed. The effect of DDRR is not significant in this specification either.

An analogous probit model serves as a baseline measure determinates of employment:

$$employed_{ij} = \delta_0 + \delta_1 X_i + \delta_2 E_i + \delta_3 C_j + \delta_4 DDRR_i + \varepsilon_{ij}\ . \qquad (2.2)$$

Table 2.3 reports results. The effect of completing high school significantly increases chances of employment, although lower levels of schooling are not significant. Single individuals were less likely to be employed, as were those living in Lofa, Montserrado

Table 2.2: OLS on Daily Income

Dependent variable:	Log of daily income				
Sub-sample				*Employed only*	
	(1)	(2)	(3)	(4)	(5)
Reintegration registered			0.02	0.12	-0.01
			(0.10)	(0.13)	(0.10)
Finished DDRR		0.05	0.01	-0.11	0.06
		(0.09)	(0.12)	(0.14)	(0.12)
age2	0.01	0.01	0.01	0.04	0.01
	(0.04)	(0.04)	(0.04)	(0.04)	(0.03)
Age^2	0.00	0.00	0.00	0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Elementary (d)	0.16	0.16	0.15	0.21	0.20**
	(0.12)	(0.11)	(0.11)	(0.14)	(0.10)
Junior high (d)	0.27**	0.26**	0.26**	0.22	0.30***
	(0.12)	(0.13)	(0.12)	(0.15)	(0.10)
Senior high (d)	0.43***	0.41***	0.43***	0.40**	0.43***
	(0.14)	(0.14)	(0.14)	(0.16)	(0.13)
Some university	0.41*	0.46*	0.41*	0.45**	0.44*
	(0.24)	(0.24)	(0.24)	(0.22)	(0.23)
Voc. Training (d)	0.54**	0.46*	0.52**	0.53*	0.37**
	(0.24)	(0.25)	(0.24)	(0.27)	(0.18)
Single (d)	-0.15*	-0.16*	-0.16*	-0.15	
	(0.09)	(0.09)	(0.09)	(0.10)	
Non-officer (d)	0.11	0.1	0.11	0.16	
	(0.09)	(0.09)	(0.09)	(0.10)	
Female (d)	0.12	-0.22**	0.12	0.28**	
	(0.09)	(0.11)	(0.09)	(0.11)	
Kpelle (d)	0.26***	0.25*	0.26*	0.16	
	(0.13)	(0.13)	(0.14)	(0.15)	
Constant	4.36***	4.54***	4.39***	3.70***	4.34***
	0.59	0.59	0.6	0.68	0.53
R-squared	0.13	0.14	0.13	0.18	0.07
Observations	381	381	381	252	381

Notes: * significant at 10%; ** significant at 5%; *** significant at 1%. Robust standard errors in brackets. (d) indicates dummy variable. a Employed only. All regressions exclude students, those currently in DDRR training at the time of the survey, individuals 65 or older, those who reported they were disabled, respondents with one or more missing relevant response and those with daily incomes of 700 LRD or higher, and includes fixed effects for county of residence at time of survey.

Table 2.3: Probit on Employment

Dependent Variable:	Employed			
	(1)	(2)	(3)	(4)
Reintegration registered			-0.30	-0.28
			-0.22	-0.19
Finished DDRR		0.57***	0.80***	0.44*
	(0.00)	(0.19)	(0.25)	(0.23)
Age	0.02	-0.01	0	0.06
	(0.08)	(0.08)	(0.08)	(0.07)
Age^2	0.00	0.00	0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)
Elementary (d)	0.15	0.11	0.11	0.18
	(0.22)	(0.23)	(0.23)	(0.18)
Junior high (d)	0.35	0.35	0.35	0.14
	(0.24)	(0.24)	(0.24)	(0.19)
Senior high (d)	0.76**	0.77**	0.72**	0.27
	(0.30)	(0.30)	(0.30)	(0.23)
Some university (d)	0.17	0.07	0	-0.47
	(0.60)	(0.61)	(0.60)	(0.54)
Single (d)	-0.72***	-0.79***	-0.78***	
	(0.17)	(0.17)	(0.17)	(0.00)
Non-officer (d)	-0.01	-0.04	-0.05	
	(0.19)	(0.19)	(0.19)	(0.00)
Female (d)	-0.40**	-0.39**	-0.42	
	(0.19)	(0.19)	(0.19)	(0.00)
Kpelle (d)	-0.12	-0.18	-0.15	
	(0.28)	(0.28)	(0.29)	(0.00)
Constant	1.6	2.04	2.04	-0.82
	(1.29)	(1.31)	(1.34)	(1.05)
Pseudo R-squared	0.2	0.21	0.21	0.02
Observations	380	380	380	380

Notes: Probit. * significant at 10%; ** significant at 5%; *** significant at 1%. Robust standard errors in brackets. (d) indicates dummy variable. a Employed only. All regressions exclude students, those currently in DDRR training at the time of the survey, individuals 65 or older, those who reported they were disabled, respondents with one or more missing relevant response and those with daily incomes of 700 LRD or higher, and includes fixed effects for county of residence at time of survey.

and Nimba counties. In contrast to the OLS model, we see significant and relatively strong effects on completing DDRR. This is in line with conclusions of Pugel (2007; 2009).

There are two potential explanations for the discrepancy between effects on income and employment: it could be the case that the DDRR program was successful in terms of employment but that employment opportunities are low paying, or this could be the result of selection bias—those with no DDRR had a higher average income than the general population, which most likely indicates that these high-earning individuals needed less help reintegrating economically. Pugel (2007) notes that those at both the top and the bottom of the income distribution were more likely to be non-participants in DDRR training, which supports this interpretation. A more skeptical view is that the employment result is driven by selection bias and that those who are more motivated to work or better suited to find employment were also more likely to complete DDRR training.

2.5.2 Propensity Score Matching

Given the lack of randomization in assignment to the treatment groups, lack of good instrumental variables and the cross-sectional nature of the data, there are no good options for identification strategies. To obtain a better estimate of the effects of the DDRR program, I employ propensity-score matching to estimate the effects of reintegration benefits and completing DDRR training on both daily income and employment. This does not solve the identification problem, as it does not account for self-selection on unobservable characteristics, however, propensity score matching does account for the influence of observable variables on selection into the treatment group, and can therefore provide a more accurate assessment of the treatment effect than simple OLS and probit.

The explanatory variables for employment and income are most likely not independent of participation in the various DDRR program components. Table 2.4 reports results from a probit regression on program variables, which demonstrates that that several personal characteristics, including age, ethnic group and region of residence are significant in determining whether an individual enrolls and completes DDRR. Model 4 from Table 2.4 is used to estimate the propensity score—the probability that an individual is assigned to the treatment group. Rosenbaum and Rubins (1984) show that by matching observations with propensity scores, the effect of a treatment can be calculated despite selection into the treatment group that is conditional on explanatory variable (Blundell and Costas 2008).

Table 2.4: Probit on Program variables

Dependent variable	Registered for benefits	Enrolled in training	Finished DDRR	Finished DDRR
Sub-sample			*Employed only*	
Explanatory Variable	(1)	(2)	(3)	(4)
Age	0.17*	0.32**	0.34***	0.48
	(0.09)	(0.13)	(0.12)	(0.15)
Age^2	0.00*	-0.01**	-0.01***	-0.01***
	(0.00)	(0.00)	(0.00)	(0.00)
Elementary (d)	0.22	0.28	0.66***	0.22***
	(0.20)	(0.21)	(0.24)	(0.27)
Junior high (d)	0.08	0.26	0.72***	-0.04
	(0.21)	(0.22)	(0.24)	(0.29)
Senior high (d)	-0.15	0.09	0.90***	0.02
	(0.24)	(0.26)	(0.26)	(0.32)
Some university (d)	-0.03	0.34	1.52***	0.54
	(0.48)	(0.47)	(0.52)	(0.56)
Single (d)	0.24	0.25		0.29
	(0.15)	(0.15)		(0.19)
Non-officer (d)	-0.04	-0.08		0.06
	(0.15)	(0.15)		(0.20)
Female (d)	-0.14	0		-0.29
	(0.16)	(0.17)		(0.23)
Kpelle (d)	0.48	0.49*		0.73**
	(0.29)	(0.28)		(0.34)
Constant	-2.29*	-4.82***	-6.08***	-9.10***
	1.29	1.73	1.72	2.09
Pseudo R-squated	0.16	0.19	0.07	0.19
Log likelihood	-278.29	-260.99	-179.64	-152.97
Observations	479	479	382	382

Notes: Probit. * significant at 10%; ** significant at 5%; *** significant at 1%. Robust standard errors in brackets. (d) indicates dummy variable. a Employed only. All regressions exclude students, those currently in DDRR training at the time of the survey, individuals 65 or older, those who reported they were disabled, respondents with one or more missing relevant response and those with daily incomes of 700 LRD or higher, and includes fixed effects for county of residence at time of survey.

To estimate the propensity score and match observations, I use the method developed by Becker and Ichino (2002). This algorithm divides the data into blocks and tests whether the balancing property is satisfied within in each block. This is the case if the mean propensity score and the means of each characteristic do no differ between treated and control units within a given block. The data is divided initially into 5 blocks, then further divided in blocks in which the balancing property is not satisfied. For the propensity score for completing the DDRR program, the final number of blocks is 6.

The pseudo R-squared of the propensity score model is around 0.19, although the goal is not to perfectly predict treatment, but rather to provide a score for matching individuals who are similar in terms of the explanatory variables (Caliendo and Kopeinig 2005).

Propensity score matching can only produce estimates of the treatment effect for individuals for whom there is common support, in which there is an overlap in characteristics for treated and untreated individuals. For this sample, the region of common support is sufficiently large, including 382 individuals, or 75% of the sample, and the analysis is restricted to the area of common support for the matching estimates.[8]

I use three matching techniques, after Becker and Ichino (2002). Each method has pros and cons, and the results of each are considered in order to obtain robust results. Firstly, stratification matching compares the outcome variable between treated and untreated individuals in each block in which the balancing property is satisfied. The stratification method has a potential drawback, in that blocks in which no treated or no control observations are observed are not counted. This is solved by the nearest-neighbor method, which matches each treated observation with the control observation that has the closest propensity score. The drawback of this method, however, is that the nearest match may have a drastically different propensity score. Finally, the kernel-density method matches all treated units with a weighted average of all controls. The weight for each control is inversely proportional to its distance from the treated observation and is calculated using a kernel-density function. The kernel-density matching method is advantageous in that it reduces variability of the average treatment effect on the treated (ATT) estimator, although it introduces bias at the edges of the propensity score's distribution (Blundell and Costa 2008).

Table 2.5 lists the estimations of the ATT and boot-strapped 95 percent confidence intervals for the effects of receiving reintegration benefits and completing a DDRR training program on log of daily income. The ATT estimate ranges between 0.08 and -0.03, although the bootstrapped 95 percent confidence intervals include zero for all three matching techniques. When considering only the sub-set of the sample that is employed, the ATT estimate produced by the nearest-neighbor method is negative, and while the others are positive, the standard errors are large and 95 percent confidence intervals include 0.

[8] Results are robust to including observations outside the area of common support.

Table 2.5: Matching on Daily Income

Dependent Variable:	Log daily income					
Sample				*Employed only*		
	Nearest-neighbor matching	Stratification matching	Kernel-density matching	Nearest-neighbor matching	Stratification matching	Kernel-density matching
	(1)	(2)	(3)	(4)	(5)	(6)
Average Treatment effect on the	-0.03	0.07	0.08	-0.06	0.08	-0.01
Standard error	0.13	0.09		0.15		
Bootstrapped standard error	0.13	0.08	0.1	0.21	0.1	0.15
95% confidence interval (bootstrapped)						
normal	-0.38	-0.08	-0.11	-0.47	-0.11	-0.32
	0.32	0.22	0.27	0.36	0.28	0.3
percentile	-0.14	-0.08	-0.1	-0.31	-0.1	-0.23
	0.41	0.22	0.24	0.46	0.26	0.44
bias corrected	-0.18	-0.08	-0.13	-0.33	-0.1	-0.23
	0.2	0.22	0.25	0.46	0.34	0.5
number treated	74	72	74	54	53	54
number control	59	214	308	41	134	140
Number of observations	382	382	382	252	252	252

Notes: All models exclude students, individuals 65 or older, those who reported they were disabled, respondents with one or more missing relevant response and those with daily incomes of 700 LRD or higher. Excluding observations that fall outside the region of common support.

Table 2.6: Matching on Employed

Dependent Variable:	Employed		
Sample			
Matching Method	Nearest-neighbor matching	Stratification matching	Kernel-density matching
	(1)	(2)	(3)
Average Treatment effect on the Treated	0.06	0.14	0.16
Standard error	0.09	0.07	
Bootstrapped standard error	0.13	0.07	0.07
95% confidence interval (bootstrapped)			
Normal	-0.2	0	0.01
	0.31	0.27	0.3
Percentile	-0.09	0.01	0.02
	0.43	0.26	0.29
Bias corrected	-0.13	-0.05	0.03
	0.27	0.26	0.31
number treated	74	74	74
number control	59	214	214
Number of observations	382	382	382

Notes: All models exclude students, individuals 65 or older, those who reported they were disabled, respondents with one or more missing relevant response and those with daily incomes of 700 LRD or higher. Excluding observations that fall outside the region of common support.

Table 2.6 reports results from matching on employment. Although the nearest-neighbor method produces a confidence interval that overlaps with 0, the stratification and kernel-density methods produce ATT's of 0.14 and 0.16 that are statistically different than 0 at the 95%-confidence level.

Again, these estimates are likely more precise than simple OLS, but may still be biased by unobservable characteristics that influence selection to the treatment group.

2.5.3 Interaction effects

Next, I add interactions between DDRR participation and education level to the OLS model (2.1) in order test the possibility that the program had an effect on some cohorts, even though the overall effect was not significant.

(2.3) $$ln\ (dailyincome)_{ij} = \delta_0 + \delta_1 X_i + \delta_2 E_i + \delta_3 C_j + \delta_4 DDRR_i + \delta_5 E_i{*}DDRR_i + \varepsilon_{ij} \qquad (2.3)$$

Similarly, (4) adds interactions between DDRR completion and level of education to (4), the probit model on employment:

$$employed_{ij} = \delta_0 + \delta_1 X_i + \delta_2 E_i + \delta_3 C_j + \delta_4 DDRR_i + \delta_5 E_i{*}DDRR_i + \varepsilon_{ij}\ . \qquad (2.4)$$

Results are reported in Table 2.7. In the OLS regressions, DDRR remains insignificant, as are the interaction terms with all levels of education. In the probit regressions, adding the education interaction terms results in a significant but *negative* coefficient, without controlling for registering for benefits, and a positive but insignificant coefficient when the registration control is added. As discussed above, education level independently affects chances of completing the program and employment, and this may account for the strong effects observed on employment in (2.2).

The survey results indicate that DDRR participation varied greatly by region, which also affects employment and income independently. Regional interactions are added to (2.1) and (2.2) and analyzed using OLS and probit, respectively:

$$employed_{ij} = \delta_0 + \delta_1 X_i + \delta_2 E_i + \delta_3 C_j + \delta_4 DDRR_i + \delta_5 C_j{*}DDRR_i + \varepsilon_{i\,j}\ . \qquad (2.6)$$

Results are reported in table 8. While there is a strong positive effect of DDRR on employment, the effect on income is negative. The coefficients for the interaction between DDDR and Montserrado county—where around 65% of those in the sample who finished DDRR training live—have the opposite signs, however. This may reflect different economic circumstances in the capital and the rest of the country, a difference in quality or availability in DDRR training or both.

$$ln\ (dailyincome)_{ij} = \delta_0 + \delta_1 X_i + \delta_2 E_i + \delta_3 C_j + \delta_4 DDRR_i + \delta_5 C_j{*}DDRR_i + \varepsilon_{ij}\ , \qquad (2.5)$$

2.6 Discussion

This analysis has shown that it is unlikely that the DDRR program had any significant effect on the daily income of participants. It is, however, possible that the program increased the chances that participants would find employment, as the results seem to

Table 2.7: Interactions with DDRR and education

Estimator	OLS			Probit	
Dependent Variable	Log daily income			Employed	
Sample			*Employed only*		
	(1)	(2)	(3)	(4)	(5)
Reintegration registered		0.02	0.12		-0.29
		-0.1	-0.13		-0.22
Finished DDRR	0.11	0.09	-0.14	-0.17***	0.08
	(0.30)	(0.31)	(0.35)	(0.74)	(0.76)
Age	0.01	0.01	0.04	-0.02	-0.01
	(0.04)	(0.04)	(0.04)	(0.08)	(0.08)
Age^2	0.00	0.00	0.00	0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Elementary (d)	0.15	0.15	0.21	0.12	0.12
	(0.12)	(0.12)	(0.15)	(0.24)	(0.24)
DDRR*elementary	0.01	0.02	0.07	0.5	0.46
	(0.34)	(0.34)	(0.38)	(0.80)	(0.79)
Junior high (d)	0.29**	0.29**	0.23	0.25	0.25
	(0.13)	(0.13)	(0.16)	(0.25)	(0.25)
DDRR*junior high	-0.14	-0.13	0	0.95	0.9
	(0.33)	(0.33)	(0.35)	(0.81)	(0.80)
Senior high (d)	0.43***	0.43***	0.40**	0.59*	0.55*
	(0.16)	(0.16)	(0.19)	(0.32)	(0.32)
DDRR*senior high	-0.07	-0.07	0.02	1.46	1.44
	(0.36)	(0.36)	(0.40)	(0.93)	(0.93)
Some university (d)	0.70***	0.70***	0.33*	0.45	0.35
	(0.26)	(0.26)	(0.18)	(0.90)	(0.90)
DDRR*some uni.	-0.58	-0.58	0.43	-0.14	-0.11
	(0.46)	(0.46)	(0.35)	(1.36)	(1.35)
Voc. Training	0.4	0.4	0.48		
	(0.36)	(0.36)	(0.38)		
Single (d)	-0.16*	-0.16*	-0.17	-0.80*	-0.8
	(0.09)	(0.09)	(0.11)	(0.17)	(0.17)
Female (d)	0.11	0.11	0.26	-0.37*	-0.40***
	(0.09)	(0.09)	(0.11)	(0.19)	(0.20)
Kpelle (d)	0.26*	0.26*	0.17	-0.18	-0.15**
	(0.14)	(0.14)	(0.16)	(0.28)	(0.29)
Constant	4.43***	4.43***	3.83***	2.17	2.17
	-(0.59)	-(0.60)	-(0.70)	-(1.33)	-(1.33)
R-squared	0.13	0.13	0.18		
Observations	381	381	252	380	380

Notes: OLS. * significant at 10%; ** significant at 5%; *** significant at 1%. Robust standard errors in brackets. (d) indicates dummy variable. a Employed only. All regressions exclude students, those currently in DDRR training at the time of the survey, individuals 65 or older, those who reported they were disabled, respondents with one or more missing relevant response and those with daily incomes of 700 LRD or higher, and includes fixed effects for county of residence at time of survey.

Table 2.8: Interactions with DDRR and county of residence

Estimator	OLS			Probit	
Dependent Variable	Log daily income			Employed	
Sample			*Employed only*		
	(1)	(2)	(3)	(4)	(5)
Reintegration registered		0.05	0.17		-0.17
		(0.10)	(0.13)		(0.24)
Finished DDRR	-0.50***	-0.53***	-0.55***	3.73***	3.81***
	(0.18)	(0.19)	(0.22)	(0.48)	(0.50)
Age	0.00	0.00	0.03	0.02	0.03
	(0.04)	(0.04)	(0.04)	(0.09)	(0.09)
Age^2	0.00	0.00	0.00	0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Bomi	0.48***	0.50***	0.59****		
	(0.14)	(0.14)	(0.16)		
DDRR*Bomi	0.04	0.03	-0.13		
	(0.27)	(0.27)	(0.28)		
Bong	0.01	0.01	0.13		
	(0.16)	(0.17)	(0.19)		
DDRR*Bong	0.03	0.02	-0.14		
	(0.20)	(0.20)	(0.25)		
Grand Cape Mt.	0.26*	0.27*	0.25	1.19**	1.15**
	(0.15)	(0.15)	(0.17)	(0.57)	(0.58)
DDRR*Grand Cape	1.31***	1.29***	0.98***	4.95***	-4.87***
	(0.33)	(0.33)	(0.29)	(1.05)	(1.06)
Grand Gedeh	0.01	0.01	0.12		
	(0.22)	(0.22)	(0.24)		
DDRR*Grand Gedeh	0.58**	0.58**	0.43		
	(0.29)	(0.29)	(0.32)		
Montserrado	0.23*	0.23**	0.32**	-0.48	-0.49
	(0.12)	(0.12)	(0.14)	(0.31)	(0.31)
DDRR*Monteserrado	0.62***	0.62***	0.56**	-3.28***	-3.24***
	(0.20)	(0.20)	(0.24)	(0.51)	(0.52)
Nimba	0.13	0.13	-0.17	-1.12***	-1.14***
	(0.19)	(0.19)	(0.17)	(0.40)	(0.41)
DDRR*Nimba	-0.02	-0.04	0.25	-2.95***	-2.90***
	(0.25)	(0.25)	(0.25)	(0.86)	(0.86)
Single (d)	-0.18**	-0.18**	-0.17	-0.88***	0.88***
	(0.09)	(0.08)	(0.10)	(0.20)	(0.20)
Female (d)	0.13	0.14	0.31***	-0.26	-0.28
	(0.09)	(0.09)	(0.11)	(0.23)	(0.23)
Kpelle (d)	0.32**	0.32**	0.22	-0.2	-0.18
	(0.14)	(0.14)	(0.16)	(0.33)	(0.33)
Constant	4.36***	4.37***	3.86***		
	(0.61)	(0.62)	(0.73)		
R-squared	0.14	0.14	0.19		
Observations	381	381	252	380	380

Notes: OLS. * significant at 10%; ** significant at 5%; *** significant at 1%. Robust standard errors in brackets. (d) indicates dummy variable. All regressions exclude students, those currently in DDRR training at the time of the survey, individuals 65 or older, those who reported they were disabled, respondents with one or more missing relevant response and those with daily incomes of 700 LRD or higher, and includes controls for education.

indicate an increase in the employment rate for those who completed DDRR training in all of the models except those that control for interactions with education. It is likely that program helped certain types of individuals find jobs, but that those jobs were not particularly high-paying. This result is consistent with other studies on job-training programs, which have a particularly poor record in developing and transition countries (Betcherman, Olivas and Dar, 2004, Puerta 2010). However, these results should be taken with a grain of salt, since there are unobservable characteristics that likely influence both the chances that an individual enrolls in the DDRR program and the chances that he or she is employed. This is a general problem associated with measuring the effects of job training in the absence of any randomized method for assigning individuals into the treatment group (Heckman et al 2006).

With limited employment opportunities, even high-quality training may be of little use to participants.

While it is not politically feasible to provide training to ex-combatants randomly, there may be some arrangements for future DDR programs that can provide a better opportunity for assessing the effects of job training, and therefore the approach as a whole.

While DDR should be ultimately be judged on its overall ability to reintegrate individuals for the sake of preventing future conflict, the linked approach depends on the economic portion of the programs actually functioning. Authors such as Muggah (2009) suggest that this approach needs to be reconsidered in light of failures to deliver real results. If DDR has been ineffective in facilitation economic reintegration for former combatants, the reason may be that the programs approach the problem in the wrong way, and job training will not be effective in certain economic situations. This would imply that effort and funds should be spent on general development rather than specifically targeting ex-combatants. This paper has attempted to demonstrate that the DDRR job training in Liberia was not necessarily effective and that possible the problem lies in the effectiveness in job training, rather than the linked approach to the reintegration of ex-combatants.

While most studies have—rightfully—concentrated on the overall impacts of DDR programs on reintegration, this paper adds to this discussion by noting that, at least in Liberia, the job training programs might have failed to produce intermediate economic outcomes. Any measure success or failure in terms of more generalized outcomes of successful reintegration should be tempered with a healthy dose of skepticism over whether the training portions of the programs were of sufficient quality to really make a difference.

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Chapter 3

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

Abstract

The stability of many post-conflict societies rests on the successful reintegration of former soldiers. 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 experience 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. We find no evidence of mistrust or preference-based discrimination against ex-soldiers among unrelated members of receiving communities. These results suggest that the impact of child soldiering on social capital, in contrast to human capital, is not necessarily detrimental.

JEL Classification: C93, D03, D74, O12

Keywords: trust, cooperation, field experiment, civil war, endogenous preferences, reintegration of soldiers

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3.1 Introduction

In conflicts around the world, the forcible recruitment of soldiers, often children, is a widespread practice among many militaries and insurgent groups (Beber and Blattman 2013, Blattman and Miguel 2010).[10] Several million children under the age of 18 are estimated to have served in combat since 2001 and the participation of child soldiers has been documented in armed conflicts in almost every region of the world (Human Rights Watch 2008). In 2013 the United Nations reported the ongoing use of children in conflicts 16 countries. 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. At the same time, pioneering surveys among this important subject pool have revealed that gaps in economic or social outcomes between former soldiers and their peers are common (Humphreys and Weinstein 2007, Restrepo and Muggah 2009, Blattman and Annan 2010). The underlying sources of these gaps, however, remain an open question.

Since reintegration outcomes are determined by economic and social interactions between ex-combatants and the communities to which they return, it is important to study both the influence of soldiering on the behavior of returnees as well as behavior towards returnees by the receiving communities. Further, in order to gain a deeper understanding of behavior towards former soldiers, it is central to distinguish whether potential differential treatment of former soldiers compared to their peers originates in preferences (taste-based discrimination) or in beliefs about future behavior of former soldiers (statistical discrimination). Similarly, the soldiering experience may impact individual behavior by shifting either preferences or beliefs. Such distinctions are important, since each of these factors has different policy as well as behavioral implications, such as understanding whether individual preferences adapt in response to key experiences. This paper contributes to the existing literature by employing experimental tools, in addition to surveys, in order to study such detailed aspects.

The second distinguishing feature is our focus on some of the key components of

[1]Civil wars have afflicted a third of all countries and two thirds of Africa since 1991 (Blattman and Miguel 2010).

social capital—trust and cooperative behavior—a crucial factor since it determines access to jobs, credit and participation in informal saving and insurance arrangements,[11] 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 percent of the population 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 impacts of soldiering is that, at the point of recruitment, soldiers were not a select group compared to their peers. This is in contrast to ex-fighters in most of the other post-conflict societies. In particular, we build on previous evidence from Blattman and Annan (2010) and argue that the LRA recruitment methods created exogenous variation in conscription, which was 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 trust towards former soldiers and what are their motivations for doing so.

A priori, it is not clear whether and how soldiering affects cooperative behavior, due to a paucity of data for this difficult-to-access subject pool.[12] 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

[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).

[12] Recently, there is a growing interest in economics to understand the endogeneity of preferences and norms to their environment. Bowles (1998) and Fehr and Hoff (2011) provide excellent overviews, Fehr, et al. (2008), Almas et al. (2010), for example, show evidence of strong developmental patterns of social preferences during childhood and early adolescence, and Kosse et al. (2014) document a causal impact of social environment on pro-social behavior in children.

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 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), in line with theories that 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 cooperation is crucial during war, social preferences may adapt in response to experiences of intergroup conflict and exposure to survival threat, and such experience may activate or intensify preferences which facilitate within-group cooperation.[13] However, the above-mentioned evidence concerns the social behavior of *recipients* of violence; there is no comparable evidence using behavioral experiments with ex-soldiers, who were often *perpetrators* of violence. The most closely related empirical evidence 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, despite the negative impacts on economic outcomes (Blattman and Annan 2010).[14] While this important piece of evidence raises the possibility that ex-soldiers have a greater willingness to help and participate in local collective action, it may also indicate distinct political interests, which may not benefit others.

Despite a widespread concern that former soldiers face stigmatization, little is known about whether receiving communities discriminate against former soldiers since, to best of our knowledge, this is the first study examining the issue quantitatively. In principle, preference-based discrimination reflects dislike or anger against certain groups; such discrimination against ex-soldiers could arise if receiving communities blame them for their violent acts while fighting. Members of receiving communities may also infer that soldiers will continue to behave anti-socially, given previous violent and destructive acts committed against the local population, or that exposure to traumatic events may cause

[13] 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, 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.

[14] 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.

psychological damage that would cause them to behave less cooperatively. These beliefs could lead to statistical discrimination.[15] 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 a strong positive relationship between being a former LRA soldier and the share send back to Senders, especially among those who soldiered at a younger age. The effect of soldiering on trustworthiness is strong for ex-soldiers who were abducted at an early age (below 14 years of age) and much muted for participants who were abducted during late adolescence or adulthood. The observed increase in cooperative behavior of former child soldiers is economically important and we show it is not driven by differences in the economic well-being, differences in understanding of the task or outliers. Strikingly, the relationship is also robust to replacing experimental measures of cooperative behavior with survey-based proxies (index of participation in local community groups). 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

[15] 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).

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 positive but statistically not 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.[16] 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.

Forcible recruitment of children, high exposure to violence and participation in combat is not a peculiar practice of LRA and is common in many other conflicts (Beber and Blattman 2013). Achvarina and Reich (2006) find, across several conflicts, that the best predictor of the percentage of child-soldiers among total combatants is simply the armed groups' accessibility to camps for refugees and internally displaced persons, while factors one would expect to influence the voluntary conscription of children, such as the number of orphaned children and poverty rates, were much weaker predictors. Once with a rebel group, high rates of exposure to violence, comparable to the Ugandan case, including the perpetration of violent acts, has been documented by studies on former child soldiers in the Democratic Republic of the Congo, Sri Lanka and Sierra Leone (Betancourt et al. 2013). Although we would caution against generalizing our findings to post-conflict settings where there is a high degree of self-selection into armed groups, we

[16] 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.

believe our findings may speak to debates about legacies of conflict and post-conflict reconstruction in settings with high prevalence of forcible recruitment of soldiers. The remainder of the paper is organized as follows. In Section 3.2 we briefly describe the background: the conflict in Northern Uganda and recruitment strategy of the Lord's Resistance Army. Section 3.3 describes the sample selection and the experimental design. In Section 3.4 we present the empirical results about behavioral differences between former soldiers and their peers. Section 3.5 presents results about differential treatment of former soldiers by their communities. Section 3.6 concludes and provides brief discussion of policy implications.

3.2 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 (where our study was conducted) was widespread, affecting virtually the entire population.[17] 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

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

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, (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. Eighty four 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), 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.

3.3 Experimental Design

3.3.1 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 3.A.1).[18]

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.

3.3.2 Experimental tasks

Senders

The individuals recruited as Senders were told that the experiment would be conducted in

[18] 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).

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[19] 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, $S \in \{0; 1000, 2000, \ldots 3S\}$. 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 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

[19] 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.

Senders care about the welfare of Receivers or adhere to sharing norms[20], 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 that included several pieces of information about the Receiver. We varied information on the Receiver's experiences during the conflict, in a between-subject design. 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.[21]

There are several noteworthy features of the profiles we provided subjects. First, in addition to information related to the Receiver's abduction status, we included several additional characteristics, in order to make relevant information about LRA experience appear more natural and to mask the fact that this 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.[22] 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).

[20] 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.

[21] This was motivated by our effort to make profiles naturally-looking. If Senders in the control condition conjectured that the Receiver is with some probability be a former soldier (given the abduction rate of around 20% in the population we study), this design choice could reduce differential treatment across conditions.

[22] 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."

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. As the decision to trust is a risky one, we also elicited Senders' attitudes towards risk and use it as a control variable.[23] 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.[24] 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,[25] 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.[26]

3.3.4 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

[23] For a similar approach to controlling for the attitudes to risk in trust decisions, see, for example, Ashraf, Bohnet and Piankov (2006).
[24] 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.
[25] 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.
[26] Former soldiers were not, however, informed that the Sender had any information regarding the length of their soldiering, simply that they had been abducted.

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 3.1.[27]

3.3.5 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),[28] and extensively used visual aids, to illustrate options and payoffs (see Appendix Figure 3.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

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

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

Table 3.1: Summary Statistics: Means (s.d.)

Sample	*Receivers* (1)		*Senders* (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)	4.18	(2.98)
No. of siblings	5.01	(2.74)		
Mother no school (d)	0.65	(0.48)	0.85	(0.36)
Father no school (d)	0.27	(0.45)	0.35	(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)
No. of current HH members	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	-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	

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] 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, soldiers. Next, subjects made decisions behind cardboard dividers to keep their choices private from the experimenter who provided the one-on-one explanation.[29] 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).

3.4 Behavior and Beliefs of Former Soldiers

3.4.1 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

[29] 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.

Table 3.2: Family characteristics and Abduction

	Abducted by the LRA				
Dependent Variable	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)
No. 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.

not 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 3.2 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.[30]

The lack of systematic correlations between observable pre-existing family (family

[30] 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.

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.

3.4.2 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:

$$D_i = \alpha + \beta A_i + \gamma X_i + \varepsilon_i \qquad (3.1)$$

where D_i is individual *i*'s action in the experiments, A_i is a variable capturing a soldiering experience, X_i is a vector of individual characteristics, 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.[31]

Does the cooperative behavior of former soldiers differ from their peers? In column 1 of Table 3.3, we regress a dummy variable that equals 1 if an individual had ever been abducted by the LRA on trustworthiness. The results indicate that former soldiers

[31] 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).

returned more in the trust game than their peers: abduction is associated with an increase of roughly 5 percentage points in trust-game back-transfers (p=0.13). We next examine the relationship between abduction length and trustworthiness, since the simple binary measure includes individuals who were abducted for very short periods of time (as little as one day) as well as those abducted for long periods of time. 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 3 we find a strong positive relationship between length of soldiering and the amount returned in the trust game (p-value<0.001).

Next, we test whether the link between soldiering and trustworthiness is more pronounced or more enduring when soldiering is 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 the development of social preferences.[32] 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).[33] 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, and test whether the effect of soldiering on trustworthiness depends on whether it was experienced during an early age, rather than during adolescence and adulthood.

We find that age of abduction matters. In Column 2 we compare three groups of subjects: those who were abducted at early age (less than 14), those who were abducted at later age and those who had never been 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 164 UGX, which is just under half the average daily cash income in our sample. At the same time, we find virtually no difference in

[32] Some evidence shows that social preferences may be innate to a certain degree (Hamlin, Wynn and Bloom, 2007), however, this does not negate evidence that children preference formation is more sensitive during certain periods.

[33] 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.

Table 3. 3: Abduction by the LRA and Trustworthiness

Dependent Variable	Trustworthiness: Average percentage returned in trust game			
Sample	*Receivers*			
	All		*All*	
	(1)	(2)	(3)	(4)
Abducted	4.99			
	(3.26)			
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)		1.42		0.37
		(3.89)		(4.05)
Abd. length x abd. adol./adult (≥14)				-2.41*
				(1.19)
Age	-0.06	0.20	0.01	0.09
	(0.42)	(0.42)	(0.41)	(0.43)
Number of siblings	-0.31	-0.36	-0.31	-0.37
	(0.40)	(0.39)	(0.39)	(0.40)
Father no school (d)	-2.11	-2.10	-1.44	-1.29
	(2.56)	(2.54)	(2.62)	(2.59)
Mother no school (d)	1.68	1.59	0.87	1.15
	(3.20)	(3.14)	(3.15)	(3.20)
Mother alive in '96 (d)	-4.32	-3.68	-3.47	-3.08
	(5.95)	(5.76)	(6.27)	(6.15)
Father alive in '96 (d)	0.36	0.42	0.68	0.78
	(2.96)	(2.73)	(2.93)	(2.83)
Log of weekly income	-0.05	-0.07	-0.09	-0.11
	(0.28)	(0.28)	(0.29)	(0.29)
Current household size	0.36	0.37*	0.34*	0.31*
	(0.21)	(0.20)	(0.18)	(0.17)
Married (d)	-6.58	-6.55	-6.16	-6.43
	(4.04)	(4.00)	(3.99)	(4.20)
Literate (d)	6.17*	6.41*	6.94**	6.57**
	(3.26)	(3.27)	(3.24)	(3.15)
Schooling (years)	-0.47	-0.44	-0.45	-0.45
	(0.59)	(0.59)	(0.56)	(0.57)
Wealth	1.26***	1.25***	1.18**	1.21***
	(0.44)	(0.45)	(0.44)	(0.43)
Partner in experiment male (d)	3.96	3.25	3.79	3.26
	(2.42)	(2.40)	(2.45)	(2.45)
Constant	36.64***	30.31**	35.39***	33.92**
	(11.96)	(11.92)	(12.47)	(12.50)
Observations	333	333	328	328
R-squared	0.07	0.08	0.07	007

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.

behavior between those who soldiered during later age and the non-abducted group (p=0.72). Table 3.A.3 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.

As with the binary measures of abduction, we observe that the effect of the length of time spent soldiering is stronger for those who were abducted when younger than 14 years of age and mute for those abducted at a later age. Column 4 of Table 3.3 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. Note that in these regressions we control for measures of wealth, education as well current family characteristics, indicating that differences in current socio-economic characteristics are unlikely to explain the differences in choices. In Table 3.A.2 we control only for characteristics that were unlikely to be changed by conflict and arrive to the similar conclusions.

Observation 1: We find a positive relationship between LRA soldiering and higher trustworthiness, which is driven by former soldiers who were abducted at an early age (younger than 14 years of age). The effect 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). While the frequency of such decisions should increase along with inequality aversion, we recognize that this is not a perfect measure of preference types. We find no link between soldiering and prevalence of choosing the equal split (columns 1-4 of Appendix Table 3.A.4.), 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 soldiering at an early age and the proportion sent back is positive for both potential amounts that could have been sent. 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 differed in their exposure various to initiation practices (prayer ceremonies, beating others, receiving beating, forcing to kill), 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 to indices of violence received, committed or witnessed by an individual. The exception is the index of violence against other family members (Panel A Table 3.A.5). Interestingly in light of previous findings, LRA experiences also seem to be related to age of abduction. We find that those who were abducted at an earlier age report more exposure to LRA prayer ceremonies, receiving more violence and were also more likely to be forced to beat and kill others while with LRA, compared to those who were abducted at a later age (Panel B of Table 3.A.5). We also find that they were more likely to participate in informal reintegration ceremonies[34] 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. To begin, we show that the result 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 3.3), excluding the control variables which may have been affected by soldiering, or including village fixed effects (Appendix Table 3.A.2). The similarity between these various specifications indicate that the relationship is not likely due to differences in socio-economic characteristics of subjects either before or after the conflict or differences across villages.

Next, we consider several sub-sample analyses. First, we test for the possibility that differences between ex-soldiers and non-soldiers in their ability to understand the task may drive our results. In Columns 1-2 of Panel A, appendix Table 3.A.6, we drop 82 individuals who did not report measures of literacy, and repeat the main analysis: results are robust. As another check on understanding, we exclude subjects who did not answer all three of the comprehension questions we administered before the experiment on the first try, which amounts to 76 subjects, the results of main specification are again robust (columns 3-4).

Second, the results are not driven by the few ex-soldiers who were with LRA for many years. Excluding 13 subjects who were with the LRA for more than five years (columns 1-2, Panel B, Appendix Table 3.A.6) does not affect the main finding.

Third, in Section 3.4.1 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

[34] 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)).

Table 3.4: 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	*Receivers*							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Abducted (d)	0.06				0.00			
	(0.07)				(0.08)			
Abduction length (years)			0.00	0.00			-0.03	-0.04**
			(0.01)	(0.02)			(0.02)	(0.02)
Abducted young (< 14 years) (d)		0.05				-0.05		
		(0.10)				(0.11)		
Abducted as adolescent/adult (≥14) (d)		0.07		0.05		0.05		0.03
		(0.09)		(0.11)		(0.12)		(0.13)
Abd. length x abd. adol./adult (≥14)				0.00				0.03
				(0.03)				(0.04)
Constant	1.65***	1.66***	1.64***	1.68***	1.19***	1.28***	1.26***	1.31***
	(0.33)	(0.32)	(0.33)	(0.32)	(0.43)	(0.46)	(0.44)	(0.44)
Observations	333	333	328	328	333	333	328	328
R-squared	0.04	0.04	0.04	0.04	0.03	0.04	0.04	0.04

Note: OLS. Robust, standard errors in parentheses, clustered at village level. * significant at 0.10%; ** significant at 5%; *** significant at 1%. (d) indicates dummy variable. In Columns 1-3 the dep. var. is the amount Receivers expected to be transferred by Senders in trust game. In Columns 4-6 the dep. var. is the amount Receivers expected to be transferred by Senders in the dictator game. In all columns, we control for the same set of variables as in Table 3.3.

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 1-2 of panel C, Appendix Table 3.A.6 show that the main findings hold when comparing this sub-sample of ex-soldiers to non-abducted peers, in line with the interpretation that LRA screening at the outset is unlikely to drive our results.

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 3-4 in panel C of Appendix Table 3.A.6).

To supplement the results from the experiment, we test whether soldiering mimics systematic differences in behavior in the naturally occurring world by replacing the experimental measure with survey-based proxies for cooperative behavior. We find strikingly similar patterns. We find a positive link between soldiering during an early age and an index of participation in local community groups and a negative correlation with the likelihood of having a physical fight in the past six months (Appendix Table 3.A.7).

3.4.3 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 1-4 of Table 3.4 we find virtually no link between soldiering and the amount that was expected to be received in trust game. In column 7 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 8), it is 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.[35]

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.

3.4.4 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 expiate guilt that they feel for acts which they have committed while with the LRA. Our

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

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, 18 percent of former soldiers report that they were ever blamed 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 surviving the conflict, and 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 3.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.

Nevertheless, it is worth noting that the existing evidence suggests that children were *less* likely to be allocated dangerous tasks and participate in battles. Annan and Blattman (2009) describe that:

> *Rebel officers questioned a young child's ability to handle a firearm, or be an effective fighter… In general, the survey evidence suggests young children below the age of 11 or 12 were entrusted with military tasks less frequently*

than older youth, while adolescents seem to have been at least as dependable and effective as young adults (and in some cases more so).

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.

3.5 Behavior of Receiving Communities towards Former Soldiers

In this section we explore whether Senders behave differently towards former soldiers and, if so, whether this is due to social preferences or beliefs about 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 3.A.9).

3.5.1 Trust

On average, Senders transfer 56 percent of their 2,000 UGX endowment to the Receivers in the trust game.[36] 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 3.5) as well as distribution of choices[37]

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

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

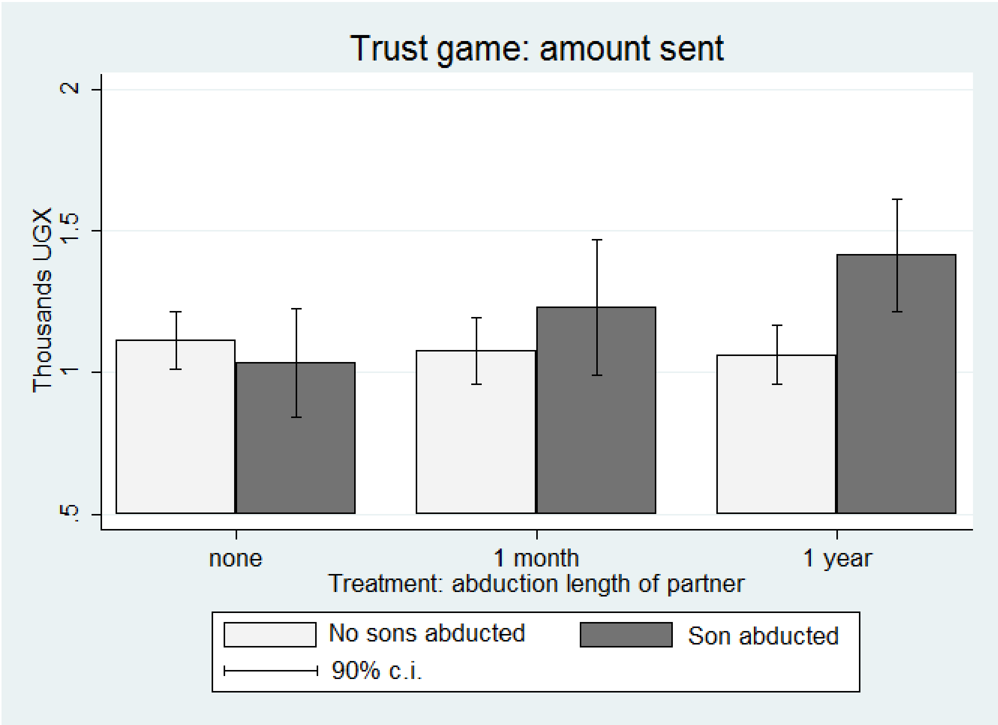

Figure 3.1
Amount sent in trust game, disaggregated by treatment and the abduction history of subjects' sons.

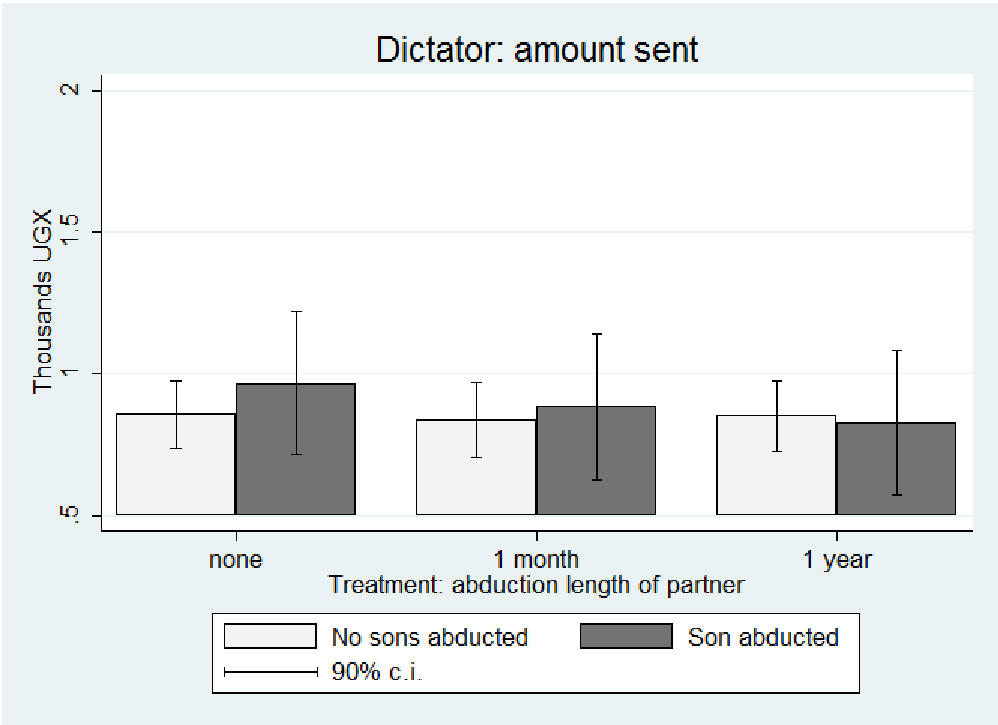

Figure 3.2
Amount sent in dictator game, disaggregated by treatment and the abduction history of subjects' sons.

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 1 and columns 2-4 of Table 3.5 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 ont trust.[38]

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

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

[38] 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).

Table 3.5: 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 (d)	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 (d)	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 (d)	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%. (d) indicates dummy variable. 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 current household size.

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

3.5.2 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 3.5). 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 among subjects who have an abducted son. This is equivalent to 0.25 standard deviations in our sample.[39] We thus conclude that there is no evidence of differences in kindness towards ex-soldiers and non-soldiers.

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.

[39] 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.

3.5.3 Expectations of trustworthiness

In order to understand possible differences in expectations of trustworthiness, we use two different measures. First, we directly examine beliefs about how much Senders expect Receivers to transfer back. The variable of interest is the mean of the percentage expected by the Sender for both possible amounts she could have sent: 1,000 UGX and 2,000 UGX.[40] 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).[41] 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 3.6, 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 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

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[40] 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).

[41] 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.

Table 3.6: Beliefs about Trustworthiness of Former Soldiers

Dependent variable:	Investment: Difference between trust and dictator allocations (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 (d)	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 (d)	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 (d)	-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 the same set of variables as in Table 3.5.

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.

3.5.4 Further Results

The set of results presented above strongly suggests that having a son who is an ex-soldier 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 3.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.

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.[42]

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

[42] 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.

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 status.

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.

3.6 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 artefactual field experiments, in contrast to responses to survey questions.

In this paper we study the impact of soldiering on two of the most important components of social capital—trust and cooperative behavior—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 soldiering for the Lord's Resistance Army is positively related to the more cooperative 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, 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. Another 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.

Our results 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 and that these war-induced behavioral responses are not too context-specific. 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.

Our results have potentially important policy implications for post-conflict societies, where the economic and social reintegration of former child soldiers may be complicated by stigmatization. We do not find evidence of such discrimination in the Ugandan context, suggesting that the concerns about stigma are less warranted in contexts where receiving population perceives participation in combat as involuntary. This does not mean that the re-integration of former soldiers was without difficulty, and several studies find an increase in mental health problems such as anxiety and post-traumatic stress syndrome in former LRA soldiers (Klasen et al., 2010). Rather, our results suggest that gaps in economic outcomes between ex-soldiers and their peers are not driven by less cooperative behavior or discrimination on the part of receiving communities, and thus it may be more efficient for interventions to focus on rebuilding human capital, especially schooling and training that was lost or delayed due to time spent fighting. Next, we find that there is limited awareness of the greater cooperativeness of former soldiers by members of receiving communities, which gives additional rationale for designing reintegration programs that provide training and services jointly with non-soldiers, instead of providing services to former soldiers separately.[43] Doing so may provide an additional benefit by facilitating the updating of beliefs and increasing social capital in those communities.

Although the psychological and human-capital costs of being a forcibly recruited soldier are enormous (see Blattman and Annan, 2010) and Table 3.A.12), 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.

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

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Appendix to Chapter 3:

Additional Tables and Figures

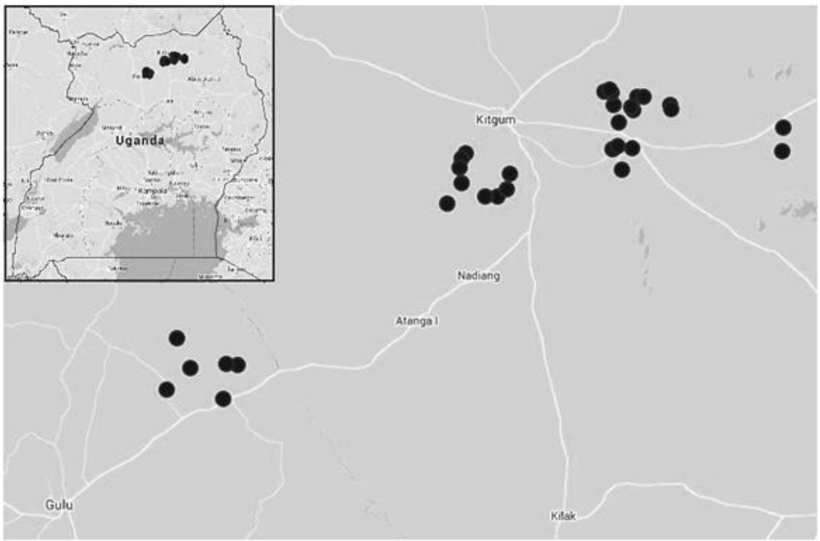

Figure 3. A.1: Location of villages in which experimental sessions were conducted in Gulu and Kitgum districts, northern Uganda

Figure 3.A.2: Group explanation of experimental task

Table 3.A.1: Exposure to Violence and Abduction History: Mean (s.d.)

Sample	*Receivers*		*Senders*	
	(1)		(2)	
Panel A: Conflict Experience				
Index of violence received (0-5) [a]	2.92	(1.82)	2.57	(1.84)
—bullets shot at home	0.62	(0.49)	0.59	(0.49)
—received beating or attacked	0.60	(0.49)	0.56	(0.50)
—tied up or taken prisoner	0.56	(0.50)	0.43	(0.50)
—received serious physical injury	0.55	(0.50)	0.48	(0.50)
—forced to carry heavy loads	0.59	(0.49)	0.51	(0.50)
Index of violence against family(0-2)[a]	1.59	(0.72)	1.40	(0.81)
—family member or friend died	0.78	(0.41)	0.68	(0.47)
—family member or friend disappeared/abducted	0.81	(0.39)	0.72	(0.45)
Index of violence witnessed (0-4)[a]	2.41	(1.30)	1.92	(1.37)
—witnessed battle or attack	0.75	(0.44)	0.56	(0.50)
—witnessed torture or beating	0.81	(0.39)	0.72	(0.45)
—witnessed a killing	0.62	(0.49)	0.46	(0.50)
—witnessed rape or sexual abuse	0.22	(0.42)	0.19	(0.42)
Index of violence committed (0-2) [a]	0.65	(0.85)		
—forced to do violent things to a soldier	0.28	(0.45)		
—forced to do violent things to a civilian	0.36	(0.48)		
Panel B: Abduction experience[b]				
Reintegration ceremonies (0-2)[a]	0.43	(0.71)		
—participated in welcoming ceremony	0.52	(0.50)		
—participated in cleansing ceremony	0.31	(0.46)		
Passed through reception center (d)	0.48	(0.50)		
Given gun (d)	0.52	(0.50)		
Allowed to sleep with gun (d)	0.41	(0.49)		
Index of initiation prayer ceremonies				
—Anointed with shea oil	0.46	(0.50)		
—Initiation prayers	0.28	(0.45)		
Forced to beat other during initiation	0.26	(0.44)		
Received beating during initiation	0.55	(0.50)		
Forced to kill other during initiation	0.15	(0.35)		
Passed through reception center	0.48	(0.50)		
Index of informal ceremonies (0-2)[a]	0.83	(0.80)		
—Participated in cleansing ceremony (Mato Oput)	0.31	(0.46)		
—Participated in welcoming ceremony	0.52	(0.50)		

(d) indicates dummy variable. [a] Index of dummy variables, elements of index listed below in italics. [b] Results shown for sub-sample of ex-abductees.

Table 3.A.2: Robustness to Different Sets of Control Variables and Fixed Effects

Dependent variable	Trustworthiness: Average percentage returned in trust game			
Sample	*Receivers*			
	(1)	(2)	(4)	(5)
Abduction length (years)		1.89***		1.80*
		(0.69)		(1.06)
Abducted young (< 14 years) (d)	6.94		7.45**	
	(4.14)		(3.52)	
Abducted as adolescent/adult (≥14) (d)	-0.18	-0.64	1.78	1.47
	(3.73)	(3.89)	(3.51)	(3.71)
Abd. length x abd. adol./adult (≥14)		-2.27*		-2.19
		(1.27)		(1.66)
Pre-abduction controls	Yes	Yes	Yes	Yes
Post-abduction controls	No	No	Yes	Yes
Village-level fixed effects	No	No	Yes	Yes
Constant	42.66***	45.53***	25.84*	29.69*
	(9.01)	(9.30)	(15.43)	(15.55)
Observations	333	328	333	328
R-squared	0.04	0.04	0.22	0.22

Note: OLS. Robust, standard errors in parentheses, clustered at village level. * significant at 0.10%; ** significant at 5%; *** significant at 1%. The dependent variable is the average percentage returned from two decisions made by Receivers, who made two separate decisions, conditional on senders' actions (strategy method). Senders could send 1 ths or 2 ths UGX, receviers could return 0-3 ths and 0-6 ths UGX, respectively. Pre-abduction controls include age, number of siblings, dummies for mother no school, father no school, mother alive in 1996, father alive in 1996 and dummies for missing information on mother's/father's level of schooling. Post abduction controls include log of monthly income, number of current household members, married, and literate. All regressions include a control for whether their partner in the experiment was male.

Table 3.A.3: Age of First Abduction (More Detailed Classification) and Trustworthiness

Dependent variable	Trustworthiness: Average percentage returned in trust game
Sample	*Receivers*
	(1)
Age of first abduction (years)[a]	
<10	5.21
	(4.81)
10-11	10.98*
	(6.03)
12-13	9.44
	(5.96)
14-15	2.02
	(4.08)
16-17	0.42
	(6.96)
18-19	2.63
	(6.31)
>20	0.94
	(4.52)
Constant	30.49**
	(12.18)
Observations	333
R-squared	0.08

Note: OLS. Robust, standard errors in parentheses, clustered at village level. * significant at 0.10%; ** significant at 5%; *** significant at 1%. [a] Set of dummy variables. In column 1, for age of first abduction variables are dummies equal to 1 if an individual was first abducted during the given age range; the excluded category is non-abductees. In all columns, we control for the same set of variables as in Table 3.

Table 3.A4: Understanding Pro-social Motivations Driving Higher Trustworthiness Among Former Soldiers

Dependent variable:	Trustworthiness: Amount returned in trust game:							
	Achieve equal allocation of rewards when 1 ths sent		Achieve equal allocation of rewards when 2 ths sent		% returned when 1 ths sent		% returned when 2 ths sent	
Sample:	*Receivers*							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Abduction length (years)		0.01		0.03		1.10		3.26***
		(0.02)		(0.02)		(1.05)		(1.01)
Abducted young (< 14 years) (d)	-0.05		0.03		8.04*		9.61**	
	(0.07)		(0.07)		(4.28)		(3.98)	
Abducted as adolescent/adult (≥14) (d)	-0.05	-0.01	-0.03	-0.05	3.14	0.90	-0.30	-0.16
	(0.08)	(0.08)	(0.06)	(0.06)	(4.73)	(4.73)	(3.95)	(4.12)
Abd. length x abd. adol./adult (≥14)		-0.02		-0.01		-0.70		-4.13***
		(0.03)		(0.04)		(1.64)		(1.40)
Constant	0.32	0.24	0.21	0.15	31.26***	34.92***	29.36**	32.93**
	(0.24)	(0.24)	(0.20)	(0.18)	(10.87)	(11.74)	(14.12)	(14.50)
Observations	333	328	333	328	333	328	333	328
R-squared	0.06	0.07	0.07	0.08	0.07	0.06	0.07	0.08

Notes: OLS. Robust standard errors in parentheses, clustered at village level. * significant at 10%; ** significant at 5%; *** significant at 1%. (d) indicates dummy variable. In Columns 1-2 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 Column 3-4 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 Columns 5-6 and 7-8 the dependent variable is the percentage returned when 1 ths UGX is transferred and 2 ths UGX is transferred, respectively. In all columns, we control for the same set of variables as in Table 3.

Table 3.A.5: Age of Abduction, Exposure to Violence and Abduction Experience

Panel A: Exposure to violence							
	Committed violent acts		Indices of violence				
Dependent Variable	Against civilians (d)	Against soldiers (d)	Received	Against family	Witnessed		
Sample	*All Receivers*						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Abducted young (< 14 years) (d)	0.49***	0.29***	2.68***	0.02	1.56***		
	(0.07)	(0.09)	(0.25)	(0.12)	(0.15)		
Abducted as adolescent/adult (≥14) (d)	0.36***	0.14	2.05***	-0.23	1.27***		
	(0.09)	(0.09)	(0.24)	(0.15)	(0.13)		
Observations	328	328	328	328	328		
(Pseudo) R-squared	(0.15)	(0.07)	0.47	0.03	0.31		
Panel B: Abduction experience							
Dependent Variable			Initiation rituals				
	Gun (index)	Prayer ceremonies (index)	Forced to beat other (d)	Received beating (d)	Forced to kill other (d)	Reception center (d)	Index of informal ceremonies
Sample	*Former Abductees*						
Abducted young (<14) (d)	0.30	0.59***	0.21***	0.17**	0.16**	0.08	0.32**
	(0.19)	(0.12)	(0.08)	(0.08)	(0.08)	(0.10)	(0.14)
Observations	177	177	177	177	169	177	168
(Pseudo) R-squared	0.05	0.12	(0.09)	(0.07)	(0.05)	(0.06)	0.08

Note: Probit: Panel A, columns 1-2, Panel B, columns 3-6; OLS: Panel A, columns 3-5, Panel B, columns 1-2 and 7. Marginal effects reported for probit regressions. Robust, standard errors in parentheses, clustered at village level. * significant at 0.10%; ** significant at 5%; *** significant at 1%. (d) indicates dummy variable. In Panel B, column 1, the dependent variable is an index =0 if the individual was never given a gun, 1 if he was given a gun, and 2 if he was given a gun and allowed to sleep with it. In all regressions we control for the same set of controls as in Table 3.

Table 3.A.6: Robustness Checks: Restricted Samples

Dependent variable	Trustworthiness: Average percentage returned in trust game			
Panel A: Understanding				
Sub-sample	*Literate*[a]		*Answered all comprehension-check questions correctly*[b]	
	(1)	(2)	(3)	(4)
Abduction length (years)		2.15**		2.75
		(1.00)		(1.77)
Abducted young (< 14 years) (d)	9.91**		8.49*	
	(3.63)		(4.18)	
Abducted as adolescent/adult (≥14) (d)	3.08	3.22	1.96	1.04
	(4.32)	(5.06)	(3.52)	(3.98)
Abd. length x abd. adol./adult (≥14)		-5.40*		-3.02
		(2.69)		(2.24)
Constant	27.96*	30.13**	32.83**	33.54**
	(14.14)	(14.40)	(14.71)	(15.69)
Observations	251	246	257	252
R-squared	0.06	0.05	0.08	0.07
Panel B: Outliers				
Sub-sample	*Abduction length up to 5 years*[c]		*Abducted before of 11 or after 16 years of age*[d]	
Abduction length (years)		3.96***		2.02*
		(1.15)		(1.11)
Abducted young (< 14 years) (d)	8.54**		11.25*	
	(4.10)		(5.93)	
Abducted as adolescent/adult (≥14) (d)	1.87	1.70	1.23	0.49
	(3.91)	(4.60)	(4.56)	(4.55)
Abd. length x abd. adol./adult (≥14)		-5.76*		-2.67
		(2.91)		(2.19)
Constant	31.17**	32.46**	21.99*	24.14*
	(11.99)	(12.36)	(12.52)	(13.27)
Observations	320	320	237	234
R-squared	0.07	0.07	0.13	0.11

Continued…

… *continued.*

Dependent variable	Trustworthiness: Average percentage returned in trust game			
	(1)	(2)	(3)	(4)
Panel C: Accuracy in reporting				
Sub-sample	*Excluding mismatched obersvations*[e]			
Abduction length (years)		2.39***		
		(0.86)		
Abducted young (< 14 years) (d)	9.16**			
	(3.88)			
Abducted as adolescent/adult (≥14) (d)	1.49	1.26		
	(4.14)	(4.35)		
Abd. length x abd. adol./adult (≥14)		-3.28**		
		(1.49)		
Constant	24.98**	28cvg.27*1*		
	(11.42)	(11.43)		
Observations	301	297		
R-squared	0.10	0.10		

Note: OLS. Robust, standard errors in parentheses, clustered at village level. * significant at 0.10%; ** significant at 5%; *** significant at 1%. The dependent variable is the average percentage returned from two 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, respectively. (d) indicates dummy variable. [a] Excluding 82 subjects who reported that they were unable to read a book. [b] Excluding 76 subjects who did not answer all comprehension questions for the trust game correctly on the first try. [c] Excluding 13 individuals who were with the LRA for more than 5 years. [d] Excluding 97 individuals abducted outside the LRA's target age range (11-16). [e] Excluding 32 individuals whose abduction status was misreported in household survey used for recruiting subjects. The dependent variable is the average percentage returned from two decisions made by Receivers. In all columns, we control for the same set of variables as in Table 3.

Table 3.A.7 Using Survey-based Measures of Pro-social Behavior

Dependent variable	Index of group membership				Reported quarrels (d)			
Sample	*Receivers*							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Abducted	0.47**				-0.02			
	(0.18)				(0.04)			
Abduction length (years)		0.70***				-0.04		
		(0.22)				(0.05)		
Abducted young (< 14 years) (d)		0.25		0.08		-0.00		0.01
		(0.18)		(0.20)		(0.06)		(0.06)
Abducted as adolescent/adult (≥14) (d)				-0.08				0.00
				(0.10)				(0.01)
Abd. length x abd. adol./adult (≥14)			0.12**	0.15*			-0.02***	-0.02***
			(0.05)	(0.08)			(0.01)	(0.01)
Constant	1.24**	0.83*	1.11**	1.12**	0.00	0.03	-0.01	0.01
	(0.45)	(0.42)	(0.49)	(0.49)	(0.10)	(0.13)	(0.11)	(0.12)
Observations	333	333	328	328	328	328	328	328
(Pseudo) R-squared	0.15	0.16	0.15	0.15	0.01	0.01	0.02	0.02

Notes: OLS. Robust standard errors in parentheses, clustered at village level. * significant at 10%; ** significant at 5%; *** significant at 1%. (d) indicates dummy variable. In Columns 1-3 the dependent variable is an index of group membership which is the number of groups a person belongs to, based on a prompted survey of 9 types of community groups. In columns 4-6, the dependent variable is equal to 1 if respondent reported quarreling with either family members, elders or people their own age. In all columns, we control for the same set of variables as in Table 3.

Table 3.A.8: Sensitivity Analysis (Bounding for Selective Mortality)

Dependent variable	Trustworthiness: Average percentage returned in trust game	
Sample	*Receivers*	
	Excluding 15 percent of most selfish non-abductees[a]	
	(1)	(2)
Abduction length (years)		1.21**
		(0.49)
Abducted young (< 14 years) (d)	1.72	
	(4.22)	
Abducted as adolescent/adult (≥14) (d)	-5.28	-4.51
	(3.74)	(3.83)
Abd. length x abd. adol./adult (≥14)		-1.54
		(1.07)
Constant	46.86***	46.93***
	(11.48)	(11.91)
Observations	311	306
R-squared	0.10	0.10

Note: OLS. Robust, standard errors in parentheses, clustered at village level. * significant at 0.10%; ** significant at 5%; *** significant at 1%. (d) indicates dummy variable. [a] Excluding 22 non-abductees (15%) who returned nothing in the trust game for both decisions (1k sent and 2k sent). The dependent variable is the average percentage returned from two decisions made by Receivers. In all columns, we control for the same set of variables as in Table 3.

Table 3.A9: Randomization Check

	Partner's length of abduction (treatment condition)						F-statistic (p-value)
Sample	*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), (treatment)	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 (current)	8.11	(3.28)	8.27	(4.18)	7.97	(3.22)	0.81
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, elements listed in Table A.1. [b] 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. [c] 1st principal component constructed from count of household assets, including: jerry cans, wash basins, bicycles, mattresses radios, plates, livestock, chairs, mobile phones and ploughs.

Table 3.A.10: Effects of Receiver's Abduction on Sender's Decisions—Ordered probit

Dependent variable:	Trust: the amount transferred in trust game (ths UGX)				Altruism: the amount transferred in dictator game (ths UGX)	
Sample	*Senders*					
	All		*No sons abducted*	*Son abducted*	*All*	
	(1)	(2)	(3)	(4)	(5)	(6)
Long-abduction treat. (d)	0.14	-0.06	-0.06	1.08**	0.01	0.03
	(0.15)	(0.18)	(0.18)	(0.42)	(0.16)	(0.19)
Short-abduction treat. (d)	0.13	0.01	0.01	0.74	0.03	0.04
	(0.19)	(0.23)	(0.23)	(0.53)	(0.16)	(0.19)
Long-abduction t. x Son abducted		0.96**				-0.12
		(0.41)				(0.44)
Short-abduction t. x Son abducted		0.55				-0.04
		(0.46)				(0.35)
Son abducted (d)	0.23	-0.26			0.21	0.26
	(0.15)	(0.23)			(0.16)	(0.25)
Observations	360	360	278	82	360	360

Notes: Ordered probit. Standard errors, in parentheses, at village level. * significant at 10%; ** significant at 5%; *** significant at 1%. (d) indicates dummy variable. 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 3.A.11: 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)
Long-abduction treatment (d)	-0.03	0.07	0.07	0.10	0.05	0.02
	(0.10)	(0.08)	(0.17)	(0.21)	(0.18)	(0.25)
Short-abduction treatment (d)	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 (d)	-0.13					-0.13
	(0.12)					(0.14)
Long-abduction t. x Son abdct.	0.49**					0.53**
	(0.21)					(0.23)
Short-abduction t. x Son abdct.	0.28					0.30
	(0.24)					(0.28)
Daughter abducted (d)		-0.12				-0.09
		(0.30)				(0.31)
Long-abduction x daughter abdct.		-0.02				-0.04
		(0.51)				(0.56)
Short-abduction x daughter abcdt.		0.01				-0.08
		(0.50)				(0.58)
Violence received (index)			0.01			0.02
			(0.03)			(0.04)
Long-abduction x violence rec.			-0.05			0.01
			(0.04)			(0.05)
Short-abduction x violence rec			0.00			-0.08*
			(0.05)			(0.04)
Violence against family (index)				0.04		0.03
				(0.07)		(0.07)
Long-abduction x violence family				-0.05		-0.09
				(0.12)		(0.10)
Short-abduction x violence family				-0.02		-0.04
				(0.11)		(0.12)
Violence witnessed (index)					0.01	-0.02
					(0.04)	(0.05)
Long-abduction x witnessed					0.01	0.03
					(0.06)	(0.06)

Continued…

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Short-abduction 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%. (d) indicates dummy variable. Regressions include controls for order of experiment, age, gender, risk aversion, wealth, log of income, and number of household members.

Table 3.A.12 Abduction by LRA and Schooling

Dependent variable	Log of income				Schooling years			
Sample	*Receivers*							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Abducted	-3.07*				0.00			
	(1.55)				(0.24)			
Abduction length (years)			0.27	0.39			-0.24***	-0.17***
			(0.37)	(0.40)			(0.06)	(0.05)
Abducted young (< 14 years) (d)		-2.07				-0.18		
		(1.89)				(0.31)		
Abducted as adolescent/adult (≥14) (d)		-3.97**		-3.37**		0.16		0.44
		(1.71)		(1.58)		(0.32)		(0.34)
Abd. length x abd. adol./adult (≥14)				-0.15				-0.19
				(0.76)				(0.11)
Constant	-13.42***	-15.03***	-13.12***	-16.16***	11.21***	11.50***	11.05***	11.24***
	(4.34)	(4.78)	(4.36)	(4.60)	(1.05)	(1.14)	(1.01)	(1.08)
Observations	328	328	328	328	328	328	328	328
(Pseudo) R-squared	0.04	0.04	0.03	0.04	0.11	0.11	0.13	0.14

Notes: OLS. Robust standard errors in parentheses, clustered at village level. * significant at 10%; ** significant at 5%; *** significant at 1%. (d) indicates dummy variable. In all regressions we control for age, number of siblings, dummies for mother no school, father no school, mother alive in 1996, father alive in 1996 and dummies for missing information on mother's/father's level of schooling.

Chapter 4

The Effect of Conflict History on Cooperation within and Between Groups: Evidence from a Laboratory Experiment

Abstract

We study cooperation within and between groups in the laboratory, comparing treatments in which two groups have previously been (i) in conflict with one another, (ii) in conflict with a different group, or (iii) not previously exposed to conflict. We model conflict using an inter-group Tullock contest, and measure its effects upon cooperation using a multi-level public good game. We demonstrate that conflict increases cooperation within groups, while decreasing cooperation between groups. Moreover, we find that cooperation between groups increases in response to an increase in the efficiency gains from cooperation only when the two groups have not previously interacted.

JEL Classification: C92, D64, D74, H41.

Keywords: within- and between-group cooperation; inter-group conflict; group identity; multi-level public good experiment; Tullock contest; other-regarding preferences.

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4.1 Introduction

In many interesting settings, a period of conflict or competition between groups is followed by the opportunity for mutually beneficial cooperation between the same groups. Examples include the formation of a coalition government following an election, the integration of work teams following a corporate merger, and the reunification of a nation after a period of civil conflict. In such situations, individuals are faced with a choice between acting in their own self-interest, in the parochial interests of their "in-group", or in the collective interests of all parties.

If, as a result of the prior history of conflict, individuals are reluctant to cooperate with members of an "out-group", the result may be substantial efficiency losses to society as a whole. Yet, at the same time, a shared experience of conflict may also reinforce cooperative norms among members of an in-group. To give an extreme example: two decades after the wars in the Balkans, Muslims and Christians in Bosnia have established separate schools and even separate fire departments (Brunwasser, 2011). This clearly illustrates these groups' preference to invest in "local" public goods that only benefit members of their in-group, as opposed to "global" public goods that benefit all parties.

There are several possible reasons why conflict may inhibit subsequent cooperation between groups. Firstly, the underlying reasons for the conflict could also have an effect on cooperation. Secondly, conflict could create or deepen in-group identity, strengthening other-regarding preferences toward in-group members and making it more attractive to cooperate within groups. Finally, conflict may create animosity towards out-group members, eroding other-regarding preferences towards out-group members and making it less attractive to cooperate between groups.

In this paper, we report a laboratory experiment designed to study how cooperative behavior, both within and between groups, is influenced by the group members' experience of a prior phase of conflict. In particular, we compare levels of within- and between-group cooperation in the situation described above—where the *same* two groups were previously in conflict—to a comparable situation in which each group previously experienced conflict involving a *different* out-group, as well as when group members have *no* prior experience of conflict. We vary this group matching on a between-subjects basis.

Since exposure to conflict is exogenous and randomly-assigned in our experiment, we can set aside the first explanation—namely that conflict and cooperation share some common and deeper source. Our group matching manipulations then enable us to disentangle the latter two mechanisms, to independently identify the effects of a shared experience of conflict upon other-regarding preferences toward members of the in- and out-groups.

Our instrument for measuring cooperation within and between groups is a multi-level public good (MLPG) game (Blackwell and McKee, 2003). In this game, all individuals have an endowment which they can retain for private consumption, contribute to a local public good that benefits only members of the in-group, and/or a global public good that benefits members of both the in- and out-groups. Our conflict manipulation takes the form of an inter-group version of the Tullock rent-seeking contest, in which parties compete by investing in a lottery that increases the chances of winning a prize (Tullock, 1980; Abbink et al., 2010). While self-interested parties invest positive amounts in the competition, in equilibrium each group has an equal chance of winning, and investment in the lottery is inherently socially inefficient. The Tullock game thus models a prior phase of inter-group conflict which is followed by a subsequent opportunity for cooperation in the form of the MLPG game.

Previous studies of the MLPG game typically find that contributions to the global public good are increasing in the relative return on the global account compared to the local one (Blackwell and McKee, 2003; Fellner and Lüsner, 2008; Chakravarty and Fonseca, 2013). However, since the Tullock contest potentially induces a much stronger form of in- and out-group identity than has previously been considered in this literature, this responsiveness to efficiency considerations may not be robust to a shared history of conflict involving the same out-group. For this reason, we vary the return on contributions to the global public good as a second dimension of our experiment design.

Our approach thus introduces a several methodological innovations. Firstly, we go beyond standard arbitrary or minimal methods of group formation, by using inter-group competition in a Tullock contest to instill a much stronger form of induced group identity–forged in conflict against another group in the laboratory. Secondly, through our manipulations of group matching across the two phases of our experiment, we are able to disentangle the in- and out-group effects of this conflict manipulation upon subsequent interactions in the MLPG game.

We find that within-group cooperation increases when groups have a shared history of conflict compared to when they play the MLPG without any prior history, while between-group cooperation diminishes when two groups have previously been in conflict. We find no significant response to an increase in the return to between-group cooperation when there has been a prior history of conflict between the groups–which is contrary to the results of previous studies that induce weaker forms of group identity. On the other hand, when two groups have not previously interacted (but each has nonetheless previously experienced conflict involving a different out-group) we find a significant increase in between-group cooperation in response to an increase in its return–which is in line with the results of the previous literature.

The paper proceeds by reviewing the relevant literature in Section 4.1 before Section 4.2 sets out our experimental design and derives hypotheses regarding the effects of our treatments. Section 4.3 presents and discusses our results, and Section 4.4 concludes.

4.2 Related literature

Our paper contributes to two strands of literature. First, we make a methodological contribution to the literature on inducing group identity in laboratory experiments. The most widely used method of doing so, the "minimal group paradigm" introduced by (Tajfel, 1971) in the social psychology literature, involves forming groups on the basis of seemingly irrelevant personal characteristics—such as preference for a particular abstract painting by either Klee or Kandinsky—and has been found to be sufficient to induce a bias in favor of members of the in-group in many psychological experiments (Tajfel and Turner, 1979). This method has also been widely applied in economic experiments, although usually in a modified form.[1]

In these studies, assigning group membership randomly according to number or color, or according to trivial preferences, has not always been sufficient to induce an in-group bias. However, using these procedures in a modified form, and/or in combination with other methods designed to increase the salience of group membership, has been found to be effective.

One notable example is (Chen and Li, 2009), who use the Klee/Kandinsky procedure and find that subjects are more likely to choose social-welfare maximizing actions in allocation games when playing with in-group members. In their setting, shared identity deriving from a trivial preference for one painter over another is only effective in producing differences in behavior toward in- and out-group members when combined

[1] As (Chen and Li, 2009) point out, the classic definition of the minimal group paradigm requires that any decisions made by a subject should not directly affect her own payoff. However this condition is violated in most economic experiments that use similar methods to induce group identity in the lab.

with anonymous communication with the in-group members during a problem-solving task. Similarly, (Charness, Rigotti and Rustichini, 2007) find that in-group preferences are stronger when an individual's choices are observed (passively) by in-group members, whereas arbitrarily labeling groups and identifying them with colors or numbers is not enough to create an in-group bias in Prisoner's Dilemma and battle-of-the-sexes games.

Eckel and Grossman (2005) compare the effects of several methods for creating group identity in a laboratory experiment, comparing cooperation in a public good game played under various degrees of induced group identity, including arbitrary group identity (in which groups are formed randomly then labeled by color only), as well as treatments in which identity is strengthened through joint participation in problem-solving tasks, and competition in a tournament (in which the group with the highest contribution to the public good receives a bonus which is deducted from the losing team's payoffs). They find that in-group cooperation is not affected by the arbitrary or problem-solving treatments, but is significantly higher when teams have participated in the tournament.

Our approach is similar to Eckel and Grossman (2005) in that we use competition as a means of making group identity more salient. However, our design differs in two key respects. First, we are able to examine and compare the effect of this group identity not only on in-group cooperation after competition, but also on cooperation with an out-group. Second, we strengthen group identity through competition that produces a social loss, which may cause a different change in preferences than productive competition.

Our design uses a Tullock contest (Tullock, 1980) to increase the salience of group membership. The Tullock contest is a simple model of rent seeking and a "natural workhorse" for experimental studies on conflict (Abbink, 2012). In the Tullock game, although any expenditure on competition is inherently socially inefficient, the equilibrium level of investment is positive. In previous experiments, investments in the contest have far exceeded equilibrium predictions (see Sheremeta (2013) for a survey). Moreover, playing the Tullock game in a group environment seems to matter: Abbink et al. 2010 find that investments in a the contest are even further above the equilibrium prediction when the game is played in teams rather than individually.

Ke, Konrad and Morath (2013) use a Tullock game in which two subjects compete together as an alliance against a third individual to study how behavior changes when the alliance members know that they will later compete against one another in a second stage competition over the "spoils" from the first stage. They find that any solidarity formed during the first stage does not diminish the intensity of conflict in the second contest, when the former alliance members compete against one another. Although behavior in ad hoc alliances is closely related to the way in which we study the effect of the Tullock game in creating group identity, our design differs from Ke, Konrad and Morath (2013) in an important way: groups in our experiment compete in the contest without any expectation of competing against team members in the future, and thus their experience in the competition is plausibly of different character. Our study is the first we are aware of to employ the Tullock game as a means of creating group identity that is salient in a subsequent and unrelated task.

Halevy et al. (2011) also examine how changes in incentives for intra-group competition affect subsequent interactions. Individuals can choose to cooperate with their in-group while simultaneously harming the out-group, and after a set number of rounds some individuals are given the option to cooperate with their in-group without harming the out-group. The authors find that individuals prefer to cooperate with in-group members without imposing negative externalities on the out-group, and this is true even after a phase of conflict in which in-group cooperation is necessarily associated with negative externalities, resulting in high rates of harm to the out-group.

Our study is similar to these experiments in that we are concerned with how conflict between groups affects subsequent interactions. However, we study an environment in which incentives favor conflict in the first phase, and then observe how this affects cooperation between groups (as opposed to further competition) in a second phase.

This brings us to the second body of literature to which our paper contributes, on cooperation within and between groups – including both groups formed endogenously in the naturally-occurring world as well as ones formed in the lab. Findings from a number of economic experiments show that group membership leads to more within-group cooperation (Puurtinen and Mappes (2009), but decreases between-group cooperation. Therefore, stronger group identity may lead to efficiency gains due to increased within-group cooperation, but efficiency losses associated with reduced out-group cooperation.

Several papers use MLPG experiments to study inter-group interactions, generally finding that although subjects contribute non-zero amounts to the local public good, contributions to the global public good are higher, and responsive to its relative efficiency. Blackwell (2003) use arbitrary group identity—groups are formed randomly and identified by color— and find weak evidence for in-group preferences, but only when the average per-capita return to the global public good is no lower than that of the local public good. Fellner and Lünser (2008) study a similar experiment, identifying groups by letters and adding a monitoring mechanism to increase the salience of group identity. They find that contributions to the global public good are high when it is socially more efficient than the local public good, but that as cooperation decays over time, subjects switch toward the local public good.

Chakravarty and Fonseca (2013) assign subjects into groups using the Klee/Kandinsky procedure, and strengthen group identity using intra-group communication (through a chat-box on the computer screen, as in Chen and Li (2009) before an MLPG game in which the efficiency of the local public good is varied across treatments. They find that even when the financial return to investing in the global public good is higher, subjects invest a considerable part of their endowment in the local ("club") good, hence reducing social efficiency. One possible reason for the somewhat stronger results of this study may be that it induces a more salient form of group identity than the two previous studies.

In addition to laboratory experiments, there are also a number of artefactual field experiments which measure the effects of naturally-occurring group identity on within- and between-group cooperation.

In a cross-cultural experiment, Buchan et al. (2009) use an adaption of the MLPG game – in which contributions to the local and global public goods do not directly affect current players, but instead accrue to individuals playing in a subsequent session – and find that increased exposure to globalization at both the individual and national level increases contributions to a global account that also accrues to citizens of other countries. The MLPG design allows them to separate preferences for cooperating with foreigners specifically from general variation in preferences for cooperation between countries.

The field experiment most closely related to our paper is Gumen (2012), who studies a variation of the MLPG game using students from fraternities at a US university.[2]

Groups of subjects from the same fraternity are matched with an out-group either from the same fraternity or from a rival fraternity, according to treatment. She finds that when subjects play with an out-group from the same fraternity, they over-invest in the global public good. However, when playing with an out-group from a rival fraternity, they invest comparatively more in the local public good.

In addition to material incentives, inter-group interactions may be motivated by preferences for cooperation within groups and competition between groups (Hirshleifer,

[2] In Gumen's design, the payoff function is non-linear, with an interior optimum for a selfish agent.

1995), which may have developed through evolutionary conditions that involved frequent conflict between small groups (Bowles and Gintis, 2011). Choi and Bowles (2007) hypothesize that war is instrumental in maintaining and strengthening “parochial altruism” (increased altruism towards in-group members coupled with hostility towards out-group members). In line with this theoretical prediction, recent empirical evidence on the effects of war on social preferences shows that war leads to more altruism towards neighbors Voors et. al. (2012), stronger egalitarian norms towards in-group members by children Bauer et al. (2014), and more within-group cooperation Gneezy and Fessler (2012).

Our study is related to this literature, but focuses on the role that simple conflict in the form competition over a fixed resource – as distinct from exposure to violence or other trauma – plays in shaping social preferences. We contribute to the discussion of group identity and cooperation by disentangling how conflict affects other-regarding preferences toward the in- and out-groups, using competition in the Tullock contest as novel means of inducing group identity in the laboratory.

4.3 Design

Our experiment consists of two stages: a group Tullock contest and an MLPG game, both of which are played between two groups of three subjects in each of our treatments. Groups are formed randomly and anonymously at the beginning of the session, and the membership of a subject's “in-group” remains the same throughout the experiment. In each stage, each group is paired with a second group (the “out-group”) for ten rounds of repeated play, with one round of each game randomly selected to count for payment at the end of the experiment. The identity of the out-group remains constant across all ten rounds of a given game. However, it may change between games according to treatment.

We use the Tullock contest primarily to manipulate subjects' experience of conflict – both as a member of their in-group, and in opposition to an out-group. We use the MLPG game to measure the effect of these conflict manipulations upon subjects' willingness to cooperate both within their in-group as well as between the in- and out-groups. In particular, in our arbitrary-groups treatment, subjects play the MLPG game as the first stage of the experiment, without any prior experience of the Tullock contest. In this treatment, subjects play the MLPG game without any previous history of interaction with the members of their in- or out-groups. This treatment thus constitutes a baseline measure of cooperativeness in the absence of any interaction history.

In our rematched-groups treatments, subjects play the Tullock contest as the first stage. However, they are rematched with a new out-group before playing the MLPG game. In these treatments, subjects have previously interacted with the other members of their in-group, but not the out-group, prior to playing the MLPG game. This enables us to identify the effect of the experience of conflict in potentially strengthening other-regarding preferences toward members of the in-group. Finally in our fixed-groups treatments, subjects play the Tullock contest first, and are then paired with the same out-group for the MLPG game. As a result, they have previously interacted with both their in- and out-groups prior to playing the MLPG game. This enables us to identify the effect of the experience of conflict in weakening other-regarding preferences toward the out-group.

In addition, in our fixed- and rematched-group treatments, we also vary the return on cooperation between groups in the MLPG game, in the form of the marginal per-capita return (MPCR) on contributions to the global account that benefits the members of both the in- and out-groups. We do this to study how our conflict manipulations influence the extent to which subjects respond to efficiency considerations.

4.3.1 Tullock contest

The Tullock contest (Tullock, 1980) models an unproductive conflict between two parties over an exogenous prize P . In our implementation of this game, we take each group to be a party to the contest, with the prize to be contested between two groups and then divided equally among the three members of the winning group. In each round, each group has an endowment y, and must choose an amount x to invest in its "contest fund" to increase its chances of winning the prize. Given the investments of the two groups, x_g and x_h, the probability that group g is the winner is given by:

$$Pr\left(P \middle| x_j . x_h \right) = \frac{x_g}{x_g + x_h}, \qquad (4.1)$$

and the expected payoff to group g in a given round of the game is:

$$E[\pi_g] = \left(y - x_g\right) + \frac{x_g}{x_g + x_h} P. \qquad (4.2)$$

The Tullock game has a unique equilibrium (in terms of total group investments) which can be found by taking the first-order condition of π_g with respect to x_g and setting $x_g = x_h = x^*$ such that each group invests $x^* = P/4$ in its respective contest fund.

We give each group an aggregate endowment of $y=300$, and the two groups compete over a prize of the same value (i.e. $P=300$), implying an equilibrium investment of $x^* =75$ for each group. Since the prize is split equally among members of the winning group, each group member receives $P/3=100$ in the event that their group is the winner.

To conduct the Tullock game in groups while preserving the unique equilibrium, and also to avoid wealth effects among the members of an in-group (which might influence their contribution decisions in a subsequent MLPG game), we determine investments in the group contest fund using a median-voter rule. Each group member is given an endowment of $y_i =y/3=100$, and is obliged to invest the median of the amounts proposed by the three members of their in-group, with no possibility to free-ride. In equilibrium, each individual's share of the group investment is thus $x^* /3=25$.

Under the median-voter rule, no individual has any incentive to deviate from proposing their own true preferred level of investment, even where this differs from the risk-neutral Nash investment, for example as a result of social or risk preferences. In each round, before the draw to determine the winning group occurs, subjects receive feedback on the median investment proposed by the members of their own group, the resulting allocation of their group to the contest fund, the allocation of the rival group, and their group's resultant probability of being the winner. After this, the draw to determine the winner takes place and subjects are informed of the result before continuing to the next round.

4.3.2 Multi-level public good game

In the MLPG game, each subject is given an individual endowment of $\omega_i =100$ in each round. Each subject must decide how to allocate this endowment between three accounts: a private account that benefits the individual alone, a local public good that benefits the three members of the in-group only, and a global public good that benefits six individuals: the three members of the in-group as well as the three members of the out-group with whom they have been matched. In the instructions, these three alternatives are framed neutrally as accounts A, B and C.

Given the contribution decisions of all six players, the monetary payoff to individual i in any given round of the game is:

$$\pi_i = (\omega_i - c_i - C_i) + \alpha \sum_{j=1}^{n} c_j + \beta \sum_{k=1}^{2n} C_k, \qquad (4.3)$$

where c_i denotes a contribution to the local public good that benefits the $n=3$ members of the in-group and C_i denotes a contribution to the global public good that benefits the $2n=6$ members of both the in- and out-groups.

Allocations to the private account always yield a return of 1, accruing to the individual alone. In all treatments, the sum of contributions to the local public good, by all three members of the in-group, is multiplied by a factor of $\alpha = 1.5$ and divided equally between them, giving an MPCR from the local account of $\alpha / n = 0.5$.

Similarly, the sum of contributions to the global public good, by all six members of the in- and out-groups, is multiplied by a factor of β and divided equally between them. We vary the return to the global account between treatments. In our low-gains-from-cooperation treatments we set $\beta = 2$, giving an MPCR on the global account of $\beta / 2n = 0.33$, while in our high-gains treatments we set $\beta = 3$, giving an MPCR of $\beta / 2n = 0.5$.

Note that in the high-gains treatment, the MPCR is equal for both the local and global public goods. In this case, subjects enrich members of their in-group equally by contributing to either of the two public goods. A contribution to the local public good, as opposed to the global, in effect excludes outgroup members from benefiting from in-group cooperation. On the other hand, in the low-gains treatments, subjects face a dilemma: contributing to the global public good—rather than the local one—is more socially efficient, but each marginal contribution benefits in-group members less than it would had equal funds been transferred to the local account.

Since $\beta / 2n \leq \alpha / n < 1$ in all treatments, an individual who cares only about her own material payoff will contribute nothing to either of the public goods, just as in a standard (single-level) public good game. On the other hand, since $\beta > \alpha > 1$ in all treatments, full contribution to the global account is always the most socially efficient outcome.

4.3.3 Treatments

By manipulating the nature of the prior group interactions (if any) before the play of the MLPG game, we are able to identify the effect that exposure to inter-group conflict, involving either the same or a different out-group, has upon preferences for both intraand inter-group cooperation, and thus examine how subjects' other-regarding preferences are shaped by the experience of conflict. Moreover, by manipulating the gains from cooperating with the out-group as well, we are also able to measure the response to efficiency considerations, and in particular whether this differs between treatments in which subjects are exposed to the same, or to a different, out-group in the MLPG game.

In total, we have five treatments in a $(2 \times 2)+1$ design. Firstly, we interact the dimension of fixed- versus rematched-groups with that of low versus high gains from cooperation. This results in four treatments: fixed groups with low/high gains from cooperation (FL and FH, respectively) and rematched groups with low/high gains from cooperation (RL and RH, respectively). In these four treatments, the Tullock contest is played as the first stage to induce a prior experience of conflict before playing the MLPG game.

In addition, we also include an arbitrary groups with low gains from cooperation treatment (AL), in which subjects play the MLPG game first (with low gains from cooperation) without any prior interaction with either their in- or out-groups, and then play the Tullock contest second (in a fixed group matching). This AL treatment captures baseline levels of cooperation with no prior experience of conflict as a group. We concentrate on the low-efficiency treatment only for arbitrary matching as it allows us to further explore the effect of conflict on in-group solidarity by comparing results with those in the RL.[3]

4.3.4 Procedures

The experiment was conducted at the Laboratory of Experimental Economics in Prague, Czech Republic between April 2012 and October 2013. We collected data for ten group pairs (with six subjects each) of the MLPG game in each of the five treatments. Subjects were recruited using ORSEE (Greiner, 2004) from a pool of students who had registered to participate in economic experiments. A total of 300 subjects took part in the experiment. Of these, 57% were undergraduates and 69% were males.

The experiment was conducted entirely in English.[4]

[3] Since subjects in the low-efficiency treatments leave in-group members worse off by contributing to the global public good—rather than the local one—comparing results with the RL treatment indicates the degree to which subjects are willing to favor in-group members at the expense of those in the outgroup after previously competing on the same team.

[4] The invitations to participate indicated clearly that the experiment would be conducted in English. All subjects completed a series of control questions (also in English) which serves to confirm that they were proficient enough in English to understand the instructions.

Sessions were conducted with 12-30 subjects at one time, and lasted around 75 minutes. All subjects in any given session were assigned to the same treatment. Two experimenters were present for each session, with the instructions read aloud by the same experimenter in all but one of the sessions. The experiment software was programmed using z-Tree (Fischbacher, 2007).

Upon entering the lab, subjects were randomly assigned to a computer terminal, and instructions were both read aloud and provided in print.[5] At the beginning of the session, subjects were informed that they would complete two tasks, but they were not told anything about the second task until after they had completed the first one. Subjects were told that they would be matched into groups, and that these groups would remain anonymous both during and after the experiment.

In all treatments except for AL, the Tullock contest was played first, followed by the MLPG game.[6] The instructions for the first game were read, and then subjects answered a series of control questions to ensure that they understood the task. After completing the first game, subjects were told that they would continue to the second task, and a similar procedure was followed again. The instructions clearly stated whether the out-group matching was the same or different from the first task (according to the treatment), and that the composition of the in-group would remain unchanged across both tasks.

Each subject was paid for one round of the Tullock contest and one round of the MLPG game, chosen at random after both games had been completed. All payments were made in private; the average payment per subject was 250 CZK, which was equivalent to approximately $13 USD at the time of the experiment.

4.3.5 Hypotheses

Recall that our experiment design has two dimensions. Firstly, we manipulate conflict history through our arbitrary, rematched and fixed group assignment conditions. Secondly, we vary the return on the global account β directly in our low and high gains from cooperation conditions. In Figure 4.1, we summarize the hypothesized effects of our treatments upon these three parameters.

Our first set of hypotheses are concerned with the effects of our group matching manipulations.

[5] The instructions for treatment RL are available in the Appendix.

[6] While this creates a potential confound, in that those in the AL treatment have spent a shorter time in the lab when playing the MLPG game, our design avoids any potential priming effects created by having subjects in the AL treatment engage in alternative activities.

In our arbitrary groups treatment, subjects had no prior interaction with either their in- or out-groups before playing the MLPG game. By contrast, in our rematched groups treatments, they had previously interacted with their in-group in the Tullock contest – but with a different out-group. Finally, in our fixed groups treatments subjects had previously interacted with both the same in- and out-groups in the Tullock game.

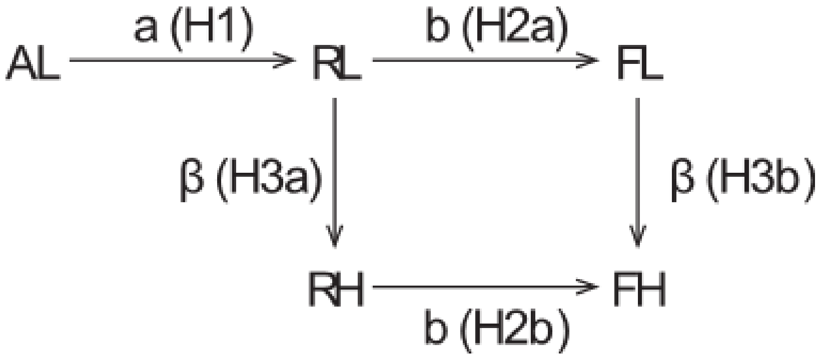

Figure 4.1:
Hypothesized effects of treatments upon model parameters

We hypothesize firstly that, relative to the arbitrary groups condition, a shared experience of conflict may strengthen subjects' other-regarding preferences toward members of the in-group. This corresponds to a rightward movement in Figure 4.1, and has the effect of making contribution to the local account more attractive under both rematched and fixed groups.

We hypothesize secondly that, relative to rematched groups, a past experience of conflict with the same out-group may weaken subjects' other-regarding preferences toward members of the out-group. This corresponds to a downward movement in Figure 4.1, and has the effect of making contribution to the global account less attractive specifically under fixed groups only. On the basis of these hypothesized effects, our model implies the following predictions:

Hypothesis 1: Contributions to the local account will be higher under the RL and FL treatments compared to the AL treatment.

Hypothesis 2a: Contributions to the global account will be lower under the FL treatment compared to the RL treatment.

Hypothesis 2b: Contributions to the global account will be lower under the FH treatment compared to the RH treatment.

Our next set of hypotheses are concerned with the second dimension of our experiment design in which we vary the gains from between-group cooperation as captured by the parameter β . An increase in β increases the marginal benefit to contributing to the global account. However, the magnitude of this increase depends on other-regarding preferences towards one's outgroup, which we have hypothesized above to be attenuated under our fixed groups treatments. Accordingly, this implies that:

Hypothesis 3a: Contributions to the global account will be higher under treatment RH compared to treatment RL.

Hypothesis 3b: Contributions to the global account will be higher under treatment FH compared to treatment FL. However, the magnitude of this increase will be smaller than under rematched groups.

4.5 Results

In Table 4.1, we report summary statistics of contributions in the MLPG game.[7]
For each treatment, we compute the mean allocation to each account pooled over all ten rounds, as well as mean earnings.[8]

The figures in parentheses are treatment standard deviations, treating group pairs as observations, i.e. we treat the mean in each group pair as a single observation and report the standard deviation for the ten group pairs in each treatment.[9]

Table 4.1 indicates that contributions to the global account are highest, while allocations to the private account are lowest, under treatment RH. On the other hand,

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[7] We report our analysis of the Tullock contest in Section 4.5.3 and Appendix Figure 4.A.1.

[8] Figure 4.3 depicts the time paths of mean individual allocations to the private, local and global accounts for each of the five treatments. It is clear that the ranking of the treatments with respect to the level of contributions to each of the accounts is fairly stable over the ten rounds of the MLPG game, and there are no obvious differences in either the nature or slope of the time trends across treatments. For these reasons, we aggregate the data from all ten rounds throughout our analysis.

[9] We acknowledge that in treatments with rematched groups, the group pairs are not strictly speaking independent observations. This is because subjects have previously interacted with members of a different out-group in the first-stage Tullock contest, and this could potentially result in inter-dependencies in behavior across two group pairs.

contributions to the global account are lowest, while allocations to the private account are highest, under treatment AL. Contributions to the local account are low across all five treatments, with the highest level (at 15%) observed under treatment FL. The ranking of the treatments with respect to earnings is identical to the ranking with respect to global contributions, even though the treatments differ with respect to the efficiency of the global public good, and there is also the opportunity to contribute to the less efficient local account.

Table 4.1 – Summary Statistics (by group pairs)

	Mean (Standard deviation)				
	Obs. (1)	Private (2)	Local (3)	Global (4)	Earnings (5)
Arbitrary Low	10	59.2	7.7	33.2	137
		(13.80)	(4.10)	(15.30)	(14.40)
Rematched Low	10	48.3	10.1	41.6	146.6
		(23.50)	(9.90)	(22.30)	(22.40)
Fixed Low	10	46.4	15	38.6	146.1
		(18.10)	(9.30)	(19.10)	(18.00)
Rematched High	10	36.1	5.4	58.5	58.5
		(15.20)	(4.40)	(16.30)	(31.90)
Fixed High	10	48	7.5	44.5	192.7
		(15.00)	(4.00)	(14.60)	(29.10)

In Table 4.2, we report two-sided p -values for Wilcoxon rank-sum tests of the equality of contributions, to each of the three accounts, in each pairwise comparison between treatments highlighted by our hypotheses. Again, this analysis treats the mean contribution of each group pair – by all six subjects and in all ten rounds – as a single observation.

Finally, in Table 4.3 we report an individual-level regression analysis of the effects of our treatments upon individual contributions to each of the three accounts. For each of the 300 subjects, we compute the mean amount allocated by that subject to each account over the ten rounds of the MLPG game. We regress these mean contributions on a set of dummies for each of the treatments, in a two-limit Tobit model with treatment AL as the omitted category.[10] Each subject contributes one observation to each of the three

[10] In Figure 4.A., we present histograms for contributions to the private, local, and global accounts for all treatments. It is clear that there is substantial censoring at both the upper and lower boundaries for both the private and global accounts, and at the lower boundary for the local account. We use a two-limit model for

Table 4.2: Wilcoxon rank-sum p -values (two-sided, by group pairs)

	Private (1)	Local (2)	Global (3)
H1: RL vs. AL	0.41	0.94	0.33
H1: FL vs. AL	0.05**	0.04**	0.50
H2a: FL vs. RL	0.82	0.30	0.82
H2b: FH vs. RH	0.11	0.30	0.06*
H3a: RH vs. RL	0.23	0.30	0.04**
H3b: FH vs. FL	0.76	0.03**	0.41

Note: * p<0.10 ; ** p<0.05 .

Table 4.3: Tobit on of mean individual contributions

	Private (1)	Local (2)	Global (3)
H1: RL	-11.68	2.94	8.58
	(8.97)	(4.28	(8.74)
H1: FL	-13.00*	9.66**	5.62
	(7.38)	(3.90)	(7.85)
RH	-23.81***	-4.83	26.24***
	(6.88)	(3.10)	(7.30)
FH	-11.36*	-1.84	11.55*
	(6.75)	(2.63)	(6.82)
Observations	300	300	300
H2a: FL vs. RL	0.89	0.21	0.75
H2b: FH vs. RH	0.08*	0.39	0.04**
H3a: RH vs. RL	0.19	0.11	0.05**
H3b: FH vs. FL	0.83	0.01***	0.44

Notes: base category AL; robust standard errors clustered by group pairs. * p<0.10 ; ** p<0.05 ; *** p<0.01 .

the latter for ease of comparison across models, though results are robust to a single-limit model (available upon request).

4.5.1 Effects of group matching

Our first two hypotheses are concerned with the effects of our *arbitrary*, *fixed* and *rematched* groups manipulations, and correspond to the horizontal dimension in Figure 4.1. We summarize the effects of this dimension of our experiment design graphically in regressions, and we report robust standard errors clustered at the level of group pairs. Table 3 also reports two-sided p-values for tests of the equality of the coefficients for each of the pairwise comparisons between treatments highlighted by our hypotheses.

The Tobit results presented in Table 4.3 are robust to controlling for individual characteristics collected from the post-experiment survey (see Table 4.A.2).[11]

On the basis of these analyses, we report our main results in the following two subsections.
Figure 4.2, separately for each of the three accounts, and for treatments with low and high gains from cooperation. The confidence bars in this figure represent ± 1 standard error of the mean, treating group pairs as observations.

Hypothesis 1 states that subjects' other-regarding preferences toward the members of their in-group may be strengthened when they have had the shared experience of playing the Tullock contest together. As a result, we expect contributions to the local public good to be higher under treatments RL and FL compared to treatment AL. Both the graphical presentation in Figure 4.2 as well as the summary statistics in Table 4.1 confirm that our results are directionally consistent with these predictions: contributions to the local account are highest under FL (15.0%) followed by RL (10.1%) and AL (7.7%). This comes at the expense of allocations to the private account, which are higher under AL (59.2%) than RL (48.3%) and FL (46.4%). Tables 4.2 and 4.3 indicate that the difference in contributions to the local account between FL and AL is statistically significant at the 5% level both in a nonparametric test at the level of group pairs, as well as in the individual-level regression.[12]

On the other hand, none of the differences between treatments RL and AL are significant in any of the analyses.

[11] Table 4.A.1 presents individual characteristics by treatment, along with test results for joint significance. Most variables are balanced by treatment, with the exception of whether subjects new at least one other subject in the session, which varies between 15% and 48% by treatment, and whether the individual resided locally.

[12] The offsetting difference in allocations to the private account is also significant at the 5% level in the rank-sum test in Table 4.2, although it is only marginally significant at the 10% level in the regression model of Table 4.3.

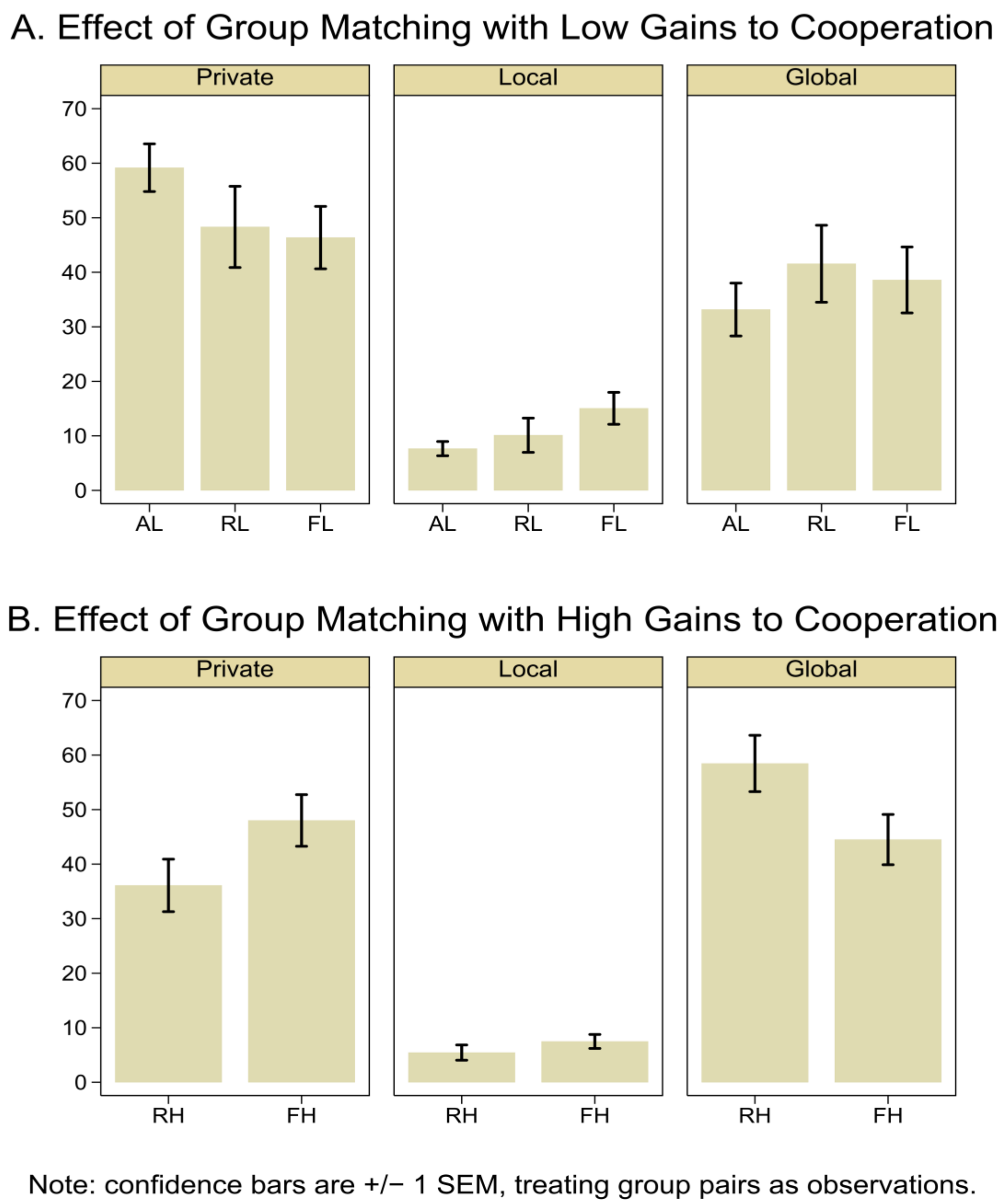

Figure 4.2: Effects of group matching

In interpreting these results, we acknowledge that the comparison between treatments FL and AL is not as clean as the one between RL and AL. Since subjects in FL also previously competed with the same out-group in the Tullock contest, the differences that we observe may also reflect an effect of negative sentiment toward the out-group as implied by Hypothesis 2. We summarize our discussion of Hypothesis 1 as follows:

Result 1: Contributions to the local public good are significantly higher under treatment FL compared to AL. Subjects are more cooperative toward the members of their in-group when they have previously jointly competed against the same out-group, compared to when they have not previously interacted with the members of either group. This result likely reflects a combination of in- and out-group effects.

Hypothesis 2 states that subjects' other-regarding preferences toward the members of their out-group may be weakened when they have previously competed against the same out-group in the Tullock contest. As a result, we expect contributions to the global public good to be lower under treatment FL compared to RL, as well as in FH compared to RH.

Once again, both Figure 4.2 and Table 4.1 confirm that our results are directionally consistent with these predictions: contributions to the global account are lower both under FL (38.6%) compared to RL (41.6%), as well as under FH (44.5%) compared to RH (58.5%). Tables 2 and 3 indicate that when gains from cooperation are high, the difference in contributions to the global account between FH and RH is significant at the 5% level in the individual-level regression, and at the 10% level in the rank-sum test at the level of group pairs.[13]

On the other hand, when gains from cooperation are low, none of the differences between treatments FL and RL are significant in any of the analyses.

As a result of their reluctance to cooperate with the members of an out-group with whom they have previously competed in the Tullock contest, subjects in our fixed groups treatments attain lower earnings and hence a lower level of efficiency. When the gains from cooperation are low, these costs are small and not statistically significant: average earnings drop fractionally from 146.6 under RL (out of a maximum of 200 in the low

[13] In addition, allocations to the private account are higher in FH compared to RH, and this difference is marginally significant at the 10% level in the individual-level regression model.

gains treatments, implying an efficiency of 73.3%) to 146.1 under FL. This difference is clearly not significant, with p=0.880 in a Wilcoxon rank-sum test.

However, when the gains from cooperation are large, the costs are more substantial: average earnings drop from 219.6 under RH (out of a maximum of 300 in high gains treatments, implying an efficiency of 73.2%) to 192.7 (64.2%). It turns out that the efficiency of the FH treatment is the lowest out of our five treatments. In an OLS regression, analogous to the Tobit models in Table 3, in which we regress each subject's mean earnings over the ten rounds of the MLPG game on dummies for each of the treatments, with standard errors clustered at the level of group pairs, we find the difference in earnings between FH and RH to be significant with p=0.047.[14]

We also find this difference to be marginally significant in a Wilcoxon rank-sum test, with p=0.070.

Result 2: When the gains to cooperation between groups are large, contributions to the global public good are significantly lower under treatment FH compared to RH. Subjects are less cooperative toward the members of their out-group when they have previously competed against that group, compared to when their previous interaction was with a different out-group. As a result of this out-group bias, subjects attain significantly lower levels of earnings and efficiency.

One potential concern with our interpretation of these group matching effects is that experience of the Tullock contest could be informative to subjects regarding the preferences of both in- and out-group members, and this could influence behavior in the MLPG game independently of the hypothesized effects upon their preferences. In particular, subjects in the fixed matching treatments might form more accurate beliefs as a result of having previously interacted with both their in- and out-groups. In this event, we might expect differences between treatments to decrease over the ten rounds of the MLPG game, as subjects in the rematched groups treatments learn the preferences of their new out-group. However, as can be seen from Figure 4.3, this is not what we observe: the differences between treatments in contributions to the local and global public goods remain stable over time, with all treatments displaying very similar time trends. Moreover, we examine contributions in the first round of teh MLPG only in Table 4.A.3.

[14] Full results are available upon request.

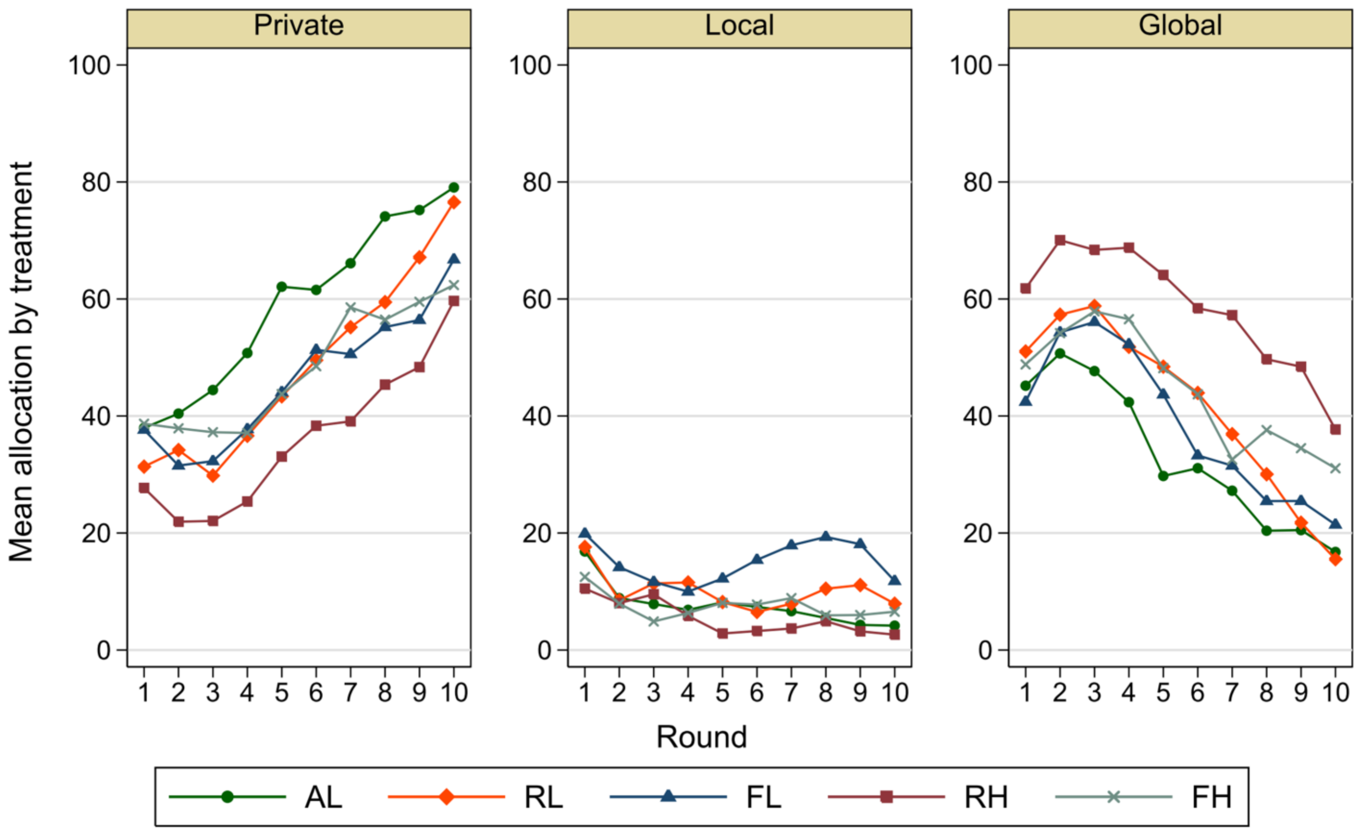

Figure 4.3: Time paths of MLPG contributions, by account and treatment

Overall results are similar, though standard errors are slightly larger when we consider first-round decisions only.

4.5.2 Effects of gains from cooperation

Our final hypothesis is concerned with the effects of our low versus high gains from cooperation manipulation, and corresponds to the vertical dimension in Figure 4.2. We summarize these effects graphically in Figure 4.4, separately for each of the three accounts, and for treatments with rematched and fixed groups. Once again, the confidence bars in this figure represent ± 1 standard error of the mean, treating group pairs as observations.

Hypothesis 3 states that an increase in the return on contributions to the global public good, representing the magnitude of potential gains from cooperation with the out-group, will increase the attractiveness of contributing to the global account. However, the response to this increase depends also on the strength of subjects' other-regarding preferences toward their out-group, which are also hypothesized to be weakened when the two groups have previously competed in the Tullock contest. Accordingly, we expect contributions to the global public good to be higher under treatment RH compared to RL. We also expect global contributions to be higher under FH compared to FL, however we expect this latter effect to be smaller in comparison to the rematched groups treatments.

Both Figure 4.4 and Table 4.1 confirm that our results are directionally consistent with these predictions: contributions to the global account are higher both under RH (58.5%) compared to RL (41.6%), as well as under FH (44.5%) compared to FL (38.6%). Moreover, the difference under rematched groups (16.9%) is almost three times larger than under fixed groups (5.9%). As a result, Tables 4.2 and 4.3 indicate that under rematched groups, the difference in contributions to the global account between RH and RL is statistically significant at the 5% level both in a nonparametric test at the level of group pairs, as well as in the individual-level regression.

On the other hand, under fixed groups the difference in global contributions between FH and FL is not significant in either of the analyses. Thus when there is a prior history of conflict between the two groups, subjects appear to be largely unmoved by an increase in the return to cooperating with the out-group.

Result 3: Contributions to the global public good are significantly higher under treatment RH compared to RL. Subjects respond to an increase in the return to cooperating with the out-group when the two groups have not previously interacted. However, when the two groups have previously competed against one another, there is no significant response.

Finally, we note one result not predicted by our hypotheses: contributions to the local public good are higher under FL compared to FH, and this difference is clearly significant – at the 5% level in the Wilcoxon test, and at the 1% level in the regression analysis.

4.5.3 Tullock contest

Since our group matching treatments were intended to manipulate subjects' experience of conflict, we now verify that the Tullock games were indeed keenly contested. Appendix Figure A1 plots the mean individual proposed investment in the group contest fund for each round of the Tullock game.[15]

In our design, the actual investment that was binding on each subject is the median of the amounts proposed by the three members of their in-group; Figure 4.A.1 also plots the mean of this group median.

Recall that each subject has an endowment of 100 in each round, and that the risk-neutral Nash investment at the individual level is 25. Figure 4.A.1 indicates that the mean proposed investments are substantially higher, and typically around double the Nash level. Further, the group medians are on average very close to the individual means, indicating that the high mean proposals are not driven by outlying group members. There is no discernible time trend in the level of contest investments over time, and in particular there is no evidence of convergence toward the Nash investment.

To give an indication of the extent of heterogeneity in the intensity of conflict in different group pairs, we compute the mean of the six individual proposals within each group pair, and depict the inter-quartile range of these means with respect to the forty group pairs as the shaded region in Figure 4.A.1. This confirms that while there are indeed differences in the intensity of conflict between group pairs, even the comparatively less competitive group pairs nonetheless invest at substantially higher than the Nash level.

In Table 4.A.4 we add a control for mean, group-level contributions in the Tullock game, averaged across the 10 rounds in the Tullock game. This gives a measure of in-

[15] This figure omits the data of the AL treatment, in which subjects played the Tullock contest as the second stage, following the MLPG game.

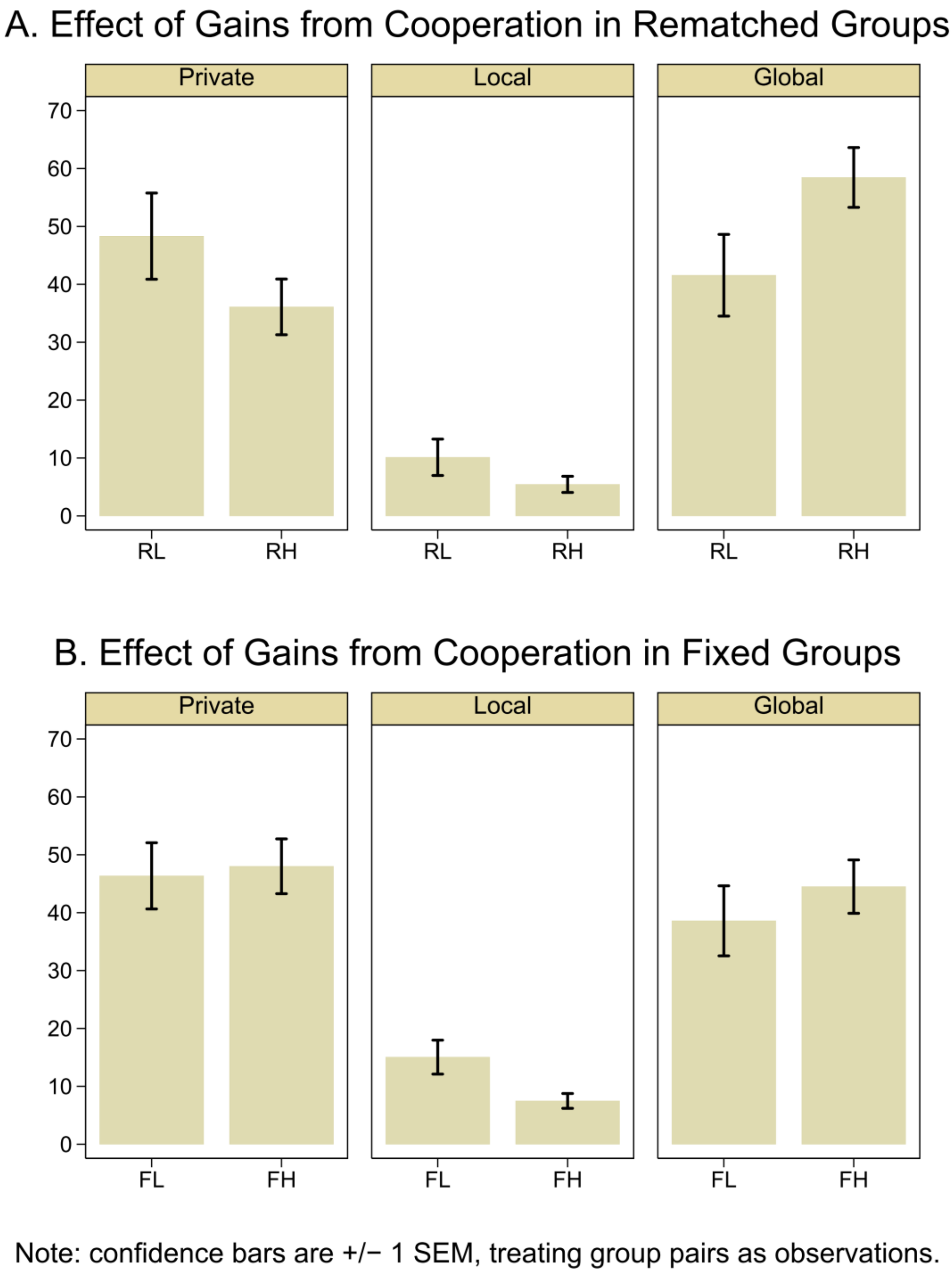

Figure 4.4:Effects of gains from cooperation

group competitiveness in the trust game. As would be expected, higher Tullock game investments are associated with higher contributions to local public good account, though the causal direction is not clear. However, the treatment effects are robust to including this control

4.5.4 Discussion

Our results demonstrate that competition does indeed make group membership more salient than arbitrary group identity in a laboratory setting. Contributions to the local account are significantly higher in the FL treatment compared to AL (Result 1). Interestingly, while the effect is directionally the same when comparing RL to AL, the difference is not statistically significant. Since decisions in the FL treatment are affected by preferences toward both in- and out-group members, the difference in allocations to the local account in FL compared to AL may be driven in part by a decrease in generosity toward an out-group with whom one has previously been in conflict (i.e. a decrease in b in equation 4, as well as an increase in a).

Another intriguing possibility is that different preferences for cooperation with the in-group may be activated when continuing to interact with the same out-group as in the contest. In terms of our model, the increase in a following conflict may be dependent on whether the in-group interacts with the same, or a different, out-group. Either of these mechanisms would suggest that inter-group competition is a useful tool for inducing salient group identity in economic experiments: the Tullock contest produced behavior that differed significantly from arbitrary group identity. This opens the possibility of using this, or other, forms of inter-group competition to examine questions relating to group identity in economic experiments.

While we observe greater cooperation within groups following conflict, there are substantial negative effects on efficiency—as a result of reduced cooperation between groups –when the MLPG game is played with the same out-group as the Tullock game. In our high returns from cooperation condition, contributions to the global account are lower in treatment FH – where the groups previously competed against one another – compared to RH (Result 2). Moreover, in our fixed-matching condition we find no significant increase in allocations to the global account in treatment FH compared to FL. On the other hand, allocations to the global public good increase significantly in the corresponding rematched-groups treatments (Result 3), which is consistent with previous research on the MLPG game using milder forms of group identity (Blackwell and McKee, 2003; Fellner and Lünser, 2008). Taken together, these results indicate that the findings of the previous studies may not be robust to the form of the group identity manipulation.

The muted response to higher returns from cooperation in our fixed matching treatments has important implications for naturally-occurring conflict. In our experiment, conflict in the Tullock contest is socially inefficient. However in other settings, competition may help to achieve socially efficient outcomes. For example, in a laboratory experiment (Nalbantian and Schotter, 1997) find that among several schemes commonly used by employers to incentivize workers, creating competition among teams is the most efficient (see also Guillen, Merrett, and Slonim, 2014). However, if conflict also decreases the potential for teams to cooperate with each other in future, the overall effect of incentivizing inter-group competition may well be negative. As a result of their prior history of competition, changes in preferences toward (or beliefs about) members of a formerly-competing team may impede future cooperation, resulting in lower profits for the firm in the long run.

Behavior in public good games has been explained by several mechanisms, including altruism (Andreoni, 1990), aversion to inequality (Fehr and Schmidt, 1999; Bolton and Ockenfels, 2000), preferences for social efficiency (Charness and Rabin, 2002) and reciprocity (Rabin, 1993; Dufwenberg and Kirchsteiger , 2004; Falk and Fischbacher, 2006). We are agnostic on which of these mechanisms drives the effects that we observe. Moreover, while our results are consistent with previous research showing that group identity affects both in- and out-group preferences, the form of conflict that we introduce in our design, namely competition over a fixed resource, may operate through distinct behavioral channels to other forms of salient group identity.

While more research is needed to explore this issue, our findings are also broadly consistent with field experiments such as Gumen (2012) and Buchan et al. (2009) who find in-group biases in similar designs using naturally-occurring group identities, as well as with previous work suggesting a link between parochialism and violent conflict (Choi and Bowles, 2007; Voors et al., 2012; Bauer et al., 2014). The fact that we find a similar effect among university students in a neutrally-framed laboratory setting suggests that the mere act of competition over a fixed-resource increases in-group bias, even in the absence of underlying ethnic divisions, cultural stereotyping, or exposure to violence.[16]

[16] While we do not study a representative sample, calling into question the generalizability of results, Cleave, Nikiforakis and Slonim (2013) find that students selected for experiments through similar procedures had social preferences in line with the general population that they were drawn from.

4.6 Conclusion

In this paper, we present the results of a laboratory experiment in which we manipulate the nature of subjects' prior exposure to conflict, to study its effects upon subsequent cooperation both within and between groups. Our design introduces a novel method to induce a stronger form of group identity in the lab, which enables us to disentangle the role of conflict in strengthening in-group identity from its effect in changing preferences towards an out-group. We also examine the response to changes in the returns to inter-group cooperation when there has been a past history of conflict between the groups.

We find that group identity is indeed strengthened by exposure to the Tullock contest, and that subjects demonstrate stronger in-group preferences when there has been a shared history of conflict between the in- and out-groups. We also find that prior exposure to conflict involving a specific out-group matters independently of the common in-group experience of conflict. Moreover, we find no response to an increase in the returns to between-group cooperation when there has been a previous history of conflict involving the same out-group. This neatly demonstrates how inter-group conflict–even in the setting of a laboratory experiment – can lead to less socially efficient outcomes.

Our results are consistent with a simple model in which an individual's other-regarding preferences are sensitive to group identity, such that increases in the material payoffs of in-group members may be weighted more heavily than corresponding increases in the payoffs of the out-group. We find that a shared experience of conflict with one's in-group increases the weight attached to in-group payoffs, while a history of conflict involving a specific out-group decreases the utility of out-group payoffs. This implies that conflict increases parochialism both by increasing preferences for in-group cooperation, and also by decreasing preferences for out-group cooperation.

Our findings are also consistent with those of several field experiments using naturally-occurring group identity and conflict. The fact that we observe similar effects suggests that competition itself plays a role in forming group identity, independent of more deeply-seated sources of group affiliation and conflict.

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Appendix to Chapter 4:

Additional Tables and Figures

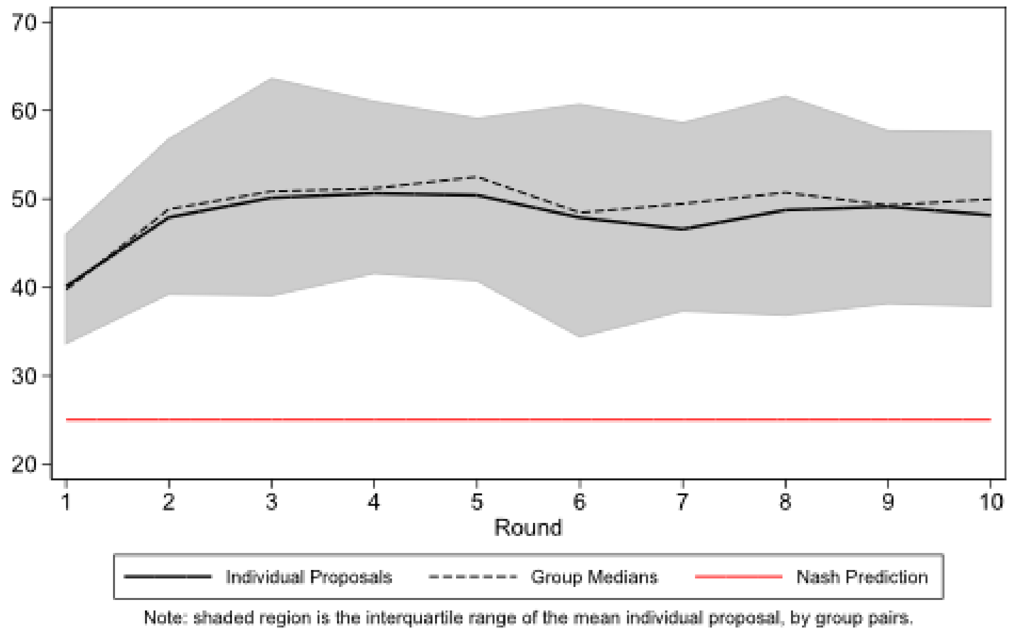


Figure 4.A.1:
Time path of Tullock contest investments (excluding treatment AL)

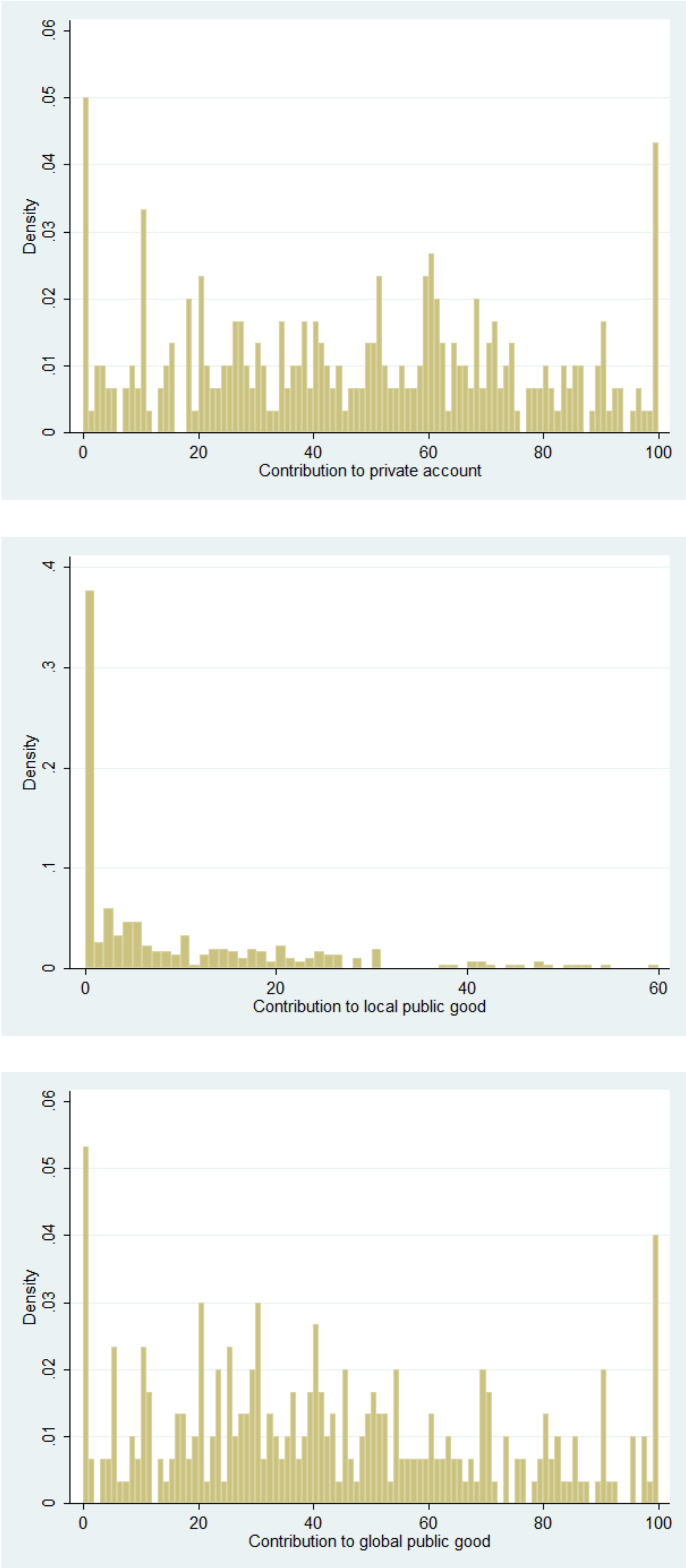


Figure 4.A.2. Histograms showing the distribution of contributions to the private account, local public good and global public good for all treatments. (Note that the scale is adjusted for the center panel).

Table 4.A.1 Observables by treatment

	Treatment					F-test for joint significance
	AL (1)	RL (2)	FL (3)	RH (4)	FH (5)	(p-value) (6)
Age (years)	23.55	22.42	22.62	22.93	23.17	1.69
	(3.35)	(2.32)	(2.43)	(2.67)	(2.42)	(0.15)
Undergraduate	0.50	0.65	0.62	0.58	0.48	1.29
	(0.50)	(0.48)	(0.49)	(0.50)	(0.50)	(0.27)
Local	0.78	0.93	0.82	0.78	0.90	2.21
	(0.42)	(0.25)	(0.39)	(0.42)	(0.30)	(0.07)
Female	0.35	0.28	0.27	0.32	0.35	0.4
	(0.48)	(0.45)	(0.45)	(0.48)	(0.48)	(0.81)
Years of study	3.78	3.27	3.38	3.52	3.72	0.9
	(1.95)	(1.76)	(1.75)	(1.58)	(1.84)	(0.46)
Hours worked	9.37	7.77	10.10	9.02	9.87	0.4
	(12.77)	(8.78)	(11.41)	(9.96)	(12.81)	(0.81)
Money spent in 1 week	790.00	918.17	906.67	936.50	788.25	0.38
	(802.84)	(779.41)	(977.45)	(1046.89)	(924.63)	(0.82)
Know 1 or more subject	0.30	0.48	0.25	0.15	0.35	4.48
	(0.46)	(0.50)	(0.44)	(0.36)	(0.48)	(0.00)

F-test from ANOVA analysis, joint test of significance for dependent variable across 5 matching treatments.

Table 4.A.2: Tobit on mean individual contributions, with Observables

	(1)	(2)	(3)
H1: RL	-12.57	3.96	8.79
	(8.02)	(4.05)	(8.06)
H2: FL	-13.21*	10.59***	5.38
	(7.07)	(3.85)	(7.44)
RH	-23.92***	-4.02	25.95***
	(6.20)	(2.81)	(6.14)
FH	-11.65*	-1.67	11.63*
	(6.54)	(2.30)	(6.38)
Age	-2.12**	0.00	2.41***
	(0.82)	(0.61)	(0.90)
Undergraduate	2.32	-1.85	-0.56
	(5.04)	(3.14)	(4.60)
Female	6.39*	4.75**	-8.44**
	(3.59)	(2.21)	(3.37)
Local	-2.51	-4.25	6.44
	(5.08)	(2.64)	(5.08)
Years of study	2.69*	0.20	-3.09**
	(1.47)	(0.93)	(1.41)
Hours worked	-0.19	0.07	0.12
	(0.18)	(0.09)	(0.17)
Money spent in 1 week	-0.00	-0.00	0.00
	(0.00)	(0.00)	(0.00)
Know 1 or more subject	1.34	2.45	-2.95
	(3.86)	(1.93)	(3.60)
Constant	100.75***	4.48	-16.23
	(19.69)	(14.82)	(21.39)
Observations	300	300	300
H2a: FL vs RL	0.94	0.21	0.71
H2b: FH vs. RH	0.07*	0.46	0.03**
H3a: RH vs. RL	0.17	0.09*	0.04**
H3b: FH vs. FL	0.84	0.00***	0.41

Notes: base category AL; robust standard errors clustered by group pairs.
* p<0.10 ; ** p<0.05 ; *** p<0.01

Table 4.A.3: Tobit on first round contributions

	(1)	(2)	(3)
H1: RL	-8.46	-0.68	7.13
	(10.45)	(6.15)	(11.24)
H2: FL	2.45	6.25	-7.37
	(11.66)	(6.78)	(13.82)
RH	-18.67*	-12.78*	24.85**
	(10.64)	(6.65)	(12.41)
FH	2.42	-9.69*	1.44
	(9.64)	(5.80)	(10.66)
Constant	28.14***	4.65	50.45***
	(7.35)	(4.18)	(7.23)
Observations	300	300	300
H2a: FL vs RL	0.36	0.33	0.33
H2b: FH vs. RH	0.04**	0.63	0.07*
H3a: RH vs. RL	0.36	0.08*	0.19
H3b: FH vs. FL	1.00	0.019**	0.54

Notes: base category AL; robust standard errors clustered by group pairs. * p<0.10 ; ** p<0.05 ; *** p<0.01

Table 4.A.4: Tobit on mean individual contributions, Tullock competition

	(1)	(2)	(3)
H1: RL	-10.94	1.92	8.60
	(8.48)	(4.25)	(8.46)
H2: FL	-12.94*	9.56**	5.62
	(7.40)	(3.95)	(7.85)
RH	-23.13***	-5.67**	26.26***
	(6.34)	(2.54)	(6.40)
FH	-10.87	-2.60	11.57*
	(6.68)	(2.40)	(6.92)
Mean Tullock group contribution	-0.10	0.13*	-0.00
	(0.17)	(0.08)	(0.17)
Constant	63.88***	-2.25	32.87***
	(9.31)	(3.64)	(9.29)
Observations	300	300	300
H2a: FL vs RL	0.83	0.16	0.74
H2b: FH vs. RH	0.06*	0.28	0.02**
H3a: RH vs. RL	0.15	0.11	0.03**
H3b: FH vs. FL	0.78	0.00***	0.44

Notes: base category AL; robust standard errors clustered by group pairs. * p<0.10 ; ** p<0.05 ; *** p<0.01

Appendix: Experiment Instructions

Instructions for Chapter 3

Script Sender:

To group

Hello, my name is xxxxxx and this is ….. Thank you for agreeing to participate in this study that concerns the economics of decision making. You will get 2000 Ush just for coming, and depending on the decisions that you make, the decisions that other make and luck, you may receive more than this. This study may take about three to four hours, so if you think you will not be able to stay that long without leaving please let us know now. If at any time you find that this is something that you do not wish to participate in for any reason, you are of course free to leave whether we have started the task or not and the initial fee is yours to keep.

You should understand that this is not our own money. This money was given to us by our University for research. This is a onetime payment and will not be repeated in the future. The simulations are part of a scientific study. They will NOT be used to evaluate you or your community. There are no "right" or "wrong" answers.

If you have heard about a task that has been done here in the past you should try to forget everything that you have been told. This is a completely different task.

I'd like to ask all of you not to talk amongst yourselves from this point on. This is really important and we will have to ask you to leave and you will not have a chance to receive extra money. If you have a question or concern at any time, feel free to ask me or one of my colleagues.

Also, you cannot leave the room during the activities and you cannot use your mobile phone. If you break any of these rules we will have to ask you to leave, and you will receive only your participation fee. Please turn off your mobile phones now.

In order for this study to be carried out correctly, we really need you to not talk about the task while we are here together. This is very important and please be sure that you obey this rule, because it is possible for one person to spoil the task for everyone, in which case we would not be able to continue with the study.

Trust Game

Group

Now we'll begin the first task. Please pay close attention, don't worry if you don't understand the task completely at first: I will go over examples and you will have plenty of time to ask questions. After I'm done explaining the instructions, you will go one by one with one of us into a separate area to make your decision so that your choice is completely confidential. You will be matched with a partner. This partner is not here in this room, and you will not know the identity of this person, nor will he or she or anyone besides us ever know with whom you are matched. The amount of money that you earn depends on your decision and your partner's decision.

In this task, you will be given 2000 Ush. We'll use these slips of paper to represent the banknotes (*hold up two 1000 Ush bills a experimental currency*). We will exchange these slips of paper for real money and come back tomorrow to pay you. You will be given the chance to send part or all of this money to your partner. We will triple everything that you choose to send, so that we add 2000 for every 1000 that you send. Then your partner has the chance to send part of this money back to you.

Please follow along as I demonstrate with these pictures (*demonstrates process with pictures)* and I'll explain this in a bit more detail, and give some examples. Please pay close attention, since your payment will be affected by your decision in this task. If you don't understand everything at first, that's fine. You will have a chance to ask questions.

In these pictures we'll use the red figure to represent you, and the blue figure to represent your partner (*point to picture)*. So, when we begin, you have got 2000 and your partner doesn't have anything.Next, you can choose to send something to him or her. You'll make this choice by putting the money that you want to keep in the red envelope that looks like this (*hold up envelope and point to picture),* and putting the money that you want to send in the blue envelope that looks like this. (*Show picture).*

You could send nothing, 1000, or 2000. We will triple anything that you send, so that your partner receives 3000 UGX for every 1000 you send. He gets nothing if you send USh 0, he gets Ush3000 if you send USh 1000 and he gets USh 6000 if you send USh 2000.

Let's say that you choose to send 1000 Ush. (*Show picture 3.a).*You put 1000 Ush in the red envelope to keep, and 1000 Ush in the blue envelope to send to your partner.

Remember anything that you send gets tripled, so we add 2000 Ush to the 1000 Ush in the blue envelope. The money you keep stays the same. (*Show picture).*

Okay, now your partner receive 3000 and you have 1000. (*Show picture).*

Next, your partner has the option of returning some of this money back to you. So he or she can send back all 3000, 2000, 1000, or return nothing. (*Show picture).* So if your partner returns nothing, you will go home with 1000 and your partner goes home with three. If your partner returns 1000, you will get this money, plus the 1000 that you kept – so 2000 in all – and your partner goes home with 2000 as well. If your partner returns 2000, then you'll have 3000 all together and your partner will go home with 1000. If your partner decides to return all 3000. Then you'll go home with this 3000 plus the 1000 you kept, so 4000. Your partner won't go home with anything.

Your partner has all of the same information about this task as you, so he or she knows that you had the chance to send money, that what you sent would be tripled, and that he or she can send it back. He or she will also know that you know that he or she would be able to send some money back., Okay. Now we'll go over another example from the beginning. To remind you, you'll start this task with 2000 Ush (*Show picture)* and your partner begins with nothing. You need to decide whether you want to send 1000, 2000 or nothing to your partner (*picture).* Let's say you decide to send nothing. This means you should put both bills into the red envelope for you to keep, and that you should leave the blue envelope empty. (*show picture).*

So now you have 2000 and your partner doesn't have anything *(picture)*. Because your partner doesn't have anything to send back, the task ends here. You go home with 2000 and your partner doesn't get anything from this task.

One last example. You begin with the 2000 Ush that we give you. (*Show picture).* You are given a red envelope for what you want to keep and a blue envelope for what you will send to your partner. (*picture 2).* Now, let's say that you want to send all 2000. In this case, you'll put both bills in the blue envelope to send to your partner and leave the red envelope empty. (*picture).*

We'll triple everything in the blue envelope. So, we add 2000 Ush for the first 1000, and 2000 Ush for the second 1000 Ush. So, that's 6000 Ush altogether. (*picture).* Your partner receives this 6000 Ush and you don't have anything. (*picture).*

Now your partner can keep all 6000, or send you back 1000, 2000, 3000, 4000, 5000 or all 6000. You'll take home whatever they choose to send. Your partner takes home whatever portion of the 6000 that they don't send back to you. So if he or she sends back 1000, then you've got 1000 and they have 5000 at the end of the day. If they send back 2000, you go home with 2000, and they go home with 4000. If they send back all 3000, you each go home with 3000. If they send back 4000, you get 4000 and they get 2000; if they send back 5000, you get this 5000 and they are left with 1000, and finally, if they send back all 6000, you'll get paid 6000 and they won't get anything from this task.

Are there any questions? (*take and answer questions).* We'll remind you of how the task works when you go individually to play.

There are a couple more things I'd like to explain.

Firstly, when you make the decision, you'll receive this slips of paper that represent bank notes, not real money. Because the paper slips will be exchanged for real money, so you should carefully think before making choices. We'll exchange the money and pay you according to what you put in the envelopes – and your partner's decision – so be sure to put all of the paper slips in the envelopes.

After we finish today, you will be paid Ush 2000 for your participation in this study. This is to compensate you for your time you spend here today. In addition, we will pay you based on your choices in one of the today's tasks. There will be two main tasks. We'll ask you a few questions about each of these tasks. I have just explained the first one. The second one will be explained later. We will use a lottery to decide which one you and your partner are actually paid for. Each of these two tasks has an equal chance of being selected. Because of this, you should make your decision in each task very carefully and independently of the decision you make in the other, because we don’t know right now for which of the two tasks you will be paid.

We won’t be able to pay you for the choices in the tasks today but tomorrow, because we need to find out what your partner decides and match it with your choices. As you know, your partner comes from a different village in the same sub-county. So we won’t know your partner’s choices until tomorrow, when your partner will make their choices during a meeting similar to the one we had here. After he/she makes his/her choices, we will determine how much you will receive and how much your partner will receive. We will prepare an envelope with money for you and come here tomorrow at XX to give you the money. We will come to the other village to give the money to you partner the day after tomorrow. So note that both you and your partner will be paid the day after making your decisions.

Secondly, this decision that you make is totally anonymous, and no one will ever know what you choose. You each received a number when you came in today *(show an example of the id number)*. Hold on to this number, because you’ll need it to collect your payment. In a little while one of us will explain the game to you again, before you make your choice by putting the paper slips into the envelopes. An “accountant” will take these envelopes and calculate how much money you receive from the game (*point at “counter”)* by checking the decision of your partner in a different village. The “accountant” will be the only one who knows how much money you put in the envelope and how much your partner chose to send back, but he won’t know whose number goes with whom! He’ll just count out your payment, put the money into an envelope with your ID and close the envelope. A different person will give you the closed envelope with money according to your ID number. This person sees your face and knows your number, but will not know how much money is inside the envelope and how you decided.

Any questions? (*take and answer questions).*

Now you will go with my colleagues so that we can check whether you understand everything. They’ll ask you some questions to make sure you understand. These questions will not be paid. Then you will make your decision for the tasks, for which you will be paid, privately.

Individual

Hello, I’m xxxxx

First, before we begin, I’m going to tell you a few things about your partner, whose choices will affect your earning. We can’t tell you specifically who your partner is, but we already have some information about this person that we can give you. He is (*insert details according to treatment.).* Your partner was told some information about you as well, we told him/her that you are … *(insert profile details).* So you know some information about your partner and he knows some information about you.

Now we’ll do the task., I’ll give you 2000 UGX in our paper “banknotes.” You can choose to send none, some or all of this money to your partner. For every 1000 UGX that you send, I’ll add 2000 UGX, so your partner will receive three times what you send. Then, your partner can return any portion of what he/she receives back to you: none, some or all.

Let's go over the process one more time to refresh your memory (*count out bills on table to demonstrate)*. Let's say that you decide to send 1000 Ush. You'll put this money into the blue envelope to send to your partner. You put the other 1000 in the red envelope to keep.

We'll triple the amount that you put in the blue envelope. So we add 2000 to the 1000 that's already there. Now you have 1000, and your partner has 3000. Now your partner will have the chance to send some of that money back to you. He could send all 3000, 2000, 1000 or he could choose not to send anything. You'll get whatever he sends back, plus the 1000 that you chose to keep.

You'll put the money inside the envelopes in this box (*point to privacy screen)* and then drop both envelopes into this box (*point to ballot box).* Your number is written on the envelope, so xxxxxx (*accountant)* will count up how much you send, how much your partner decided to return, and then prepare your payment. He won't know whose number goes with whom, so your decision is totally anonymous.

Now let's say you decide keep the whole USh 2000. You'll put all the money into the red envelope and nothing the blue envelope to send your partner. You'll get USh 2000 and your partner nothing.

Let's say that you send all 2000. You'll put this money in the blue envelope to send to your partner. We'll triple the amount that's sent.

Comprehension questions for main task (answers in brackets). Mark responses on experiment sheet. If answered incorrectly, re-explain task. Ask questions again. If answered incorrectly on second attempt, go to decision.

1. So, how much can you send? (0, 1000, 2000)
2. How much does your partner get if you send 0?
3. How much does he get if you send 1000? (3000)
4. How much does he get if you send 2000? (6000)
5. Which amounts can your partner choose to send back if you send 0? (nothing)
6. How can he send back if you send 1000? (nothing, 1000, 2000, 3000).
7. If you send 2000? (nothing, 1000, 2000, 3000, 4000, 5000, 6000).

Now I'll give you the 2000 Ush.

Now, please make your decision by placing the amount you wish to keep in the red envelope and place the amount you wish to send to your partner in the blue envelope. To remind you… (*repeat partner information*) (*Wait for decision).* Now please put both the red and the blue envelope in this larger envelope. The accountant will look at how much you sent and how much your partner returns tomorrow and calculate how much you will receive and we'll return tomorrow to pay you.

Now I will ask you a few questions about your partner's decision. I'll ask you some questions and you'll get 500 Ush for each correct answer. This is in addition to the money you may receive from the decision you've just made and for coming today. Your answers will be anonymous in the same way that the task was.

We will ask your partner how much he/she would send back for each possible amount that you might have sent – so how much he'll send back if you send 1000 and how much if you send 2000. We'll use this information to count out how much you'll receive. Now I'm going to ask you to guess what your partner will decide. In other words, to guess how much was your partner is willing to return for each amount that you could have sent? You have already chosen how much to send and that's not going to change. But, if you can correctly guess how much your partner would have sent back for each amount you could have sent, you can earn some extra money. You'll get 500 for each correct answer. This will be anonymous as well. No one will know what you answer. Therefore, you should answer what you really think, as this gives you the best chance of earning more money.

Imagine that you had sent 1000, which means your partner would have received 3000 (*count out money on the table*). So, now you've got 1000, and your partner has 3000. Please look at these pictures. This represents the amount that your partner returns to you. In this one, he sent back 1000 so you have 2000 and he's got 2000. In this picture, he didn't send anything, so you've got 1000 and your partner has 3000. In this one he sent back all 3000, so you've got 4000 and he doesn't have anything. In this one, he send 2000, so you've got 3000 and he has 1000. You will choose what you think your partner will send back by selecting the corresponding picture and placing it in this envelope. Fold the rest of the paper in half and put it in this box (*hold up box for discarded papers).* You'll get 500 Ush for guessing what your partner will actually return.

To make sure you understand, *(explain again if answered incorrectly)*

8. let's say you think the partner will not send back anything. Which picture will you choose?
9. Now let's say you think he will send back 3000? Which picture will you choose?
10. What if you think he will send back 2000?
11. What if you think he will send back 1000?

Remember, your partner is… (*repeat partner details).* Now please choose the picture that you think represents what your partner will return.

Now, I'll ask you the same question again, imagining that you had sent 2000, which means your partner would have received 6000 (*count out money on the table*). So, now you've got nothing and your partner has 6000. Please look at these pictures. This one represents the amount that your partner returns to you. In this one, he sent back nothing, so you've got nothing and your partner has 6000. In this one …(*explain **all** other options).*

To make sure you understand, *(explain again if answered incorrectly)*

12. let’s say you think the partner will choose to send back all 6000. Which picture would you choose?
13. Now let’s say you think he will send back 2000. Which picture will you choose?
14. What if you think he won’t send anything?
15. What if you think he will send 5000?
16. What if you think he will send 3000?
17. What if you think he will send 4000?
18. What if you think he will send 1000?

Please choose what you think your partner will send back by selecting the corresponding picture and placing it in this envelope. Fold the rest of the paper in half and put it in this box (*hold up box for discarded papers).* You’ll get 500 Ush for guessing what your partner will actually return.

Remember, your partner is… (*repeat partner details).* Now please choose the picture that you think represents what your partner will return.

Now I’m going to ask you to answer a different question about your partner. It’s a different question, but still about the same task.

When we complete this task with your partner, we’ll ask him how much he thinks you have sent. He will get 500 if he guesses correctly your choice. Now I’m going to ask you to guess what he will say. In other words, I am asking you to guess how much your partner expects you to send. You’ll get another 500 Ush for guessing correctly. Remember, you have already chosen what to send and what to keep, and that won’t change.

Please look at these pictures. Remember, the red envelope is for the money that you keep, the blue one is the money you sent. In this one, you send 1000 and keep 1000. In this one, you send nothing and keep 2000, in this one you send 2000 and keep nothing.

You’ll choose the picture that corresponds to what you think your partner expects you to do.

Just to make sure that you understand, (*explain again if answered incorrectly),*

19. Which picture will you choose if you think that your partner expects you to keep 1000 and send 1000?
20. Which picture will you choose if you think that your partner expects you to send 2000 and keep nothing?
21. Which picture will you choose if you think that your partner expects you to keep 2000 and send nothing?
22. In this question, are we asking about what you have sent? N
23. In this question are we asking about what your partner would do in your place?N
24. In this question are we asking about what your partner expects you to send?Y

Please make your choice by putting the picture that represents what you think your partner expects you to do in the big envelope. You'll get an extra 500 Ush for guessing the right answer. Remember, this decision is anonymous like the other ones you've made today. To remind you, your partner is (*insert partner details).* Please make your decision now.

Dictator game

Now we have finished with task 1 and we'll begin the second task. So all of the decisions you've made so far have been part of task 1, and everything from now on will be part of task 2. To remind you, we'll have a lottery to decide which of these tasks will actually be paid. There's an equal chance that each of these tasks will be chosen. In this task, you're matched with the same partner as before. To remind you… (*repeat partner's details).* In the second task, you'll make another decision that will affect your payoff and your partner's payoff. Since we don't know right now whether this task or the previous task will be paid, you shouldn't consider your decision in the first task when making your decision in the second task.

The decision that you'll make in the second task is actually very similar to what you decided in the previous task: you will be given 2000 Ush. We'll use these slips of paper to represent the banknotes – as we did in the last task. We'll pay you in real money when we come back tomorrow, according to your decision. You will be given the chance to send part or all of this money to your partner. We will triple everything that you choose to send, so that we add 2000 for every 1000 that you send. However, after you send this money, the task is over. Your partner will not be able to return anything as they were in the last task.

Let's go over the process one more time to refresh your memory (*count out bills on table to demonstrate)*. Let's say that you decide to send 1000 Ugx. We'll triple the amount that you put in the blue envelope. So we add 2000 to the 1000 that's already there. Now you have 1000, and your partner has 3000. The task ends here and you go home with 1000, while your partner goes home with 3000. If you send 2000, we'll add 2000 to the first 1000 and add 2000 to the second 1000, so your partner gets 6000 altogether. This is the end of the task and you go home with nothing, and your partner goes home with 6000. Let's say you decide to keep 2000 and not to send anything. This is the end and you go home with 2000 and your partner goes home with 0.

Just to make sure that you understand,

1. What amounts can you choose to send?
2. If you choose to send 1000, how much do you have and how much does your partner have?
3. How much do you each go home with?
4. If you choose to send 2000, how much do you have and how much does your partner have?
5. If you choose not to send anything, how much do you each go home with?

Now, please make your decision by placing the amount you wish to keep in the red envelope and place the amount you wish to send to your partner in the blue envelope. To remind you… (*repeat partner information*) (*Wait for decision).* Now please put both envelopes in this box. One of us will look at how much you sent and calculate how much you will receive and we'll pay you when we come back tomorrow.

Now I will ask you a question about your partner's expectation. This is in addition to the money you may receive from the task and for coming today. I'll ask you a question and you'll get 500 Ush if you can guess the correct answer. Your answers will be anonymous in the same way that the task was.

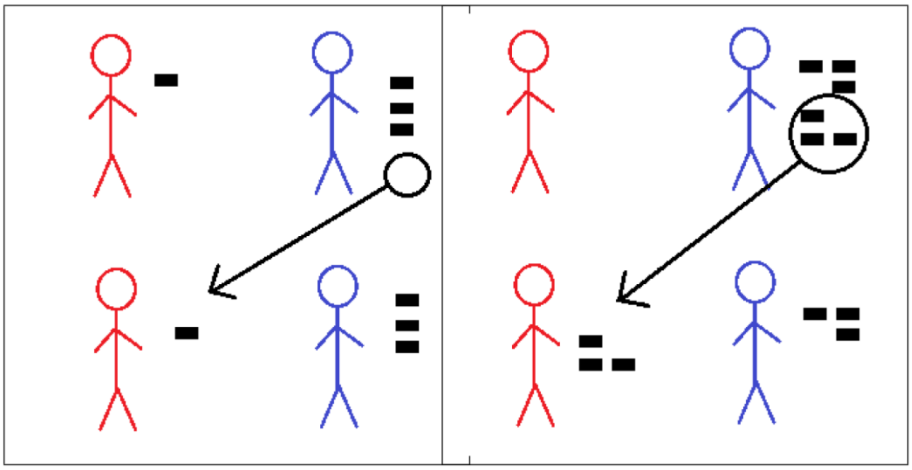
We asked your partner how much he thought that you would send. He will get 500 if he guesses correctly the amount that you sent. Now I'm going to ask you what you think he said. In other words, you will guess how much do you think your partner expected you to send. if you think that your partner expected you to send nothing, choose this picture, if you think he expected you to send 1000, choose this one, and if you think he expected you to send 2000, choose this one. (*point at pictures).*

Just to make sure that you understand, which picture would you choose if

1. you think that your partner expects you to send 1000?
2. You think your partner expects you to send 2000?
3. You think your partner doesn't expect you to send anything?

Please make your choice by putting the piece of paper with the amount appropriate amount of money in the envelope. Remember, the red envelope is the money that you keep, the blue one is the money you sent. You'll get an extra 500 Ush for guessing the right answer. Remember, this decision is anonymous like the other ones you've made today. To remind you, your partner is (*insert partner details).*

Thanks for completing the tasks!



(a) 1000 sent, nothing returned. (b) 2000 sent, 3000 returned.

Online Appendix Figure B.1: An example of pictures used for the explanation of the trust game. The Sender and Receiver are represented by the red and blue figures, respectively.

Instructions for Chapter 4

RL treatment

1. General instructions

Welcome to this session. From now on, please do not talk to the other participants, or communicate with them in any other way. Mobile phones must also be switched off. If you have a question, please raise your hand and one of us will come to you and assist you in private. These rules are important. If you break any of these rules, we will cancel the session and dismiss all of you without any payment.

In this experiment you will make a number of decisions. These instructions explain the decisions that you will make and their consequences. Depending on your decisions you will earn money which will be paid to you in cash at the end of the experiment.

Throughout the experiment we will record all earnings in "tokens". At the end of the session, we will randomly select two decision rounds to count toward your earnings. The tokens that you earn in these two rounds will be converted into Czech crowns at the following exchange rate:

1 token = 1 CZK

2. First task

This task will consist of 10 rounds. At the end of the session, we will randomly draw a number between 1 and 10 from a bag to select one of these rounds, and you will be paid your earnings from this randomly chosen round.

At the beginning of this task you will be matched with two other randomly selected people in the room, to form a group of three. Your group will play against one of the other groups who will be your opponents. The other members of your group, as well as your opponents, remain the same through all ten rounds of this task. You will not learn who your group members or opponents are, either during or after today's session. Likewise, neither your group members nor opponents will learn of your identity.

In each round your group and your opponents will compete for a prize as we will now explain. At the beginning of each round, you will be given an endowment of 100 tokens. Each group must decide how many tokens to allocate to its "contest fund". This decision is made in the following way. Each group member will be asked to propose a number of tokens to allocate to the contest fund. The computer will then determine the median amount proposed by the three members. (The median is the middle number of an increasing series of numbers: the median of 1, 2 and 3 is 2; the median of 1, 98 and 100 is 98.) This amount will be automatically deducted from each member's endowment and allocated to the group's contest fund. Any tokens not allocated to the contest fund will be yours to keep. Since each group member must allocate the same amount to the contest fund, each member will end up with the same balance of tokens. Likewise, your opponents will decide how many tokens to allocate to their contest fund in exactly the same way.

After each group has chosen its allocation to the contest fund, the computer will conduct a random draw to determine whether your group or your opponents win the prize. The prize is worth 300 tokens, which is divided equally among the members of the group that wins it (100 for each member). Your group's chances of winning depend on how many tokens are in its contest fund. This works as follows: imagine that each token allocated to the contest fund by your group and by the other group are placed in a bag, and then one token is randomly drawn from this bag. If the token that is drawn belongs to your group, then your group wins the prize. If the token belongs to the other group, then the other group wins the prize. Each group's chances of winning depend on the number of tokens that it has allocated relative to the number of tokens allocated by the other group.

For instance, if your group and your opponents each allocate the same amount to the contest fund, each group has the same number of tokens in the bag and an equal chance of winning (a 1/2 chance). If your group allocates twice as many tokens to its contest fund as your opponents, your group has twice the chances of winning (your group has a 2/3 chance of winning and the other group has a 1/3 chance). Thus, your group's chances of winning increase with the amount that it allocates to the contest fund, and decrease with the amount allocated by your opponents. If your group allocates nothing and the other group allocates 1 or more tokens, than the other group automatically wins. If neither group allocates anything to the contest fund, then both groups have a 1/2 chance of winning.

After the computer has determined the winner, you will be informed which group won the prize and shown your earnings for that round. Your earnings are equal to your initial endowment of 100 tokens, minus the number of tokens you allocated to the contest fund, plus 100 tokens if your group won the prize. Since each group member must allocate the same number of tokens to the contest fund in each round, each group member's earnings will also be the same in each round.

3. Second task

This task will again consist of 10 rounds. At the end of the session, we will randomly draw a number between 1 and 10 from a bag to select one of these rounds, and you will be paid your earnings from this randomly chosen round.

For this task, you will again be a member of the same group of three people with whom you were matched in the previous task. This group will now be paired with a second group of three, who were also matched with one another in the previous task, and who we will refer to as the "other group". As before, you will never know the identities of any of these people, and they will never know your identity.

The other group will not be the same as the group that was your opponent in the first task.

At the beginning of each round, you will be given an endowment of 100 tokens. You will be asked to decide how many of these tokens you will allocate to three accounts: Account A, Account B and Account C. Your total earnings will depend on the amount that you and others allocate to each of the accounts as explained below:

<u>Your earnings from Account A</u>

Each token that you allocate to Account A will earn one token for you alone. Therefore if you allocate X tokens to Account A, you will earn exactly X tokens from Account A.

No-one other than you earns anything from the tokens you allocate to Account A. Likewise, you will not earn anything from any tokens allocated to Account A by any other person.

Your earnings from Account B

Tokens allocated to Account B only affect the earnings of the three members of your own group. For every token allocated to Account B, by any member of your group, each member of your group will earn 1/2 tokens, regardless of whether he or she allocated any tokens to Account B.

Your earnings from Account B = 1/2 x (sum of three allocations to Account B)

The earnings from Account B are calculated in the same way for all three members of your group, so all members of your group each receive the same earnings from Account B as you do. Therefore, all members of your group each benefit equally from every token that any member allocates to Account B.

For each token that you allocate to Account A, you earn one token. Suppose that you allocate this to Account B instead. Then the total amount allocated to Account B increases by one token, and your earnings from Account B increase by 1/2 tokens.

At the same time, the earnings of the other members of your group also increase by 1/2 tokens each, so the total earnings from Account B for all three group members would increase by 3/2 tokens in total.

Your allocation to Account B therefore increases the earnings of the other two members of your group, and similarly their allocations to Account B also increase your earnings. For each token that another member of your group allocates to Account B, you also earn 1/2 tokens.

So, if all 3 members of your group allocate 1 token to Account B, then Account B contains 3 tokens in total, and each group member will receive 3/2 tokens. In total, your group would earn 3 x 3/2 = 9/2 tokens from Account B.

Tokens allocated to Account B only affect the earnings of the members of your own group. They do not affect the earnings of the other group of three with whom your group is matched.

The members of the other group can allocate tokens to their own Account B, and this will not affect the earnings of you or the other members of your own group.

Your earnings from Account C

Tokens allocated to Account C affect the earnings of both the three members of your own group, as well as the three members of the other group. For every token allocated to Account C, by any member of either group, each member of both groups will earn 1/3 tokens, regardless of whether he or she allocated any tokens to Account C.

Your earnings from Account C = 1/3 x (sum of six allocations to Account C)

The earnings from Account C are calculated in the same way for all six members of both groups, so all members of both groups each receive the same earnings from Account C as you do. Therefore, all members of both groups each benefit equally from every token that any member of either group allocates to Account C.

For each token that you allocate to Account A, you earn one token. Suppose that you allocate this to Account C instead. Then the total amount allocated to Account C increases by one token, and your earnings from Account C increase by 1/3 tokens.

At the same time, the earnings of the other members of both groups also increase by 1/3 tokens each, so the total earnings from Account C for all six members of both groups would increase by 2 tokens in total.

Your allocation to Account C therefore increases the earnings of the other five members of both groups, and similarly their allocations to Account C also increase your earnings. For each token that another member of either group allocates to Account C, you also earn 1/3 tokens.

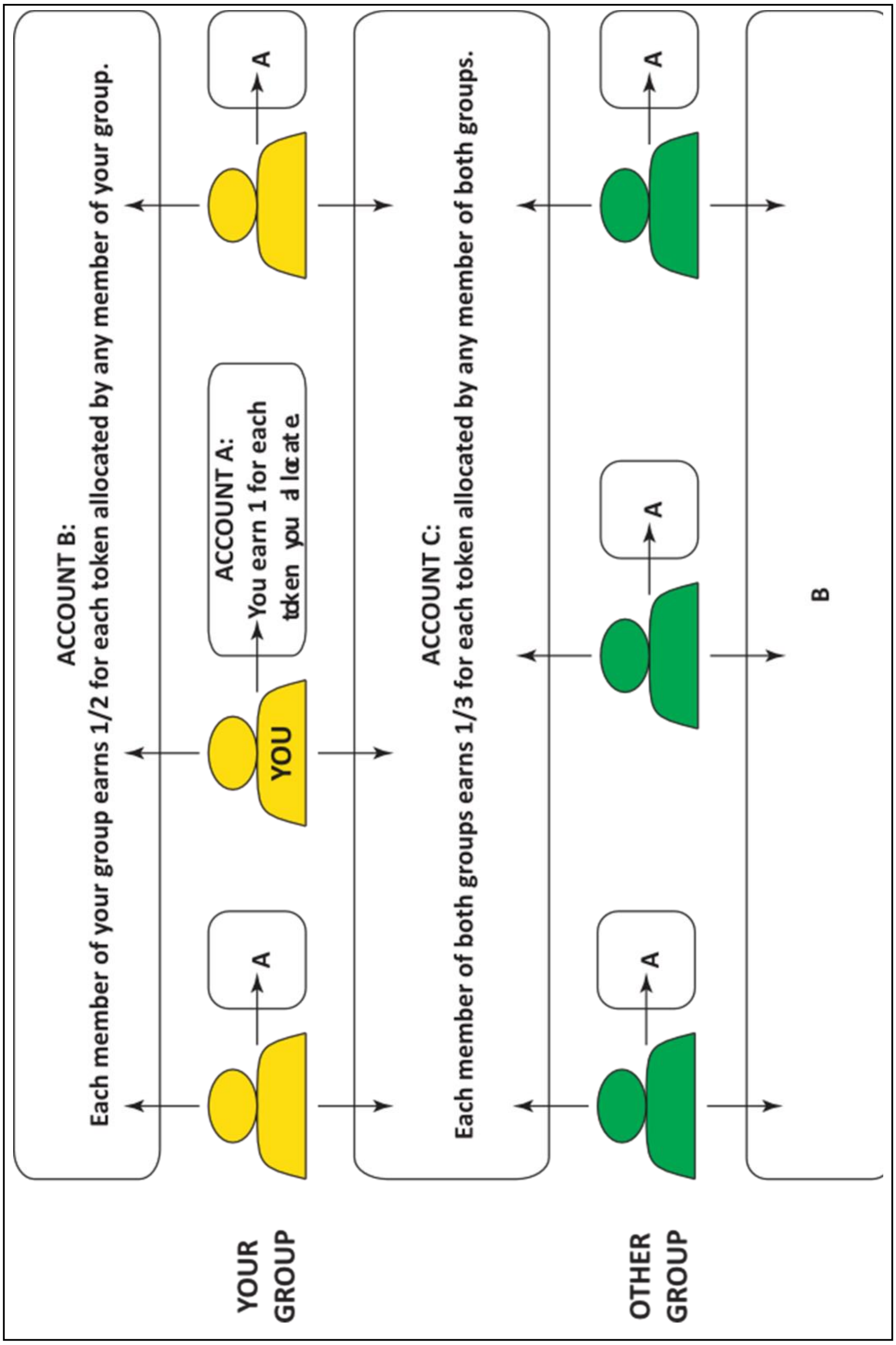
So, if all 6 members of both groups allocate 1 token to Account C, then Account C contains 6 tokens in total, and each member of both groups will receive 2 tokens. In total, both groups would earn 6x2=12 tokens from Account C.

Your total earnings from this task

Your earnings = Your own allocation to Account A

+ 1/2 x (sum of three allocations to Account B)

+ 1/3 x (sum of six allocations to Account C)


ACCOUNT B:
Each member of your group earns 1/2 for each token allocated by any member of your group.
YOUR GROUP
YOU
ACCOUNT A:
You earn 1 for each token you allocate
A
ACCOUNT C:
Each member of both groups earns 1/3 for each token allocated by any member of both groups.
OTHER GROUP
B

Response to opponents:

Defense: December 2, 2015

Opponents:

Randy Filer*, Professor of Economics at Hunter College*
Jaromír Kovářík*, Assistant Professor of Econimcs at the University of the Basque Country*
Silvester van Koten*, Associate Professor of Economics at the University of Economics in Prague*

I would like to wholeheartedly thank all three reviewers for spending the time to read and comment on my dissertation. I found the comments useful, both for this current revision as well as for the future publication of the unpublished papers it contains.

All of the reviews I received requested only minor changes to the work, which I have, for the most part addressed. I will summarize my responses and changes to the text by chapter:

Chapter 2:

Professor Filer points out that that the definition of employment is not as clear-cut in developing countries, such as Liberia, and that for this reason it is necessary to use a more comprehensive definition of employment when analyzing economic activity. In fact, the survey upon which I draw my data used a broad definition, which included informal economic activity. However, this was not clear from the original text, and has been made explicit on page 26 of the revised version. Professor Filer also suggests looking into family businesses, and while this is an excellent suggestion, I unfortunately do not have the data necessary to follow-up on this.

Professor Kovářík suggests that data from the survey be combined with regional employment levels to explain differences across location in the employment rates. Again, I unfortunately do not have access to reliable data to make this possible.

Chapter 3:

Professor Filer notes that some literature suggests that social preferences (and awareness of social interactions) might reach back to infancy, and this implies that such preferences might be innate. I have added the citation he recommends, Hamlin, Wynn and Bloom (2007) to the text, on page 62, in footnote 32. While this is an important disclaimer, our argument in the paper—that we observe an effect on social preferences among only those who were exposed to soldiering at an early age—still stands, due to the literature we cite, establishing that children are more sensitive to environmental influences during certain time periods.

In addition, professor Filer points out that we study a population that has a very high baseline-exposure to violence, and our results do not speak to this issue. Unfortunately, we have no instrument available to study exposure to violence causally. While we claim that abduction was exogenous, other types of war-time violence may not have been. We do note in the paper, on page 61, in footnote 31, that a recent meta-study of trust games conducted by Johnson and Mislin (2011), reports average trust game results that are in line with what we find in our study. Another danger in reporting results from this paper is that it may be interpreted as minimizing the effects of warfare in general, or child soldiering in particular. We try to make it clear in the conclusion that this is not the case, and there are many other, negative psychological effects linked to child soldiering.

Professor Kovářík had two main concerns: First, we examine the frequency of choices made by receivers in the trust game that lead to equal allocations, and interpret this as being indicative of inequality aversion. He is right to point out that this activity is not the perfect mechanism for identify preferences. Nonetheless, I feel that the exercise is worthwhile, and helps to explain our results. We have added a short caveat on page 65 expressing this.

Secondly, Professor Kovářík points out that we should refer to the study in Chapter 3 as an artefactual field experiment. The terminology has been updated throughout.

Chapter 4:

This chapter is the least polished of the three, and as such, there have been more substantial revisions than in the previous chapters. I appreciate all of the recommendations and have tried to incorporate as much of the feedback as possible. My co-authors and I plan on continuing to improve the article before submitting it to an academic journal. Here, I will not go point-by-point through the recommendations, but will summarize some of the more substantial changes that have been made:

- Per Professor Kovářík's recommendations, the theoretical section has been deleted. We agree that it adds little to the understanding of our results, and most likely confuses readers more than it illustrates our arguments.
- According to Professor Kovářík‘s recommendation, we have analyzed first-round contributions in the multi-level public good (MLPG) game, in addtion to averages. (Table 4.A.3)
- On page 113 we give a clearer explanation of the parameters in the MLPG game, giving the reader a better intuition as to why they were chosen.
- Professor Kovářík asks why we chose the "arbitrary-low" (AL) treatment (but not the AH treatment). This is now addressed on page 114: In short, the low-efficiency treatments capture in-group favoritism, and our aim was to add an extra comparison of this effect with the re-matched-low treatment.
- Professor Filer raises issue with the AL treatment as well, in that subjects spent less time in the lab before playing the MLPG. While I agree with this point, we any manipulation to adjust for time spent in the lab would likely introduce an additional confound. This is now addressed in footnote 6 on page 116.
- Professor Filer raises concerns about the sample and self-selection into the experiment. We now re-run the regressions with a set of control variables to, in part, address this concern. The results are reported in tables 4.A.1 and 4.A.2, and discussed on page 120.

References

Hamlin, J. Kiley, Karen Wynn and Paul Bloom. 2007. "Social evaluation by preverbal infants," *Nature*, 450(6288): 557-559.

Johnson, Noel, and Alexandra Mislin. 2011. "Trust games: A meta-analysis." *Journal of Economic Psychology* 32 (5): 865-889.