Shocks, Preferences, and Institutions

Experimental Evidence from Sub-Saharan Africa

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To my families.

Contents

CHAPTER 1
Introduction

1.1 Overview

Both preferences and institutions are central to economic theory (Simon, 1959; Acemoglu et al., 2005). Insofar as they cannot be taken as given, it is important to understand how they are formed, and how they "respond" to shocks. This thesis investigates the endogenous formation of preferences and institutions. It presents field-experimental evidence from Sub-Saharan Africa – specifically Uganda, Sierra Leone, and Ethiopia – gradually zooming out through different levels of responses to shocks. It starts by looking at the formation of individual preferences in utero and during childhood. Next, it explores the endogeneity of rational choice among adults. Finally, it looks at the cumulative outcome of these responses in terms of changes in local norms and informal institutions. Shocks are thought of in their broadest possible definition. Conflict is a shock, but so is the introduction of exogenously planned and implemented institutions, or the penetration of statutory law into predominantly customary settings.

While each chapter is envisioned as a self-standing contribution to economic literature, the crosscutting thread is equally crucial. Not always do endogenous responses to shocks fit existing economic theory. Rather, the evidence presented sometimes highlights unforeseen dynamics. It moreover strongly rejects the notion of passive acceptance of shocks; individuals and institutions "respond" to shifting circumstances through "rational" – although not necessarily conscious – behavioral changes. These findings contribute to the understanding of the micro-foundations of preferences and institutions, and emphasize the need to continuously underpin theoretical predictions with empirical evidence.

1.2 Preferences

'Where do preferences come from? Do they drop from the skies? Are they innate in the mind? No, they come from social practice. [...] In their social practice men engage in various kinds of struggle, gaining rich experience both from their successes and their failures' (Mao, 1966: p.1). This loosely translated quotation from Mao Zedong involuntarily makes him an early proponent of the "endogeneity of preferences"—the notion that individual preferences may not be as hardwired as economic welfare theory assumes. Actually, despite Stigler and Becker's (1977) arguments in favor of the opposite, economists have long acknowledged that shocks and experience may alter individual preferences (e.g. Marshall, 1920; Friedman, 1962). 'The question is whether or not economists should concern themselves with such changes' (Albert and Hahnel, 1990: p.76).

The answer may lie in the very events that preceded Chairman Mao's thoughts on the subject. The agrarian reforms implemented during the Great Leap Forward caused the death of over 30 million Chinese people between 1958 and 1961 (Ashton et al., 1984); such "struggle" may have affected the preferences of those suffering from it. In fact, an increasing body of literature investigates behavioral responses to individual life experiences. They show that shocks such as conflicts (e.g. Voors et al., 2012) and natural disasters (e.g. Cassar et al., 2011) considerably alter individual risk, time and other-regarding preferences—fundamental drivers of consumption, saving and investment decisions. This in turn has strengthened the hypothesis that shocks impact the performance of economies far beyond their immediate consequences. If so, the traumatic experience of the Great Leap Forward may not only have altered individual preferences, but also the long-run growth trajectory of the whole Chinese economy. Understanding the relationship between shocks and individual preferences may therefore be central to studying the long-term dynamics of economic development.

What Mao Zedong's quotation does not acknowledge is that preferences are, to a certain extent, "innate to the mind". In fact, people acquire preferences not only through life experiences and learning, but also genetic inheritance. In the lab, monozygotic twins display consistently more similar trust and risk behavior in comparison with dizygotic twins—a sign that genetic variation accounts at least partially for differences in individual preferences (Cesarini et al., 2008; Zyphur et al., 2009). Preferences are thus transmitted to the progeny through at least three channels: deliberate efforts by others to shape behavior and beliefs, independent learning from life experience, and genetics—the so called gene-culture coevolution (Bowles, 1998, 2006; Henrich, 2004; Dohmen et al., 2011). This suggests that economic development is affected by traits selected and transmitted across generations over the very long run (see Spolaore and Wacziarg, 2013), providing a 'solution to the puzzle of strong reciprocity and large-scale human cooperation' (Fehr and Fischbacher, 2003: p. 789).

At the other end of the time spectrum – the very short run – behavioral economists have looked at how the brain generates decisions. Neuroscience is becoming ever more complementary to economic theory in understanding human decision making (Camerer et al., 2005). Studies using neuroimaging and hormonal manipulation are increasingly supporting the neuroeconomic foundations of trust and social preferences (see Fehr et al., 2005). Variations in hormone levels have been found to influence a wide range of behaviors (see Mehta and Josephs, 2010), including fairness in ultimatum bargaining (e.g. Eisenegger et al., 2010), trust (e.g. Kosfeld et al., 2005), and parochial altruism in public good games (e.g. De Dreu et al., 2010). Moreover, sudden maternal hormonal variations are known to interact with the fetus during early development, triggering epigenetic modifications that may shape the child's brain evolution and behavior (Dörner et al., 2001; Keverne and Curley, 2008). If these prenatal effects extend to the domain of preferences, a fourth channel of preference transmission may be at play.

The epigenetic channel lies at the intersection of "nature" and "nurture". Experiencing a shock during pregnancy will not result in a modification of the genetic imprint of the fetus, but may alter the "gene expression". In other words, identical genetic information may produce very different outcomes. This epigenetic dimension is widely recognized within biological and medical literature (e.g. Cavalli-Sforza and Feldman, 1981; Jablonka and Lamb, 2014). Nevertheless, it has only recently come under scrutiny by economists (see Cunha and Heckman, 2007; Spolaore and Wacziarg, 2013). In the lab, markers of *in utero* hormone exposure have been found to correlate in different ways with altruism, cooperation, and risk preferences (Garbarino et al., 2010; Buser, 2012; Brañas-Garza et al., 2013). Chapter 2 in this thesis explores this channel by looking at prenatal hormonal shocks, and how they affect contributions to a public good among children born during a violent conflict.

Conflicts affect individuals and societies along multiple dimensions. 'Humans regulate intergroup conflict through parochial altruism; they self-sacrifice to contribute to in-group welfare and to aggress against competing out-groups' (De Dreu et al., 2010: p. 1408). Parochial altruism has likely developed as result of coevolutionary pressures that favored both in-group cooperation and out-group antagonism (Bernhard et al., 2006; Choi and Bowles, 2007; Fehr et al., 2008). Coevolutionary economic models are concerned with changes happening over the very long run, but typically recognize that parochial altruism may also be affected by individual experience (Choi and Bowles, 2007). In fact, several studies have looked at the effect of war exposure on in-group altruism and egalitarianism (e.g. Bellows and Miguel, 2009; Blattman, 2009; Voors et al., 2012; Gilligan et al., 2014); fewer at out-group aggressiveness (e.g. Miguel et al., 2011; Bauer et al., 2014). Arguably, however, spite and aggressiveness are but one aspect of increased antagonism; greater willingness to compete may well be the other side of the coin. Chapter 3 looks at both aggressiveness and competitiveness towards in- and out-groups, and how these are influenced by individual exposure to violent conflict during childhood.

Competitiveness is a keystone to the economic problem. Competitive markets, populated by traders willing to compete over finite resources to satisfy "infinite wants", are a fundamental prerequisite to reach competitive equilibria over prices and quantities—and Pareto efficient allocations of resources (Walras, 1889; Marshall, 1920). This however implies that traders maximize a certain utility function. Economic theory typically assumes that preferences are stable at least in the very short run, and that individual choices satisfy the axioms of revealed preference theory (Samuelson, 1938). More often than not, however, empirical studies have shown that people exhibit "irrational" violations of these axioms (e.g. Sippel, 1997; Mattei, 2000; Harbaugh et al., 2001). Chapter 4 tests the notion that people may endogenously "learn" to behave more "rationally", studying how rational choice is affected by the exogenous exposure to an institutional setting that mimics a competitive market.

1.3 Institutions

Why did Mao Zedong initiate the purges of the Cultural Revolution, only a few years after the failures of the Great Leap Forward? An increasing number of scholars would argue that both these disastrous policies – and the human and socio-economic consequences they entail – are the result of "bad institutions" (North, 1990; Platteau, 2000; Acemoglu et al., 2005). Institutions are humanly devised incentive frameworks that shape social, political and economic interactions, determining the choices individuals make, and affecting the performance of economies over time (North, 1990, 1991). They comprise both the "institutional environment" – formal laws and rules, informal conventions, social norms and beliefs – and the "institutional arrangements"—governance structures and organizational modes that define the actual "play of the game" within the institutional environment (Williamson, 2000). In the words of Acemoglu et al., 'bad institutions are [...] kept in place, not for the benefit of society as a whole, but for the benefit of the ruling elite' (2005: p.407).

Like preferences, institutions affect individual decisions about savings, investments, and consumption. Institutional features like security over property rights are positively correlated to economic performance (Hall and Jones, 1999; Acemoglu et al., 2001). Also, Dell (2010) finds that "bad" historical institutions such as forced labor have persistent effects on contemporary underdevelopment, over two centuries after being abolished. More generally, in any "social dilemma" situation – where individual interests conflict with the maximization of social welfare – institutions rewarding cooperators and/or sanctioning free-riders are essential to sustain high levels of cooperation (Kosfeld et al., 2009; Sutter et al., 2010). The quality of formal and informal institutions is thus widely believed to be a fundamental determinant of economic development (Acemoglu et al., 2005), if not the primary one (Rodrik et al., 2004). At the same time, much remains to be understood about their formation and functioning.

In recent years, the interaction between formal and informal institutions has stimulated a wealth of literature. Formal institutions are typically treated as exogenous constraints, while informal institutions are modeled as endogenous self-enforcing rules (Greif, 1993; Aoki, 2001a, 2001b). Within this framework, economic theory predicts a certain degree of interdependence between formal and informal institutions, viewing them either as complements or as substitutes. Some scholars argue that formal constraints facilitate the self-enforcement of non-contractible dimensions (Lazzarini et al., 2004), increasing the effectiveness of informal rules by lowering information, monitoring, and enforcement costs (North, 1990). Others that formal institutions 'do not produce trust but instead are a functional substitute for it' (Granovetter, 1985: p.489), or even that, as long as there are non-contractible dimensions, formal institutions may only "imperfectly" substitute for informal arrangements (Bernheim and Whinston, 1998). Chapter 5 studies whether formal institutions substitute informal arrangements, investigating the effect of introducing a formal insurance on the within-village dynamics of social capital and cooperation.

Those who challenge existing laws and norms are typically subject to some sort of punishment. Formal legal institutions oversee the compliance to formal constraints, such as laws, while informal ones preside over norms and customs. As a result of this duality, 'virtually every society is legally plural' (Merry, 1988: p.869). The punishment of non-compliers is thus often regulated through competing and sometimes overlapping institutional arrangements. The penetration of formal legal institutions, however, is far from homogeneous. Many regions worldwide remain effectively excluded from its direct influence, especially relatively poorer rural areas. Sandefur and Siddiqi (2013) suggest that improving access to formal law may result in direct gains for those disadvantaged by informal institutions. Yet, if formal legal institutions become accessible and competitive, indirect changes in customary outcomes may be even more salient (see Aldashev et al., 2012a, 2012b). Chapter 6 explores the effect on customary arbitration outcomes of increased "competition" by formal law.

1.4 Objectives

Preferences are endogenous to the institutional environment and arrangements (Bowles et al., 2003). On the other hand, institutions emerge as a result of cumulative individual choices, and are continually reshaped by changing beliefs and preferences (Williamson, 2000). Shocks may thus have three orders of effects on economic development: directly, though changes in physical and human capital; indirectly through changes in preferences and institutions; and through the changes that these may induce in each other (see Palacios-Huerta and Santos, 2004). The overarching objective of this thesis is to identify behavioral responses to exogenous shocks at the individual and community levels, contributing to the understanding of the endogenous formation of preferences and institutions—and the dynamics of economic development.

The chapters separately address the following research questions:

Chapter 2: Does prenatal trauma affect individual preferences for cooperation?
Chapter 3: Does conflict exposure alter the willingness to compete?
Chapter 4: Does market experience promote rational choice?
Chapter 5: Does formal insurance crowd-out social capital?
Chapter 6: Does (the threat of) formal law affect customary legal outcomes?

The individual preferences discussed in Chapters 2 and 3 are shaped by existing norms, customs, and laws, but at the same time represent the very building blocks of the latter. Similarly, the institutional changes discussed in Chapter 5 and 6 are underpinned by individual choices. Chapter 4 embodies the juncture between these chapters, as it investigates individual behavioral changes induced by an institutional shock. Hence, it should not come as a surprise that the strongest common thread across the case studies is the focus on individual decision making (see Williamson, 2008). The next section outlines the methodological similarities across chapters, discussing the advantages and pitfalls of the field experimental approach.

1.5 Methodology

1.5.1 Field experiments

Theoretical propositions are the backbone of economic science. Yet, after modeling a general hypothesis scholars must illustrate its functioning, and test the underlying causal mechanisms, through empirical case studies. At the same time, case studies 'enable the analyst to examine off-path behavior' (Alston, 2008: p.120), upon which more solid theoretical foundations can be developed (Becker, 1993). Traditionally, micro-economic data is gathered through questionnaires and other forms of field data. These are then analyzed quantitatively – through econometric and statistical inference – to identify patterns and relationships. In addition, economists may use incentive compatible laboratory experiments, randomizing "treatment" and "control" groups, to improve the identification of causal mechanisms (see Wilde, 1981; Smith, 1982).

To bridge field and lab-generated data, experimental economists have increasingly turned to "field experiments" (see Harrison and List, 2004; List, 2007; Gerber and Green, 2012). In essence, field experiments can be classified within three broad categories (List, 2014). Artefactual field experiments – also known as lab-in-field experiments – mimic experiments in the lab, except for drawing participants from the "field" of interest (e.g. Binswanger, 1980; Fehr and List, 2004; Gneezy et al., 2009). Framed field experiments add the contextualization of the experimental setting—in terms of the tasks, commodities, or information sets used by participants (e.g. Duflo and Saez, 2003; Chattopadhyay and Duflo, 2004; Gneezy and Rustichini, 2004). Finally, natural field experiments take place in the very environment where participants normally undertake a certain task, and subjects may be unaware that they are taking part in an experiment (e.g. Thaler and Benartzi, 2004; Gneezy and List, 2006; DellaVigna et al., 2012). Field experiments are becoming a popular tool in economic research. Not only have they proven useful in assessing the effectiveness of development interventions, but – when informed by economic theory – may help to study relationships that can hardly be gauged through observational data (Banerjee and Duflo, 2008).

This thesis roughly covers the entire spectrum of field experiment categories. All chapters in the main body make use of one or more decontextualized artefactual field experiments, such as public goods games, ultimatum games, or risk games. Chapter 2, 4, and 6 also expose participants to contextualized dilemmas, such as dispute arbitration by a local customary judge, or the trade of sesame produced by local farmers. Finally, Chapter 2, 3, and 5 make use of quasi-natural experimental settings, such as the varying exposure to violent conflict, a street football tournament, or the introduction of a formal health insurance in some parishes prior to others. The case studies often stem from broader research projects in the areas of study. The experiments conducted in Uganda, for instance, are part of a broader impact evaluation effort of development initiatives that are co-financed by the Dutch Ministry of Foreign Affairs.

1.5.2 Ethical dimension

The improvement of identification and data collection methodologies – and increasing accessibility of relatively remote areas of the world – has permitted an unprecedented surge in both the quantity and quality of information gathered from developing countries. In particular, the Sub-Saharan African context offers a wealth of opportunities to understand the dynamics of poverty and development—'much of the economics that really matters' (Schultz, 1980: p.639). Yet, using experimental methods in the field raises several ethical concern, especially because it involves "manipulating" the setting and opportunities available to subjects (see Teele, 2014). Interventions that randomly assign one group to a treatment, and take another group as control, raise immediate worries with respect to fairness, as well as about the extent of the "mandate" one has to do so. Similarly, artefactual field experiments that provide monetary incentives to some participants and not to others may have downstream consequences on community dynamics, even when played anonymously. In other words, even though an experiment is intended to be entirely beneficent, it may bring about unforeseen risks for those involved. This issue is not unique to experimental settings. A notable non-experimental example is that of a gender-targeted microfinance program which unintentionally resulted in increased violence against participating women (Rahman, 1999). Yet, researchers' capacity to manipulate the "rules of the game" makes voluntary participation and informed consent even more essential.

On top of this, a more utilitarian argument is often omitted. In the words of Hicks, data collection has 'been most successful in those cases where it is possible to induce the people questioned to take some trouble over the answers; generally this means paying them to do so. [...] Poor people can be induced to take this trouble for a very small fee; to give the same inducement to richer people would be impossibly expensive' (1942: p.3). The lower the opportunity cost of participants, the cheaper it is for researchers to collect data—even more so when using experiments.

In the research we conducted for this thesis we carefully explained the risks related to participation to all involved subjects. We stressed the importance of anonymity, both to participants and those executing the experiment, clearly marking non-anonymous choices before these where taken. We required informed consent from participants, granting the freedom to voluntarily terminate interviews and experiments at any time.

The use of field experimental methodologies in developing countries can contribute immensely to target global food security and the eradication of poverty. Moreover, field experiments may help speed up policy change—as long as they do not divert resources destined to development interventions. A well-planned field experiment, for instance, may have identified the unintended consequences of the targeted microfinance example mentioned above both more rapidly and more precisely than the way it happened. Nonetheless, Sub-Saharan Africa should never become a giant testing field, and researchers should never underestimate the ethical dimension.

1.6 Outline

Chapters are organized as follows. Chapter 2 looks at the effect of prenatal trauma on the cooperation of those born during the Lord's Resistance Army insurgency in northern Uganda. Chapter 3 investigates the effects of war exposure on the preferences for competition of youth in Sierra Leone, using the group dynamics generated by a local football tournament to separate in- and out-group behavior. Chapter 4 studies rational choice and its relationship to market exposure in rural Ethiopia, through a laboratory experiment involving local brokers and farmers. Chapter 5 explores the dynamics of social capital – proxied by individual public good contributions – in response to introducing a formal insurance scheme in southwestern Uganda. Chapter 6 studies the customary changes induced by the increased penetration of the rule of law, by looking at arbitration decisions made by real customary judges in Ethiopia over lab-in-field disputes. Chapter 7 concludes.

CHAPTER 2
Prenatal Trauma and Cooperation

Evidence from a Public Goods Game in Post-Conflict Uganda

Abstract

We look at the impact of prenatal trauma on the social preferences of children born during an armed conflict. We play a dichotomous one-shot public goods game in Northern Uganda with children born during a period of intense fighting and civilian victimization. To proxy for prenatal trauma we use the 2D:4D digit ratio—a marker of in utero hormone exposure negatively associated with high maternal distress during early fetal development. We find that a rise in our marker of prenatal hormonal shock robustly reduces the child's probability of contribution to the public good. Our findings are consistent with literature on the fetal origins of preferences. If prenatal trauma affects next generation's taste for cooperation, violent conflict may have farther reaching socio-economic consequences than previously thought.

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2.1 Introduction

The nine months *in utero* may well be the most critical time in a person's life (Almond and Currie, 2011). During pregnancy, suffering from severe trauma and stress alters the hormone exposure of the child, triggering epigenetic processes that may shape brain evolution and behavior (Dörner et al., 2001; Keverne and Curley, 2008). Later-life behavioral characteristics are in fact increasingly associated with "fetal origins". Different scientific disciplines have studied the correlation of *in utero* hormone exposure with sexual identity, personality traits, and even financial trading ability (Csathó et al., 2003; Luxen and Buunk, 2005; Coates et al., 2009). In the lab, economists have investigated its relationship to altruism, cooperation in public goods games, and risk preferences (Garbarino et al., 2010; Buser, 2012; Brañas-Garza et al., 2013). We bring these studies to the field, and look at the impact of prenatal trauma on the social preferences of children born during an armed conflict. We shed light on an alternative, epigenetic mechanism of preference transmission—beyond the standard nature-nurture debate. If prenatal trauma affects next generation's taste for cooperation, violent conflict may have farther reaching socio-economic consequences than previously thought. It may affect regional long-run development trajectories and post-conflict recovery across generations, even if the episodes of violence are limited in time.

We play a dichotomous one-shot public goods game with 440 children born in Pader district in Northern Uganda during the 1998-2006 period of intense fighting between government forces and the Lord's Resistance Army (LRA). Simultaneously, we conduct an extensive socio-economic questionnaire including war exposure, post-traumatic stress disorder (PTSD) symptoms, and a closely related public goods game, with their main caregiver. To proxy for prenatal trauma, we use the 2D:4D digit ratio—a marker of *in utero* hormone exposure (Manning et al., 2003; Lutchmaya et al., 2004; Zheng and Cohn, 2011). The 2D:4D digit ratio is established during early fetal development, and remains relatively stable

throughout life. It measures the relative length of the index finger with respect to the ring finger, and is negatively related to the fetal testosterone to estradiol ratio (T:E2) (Manning et al., 2003; Lutchmaya et al., 2004). High levels of prenatal distress are associated with higher offspring T:E2 ratios (Vom Saal et al., 1990), and lower 2D:4D ratios (Lilley et al., 2010).

We find that a one standard deviation rise in our marker of prenatal distress reduces the child's probability of contribution to the public good by about 8 percentage points (or 20% of the mean prevalence). We control for alternative mechanisms such as early life deprivation, caregiver public good contributions, and war exposure after birth. We show that adult PTSD predicts lower digit ratios in children, and that the average digit ratio of biological mothers and siblings born before the war is significantly higher. We discuss the sensitivity to exogeneity assumptions, and the likelihood that our findings are driven by unobserved characteristics.

The rest of the paper is organized as follows. Section 2.2 reviews the literature on the subject. Section 2.3 briefly describes the context and background. Section 2.4 outlines the experimental design. Section 2.5 discusses the empirical strategy and identification. Section 2.6 illustrates the results, and Section 2.7 concludes.

2.2 Conflict, trauma, and preferences

This paper explores the fetal origins of preferences for cooperation in a post-conflict setting. It builds upon three strands of literature—that on the role of violent conflict in shaping preferences, that on the consequences of trauma *in utero*, and that on the relationship between prenatal hormone exposure and economic behavior.

The relationship between violent conflict and the functioning of societies has been at the forefront of economic debate for years. War violence persistently impacts health, education, and poverty (Ghobarah et al., 2003; Chamarbagwala and Morán, 2011; Gates et al., 2012), but also affects

individual preferences and behavior. It has been found to increase community participation and political engagement (Bellows and Miguel, 2009; Blattman, 2009), out-group aggressiveness and competitiveness (Miguel et al., 2011; Cecchi et al., 2014), as well as risk propensity, and discount rates (Voors et al., 2012). Individuals exposed to inter-community violence display more altruistic behavior, higher public good contributions, and trust within their networks (Voors et al., 2012; Gilligan et al., 2014). Intra-community violence, instead, decreases social cohesion and trust, and increases sentiments of group identity (Cassar et al., 2013; Rohner et al., 2013). These studies focus on the postnatal impact of violent conflict exposure. The effect of prenatal exposure on preferences may follow distinct pathways, perhaps more associable to those related to extreme prenatal stress; these have yet to be investigated in a post-conflict setting.

Several studies have looked at the physical and psychological consequences of stress and traumatic events *in utero*. Maternal prenatal anxiety may suppress the development of a functioning immune system, increasing the incidence of several health complications in infants (Stott, 1973). Moreover, exposure to violence during pregnancy has been found to deteriorate birth outcomes, typically in terms of birth weight, fetal growth, and preterm delivery (Mancuso et al., 2004; Lauderdale, 2006; Camacho, 2007; Koppensteiner and Manacorda, 2013; Black et al., 2014; Quintana-Domeque and Rodenas, 2014). Independent positive shocks, such as a rise in cocoa price at birth – expected to reduce financial distress in cocoa-producing areas of Ghana with respect to other regions – decrease the likelihood of mental distress during adulthood (Adhvaryu et al., 2014). Prenatal negative shocks, such as extreme weather or military invasions, are instead associated with higher prevalence of schizophrenia and autism (van Os and Selten, 1998; Walder et al., 2014).

The prolonged emotional disturbance and distress induced by conflict increases the likelihood of trauma, mental health problems, and PTSD (de Jong, 2002; Lopes Cardozo et al., 2004; Miller et al., 2008). The higher the

post-traumatic hormonal release, the greater the chance that subjects develop PTSD (Delahanty et al., 2000). PTSD, in turn, increases the likelihood of persistent hormonal imbalance, particularly with respect to cortisol—the stress hormone in humans (de Kloet et al., 2008; Song et al., 2008; Steudte et al., 2011). During pregnancy, maternal stress is transmitted to the fetus through hormonal releases (Mancuso et al., 2004; Weinstock, 2008). Prenatally stressed rodents exhibit higher concentrations of serum testosterone and higher testosterone to estradiol ratios (Ward and Weisz, 1980; Vom Saal et al., 1990). In turn, the fetal testosterone to estradiol ratio is negatively related to the 2D:4D digit ratio (Manning et al., 2003; Lutchmaya et al., 2004). Connecting the dots, Lilley et al. (2010) show that high levels of maternal corticosterone, the rodent equivalent to cortisol, are associated with lower offspring 2D:4D digit ratio. They suggest that the latter may be a useful phenotypic indicator of maternal distress during early fetal development.

The 2D:4D digit ratio measures the relative length of the index finger with respect to the ring finger. It is established through changes in gene expression which take place without a change in the DNA sequence – known as epigenetic modifications (Jirtle and Skinner, 2007) – and is widely accepted as a noninvasive marker and 'lifelong signature of prenatal hormonal exposure' (Zheng and Cohn, 2011: p. 16289). During early fetal development, increased androgen hormones (e.g. testosterone) or the inactivation of the estrogen receptor (ER-α) stimulate the ring finger growth, which leads to a lower 2D:4D ratio. On the other hand, the addition of estrogen (e.g. estradiol) or the inactivation of the androgen receptor (AR) decrease the ring finger growth, resulting in a higher 2D:4D ratio (Lutchmaya et al., 2004; Zheng and Cohn, 2011).

In the lab, Garbarino et al. (2010) show that a low 2D:4D is associated with greater risk-taking. This is confirmed by evidence that low digit ratio MBA students self-select more into risky finance careers (Sapienza et al., 2009), and that the financial ability among male high-frequency traders is

negatively related to their 2D:4D ratio (Coates et al., 2009). Its relationship to other-regarding preferences is relatively less studied, especially on non-experimental populations. Among undergraduate students, Brañas-Garza et al. (2013) find a non-monotonic impact of the digit ratio on altruism. Also, self-assessed low digit ratios (2D<4D) predict lower giving in ultimatum, trust, and public good games (Buser, 2012). We use the 2D:4D digit ratio as a marker of maternal distress, to explore how prenatal trauma induced by violent conflict may reflect on the preferences for cooperation of the next generation.

2.3 Context and background

In the last 25 years Uganda achieved high and steady GDP growth rates, averaging about 6.7% per year (World Bank, 2014). This, however, has been more of an exception than the rule. Since independence in 1962, Uganda has witnessed few short periods of peace and relative prosperity, and many long periods of violence and constitutional suspension. In fact, even while the country's overall growth rate was faster than that of many of its neighbors, the North was enduring the last of a long series of conflicts: Joseph Kony's LRA insurgency (1987-2006).

Violence has been escalating recurrently in Uganda since 1971, when Idi Amin took power from the discredited President Milton Obote.[1] Amin ruled the country until the 1979 Uganda-Tanzania War led to his ousting. Obote's comeback triggered instead the Ugandan Bush War, against the southern rebels of the National Resistance Army (NRA) headed by the current President of Uganda, Yoweri Museveni. Obote lost power for the second time in 1985, short before the NRA faction assaulted Kampala—gaining the power it still holds today (Finnström, 2008).

[1] In 1966 Obote was implicated in a corruption scandal together with the then deputy commander of armed forces, Idi Amin. He responded by suspending the constitution.

As the balance of power shifted southwards again, rebel movements in the North gathered under the flag of the LRA (Doom and Vlassenroot, 1999). Limited in numbers and resources, the LRA resorted to pillaging villages and abducting local youth: an estimated 60,000 to 80,000 people were abducted across two decades (Annan et al., 2006; Pham et al., 2007). Throughout the years, weak government responses and the setup of an Acholi self-defense militia invigorated the LRA, which scaled up operations to discipline the local population.[2] Yet, civilian victimization in these years was not only the result of LRA violence, and abuse from government troops was not uncommon (Finnström, 2008; Dolan, 2009). The widespread killing and mutilation of Acholi civilians escalated dramatically after 1996, especially in the Acholi districts of Gulu, Kitgum, and Pader. In these districts, more than a quarter of the males aged 10 to 25 at the end of the war had been abducted for at least two weeks, and only 80% of them returned from captivity (Blattman and Annan, 2010).

Pader district was particularly hit from 1998 onwards, when LRA operations gained momentum and moved southwards (Figure 1 and Appendix Figure A1). Civilian fatalities peaked in 2002, after the start of "Operation Iron Fist" against rebel bases in South Sudan set off a bloody reaction by LRA forces. A truce between the LRA and the government was signed in 2006, and fighting in Uganda has been sporadic ever since.[3]

[2] Between 1994 and 2002, in response to Uganda's support for the rebels in South Sudan, the Sudanese government provided the LRA with logistic support and military equipment.
[3] Since then, the LRA has not been disarmed nor demilitarized, and has been active in Democratic Republic of the Congo, the Central African Republic and South Sudan.

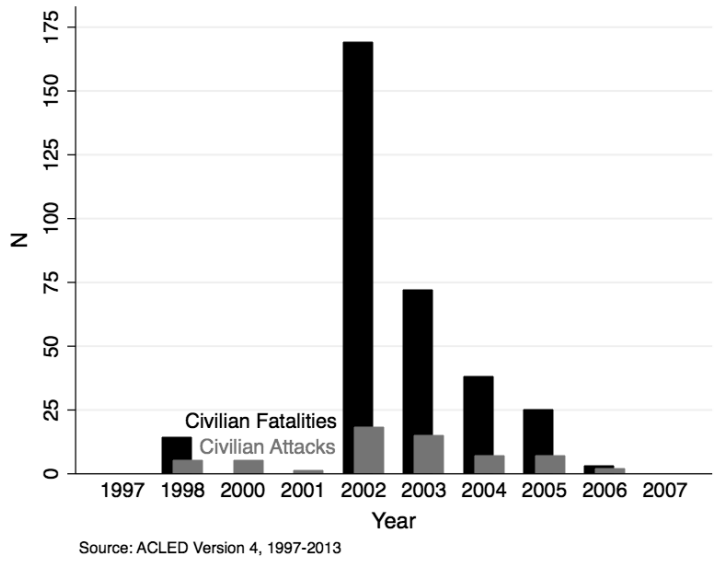

Figure 1: Civilian targeting in Pader district

2.4 Experimental design

2.4.1 Sample and setting

Our sample includes 440 children and their caregivers from Pader district in Northern Uganda. In November 2012, we visited 42 primary schools in the district, and randomly selected 12 students from a list of pupils enlisted at the beginning of the year.[4] The descriptive statistics for children and caregivers are presented in Table 1.1 and 1.2 respectively. On average the children are 11 years old, half of them are female, and their body mass and height are respectively 0.4 and 1 standard deviation below the mean for their age (de Onis et al., 2007). Caregivers are instead 42 years of age, 53% are female, and 79% did not complete primary education.[5] Almost the

[4] The randomization was stratified according to grade: 4 students were selected from grade 2, 4 from grade 4, and 4 from grade 6.Out of a total of 504, 64 students born prior to the intensification of violence in the area, around 1998, were excluded from the analysis.

[5] 188 caregivers are biological mothers. Another 161 are biological fathers, while the remaining 91 were uncles/aunts, grandparents, siblings or other relatives (in descending order of prevalence). On average, caregivers had taken care of the child for 92.8% of the child's life. Only 10 are not blood-related to the child (see Appendix Table A1).

Table 1.1: Descriptive statistics (Children)

Variable	Observations	Mean	Std. Dev.	Min.	Max.
2D:4D	440	0.94	0.04	0.73	1.11
Public good contribution	440	0.41	0.49	0	1
Age	440	11.1	2.2	6	14
Female	440	0.50	0.50	0	1
School grade	440	3.6	1.49	2	6
Height-for-age	439	-0.43	1.38	-4.82	5.8
BMI-for-age	438	-1.04	1.09	-5.98	2.01
IQ-for-age	440	94.98	14.99	79	162
Time preferences	440	0.25	0.44	0	1
Risk preferences	440	0.64	0.33	0	1
War exposure	436	0.57	0.27	0	1

Notes: See Appendix for variable definitions.

Table 1.2: Descriptive statistics (Caregivers)

Variable	Observations	Mean	Std. Dev.	Min.	Max.
Public good contribution	440	0.51	0.50	0	1
Age	435	42.1	12.1	19	92
Female	440	0.53	0.50	0	1
Education level	440	0.84	0.80	0	4
Risk preferences	440	0.14	0.24	0	1
War exposure	436	0.75	0.17	0.17	1
PTSD (dummy)	440	0.40	0.49	0	1
PTSD (factor)	440	0.00	1.00	-1.92	2.34
Christian	440	0.99	0.10	0	1
Acholi	440	0.97	0.16	0	1
Household size	440	8.08	2.99	2	25
Assets index	440	0.00	1.00	-0.56	5.62

Notes: See Appendix for variable definitions.

entire sample is ethnically Acholi and Christian by religion. Households are typically composed of 8 people. To control for additional potential confounds, we also collect information about the children's cognitive ability (IQ) through standard Raven's progressive matrices (Kaplan and Saccuzzo, 2012), time and risk preferences (Voors et al., 2012), as well as the household size and relative asset wealth (Sahn and Stifel, 2003).

2.4.2 The digit ratio

The index and ring finger lengths were measured on the ventral surface of the right hand from the midpoint of the basal crease to the tip of the digit. [6] Given the contextual constraints and instruments available, measurement precision does not exceed 1 mm, resulting in an error of ±3.3% at the mean of our estimations.[7] While this is still far from the precision obtained in the lab (see Voracek et al., 2007), it represents a significant improvement with respect to Buser (2012)—whose subjects self-report if they have a shorter, equal, or longer ring than index finger. Independent raters measured the digit lengths unaware of their scientific significance; errors should therefore result in unbiased random noise.[8]

2.4.3 Public goods game

We played a one-shot dichotomous public goods game with both the children and the caregivers. In each school, children played in randomly assigned groups of 6, and anonymously decided whether to select a "private card" or a "group card".[9] Children could in no way infer which other 5 participants belonged to their group (out of the 11 other children selected in that school). The private card allotted 3 candies to themselves and none to other unknown group members. The group card gave instead 1 candy to each group member including themselves (a graphical

[6] The 2D:4D ratio varies between ethnic groups (Manning et al., 2000). The average ratio measured in our sample was around 0.94.

[7] In a pilot 30 raters separately measured 35 right hands, revealing comparable margins.

[8] Approximately two years after the main data collection we re-measured the digit lengths for a sub-sample of 258 respondents. While the absolute length of the fingers had undoubtedly changed in the meanwhile, their ratio should remain relatively stable throughout lifetime. In line with expectations, the average error was ±3.7%. Results are not driven by systematic measurement error, and excluding measures with potentially greater error does not significantly alter the results (see Appendix Table A2).

[9] Contrarily to many public good games in which participants can choose their preferred contribution level, we opted for a dichotomous choice: respondents could either cooperate or not. While this reduces the nuances present in the experimental sample, we believe that it facilitated the decision making process, especially for the youngest.

representation of the two cards can be found in the Appendix, Figure A2). The joint surplus is therefore maximized when all participants choose the group card, such that each group member receives 6 candies. Nevertheless, free riders selecting the private card may obtain up to 8 candies. The Nash equilibrium is reached if everyone selects the private card, receiving 3 candies only. Caregivers played a very similar game, but played in groups of 12 instead of 6. The game was played in an isolated environment – typically their home – and caregivers where unaware of the identity of other participants. The private card was worth 4000 UGX, equivalent to approximately 1.5 USD; the group card was instead worth 500 UGX. The non-cooperative equilibrium thus yielded 4000 UGX each, joint maximization returned 6000 UGX each, and free riders could earn as much as 9500 UGX.[10] On average, 41% of the children and 51% of the caregivers opted for the cooperative option offered by the group card.

2.4.4 War exposure and trauma

We do not ask war-related questions to children. Instead, we use information on the individual war exposure of their caregivers, and weigh it against the war violence that happened after the year of birth of the child. We use an adapted version of the War Trauma Questionnaire (WTQ), excluding the questions about shelling and bombardment which are not relevant to our setting (Macksoud, 1992; Papageorgiou et al., 2000). This questionnaire provides information on 23 war related traumatic events that a person may have witnessed, rated through 'yes' or 'no' statements. We create a victimization index using the average of positive responses to these violence related questions (Bellows and Miguel, 2009).

[10] The variation in pay-outs between the child and caregiver versions of the game was determined during a pilot. We adjusted the relative values of the "private" and "group" cards to obtain relatively similar cooperation prevalence ratios across the two samples, with around half the sample opting for cooperation. Specifically, the number of candies assigned by the "private" card in the child version was dropped from 4 to 3 to increase the likelihood that children would select the "group" card.

On average, caregivers responded positively to 75% of the questions, with a minimum observed exposure of 17%.[11] To proxy the child's postnatal war exposure we weight the caregiver's victimization index by the portion of violence potentially witnessed by the child after birth. To this end, we take the fraction of total civilian fatalities that occurred in Pader district following the child's birth (see Figure 1) or, in alternative, the fraction of LRA-related violent conflict events after birth (see Appendix Figure A1).

We measure PTSD symptoms in adults using the civilian version of the PCL self-report checklist (Weathers et al., 1993).[12] We convert individual scores into a PTSD dummy, following the recommendations of the US Department of Veteran Affairs, and the Diagnostic and Statistical Manual of Mental Disorders, Fourth Edition (DSM4).[13] As additional robustness check, we perform a factor analysis to extrapolate a factor for "trauma". We find a robustly positive and significant relationship between war exposure and trauma in our adult sample, with a higher prevalence of PTSD for women. A one standard deviation increase in our victimization index increases the likelihood of PTSD by almost 10%, and scores on the trauma factor by 20% of a standard deviation (see Appendix Table A3).

[11] Unfortunately, 4 caregivers asked to terminate the module prior to its completion; any analysis including war exposure measures will be made on the remaining 436 respondents.

[12] This 17-item questionnaire has been found to have strong psychometric properties, high internal consistency, and high test-retest reliability (Blanchard et al., 1996; Ruggiero et al., 2006; Conybeare et al., 2012). Moreover, it is strongly correlated with alternative measures of PTSD such as the Mississippi, MMPI-2 Keane, IES, and CAPS scales (Weathers et al., 1993; Dobie et al., 2002; Freedy et al., 2010).

[13] In our setting we expect high rates of PTSD (Roberts et al., 2008; Pfeiffer and Elbert, 2011). We therefore take a conservatively high threshold for PTSD, at >66% of the maximum item score, to minimize the likelihood of false positives (Keen et al., 2008). We therefore find a lower PTSD prevalence (40%) than previous studies in the region that do not apply this correction. The DSM4 cut-off point requires at least 1 moderately positive answer in questions 1-5, 3 in 6-12, and 2 in 13-17. At the selected threshold, only 4 out of 440 caregivers do not meet this requirement. Our analysis is robust to their inclusion or exclusion from the PTSD count.

2.5 Empirical strategy

2.5.1 Main result

We hypothesize that maternal distress during pregnancy may reflect on the cooperation preferences of the offspring. The 2D:4D digit ratio is an indicator of maternal distress during early fetal development, negatively correlated to maternal corticosterone levels during pregnancy as well as to *in utero* testosterone to estradiol ratios. We use the negative standardized digit ratio as our (relative) measure of prenatal trauma:

$$DigitRatio_{is} = -\frac{2D{:}4D_{is} - \overline{2D{:}4D}}{\sigma} \quad (1)$$

where $2D{:}4D_{is}$ is the digit ratio of individual i; $\overline{2D{:}4D}$ is the mean digit ratio in the sample; σ is the standard error; and the negative sign produces a positive relationship between our proxy and actual prenatal trauma—for ease of interpretation.

We estimate a specification with only the prenatal trauma proxy as a regressor, and gradually include other variables to reach the following fully specified linear probability model:[14]

[14] Literature highlights several trade-offs between linear probability (LPM) and probit models. First, compared to a probit, the LPM does not estimate the structural parameters, but this paper is mostly concerned with marginal effects (intuitively interpretable with LPM). Second, LPM error terms are heteroskedastic by construct; we thus use cluster robust standard errors, which are heteroskedasticity-consistent. Finally, Horrace and Oaxaca (2006) show that the potential bias of LPM increases with the fraction of predicted probabilities that lie outside the (unconstrained) unit interval. In our main specification, the predicted probabilities lie between 0.003 and 0.815; we thus expect our estimations to be largely unbiased and consistent. In fact, marginal effects calculated through a probit very closely resemble those of our selected LPM (see Appendix Table A4, column 1).

$$Cooperation_{is} = \alpha + \beta DigitRatio_{is} + \gamma X'_{is} + \delta_s + \zeta Z'_{is} + \varepsilon_{is} \quad (2)$$

where $Cooperation_i$ is a dummy taking value of 1 if individual i played the group card in the public goods game; $DigitRatio_{is}$ is our measure of prenatal trauma; X'_{is} is a vector of child characteristics including age, gender, age × gender, education, and caregiver characteristics including age, gender, age × gender, education and ethnicity; δ_s represents spatial fixed effects at the sub-county level; and Z'_{is} is a vector of potentially endogenous covariates such as household size, assets, time preferences and risk preferences of child i. Standard errors are clustered for 42 villages. From Buser (2012), we expect $\beta < 0$.

2.5.2 Alternative mechanisms

Next, we investigate potential alternative mechanisms. First, the literature discussed in Section 2 predicts that prenatal stress may capture the effect of early life deprivation. Height, for instance, is an anthropometric indicator of early-life experiences comparable to longitudinal measures such as height and weight at birth (Currie and Vogl, 2013). Similarly, low birth weight is associated with later-life low BMI (Walker et al., 2002), and severe deprivation at an early stage has persistent effects on cognitive ability (Beckett et al., 2006; Figlio et al., 2014). Second, the preferences of children may be driven by those of their caregivers through environmental as well as genetic mechanisms (Dohmen et al., 2011). Third, Section 2 highlights the role of postnatal war exposure in shaping individual preferences. Bauer et al. (2014) find that experiencing war may affect social preferences even at a very young age. We control for such covariates through the following fully specified linear probability models:

$$Cooperation_{is} = \alpha + \beta DigitRatio_{is} + \gamma X'_{is} + \delta_s + \eta H_{is} + \theta BMI_{is} + \iota IQ_{is} + \varepsilon_{is} \quad (3)$$

$$Cooperation_{is} = \alpha + \beta DigitRatio_{is} + \gamma X'_{is} + \delta_s + \kappa CaregiverCoop_{is} + \lambda CaregiverRisk_{is} + \varepsilon_{is} \quad (4)$$

$$Cooperation_{is} = \alpha + \beta DigitRatio_{is} + \gamma X'_{is} + \delta_s + \mu PostWar_{is} + \varepsilon_{is} \quad (5)$$

where H_{is} is the height-for-age of child i, BMI_{is} is the body-mass-index-for-age of child i, and IQ_{is} is an age-standardized measure of cognitive ability; $CaregiverCoop_{is}$ is a dummy taking value of 1 if the caregiver of child i played the group card in the public goods game, and $CaregiverRisk_{is}$ is a measure of the caregiver's risk propensity; $PostWar_{is}$ is the measure of child postnatal war exposure discussed in section 4; and all other notations have the same meaning as in (2).

2.5.3 Causality and unobserved selection

We use the 2D:4D digit ratio as an indicator of maternal distress during early fetal development (Ward and Weisz, 1980; Vom Saal et al., 1990; Lilley et al., 2010). If prenatal traumatization results in smaller digit ratios, we should observe this pattern in our data. First, we test this using the following equation:

$$2D{:}4D_{is} = \beta_0 + \beta_1 CaregiverPTSD_{is} + \beta_2 X'_{is} + \delta_s + \varepsilon_{is} \quad (6)$$

where $CaregiverPTSD_{is}$ is a measure of the PTSD symptoms of the caregiver; and all other notations have the same meaning as in (2).

Next, we compare the digit ratio of (83) biological mothers to that of their offspring, and of their female offspring only (43). Moreover, we identify same-mother siblings born between 1990 and 1996, a relatively non-violent period in Pader district. We test the hypothesis that (42) siblings born prior to the intensification of war violence have a significantly higher digit ratio, and verify its robustness by looking at (26) same-sex siblings.

Finally, our results may be biased by unobserved selection. Following Blattman and Annan (2010), we explicitly model relaxations of unconfoundedness by applying the sensitivity analysis proposed by Imbens (2003) and further developed by Harada (2012). Unobservable covariates may induce bias only if sufficiently correlated to the assignment and outcome variables. We identify a contour representing the degree of partial correlation with prenatal trauma and cooperation required for a pseudo-unobservable to reduce the coefficient by ¼. For comparison, we also plot the partial correlations of five benchmark covariates.

2.6 Results

We test the effect of prenatal trauma on preferences for cooperation of 440 children born during the Lord's Resistance Army (LRA) insurgency, in Northern Uganda. We find that our marker of prenatal distress is negatively correlated with the child's probability of contribution to the public good (See Appendix Figure A3). Parametrically, one standard deviation drop in the digit ratio reduces the child's probability of contribution by about 8 percentage points (Table 2). At the mean prevalence (41%), this results approximately in a 20% lower likelihood of cooperation. The effect is robust to individual, caregiver, spatial and potentially endogenous controls (Table 2, columns 2-4).[15]

> ***Result 1:*** *Prenatal trauma reduces the taste for cooperation. One standard deviation drop in the digit ratio decreases the child's probability of contribution to the public good by 20%.*

[15] Results are robust to using a probit model; including village fixed effects or enumerator fixed effects; and to two-way clustering of standard errors for village and year of birth. See the Appendix Table A4 for details.

Table 2: Prenatal trauma reduces cooperation

	Public Good Contribution			
	(1)	(2)	(3)	(4)
Standardized digit ratio (-)	-0.078***	-0.083***	-0.084***	-0.085***
	(0.025)	(0.026)	(0.026)	(0.025)
Child controls	N	Y	Y	Y
Caregiver controls	N	Y	Y	Y
Sub-county fixed effects	N	N	Y	Y
Child risk and time preferences	N	N	N	Y
Household size and assets index	N	N	N	Y
Observations	440	435	435	435
R^2	0.024	0.042	0.045	0.047

Notes: Standard errors corrected for village level clustering (42) are in parentheses. Confidence: *** ↔ 99%, ** ↔ 95%, * ↔ 90%. Child controls: Age, Female, Age × Female, Grade; Caregiver controls: Age, Female, Age × Female, Education, Acholi. See Appendix Table A5 for the full list coefficients.

Next, we control for potential alternative mechanisms driving such effect. First, we look at the impact of early life deprivation. We find that height-for-age is positively associated with cooperation: one standard deviation increase in the height-for-age increases the likelihood of contribution to the public good by 6 percentage points. BMI-for-age and IQ-for-age do not enter significantly (Table 3, columns 1-2). Nonetheless, prenatal trauma remains significant: its coefficient is stable and robust to controlling for markers of early life deprivation (Table 3, columns 3-4).

Second, we study the intergenerational transmission of preferences by controlling for caregiver public good contributions and caregiver risk propensity. Notably, we find a strong relationship between the social preferences of the caregiver and the child, but not between the risk preferences of the caregiver and the social preferences of the child. Children are 16 percentage points more likely to contribute if their main caregiver contributes to the public good in a separate game (Table 4, columns 1-2). Prenatal trauma is not affected by these controls (Table 4, columns 3-4).[16]

[16] For a sub-sample of 154 parents we verify that the parents' digit ratio is not driving our result—i.e. the genetic component does not foreshadow the epigenetic effect We find no effect of parental digit ratios on the cooperation of children (see Appendix Table A6).

Table 3: Alternative mechanism 1: early life deprivation

	Public Good Contribution			
	(1)	(2)	(3)	(4)
Standardized digit ratio (-)			-0.078***	-0.081***
			(0.026)	(0.028)
Height-for-age	0.027*	0.060***	0.027*	0.056***
	(0.015)	(0.021)	(0.015)	(0.021)
BMI-for-age	-0.019	-0.014	-0.014	-0.007
	(0.025)	(0.025)	(0.025)	(0.026)
IQ-for-age	-0.010	-0.0012	-0.012	-0.005
	(0.022)	(0.022)	(0.022)	(0.023)
Child controls	N	Y	N	Y
Caregiver controls	N	Y	N	Y
Sub-county fixed effects	N	Y	N	Y
Observations	438	433	438	433
R^2	0.006	0.038	0.029	0.062

Notes: Standard errors corrected for village level clustering (42) are in parentheses. Confidence: *** ↔ 99%, ** ↔ 95%, * ↔ 90%. Child controls: Age, Female, Age × Female, Grade. Caregiver controls: Age, Female, Age × Female, Education, Acholi.

Table 4: Alternative mechanism 2: caregiver preferences

	Public Good Contribution			
	(1)	(2)	(3)	(4)
Standardized digit ratio (-)			-0.072***	-0.079***
			(0.026)	(0.026)
Caregiver cooperation	0.161***	0.162***	0.148***	0.150***
	(0.040)	(0.043)	(0.041)	(0.044)
Caregiver risk preferences	0.037	0.059	0.044	0.077
	(0.099)	(0.010)	(0.100)	(0.098)
Child controls	N	Y	N	Y
Caregiver controls	N	Y	N	Y
Sub-county fixed effects	N	Y	N	Y
Observations	440	435	440	435
R^2	0.027	0.047	0.047	0.070

Notes: Standard errors corrected for village level clustering (42) are in parentheses. Confidence: *** ↔ 99%, ** ↔ 95%, * ↔ 90%. Child controls: Age, Female, Age × Female, Grade. Caregiver controls: Age, Female, Age × Female, Education, Acholi.

Third, we look at the effect of postnatal conflict-related violence.[17] The children in our sample were at most 8 years of age at the end of hostilities,

[17] Here we use the fraction of civilian fatalities after birth to weight the caregiver's war exposure. Using the fraction of LRA-related violent events does not change the results.

but postnatal witnessing of conflict-related violence may still have affected their taste for cooperation. In our sample, however, postnatal war exposure does not significantly affect cooperation, and does not wash out the effect of prenatal trauma (Table 5).

> ***Result 2:*** *The relationship between prenatal trauma and cooperation is stable and robust to controlling for early life deprivation markers, caregiver preferences and postnatal war exposure.*

Next, we address the causality mechanism by regressing caregiver PTSD on child digit ratios and by investigating mother fixed effects. We find that stronger symptoms of PTSD in caregivers are associated with lower child 2D:4D digit ratios (Table 6).[18] While this is reassuring, it is by no means conclusive evidence. First, out of 440 caregivers, our sample comprises only 188 biological mothers. Second, we do not know for sure whether the caregiver's traumatization took place before or after the birth of the child. Third, we measure PTSD symptoms 6 years after the end of the war, and an average of 11 years after the birth of the child. While literature shows that war related PTSD symptoms in Northern Uganda have persisted for such a long time period (Roberts et al., 2008; Pfeiffer and Elbert, 2011), it is plausible that *post bellum* events may have caused the observed PTSD. We thus complement the evidence with a mother fixed effects analysis, which involved a separate step of data collection (Table 7). We compare mean digit ratios of a sub-sample of the children with that of their biological mother through a paired t-test (Table 7, rows 1 and 2). Similarly, we compare the digit ratio with that of same-mother siblings born prior to the intensification of violence in Pader (Table 7, rows 3 and 4). Children born during the conflict have significantly smaller digit ratios.

[18] Here we take the 4 observations that do not comply with the DSM4 requirements for clinical PTSD as negative. Including them as positive cases does not change the results. Also, the effect is quantitatively stable and robust to excluding caregivers that are not: blood-related, parents, mothers (although the latter result is statistically insignificant).

Table 5: Alternative mechanism 3: postnatal war exposure

	Public Good Contribution			
	(1)	(2)	(3)	(4)
Standardized digit ratio (-)			-0.076***	-0.082***
			(0.025)	(0.026)
Postnatal war exposure	0.122*	0.029	0.120	0.062
	(0.071)	(0.110)	(0.072)	(0.110)
Child controls	N	Y	N	Y
Caregiver controls	N	Y	N	Y
Sub-county fixed effects	N	Y	N	Y
Observations	436	431	436	431
R^2	0.005	0.020	0.027	0.045

Notes: Standard errors corrected for village level clustering (42) are in parentheses. Confidence: *** ↔ 99%, ** ↔ 95%, * ↔ 90%. Child controls: Age, Female, Age × Female, Grade. Caregiver controls: Age, Female, Age × Female, Education, Acholi.

Table 6: Caregiver trauma predicts lower digit ratios in children

	2D:4D			
	(1)	(2)	(3)	(4)
PTSD	-0.300***	-0.265**		
	(0.108)	(0.116)		
Trauma factor			-0.116**	-0.098*
			(0.050)	(0.055)
Child controls	N	Y	N	Y
Caregiver controls	N	Y	N	Y
Sub-county fixed effects	N	Y	N	Y
Observations	440	435	440	435
R^2	0.023	0.041	0.014	0.033

Notes: Standard errors corrected for village level clustering (42) are in parentheses. Confidence: *** ↔ 99%, ** ↔ 95%, * ↔ 90%. Child controls: Age, Female, Age × Female, Grade. Caregiver controls: Age, Female, Age × Female, Education, Acholi.

Table 7: Children born during the conflict exhibit significantly lower digit ratios

	Observations		2D:4D			
	Child	Control	Child	Cont.	Diff.	Std. Err
Biological mothers	83	83	0.941	0.956	-0.015***	0.005
Biological mothers (female child)	43	43	0.940	0.953	-0.013**	0.006
Same-mother sibling	42	42	0.935	0.948	-0.013*	0.008
Same-mother sibling (same sex)	26	26	0.941	0.950	-0.009	0.008

Notes: Confidence: *** ↔ 99%, ** ↔ 95%, * ↔ 90%.

> ***Result 3:*** *Caregiver trauma predicts lower digit ratios in children. Also, children that are born during the conflict exhibit significantly lower digit ratios than their biological mothers and same-mother siblings.*

As is true for most observational studies, we cannot control for the bias due to unobserved selection. A certain type of mothers – with a certain preference for cooperation or risk before the war, for instance – may have self-selected into or out of traumatic events during pregnancy. Similarly, we are not able to verify the role of the behavior of armed groups or the influence of pre-war unobservable household characteristics. Our causal interpretation of the results may suffer from the potential bias due to the omission of such unobservables. Following Blattman and Annan (2010), we provide a graphical benchmark of the sensibility of our results to exogeneity assumptions (Imbens, 2003; Harada, 2012). The curve in Figure 2 represents the locus of partial correlation points of a hypothetical pseudo-unobservable with our assignment and outcome variables, that would lead our estimated effect to be reduced by ¼. The selected contour is conservative,[19] and still yields a significant coefficient at the 5% level (t=2.16). Yet, the alternative mechanisms we identify and discuss in Section 5.2 lie far below the selected threshold. To rule out the effect of prenatal trauma, any unobserved covariate not considered in our analysis would require a partial correlation with both the treatment and the assignment well above the curve of Figure 2.

[19] Blattman and Annan (2010), for instance, plot a contour that decreases the effect of the assignment by ½.

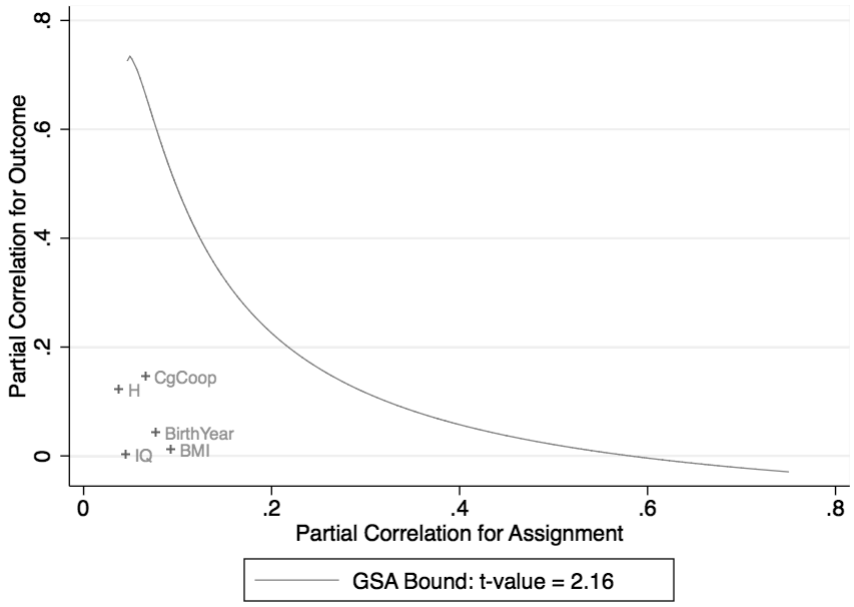

Figure 2: Sensitivity to the unconfoundedness assumption

2.7 Conclusions

'The womb may be more important than the home', wrote the late David J. Barker (1990: 1111) in his seminal work on the fetal origins of adult disease. Barker's hypothesis has spawned a large volume of literature exploring its economic implications. This study builds upon the fetal origins literature, and tests the hypothesis that prenatal events may not only alter later-life individual abilities and health trajectories (Almond and Currie, 2011), but also other-regarding preferences. In particular, we look at the impact of prenatal trauma on the social preferences of children born during an armed conflict. We play a dichotomous one-shot public goods game in Pader, a district in Northern Uganda, with children born during the 1998-2006 period of intense fighting between government forces and the Lord's Resistance Army (LRA). Our identification strategy exploits variations in the 2D:4D digit ratio—a marker of *in utero* hormone exposure negatively associated with high maternal distress during early fetal development. We find that a rise in our marker of prenatal distress robustly reduces the child's probability of contribution to the public good.

The estimated effect is quantitatively large, stable, and robust to controlling for alternative mechanisms such as early life deprivation, caregiver public good contribution preferences, and war exposure after birth.

Our results thus support three separate findings from previous studies. Firstly – and perhaps obviously – violent conflict exposure is traumatizing. Secondly, a mother's traumatization during pregnancy affects the hormonal balance of the fetus. Thirdly, *in utero* hormonal balance affects later-life other-regarding preferences. By analyzing these three relationships concurrently in a post-conflict context – where violence has differentially impacted large portions of the population – we find evidence supporting the entire causal chain: from conflict in one generation to economic behavior in the next one. Prenatal trauma triggers adaptive mechanisms that go far beyond the well-established relationship between postnatal war exposure and preferences. The socio-economic consequences of conflict may thus be reaching much further than previously thought, and the womb may well be far more crucial than David Barker ever imagined.

Appendix

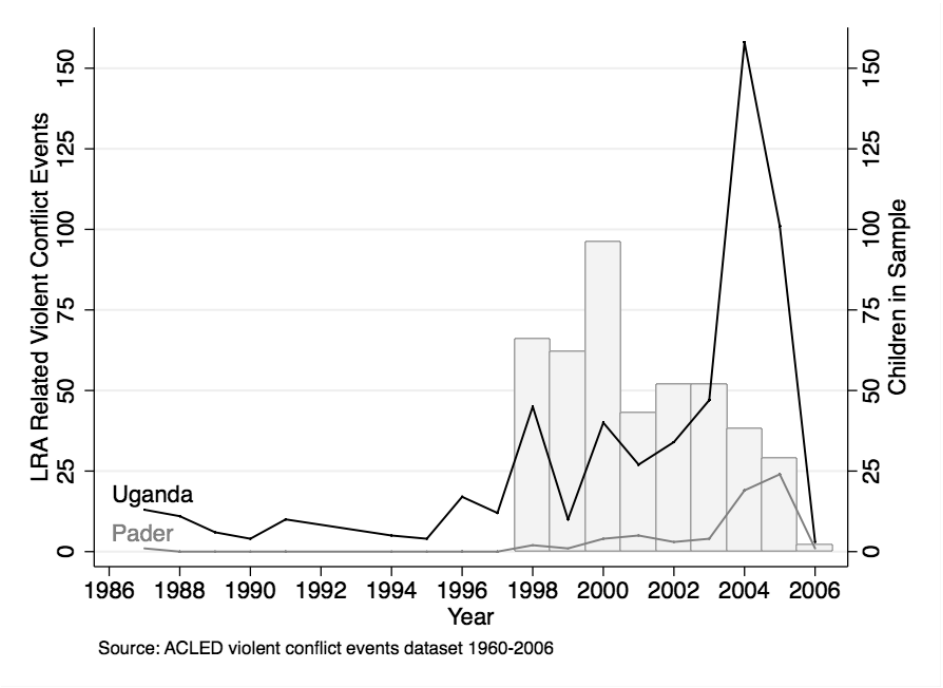

Figure A1: LRA related conflict events, Pader and Uganda 2006

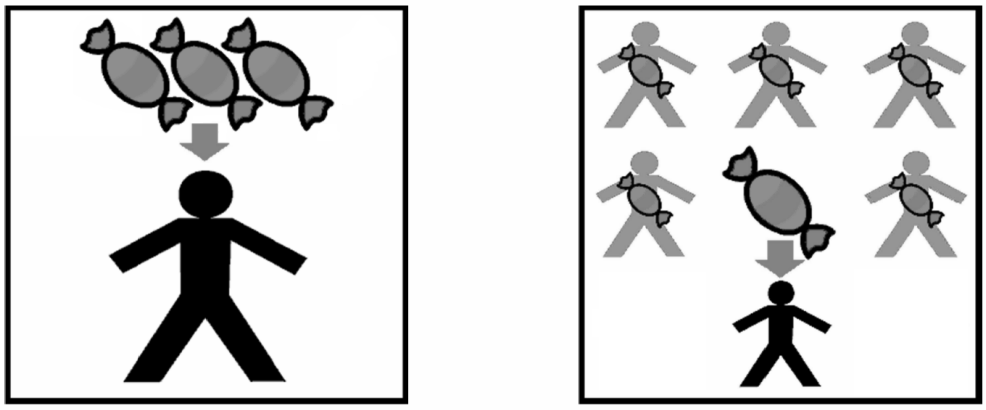

Figure A2: Private and group cards in the public goods game for children

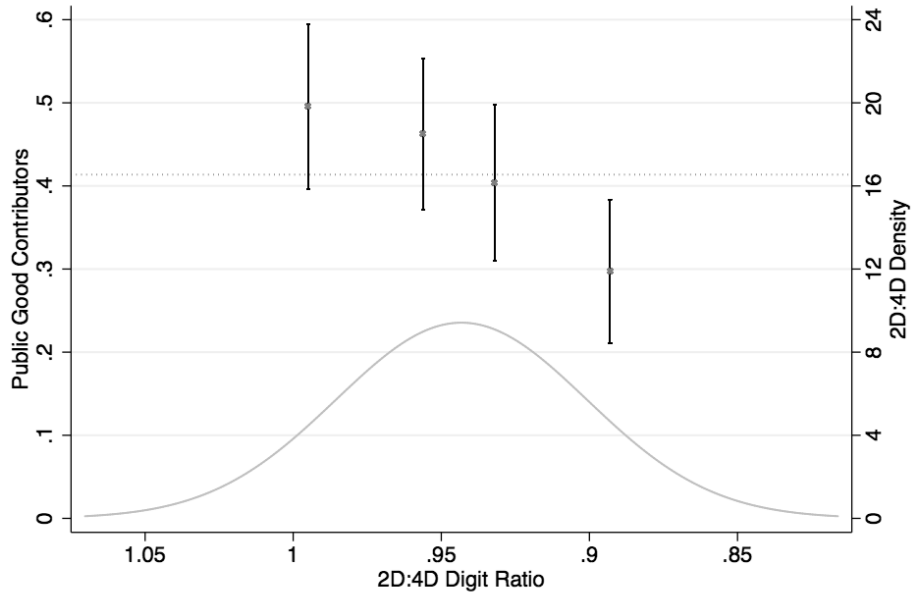

Figure A3: Digit ratio quartiles and prevalence of public good contributions

Table A1: Sensitivity of results to caregiver relationship to the child

	Public Good Contribution			
	All	Blood-related	Parents	Mothers
	(1)	(2)	(3)	(4)
Standardized digit ratio (-)	-0.084***	-0.081***	-0.080**	-0.091**
	(0.026)	(0.040)	(0.029)	(0.037)
Child controls	Y	Y	Y	Y
Caregiver controls	Y	Y	Y	Y
Sub-county fixed effects	Y	Y	Y	Y
Observations	435	425	345	185
R^2	0.045	0.045	0.043	0.083

Notes: Standard errors corrected for village level clustering (42) are in parentheses. Confidence: *** ↔ 99%, ** ↔ 95%, * ↔ 90%. Child controls: Age, Female, Age × Female, Grade; Caregiver controls: Age, Female (excluding column 4), Age × Female (excluding column 4), Education, Acholi.

Table A2: Sensitivity of results to the exclusion of potentially biased measures

	Public Good Contribution			
	All	$\Delta < 5\%$	$\Delta < 3\%$	$\Delta < 1\%$
	(1)	(2)	(3)	(4)
Standardized digit ratio (-)	-0.097***	-0.119***	-0.097*	-0.189
	(0.033)	(0.040)	(0.052)	(0.171)
Child controls	Y	Y	Y	Y
Caregiver controls	Y	Y	Y	Y
Sub-county fixed effects	Y	Y	Y	Y
Observations	258	186	129	39
R^2	0.045	0.054	0.040	0.153

Notes: Δ is the two-year inter-observer measurement difference. Standard errors corrected for village level clustering are in parentheses. Confidence: *** ↔ 99%, ** ↔ 95%, * ↔ 90%. Child controls: Age, Female, Age × Female, Grade; Caregiver controls: Age, Female, Age × Female, Education, Acholi.

Table A3: War exposure predicts trauma in adults

	PTSD		Trauma factor	
	(1)	(2)	(3)	(4)
Victimization index	0.082***	0.099***	0.182***	0.226***
	(0.023)	(0.023)	(0.053)	(0.051)
Female		0.278***		0.569***
		(0.053)		(0.108)
Caregiver controls	N	Y	N	Y
Sub-county fixed effects	N	Y	N	Y
Observations	436	431	436	431
R^2	0.029	0.098	0.034	0.127

Notes: Standard errors corrected for village level clustering (42) are in parentheses. Confidence: *** ↔ 99%, ** ↔ 95%, * ↔ 90%. Caregiver controls: Age, Female, Age × Female, Education, Acholi.

Table A4: Robustness of the main result to alternative specifications

	Public Good Contribution			
	Probit model	Village f.e.	Enumerator f.e.	Two-way clustering
	(1)	(2)	(3)	(4)
Standardized digit ratio (-)	-0.090***	-0.070***	-0.059**	-0.088***
	(0.029)	(0.024)	(0.025)	(0.029)
Child controls	Y	Y	Y	Y
Caregiver controls	Y	Y	Y	Y
Sub-county fixed effects	Y	N	Y	Y
Village level clustered s.e.	42	N	42	42
Year of birth clustered s.e.	N	N	N	9
Observations	435	429	435	435
R^2	0.035	0.185	0.181	0.032

Notes: Confidence: *** ↔ 99%, ** ↔ 95%, * ↔ 90%. Child controls: Age, Female, Age × Female, Grade; Caregiver controls: Age, Female, Age × Female, Education, Acholi.

Table A5: Prenatal trauma reduces cooperation (all coefficients)

	Public Good Contribution			
	(1)	(2)	(3)	(4)
Standardized digit ratio (-)	-0.078***	-0.083***	-0.084***	-0.085***
	(0.025)	(0.026)	(0.026)	(0.025)
Child age		0.018	0.016	0.016
		(0.019)	(0.019)	(0.019)
Child female		-0.108	-0.126	-0.139
		(0.264)	(0.273)	(0.274)
Child age × female		0.010	0.012	0.013
		(0.023)	(0.024)	(0.024)
Child grade		-0.022	-0.020	-0.021
		(0.024)	(0.025)	(0.024)
Caregiver age		-0.004	-0.004	-0.004
		(0.003)	(0.003)	(0.003)
Caregiver female		-0.298	-0.297	-0.306*
		(0.179)	(0.178)	(0.180)
Caregiver age × female		0.008**	0.008**	0.008**
		(0.004)	(0.004)	(0.004)
Caregiver education		0.013	0.014	0.009
		(0.035)	(0.036)	(0.038)
Caregiver Acholi		0.004	0.018	0.021
		(0.116)	(0.122)	(0.129)
Lira Palwo sub-county			0.014	0.009
			(0.061)	(0.064)
Lukole sub-county			-0.037	-0.041
			(0.073)	(0.074)
Parabongo sub-county			-0.008	-0.016
			(0.090)	(0.093)
Patongo sub-county			0.033	0.029
			(0.139)	(0.137)
Awere sub-county			-0.090	-0.097
			(0.100)	(0.100)
Pajule sub-county			0.029	0.025
			(0.096)	(0.097)
Puranga sub-county			0.005	0.002
			(0.071)	(0.074)
Assets index				0.014
				(0.020)
Household size				0.004
				(0.010)
Child time-preferences				-0.002
				(0.055)
Child risk-preferences				0.001
				(0.076)
Observations	440	435	435	435
R^2	0.024	0.042	0.045	0.047

Notes: Standard errors corrected for village level clustering (42) are in parentheses. Confidence: *** ↔ 99%, ** ↔ 95%, * ↔ 90%. Atanga sub-county is taken as reference category, and thus omitted.

Table A6: Robustness of the main result to controlling for parental digit ratios

	Public Good Contribution			
	(1)	(2)	(3)	(4)
Standardized digit ratio (-)	-0.130***	-0.134***	-0.142***	-0.120***
	(0.040)	(0.039)	(0.036)	(0.038)
Caregiver digit ratio		0.064	0.054	0.070*
(biological parents only)		(0.038)	(0.041)	(0.039)
Child controls	N	N	Y	Y
Caregiver controls	N	N	Y	Y
Sub-county fixed effects	N	N	N	Y
Observations	154	153	153	153
R^2	0.045	0.061	0.094	0.145

Notes: Confidence: *** ↔ 99%, ** ↔ 95%, * ↔ 90%. Child controls: Age, Female, Age × Female, Grade; Caregiver controls: Age, Female, Age × Female, Education, Acholi.

Variable Definitions:

2D:4D. A child level measure of the relative length of the index finger of the right hand with respect to the ring finger (in cm).

Standardized digit ratio (-). A child level variable derived by standardizing the 2D:4D (z-score). The negative sign is added for ease of interpretation.

Public good contribution (child and caregiver). An individual level dummy for both child and caregiver representing the individual choice in the public good game: "group card" or "private card". The choice "group card" takes value 1, 0 otherwise.

Age (child and caregiver). Age of respondent (child and caregiver) i in years, rounded down to the last birthday.

Female (child and caregiver). Individual level dummy taking value of 1 if respondent (child and caregiver) is female, 0 otherwise.

School grade. A child level variable indicating the current school grade of child i.

Height-for-age. The height of child i standardized for his age class (WHO, 2007).

BMI-for-age. The body-mass-index of child i standardized for his age class (WHO, 2007).

IQ-for-age The IQ of child i, measured using standard Raven's matrixes and standardized for his age class (in sample).

Time preferences. A child level dummy taking value of 1 if child prefers to receive two candies at the end of the survey rather than one half way, 0 otherwise.

Risk preferences (child and caregiver). An individual level index (child and caregiver) spanning from 0 (i.e. never gamble) to one (i.e. always gamble), based on two dichotomous lottery choices.

War exposure (child and caregiver). A caregiver level victimization index derived from answers to 23 war witnessing questions (see Macksoud, 1992). The postnatal war exposure of child i is proxied by the war exposure of the caregiver multiplied by the fraction of violent conflict events that took place after the birth of child i.

Education level. A caregiver level variable indicating the number of completed years of education of respondent i.

PTSD. A caregiver level measure of post-traumatic stress disorder, calculated using the PCL-civilian checklist.

Christian. A dummy taking value of 1 if the caregiver is Christian by religion, 0 otherwise.

Acholi. A dummy taking value of 1 if the caregiver is ethnically Acholi, 0 otherwise.

Household size. The number of people sharing the same roof and sharing the same pot.

Assets index. A principal factor (see Sahn and Stifel 2003) of assets possessed by the caregiver's household (Radio, Phone, Bicycle, Motorbike, Television, Car, Generator).

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CHAPTER 3
Conflict Exposure and Competitiveness

Experimental Evidence from the Football Field in Sierra Leone

Abstract

We use data from a street football tournament and a series of field experiments in post-conflict Sierra Leone to examine the impact of exposure to conflict violence on competitive behavior. We find that football players that experienced more intense exposure to violence are more likely to get a foul card during a game. In the lab we find that these individuals are significantly less risk averse on average, and more altruistic towards their in-group. We then isolate competitiveness from aggressiveness and find that conflict exposure increases the willingness to compete, but only towards the out-group. These results are in line with evolutionary theory, which highlights the role of inter-group conflict in increasing in-group cooperation while exacerbating out-group antagonism. Next to risk and other-regarding preferences, changes in individual preferences for competition may impact regional long-run development trajectories and post-conflict recovery.

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3.1 Introduction

More than two-thirds of the African countries have experienced civil war during the past few decades (Themnér and Wallensteen, 2014). Research on the consequences of these conflicts documents the persistent effect of violence on education (Lai and Thyne, 2007; Chamarbagwala and Morán, 2011), health and disability (Ghobarah et al., 2003; Iqbal, 2006; Iqbal and Zorn, 2010), food security and poverty (Gates et al., 2012). The impacts on institutions, individual behavior, and preferences, are less well understood (Blattman and Miguel, 2010). There is a small but growing body of literature examining these impacts, predominantly highlighting changes in social and political preferences, such as participation in local collective action, voting and sharing both within and across communities. Evolutionary theory highlights the role of inter-group conflict in shaping pro-egalitarian parochial preferences—increasing in-group cooperation while exacerbating out-group antagonism (Bernhard et al., 2006; Bowles, 2006; Choi and Bowles, 2007). At shorter time-scales this theory has been corroborated with respect to increased in-group cooperation after civil war (Bellows and Miguel, 2009; Voors et al., 2012; Gilligan et al., 2014), and increased out-group antagonism (Miguel et al., 2011).

Increased out-group antagonism may impact the aggressiveness of individuals (Miguel et al., 2011), but it may also affect their willingness to compete. Taste for competitiveness is an important non-cognitive determinant of human capital indicators, such as adult economic achievements and productivity (Niederle and Vesterlund, 2007). If less competitive people shy away from direct competition (Bartling et al., 2012), non-first-best contenders have a higher chance of winning a contest—affecting allocative efficiency (see Eriksson et al., 2009). For this reason, 'competitions and the right dose of competitiveness significantly determine not only the future of the individual but even the evolution of the whole species' (Leibbrandt et al., 2013: p.9305). Yet, individual variations in competitiveness need not to be solely explained by long-run

evolution. They may result from exposure to different environments and pressures. Leibbrandt et al. (2013) find that fishermen from individualistic societies are far more competitive than those from neighboring collectivistic societies, and that this difference emerges with time. In conjunction with altered preferences for local collective action and trade-offs over risk and time, shifts in competitiveness may be a crucial determinant of regional post-war political and economic recovery and development.

Using data from a football tournament in Sierra Leone, we assess the impact of war-related violence on preferences of local youth. We carefully record the details of each match and player. After the game, we invite players to participate in a series of lab-in-field experiments and a short survey. We measure preferences towards teammates and opponents, making use of the bi-lateral antagonism and group dynamics generated by the game itself (Weinstein et al., 1995; Duggan and Levitt, 2002; Garicano and Palacios-huerta, 2006; Miguel et al., 2011). We find that individuals that experienced more intense conflict-related violence during childhood are more likely to receive a foul card during a football game, are less risk averse and more altruistic towards their in-group, but not towards the out-group. Next, we test willingness to compete through an effort game that disentangles competitiveness from aggressiveness. Violent conflict appears to exacerbate out-group competitiveness: conflict exposed subjects are on average 51% more likely to enter a competition than the non-exposed, whereas in-group competitive dynamics are not significantly altered.

Obviously, it is challenging to identify the exact mechanisms via which conflict affects behavior. We argue that our results are consistent with a perspective on how conflict changes preferences and beliefs, and discuss several potential alternative mechanisms. In a sensitivity analysis, we show that the magnitude of the effect increases when focusing solely on the most relevant age sub-sample. Also, we find little evidence of self-selection into violence, which is consistent with literature on the Sierra Leonean civil war. Our results are robust to the introduction of forced displacement as

an additional source of war-related trauma, as well as to clustering standard errors at the football team level, and to football match fixed effects. Finally, competitiveness may indirectly change in response to altered social and risk preferences, or aggressiveness, and not as an independent process of endogenous preference formation. We show that our results maintain when controlling for such endogenous covariates.

The study is organized as follows. Section 3.2 discusses literature on conflict and preferences and on the determinants of competitiveness, presenting our key hypothesis. Section 3.3 presents the context and background to the field and lab experimental data, and outlines the experimental design and data. Section 3.4 discusses our identification strategy and Section 3.5 contains our results. Section 3.6 offers a discussion and conclusion.

3.2 Conflict, preferences, and competition

This paper seeks to connect and contribute to two literatures: that on the determinants of competitiveness and that on the impact of civil war. Competitiveness is a key determinant of individual economic achievements and productivity (Niederle and Vesterlund, 2007). There are significant differences in willingness to compete both within and across societies (Leibbrandt et al., 2013). These differences can be attributed to variations in genetic endowments, abilities and preferences (Niederle and Vesterlund, 2007; Gneezy et al., 2009), as well as individual exposure to various environmental pressures and life events (Roth and Erev, 1995). Most empirical studies on the origins and consequences of competitiveness use data from laboratory experiments. Using effort games, behavioral economists document that when the type of payment is exogenously imposed on subjects, competitive tournaments reveal a much larger variance of effort than equivalent piece-rate schemes (van Dijk et al., 2001; Harbring and Irlenbusch, 2003). This in turn reduces their overall efficiency (Eriksson et al., 2009). Such an unexpected finding may be driven by the

unwillingness of some people to enter competition. In fact, Eriksson et al. (2009) show that allowing for self-selection into a competitive tournament results in higher average effort rates and lower between-subject variance for subjects choosing to compete. Competitive environments are thus more efficient than non-competitive ones only if populated by a sufficient share of agents willing to compete.

While a complete insight is lacking, literature has highlighted several individual and behavioral determinants of competitiveness. For example, Niederle and Vesterlund (2007) find important differences with respect to gender and performance expectations. Bartling et al. (2009) find that overconfident, skilled and risk prone subjects are more likely to join a contest, while inequality-averse subjects less. Liebbrandt et al. (2013) compare individualistic and collectivistic societies, and show that life experiences may alter individual tastes for competition. Individuals develop their preferences mostly during childhood (Benenson et al., 2007; Fehr et al., 2008), learning from the society and environment surrounding them. Intense shocks during childhood should thus alter individual preferences for competition. Yet, the role of early life events such as exposure to conflict as a determinant of competitiveness is still ill-understood.

Research into the conflict induced chances in behavior is equally limited but growing (Blattman and Miguel, 2010).[20] A key research line focusses on the impacts on pro-social preferences. An emerging insight points to the fundamental role of the boundary between in-groups relative to the out-groups in shaping post conflict preferences: intra community violence

[20] Psychological literature documents the relationship between war exposure and trauma, focusing mostly on post-traumatic stress disorder (PTSD), anger and anxiety. Macksoud & Aber (1996) find that the number of war-related traumatic events experienced by a child is positively related to Post Traumatic Stress Disorder (PTSD) symptoms and differentially related to other behavioral outcomes. Similar attitudinal outcomes were found among conflict exposed children in Bosnia (Papageorgiou et al., 2000; Layne et al., 2010). Dyregrov et al. (2002) find highly time-persistent intrusive and avoidance reactions among Iraqi children exposed to a deadly aerial bombing. Other studies explore instead positive responses to trauma—often referred to as "post-traumatic growth" (Tedeschi and Calhoun, 1996; Powell et al., 2003; Staub and Vollhardt, 2008).

appears to decrease within community social cohesion whereas inter community conflict increases it. This mirrors contributions in evolutionary theory, which predicts how inter-group conflict shapes parochial preferences –increasing in-group cooperation while exacerbating out-group antagonism (Bernhard et al., 2006; Bowles, 2006; Choi and Bowles, 2007). For example, Cassar et al (2013) find that intra-community violence in Tajikistan undermined social cohesion and within-village trust (see also Rohner et al., 2013). On the other hand, Bellows and Miguel (2009) find that individuals whose households directly experienced more intense violence by the RUF are more likely to attend community meetings, join local political and community groups, and vote. Blattman (2009) finds that experiencing abduction and violence increased political engagement, voting and community leadership among ex-combatants in Northern Uganda. Blattman and Miguel (2010) present a survey of literature on civil war and argue that the existing literature omits advances in behavioral economics, and advocate micro-level analysis and case studies as crucial to understand war's causes, conduct, and consequences, in particular in the behavioral and institutional domain.

In recent years, a number of studies have used lab-in-field experiments to gauge the consequences of civil wars. Voors et al. (2012) show that individuals exposed to violence display more altruistic behavior towards their neighbors, are more risk-seeking, and have higher discount rates. Gilligan et al. (2014) show that communities that suffered war-related violence during Nepal's ten-year civil war exhibit significantly greater levels of altruistic giving, public good contributions, investment in trust-based transactions, and willingness to reciprocate trust-based investments. Bauer et al. (2014) investigate how conflict experiences shape the beliefs and preferences of youth. They present two case studies – one in Georgia and one in Sierra Leone – indicating that experiencing inter-group conflict during childhood and adolescence increases egalitarian motivations toward the in-group, but not the out-group. Miguel et al. (2011) explicitly investigate behavioral changes in out-group antagonism. They examine the

consequences of civil war on aggressiveness of players in European football leagues. They find that the number of years the home country of a player has been in violent conflict before the player reaches the age of eighteen is strongly and positively related to the amount of foul cards received.

We build upon the work of Miguel et al. (2011), by combining data from a field setting – the football tournament – and the lab-in-the-field experiments. After providing confirmatory evidence of increased aggressiveness, increased parochial altruism and risk propensity, we test willingness to compete through a competitiveness game that disentangles competitiveness from aggressiveness—through a game where players cannot affect another player's payoffs. While the role of conflict exposure in shaping social preferences has been explored in several experimental settings, to our knowledge this is the first work attempting to investigate its effect on competitiveness.

3.3 Context, data and experimental design

Sierra Leone is amongst the poorest countries in the world and is recovering from an eleven years long civil war. In 1992, a small group of rebels entered the East of the country. They found fertile ground for popular grief and discontent towards 'a decayed neo-patrimonial one-party regime' (Richards, 1999: p.433), and were nurtured by Sierra Leone's diamond wealth (Keen, 2005). It was the start of a country-wide civil war that cost over 50,000 lives, leaving many civilians amputated and abused, and hundreds of thousands temporarily displaced (Dufka, 1999; Doucet and Denov, 2012). At present violence and intimidation have disappeared from Sierra Leone and the country has now known several years of peace. In 2003, after a brief intervention by the British Army an internationally-brokered peace agreement was signed, paving the way for a transition process that led to an integrated defense force, elections, the establishment of a new constitution, and opened the country to foreign aid. While the country still ranks low on close to all development indicators, the local

economy is improving each year – the 2012 growth rate was 16% – and locally business are developing.

We use data collected during a youth street football tournament organized in Kenema, a regional town in Eastern Sierra Leone. The tournament spanned several weeks between November and December 2010. For this knockout tournament, streets within the city each assembled in a team. Matches were centrally organized and a substantial cash reward awaited the winner. Team identity was strong and the players took pride in defending their street. Referees oversaw adherence to rules and distributed yellow and red cards in response to minor and major faults. We carefully recorded details of the matches and players of the performance of 14 teams and 162 players. Table 1 presents the descriptive statistics. A total of 47 yellow and 3 red cards were given, involving 20% of the players. After each football match, we invited players to participate in a survey and a series of lab-in-field experiments. Our close collaboration with tournament organizers and team managers effectively cancelled attrition.

Our respondents are young males, between 14 and 31 years old. They are predominantly Muslim, and of the Mende tribe and 50% are enrolled in senior secondary education. We identify a series of plausible non-experimental proxies of athletic ability, which may influence the willingness to compete and to receive a foul card. Substitutes could enter and exit at any time of the match, with no limit with respect to the number of substitutions. Therefore, whether a player had not been substituted during the entire duration of the match (46%) may be seen as a good approximation of relatively greater football skills, most likely correlated to general athletic ability. In addition, we ask our respondents to rate their own level of skills compared with their teammates. We create an index ranging from 0 (self-declared least skilled) to 1 (self-declared most skilled).

Table 1: Descriptive statistics

Variable	Observations	Mean	Std. Dev.	Min	Max
Individual characteristics					
War Exposure	162	0.57	0.26	0	1
Parents Fought in War	162	0.12	0.33	0	1
Age	162	19.75	3.44	14	31
Education Level	162	2.64	0.75	1	4
Meals per Day	162	2.41	0.63	1	3
Muslim Religion	162	0.79	0.41	0	1
Mende Tribe	162	0.54	0.50	0	1
Fula Tribe	162	0.16	0.37	0	1
Mandingo Tribe	162	0.11	0.32	0	1
Temne Tribe	162	0.07	0.26	0	1
Football tournament					
Foul Card in Football Game	162	0.20	0.40	0	1
Played the Whole Football Game	162	0.46	0.50	0	1
Self-declared Football-skills	162	0.86	0.23	0	1
Scored	162	0.17	0.38	0	1
Won the Football game	162	0.42	0.50	0	1
Left Footed	162	0.19	0.39	0	1
Lab-in-Field experiments					
Risk Propensity	162	0	1.00	-1.6	1.2
Sharing in Out-Group Dictator Game	162	0	1.00	-2.9	2.4
Sharing in In-Group Dictator Game	162	0	1.00	-3.7	4.4
Self-selection into Out-Group Competition	70	0.43	0.50	0	1
Self-selection into In-Group Competition	92	0.41	0.50	0	1

Notes: See Appendix for variable definitions.

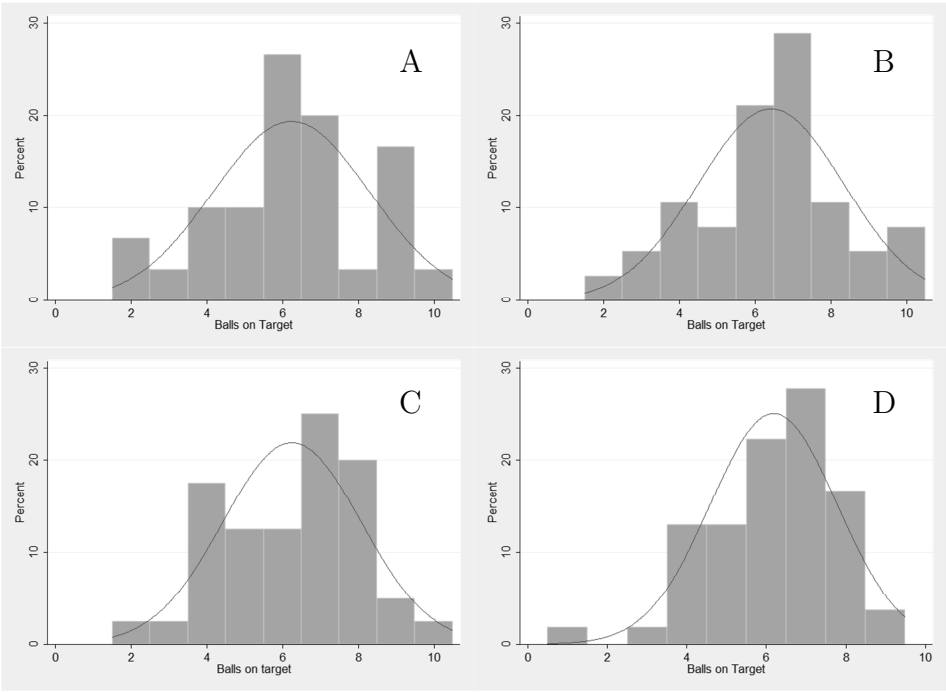
While we could not record the positioning of players on the football field due to the high fluidity of play, we also recorded which players scored a goal. Finally, we recorded which team won the football game and if the participant is predominantly left- or right-footed.[21] To measure exposure to conflict-related violence we ask respondents about a range of war related events, covering information on personal injury, seeing one or more injured person, seeing and hearing combat. Following Bellows and Miguel (2009),

[21] Psychological literature highlights correlations between handedness (footedness) and several non-cognitive dimensions (Goldberg et al., 1994), as well as cognitive skills (Sanders et al., 1982; Faurie et al., 2006). More recently, handedness has been placed in correlation with economic outcomes (Denny and O Sullivan, 2007), and competitiveness (Hoffman and Gneezy, 2010).

we create a victimization index using the average of positive responses to these violence related questions.

We implement a range of lab-in-field experiments. We measure willingness to self-select into competitive environments using an effort game, based on Niederle and Vesterlund (2007) and Bartling et al. (2009). Respondents are invited to participate in a game where they throw a football into a standard sized basket secured to the floor, from a distance of four meters. They choose whether to play individually – at a piece rate payment scheme of 500 Leones per ball on target – or to enter a competition against an anonymous counterpart.[22] In the competition, the respondent wins 1500 Leones for every ball on target if the total number is higher than the counterpart—zero if lower. In case of a draw both respondents receive 500 Leones per ball on target. This experiment disentangles willingness to compete from aggressiveness, as the decision of each participant can only affect their own private outcome, and not that of the counterpart. Even if deciding to compete, they would not alter their counterpart's utility and earnings if the counterpart chooses not to compete. Similarly, even if choosing not to compete they may affect their counterpart's utility and earning if the counterpart chooses to compete and does not win. In other words, aggressiveness should not determine willingness to compete in this experiment. Respondents are randomly divided into two groups: one group plays against an anonymous player of the opponent team (out-group) and another against an anonymous player of their own team (in-group). 42% of the respondents chose to participate in the tournament. Figure 1 shows the distribution of shots and relative frequency across groups. On average, respondents scored 6.27/10, with a standard deviation of 1.82.

[22] 4400 Leone s was about 1 USD at the time of the data collection.



A: Competitive out-group; B: Competitive in-group; C: Non-competitive out-group; D: Non-competitive in-group

Figure 1: Balls on target across treatments and completion choice

To measure risk preferences, we use a simple dichotomous choice game based on Harbaugh et al. (2002). In this risk game subjects are required to choose several times between receiving an amount of money for certain, and playing a simple gamble. Six choice sets are presented; each time we ask whether the respondent prefers (1) to toss a coin and make the chance of winning 3000 Leones or zero (if tails), or (2) not toss a coin and win an amount of money for certain, growing in each choice set, from 100 Leones to 2500 Leones. The expected value of the gamble is thus kept constant, while the certain option increases progressively: the point of switch from the gamble to the certain option is used to determine the risk preferences of the respondent—the later the switch, the less risk-averse (Table 2). Next, we standardize the resulting variable to improve interpretability.

To gauge other-regarding preferences we use a simple non-strategic dictator game. Each participant made two random order choices on how to allocate an endowment, once paired with a teammate and once with an opponent.

Table 2: Risk propensity game choice sets

Choice set	Coin toss		For certain
	If heads	If tails	
(1)	3000	0	100
(2)	3000	0	500
(3)	3000	0	1000
(4)	3000	0	1500
(5)	3000	0	2000

Players received 1000 Leones and were told these were theirs to keep at the end of the experiment. Alternatively, they could donate any 50 Leones portion of it to an anonymous counterpart. To avoid income effects potentially confounding our results, participants were notified that their final pay-off would be determined by the outcome of one randomly selected game they played, plus a possible donation from either a teammate or an opponent. Also in this case, we standardize out-group and in-group donations for the sake of interpretability.

3.4 Identification and empirical strategy

Our empirical strategy relies on local comparisons across war and non-war exposed subjects. The key identifying assumption is that exposure to violence was exogenous with respect to individual characteristics. This assumption may be violated in the presence of systematic targeting by belligerents along some individual dimension—i.e. religion, ethnic group, etc. While, undoubtedly some elements of violence were targeted, most violence in Sierra Leone was essentially a random process. It was not motivated by religious or ethnic cleavages (Bellows and Miguel, 2009), and no ethnic group was disproportionally targeted by rebels (Conibere et al., 2004; Humphreys and Weinstein, 2006). To test these assumptions on our sample of respondents, below we regress war exposure on a set of variables capturing individual characteristics. We find no evidence of selective

violence, except for age (and age squared); responding to intuition, older participants had a higher probability of war exposure (Table 3).[23]

Previous experimental evidence shows that children develop their preferences mostly between the age of three and eight (Benenson et al., 2007; Fehr et al., 2008), reaching stability around the early twenties (Sutter, 2007; Sutter and Kocher, 2007). Our sample's mean age at the beginning of the civil war was less than one-year-old, eleven by the end of it. They acquired the normative rules of the society surrounding them, and shaped their individual preferences, throughout the war period. This provides additional supporting ground for the causal relationship between exposure to violence during childhood, and the behavioral changes we observe. If anything, given the slightly wider age range, we are likely to underestimate the true impact of exposure to war violence. Nonetheless, the absence of base-line behavioral data – rarely available for this type of studies – makes it impossible to completely rule out potential correlations between pre-war parental behavioral characteristics and the degree of war exposure experienced by children.

Results could be biased by selective migration. If displaced people are significantly different from people who did not migrate, selective migration might play a role in determining who experienced violence. Gilligan et al. (2014) identify two mechanisms through which war may impact social preferences: (1) a collective coping mechanism by which people band

[23] Our sample does not include traditional authority households – significantly more likely to experience violence during the civil war according to Bellows and Miguel (2009). We do have information on participation in civic defense forces (CDF). Individuals whose parents participated in CDFs or independently fought during the civil war may have experienced more violence. In particular, if those individuals were more competitive, and competitive behavior is correlated across generations, the main coefficient might reflect selection rather than the treatment effect of exposure to violence. Columns 3 and 4 of Table 3 show no evidence of a significant self-selection effect into war related violence for the children of combatants. Also, column 4 shows that war exposure does not significantly correlate with any of the proxies for athletic ability identified during the football game.

Table 3: Age predicts conflict victimization

	Exposure to conflict			
	(1)	(2)	(3)	(4)
Age	0.196***	0.172***	0.193***	0.158***
	(0.053)	(0.056)	(0.053)	(0.056)
Age squared	-0.004***	-0.004***	-0.004***	-0.003**
	(0.001)	(0.001)	(0.001)	(0.001)
Muslim religion		0.001		0.018
		(0.046)		(0.051)
Mende tribe		0.025		0.034
		(0.061)		(0.065)
Fula tribe		-0.095		-0.069
		(0.084)		(0.090)
Mandingo tribe		-0.086		-0.063
		(0.089)		(0.097)
Temne tribe		-0.055		-0.066
		(0.095)		(0.096)
Parents fought in war			0.053	0.049
			(0.058)	(0.064)
Left footed				0.045
				(0.056)
Play whole football match				-0.011
				(0.040)
Self-declared skills				0.105
				(0.085)
Scored				-0.006
				(0.052)
Won the football match				-0.022
				(0.041)
Education level				-0.000
				(0.031)
Meals per day				-0.047*
				(0.029)
Observations	162	162	162	162
R^2	0.151	0.188	0.156	0.215

Notes: Confidence: *** ↔ 99%, ** ↔ 95%, * ↔ 90%.

together to deal with threats, and (2) a purging mechanism by which less social individuals disproportionately flee communities. In our case, more

competitive people may have permanently migrated into Kenema, and less competitive people may have migrated out of Kenema and would therefore not be part of the investigation.[24] Yet, our study focuses on comparisons across individuals that have experienced varying degrees of war exposure and are currently residing in Kenema. It does not attempt to draw conclusions on the overall intent-to-treat impact of the Sierra Leone civil war on the competitiveness and willingness to compete of Sierra Leoneans, nor does it expect to generalize the conclusions across countries.[25]

The core of our analysis lies in a set of regressions that seek to explain differences in our outcome variables through a set of individual and football-related characteristics, and our measure of exposure to war violence. We set out by assessing the probability of receiving a foul card:

$$\Pr(FoulCard_i = 1 \mid War_i, X'_i, S'_i) = \alpha + \beta War_i + \gamma X'_i + \zeta S'_i + \varepsilon_i \qquad (1)$$

where $FoulCard_i$ is a dummy taking value of 1 if the player i received at least one foul card during the football game (where i=1,…,162), War_i is our victimization index, X'_i a vector of individual characteristics and S'_i is a vector of football match related controls.

We continue by examining the impacts of violence in a series of field experiments:

$$Risk_i = \alpha + \beta War_i + \gamma X'_i + \varepsilon_i \qquad (2)$$
$$Donation_i = \alpha + \beta War_i + \gamma X'_i + \varepsilon_i \qquad (3)$$

where $Risk_i$ refers to individual risk propensity, $Donation_i$ to the portion of endowment donated in the dictator game, to an anonymous teammate or opponent, and other notations are the same as in (1).

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[24] According to the UN, from April 2001 to November 2002, all the 223,000 registered IDPs were reintegrated within their original communities and many more unregistered refugees have been returning home ever since (Norwegian Refugee Council, 2003). In our sample, 82% of respondents declared to have been temporarily displaced during the war—a slightly higher percentage than the national average (60%). This indicates that our dataset encompasses a large portion of returnees.

[25] For a cross-country perspective see Adhvaryu and Fenske (2014) .

Finally, we empirically investigate the effect of war-related violence exposure on the willingness to compete:

$$\Pr(Competition_i = 1 \mid War_i, X'_i, S'_i) = \alpha + \beta War_i + \gamma X'_i + \zeta S'_i + \varepsilon_i \qquad (4)$$

where $Competition_i$ takes value of 1 if the participant has opted for the competitive choice, 0 if he opted for the piece-rate payment in the effort game. All other notations are the same as in (1).

3.5 Results

We start by analyzing our football field data. In Table 4, column (1) and (2) we find that individuals strongly exposed to conflict-related violence are 28% more likely to commit a card-deserving foul during the football game, significant at $\alpha = 0.05$.[26]

Next, we regress violence exposure on our standardized measure of risk propensity. We find that it increases the propensity to risk by around 2/3 of a standard deviation (Table 4, columns 3 and 4).

> ***Result 1:*** *Conflict exposure significantly increases aggressiveness on the football pitch, as well as risk-seeking behavior in the lab.*

In Table 5, we test the hypothesis that individual war exposure may foster parochial pro-egalitarian preferences. Indeed exposure to conflict-related violence increases in-group donations by over 4/5 of a standard deviation (columns 1 and 2). On the other hand, war exposure does not seem to significantly alter altruistic behavior towards out-groups (columns 3 and 4).

> ***Result 2:*** *Conflict exposure significantly increases altruistic motives towards the in-group, not the out-group.*

[26] Also, a Pearson χ^2 test on victimization strongly rejects the null hypothesis of independence between war exposure and receiving a foul card (p=0.02).

Table 4: Aggressiveness and risk propensity

	Foul card		Risk propensity	
	(1)	(2)	(3)	(4)
Exposure to conflict	0.266**	0.284**	0.580**	0.668**
	(0.125)	(0.133)	(0.282)	(0.332)
Age		-0.031		0.081
		(0.082)		(0.199)
Age squared		0.000		-0.002
		(0.002)		(0.004)
Education level		0.042		-0.081
		(0.049)		(0.119)
Meals per day		0.046		0.092
		(0.049)		(0.136)
Muslim religion		-0.082		0.118
		(0.091)		(0.203)
Mende tribe		0.081		-0.064
		(0.061)		(0.170)
Play whole football match		0.083		
		(0.064)		
Self-declared skills		-0.231*		
		(0.123)		
Scored		0.166*		
		(0.099)		
Won the football match		0.175**		
		(0.068)		
Left footed		-0.108**		
		(0.052)		
Observations	162	162	162	162
Pseudo R^2 and R^2	0.025	0.157	0.022	0.036

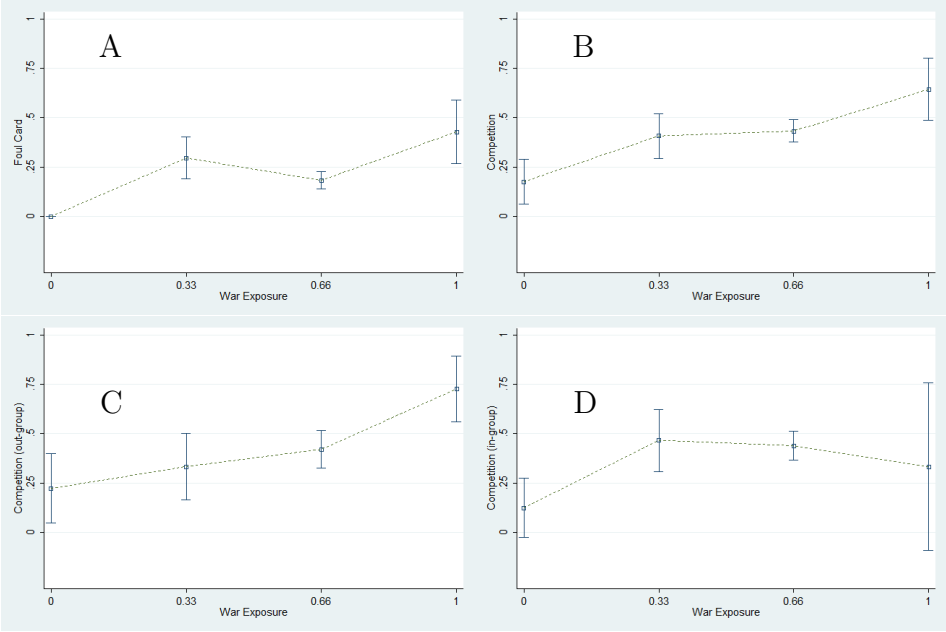
Notes: Probit marginal effects; Robust standard errors in parentheses. Confidence: *** ↔ 99%, ** ↔ 95%, * ↔ 90%.

Our main results on the relationship between civil war exposure and competiveness is presented in Figures 2A-D and Table 6. Figure 2A shows the percentage of football players receiving a foul card during the football tournament for each level of war exposure. None of the un-exposed players received a foul card. While indicative of increased out-group antagonism, this result *per-se* is not symptomatic of increased willingness to compete. We therefore proceed to look into our laboratory style competitiveness experiment. We find that the results parallel the field setting: across the two treatments, 18% of the completely war un-exposed respondents decide

Table 5: Dictator game donations

	Donations (z-score)				
	In-group		Out-group		Pooled
	(1)	(2)	(3)	(4)	(5)
Exposure to conflict	0.443*	0.667**	0.293	0.218	0.368
	(0.238)	(0.282)	(0.394)	(0.416)	(0.411)
In-group					0.465*
					(0.272)
Exposure to conflict X in-group					0.150
					(0.445)
Age		-0.041		0.220	0.089
		(0.150)		(0.184)	(0.115)
Age squared		-0.000		-0.004	-0.002
		(0.003)		(0.004)	(0.002)
Education level		0.017		-0.058	-0.021
		(0.092)		(0.111)	(0.074)
Meals per day		-0.019		0.291*	0.136
		(0.107)		(0.166)	(0.104)
Muslim religion		-0.001		0.137	0.068
		(0.157)		(0.219)	(0.141)
Mende tribe		-0.189		-0.066	-0.128
		(0.131)		(0.165)	(0.101)
Observations	162	162	162	162	324
R^2	0.020	0.061	0.005	0.072	0.101

Notes: Robust standard errors in parentheses. 162 clustered s.e. in (5). Confidence: *** ↔ 99%, ** ↔ 95%, * ↔ 90%.



A: Foul card all; B: Competition all; C: Competition out-group; D: Competition in-group

Figure 1: Balls on target across treatments and completion choice

to join the competition, compared to 64% of the fully war exposed respondents. (Figure 2B). Figure 2C and 2D show a breakdown for subjects playing against the out-group and in-group respectively.

In Table 6 we analyze these patterns parametrically. At the median of all covariates, subjects most exposed to conflict-related violence are 51% more likely to join a competition against the out-group, significant at $\alpha = 0.05$ (columns 1 and 2).[27] On the other hand, we find no evidence of an impact on in-group competitive behavior (columns 3 and 4).

> ***Result 3:*** *Subjects most exposed to conflict violence are 51% more likely to be willing to compete against the out-group. The effect of conflict exposure on in-group competition is positive but not significant.*

Our results are robust to the introduction of forced displacement as an additional source of war-related trauma, as well as to clustering standard errors at the football team level, and to football match fixed effects (see Appendix Table A1 for details). The magnitude of the effect increases when focusing solely on the most relevant age sub-sample [28] Also, competitiveness may indirectly change in response to altered social and risk preferences, or aggressiveness, and not as an independent process of endogenous preference formation. We introduce foul cards, risk and dictator choices as endogenous controls into equation (4), first separately (Appendix Table A2, columns 1 and 2) and then jointly (Appendix Table A2, column 3). Our results remain significant and the coefficients maintain relative constancy.

[27] The coefficient on exposure to war-related violence increases when observable controls are included. Following Bellows and Miguel (2009), this suggests that omitted bias is unlikely to explain away the effect (see also Altonji et al., 2005).

[28] i.e. not older than eight at the start of the war and at least eight at the end of it (see Table A1 for details).

Table 6: Willingness to compete

	Competition			
	Out-group		In-group	
	(1)	(2)	(3)	(4)
Exposure to conflict	0.485**	0.510**	0.274	0.266
	(0.222)	(0.244)	(0.227)	(0.270)
Age		0.104		-0.228
		(0.183)		(0.160)
Age squared		-0.002		0.005
		(0.004)		(0.004)
Education level		0.171*		0.130
		(0.099)		(0.083)
Meals per day		-0.018		-0.137
		(0.110)		(0.097)
Muslim religion		0.305***		-0.079
		(0.118)		(0.150)
Mende tribe		-0.084		0.011
		(0.148)		(0.116)
Play whole football match		0.405***		0.191*
		(0.136)		(0.111)
Self-declared skills		0.021		-0.222
		(0.254)		(0.299)
Scored		-0.267*		-0.125
		(0.147)		(0.138)
Won the football match		0.123		-0.075
		(0.135)		(0.114)
Left footed		-0.178		-0.213*
		(0.140)		(0.121)
Observations	70	70	92	92
Pseudo R^2	0.055	0.207	0.011	0.106

Notes: Probit marginal effects; Robust standard errors in parentheses. Confidence: *** ↔ 99%, ** ↔ 95%, * ↔ 90%.

3.6 Conclusions

We explore whether exposure to war-related violence affects the competitiveness of youth participating in a local street football tournament and a series of lab-in field experiments in Sierra Leone. Previous economic literature on the consequences of civil war on preferences documents increases in-group cooperation, political activeness and altruism. The main contribution of this study is to provide insight into the determinants of competitive behavior and its relation with exposure to conflict. We bring

new evidence that increased parochial altruism is a two-fold process—increasing in-group cooperation while exacerbating out-group antagonism.

Increased antagonism matters for post-conflict development as it shapes aggressiveness and, perhaps more saliently, competitiveness. To study war induced out-group dynamics we look both at aggressiveness during a football game and competitive behavior in laboratory experiment. We find that subjects more exposed to war violence during early childhood and preadolescence are not only robustly more likely to commit fouls during a football game, but are also more likely to self-select into a competition against an out-group in our experiment. Civil war does not only seem to foster cooperation towards perceived in-groups, but curbs distaste for competition against perceived out-groups. Being more prone to cooperate and engage in public debates affects the community level provision of public goods, potentially promoting economic development (Bellows and Miguel, 2009). Similarly, accepting inequality-averse outcomes driven by a fair and regulated competition is a fundamental element of economic growth (Bartling et al., 2009).

Our findings are tentative; different types of conflicts could have varying legacies, and the human cost of conflict may never be justified by its "externalities" (Cassar et al., 2013). Yet, a growing body of evidence about war violence victims' profound changes in individual beliefs, values, and preferences poses new challenges to policy makers and post-conflict recovery strategists. Indisputably, it profoundly rejects the notion of conflict as development in reverse (Collier et al., 2003). Not only has war historically promoted state formation and nation building – ultimately strengthening institutional capacity (Tilly and Ardant, 1975) – it may also be at the core of inclusive and dynamic societal transformations. Policymakers responsible for post-war recovery should be aware of the extent of these transformations and recognize heterogeneity among communities and individuals, not overlooking the significance of autonomous responses.

Appendix

Table A1: Robustness of main result to alternative specifications

	Competition (out-group)			
	Exposure + displacement	Cluster robust s.e.	Match fixed effects	Most relevant age sub-sample
	(1)	(2)	(3)	(4)
Exposure to conflict	0.709**	0.510**	0.574**	0.615**
	(0.292)	(0.239)	(0.255)	(0.280)
Age	0.095	0.104	0.156	-0.265
	(0.185)	(0.187)	(0.183)	(0.568)
Age squared	-0.001	-0.002	-0.003	0.007
	(0.004)	(0.004)	(0.004)	(0.013)
Education level	0.161	0.171**	0.139	0.184
	(0.102)	(0.077)	(0.098)	(0.115)
Meals per day	-0.039	-0.018	0.046	0.054
	(0.112)	(0.107)	(0.120)	(0.127)
Muslim religion	0.304***	0.305**	0.359***	0.357**
	(0.117)	(0.148)	(0.131)	(0.149)
Mende tribe	-0.125	-0.084	-0.064	-0.013
	(0.154)	(0.150)	(0.176)	(0.165)
Play whole football match	0.399***	0.405***	0.342**	0.487***
	(0.137)	(0.074)	(0.153)	(0.149)
Self-declared skills	0.046	0.021	-0.012	0.089
	(0.260)	(0.259)	(0.259)	(0.296)
Scored	-0.288**	-0.267**	-0.189	-0.460***
	(0.141)	(0.127)	(0.185)	(0.135)
Won the football match	0.161	0.123	0.062	0.122
	(0.143)	(0.108)	(0.152)	(0.159)
Left footed	-0.201	-0.178**	-0.186	-0.257
	(0.136)	(0.078)	(0.149)	(0.168)
Observations	70	70	70	55
Pseudo R^2	0.218	0.207	0.258	0.213

Notes: Probit marginal effects; Robust standard errors in parenthesis. Confidence: *** ↔ 99%, ** ↔ 95%, * ↔ 90%.

Table A2: Robustness of main result to endogenous covariates

	Competition (out-group)		
	Foul card	Risk and dictator choices	Foul card, risk and dictator choices
	(1)	(2)	(3)
Exposure to conflict	0.481*	0.489**	0.449*
	(0.258)	(0.245)	(0.258)
Foul card	0.116		0.158
	(0.182)		(0.191)
Risk Preferences		0.034	0.038
		(0.073)	(0.073)
Dictator Donation		0.050	0.061
		(0.073)	(0.078)
Age	0.110	0.098	0.105
	(0.185)	(0.183)	(0.185)
Age squared	-0.002	-0.001	-0.002
	(0.004)	(0.004)	(0.004)
Education level	0.168*	0.175*	0.175*
	(0.101)	(0.098)	(0.100)
Meals per day	-0.019	-0.041	-0.048
	(0.110)	(0.112)	(0.113)
Muslim religion	0.327***	0.307**	0.340***
	(0.116)	(0.120)	(0.119)
Mende tribe	-0.085	-0.083	-0.087
	(0.150)	(0.153)	(0.156)
Play whole football match	0.402***	0.388***	0.382***
	(0.137)	(0.143)	(0.144)
Self-declared skills	0.054	0.035	0.079
	(0.263)	(0.252)	(0.264)
Scored	-0.270*	-0.251	-0.250
	(0.149)	(0.156)	(0.160)
Won the football match	0.094	0.134	0.097
	(0.149)	(0.137)	(0.152)
Left footed	-0.182	-0.187	-0.193
	(0.140)	(0.138)	(0.138)
Observations	70	70	70
Pseudo R2	0.210	0.214	0.218

Notes: Probit marginal effects; Robust standard errors in parenthesis. Confidence: *** ↔ 99%, ** ↔ 95%, * ↔ 90%.

Variable definitions:

Exposure to conflict. An individual victimization index resulting from the average response to violence related questions: "*during war time...*" "*did you ever witness combat, shooting and explosions?*", "*did you ever see a person injured because of war-related violence?*", "*did you personally suffer from physical injury because of war-related violence?*".

Parents fought in war. Individual level dummy variable taking value of unity if any one of parents of respondent i have been active belligerents during the civil conflict.

Age. Age of respondent i in years, rounded down to the last birthday.

Education level. Individual level variable taking value 1 if the respondent was currently in primary school, 2 if the respondent was currently in junior secondary school, 3 if the respondent was currently in senior secondary school, 4 if respondent was enrolled or had completed tertiary education.

Mende (Fula, Mandingo, Temne) tribe. Individual level dummy taking value of unity if the i-th respondent is ethically Mende (Fula, Mandingo, Temne), 0 if else.

Muslim religion. Individual level dummy taking value of unity if the i-th respondent self-declared to be Muslim by religion, 0 if else.

Meals per day. Household level index representing the self-reported full meal consumption patterns of respondent i's household.

Left Footed. Individual level dummy variable taking value of unity if the i-th respondent self-declared to be predominantly left-footed, 0 if else.

Played whole football match. Individual level dummy variable taking value of unity if the i-th respondent responds positively to the question "*did you play the whole football game?*", 0 if else. The answer was crosschecked with the questions "*how many minutes did you play in this game*" and "*How many minutes did the game last in total?*"; the dummy would take a value of 0 if the ratio of their responses differed from unity.

Self-declared skills. Individual level index constructed as the answer to the question "C*ompared to your team mates, how skillful would you say you are?";* on a scale of 1 (least skilled) to 5 (most skilled), standardized between 0 and 1.

Scored. Individual level dummy variable taking value of unity if the i-th respondent had scored at least one goal during the football game, 0 if else.

Won the football match. Team level dummy variable taking value of 1 if the team of respondent i has won the football game, 0 if else. Out of 14 games 1 ended up in a draw and the penalty kicks were postponed to the next day due to insufficient light.

Foul card. Individual level dummy variable taking value of unity if the i-th respondent had received at least one yellow/red card in the tournament.

Risk propensity. Individual level variable based on the respondents' six choices in the risk game, spanning from 0 (i.e. never gamble) to one (i.e. always gamble), and allowing for indifference by taking the last switch point. The resulting index is then standardized.

Donations. The standardized value of the donation in the relevant dictator game.

Competition. Individual level dummy variable taking value of unity if the i-th subject decides to enter the competition in the effort game, 0 if else.

CHAPTER 4
Market Experience and Rational Choice

Experimental Evidence from Rural Ethiopia

Abstract

We organize a field experiment with sesame farmers and brokers in northern Ethiopia to explore whether market experience fosters rational behavior—proxied by fewer Generalized Axiom of Revealed Preference (GARP) violations. In the baseline study farmers and brokers perform equally well or badly, which is consistent with qualitative evidence that the prior "trading experience" of our brokers is not obtained in a competitive setting. Following random assignment to a competitive market setting – a one-day trading session in a sesame auction – we find that treated farmers and brokers behave more rationally than their peers in the control group.

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4.1 Introduction

People make choices to satisfy their potentially endless needs and desires, given a finite set of resources. Scarcity requires choice, and economists use rational choice theory to predict "optimal" behavior of agents or infer underlying preferences by analyzing actual behavior. Rational choice is one of the cornerstones of economics. It is routinely assumed that respondents maximize a continuous, concave and monotonic utility function, which requires completeness, reflexivity and transitivity of preferences. The Generalized Axiom of Revealed Preference (GARP) is a necessary and sufficient condition to meet these requirements (Varian, 1982). A GARP violation occurs when a bundle x is chosen when a bundle y is available, where bundle y has at least as much of all goods and strictly more of at least one good than a third bundle z, and z has been directly or indirectly revealed to be preferred to x.

Rational choice theory has been a powerful basis for the development of theories, policies and ideologies. Until fifteen years ago, consistency of choices of real agents was assumed, rather than measured. However, the assumption of rationality proved untenable. Experiments showed that between 10 and 75% of subjects violated the GARP predictions (e.g. Sippel, 1997; Harbaugh et al., 2001; Andreoni and Miller, 2002), acting more or less irrationally according to the neoclassical paradigm. Such studies were typically carried out in controlled environments with a non-random sample of respondents (e.g., students), with limited effort to test the external validity of the findings.

It has been hypothesized that rationality violations co-vary with the nature of the context and market experience. Specifically, evidence suggests rationality violations are less common in an environment that more closely resembles the market-type of setting that is the natural habitat of agents in neoclassical models (List and Millimet, 2008; List and Haigh, 2009; but see also the discussion below). Professional stock brokers may reveal more rational behavior in a choice experiment than a person who has rarely

engaged in competitive markets, and has exchanged goods according to other mechanisms. If so, rationality is not a hard-wired characteristic of humans. Rather, it is a trait that will be acquired through learning and exposure to markets, or, in the words of List and Millimet: 'economic rationality is a social, not an individual construct' (2008: p.36). Markets, then, are not simply mechanisms to allocate goods and services; by making participants feel the losses associated with irrational behavior, markets may help individuals learn to express their preferences in ways more consistent with a rational model.[29]

We use a field experiment in Ethiopia to explore whether competitive trading experience affects rationality, and analyze which factors affect the "learning process" associated with reducing rationality violations. We developed a lab-in-field experiment that involved local farmers and brokers. The market treatment was designed to be closely related to the core of the livelihoods strategies of the respondents—selling and buying sesame. Initially, both farmers and brokers rarely satisfied the GARP requirements—rationality violations were common. However, after a randomly selected subsample of the respondents participated in a competitive real sesame market, we observe that treated farmers and brokers showed statistically and economically significant reductions in the number of GARP violations.

The paper is organized as follows. In section 4.2 we discuss the existing literature on market experience and rationality. In section 4.3 we present

[29] Henrich et al. (2004) suggest that exposure to markets influences preferences, including social preferences and the propensity to trust others. The focus of this paper is on learning how to implement preferences, which extends the earlier work on endogenous preferences. Also see Cherry et al. (2003) for early evidence on "rationality spillovers"—they study preference reversals among undergraduate students in the context of (environmental) lotteries, and present evidence that "induced" market-like discipline extends to a non-market setting (hypothetical choices and environmental lotteries). They conclude that arbitrage feedback can help respondents to formulate more consistent stated values for their environmental preferences.

the context and background to the field experiment. Section 4.4 outlines the experimental design and presents the data, and section 4.5 contains all the results. Finally the discussion and conclusions are presented.

4.2 Rationality and market experience

Since Becker (1962), authors have tried to capture apparently "irrational behavior" into economic theory to account for behavioral patterns that were seemingly at odds with prescriptions for *Homo Economicus*. For example, Simon (1984) explored relaxing certain assumptions (e.g. perfect information) in his models of bounded rationality. Rieskamp et al. believe that violations of the bounds of rationality might 'reflect subtle, yet reasonable, dependencies on the environment' (2006: p.631), suggesting that different market environments lead to different behavioral strategies.

Several lab experiments show that, when placed in the "right environment", agents tend to behave according to neoclassical theory. For example, List (2003) provides empirical evidence that certain market anomalies tend to vanish as market experience increases, confirming a hypothesis dating back to Koopmans (1964). Inspired by a famous experiment by Knetsch (1989), List (2003) investigates the strength of the endowment effect—the tendency of subjects to keep a randomly allocated gift, rather than swapping it for an equally valuable alternative good. He shows that this tendency is strongest among inexperienced non-traders, and that, as market experience intensifies, behavior converges to the neoclassical predictions (no endowment effect). While suggestive, such studies cannot address an important concern regarding self-selection: people who behave more rationality are perhaps more likely to self-select into occupations that involve trading. If so, the correlation between market experience and rationality does not necessarily imply a causal effect of market exposure on rationality and coherence of choice.

This challenge is addressed in an important follow-up study. List and Millimet (2008) designed a field experiment where randomly selected respondents (youths) are exposed to a market setting, and analyze how market experience influences the rationality of choices. They sample young sportscard traders and young subjects shopping at a mall. First, they test the pre-existing levels of rationality violations among these subjects, using a GARP test (see Harbaugh et al., 2001).[30] This involves "gift bundles" of two goods: juice boxes and chips. Subjects were offered three to seven different gift bundles from which to select their preferred gift. The composition of the gift bundles varied with the relative price of the two goods in the choice sets and the budget (Figure 1). Once the choice sets had been evaluated and preferred gifts chosen, the experimenters randomly selected one of the sheets and offered the preferred gift to the subject.

List and Millimet find that only a minority of the subjects made rational choices. Overall, some 70% of the subjects exhibited at least one GARP violation. Consistent with the evidence above, they also found that self-selected traders are significantly more rational than their counterparts. But again, potential self-selection into the trading business implies this is not necessarily indicative of a causal effect.

[30] When an individual's choices do not violate GARP, such choices are consonant with the individual maximizing a continuous, concave and monotonic utility function. Yet, Cox raises the concern that 'finding low rates of violation of the one type of necessary condition (i.e. GARP) is an important result, but it does not support the conclusion that the subject's behavior can be rationalized by a utility function' (2010: p.27). In other words, we can test whether necessary conditions for utility maximization are satisfied, but not whether the same holds for sufficient conditions. Moreover, in addition to GARP violations there are alternative measures of rationality, and it is not evident that subjects who have many violations in one setting will also perform badly along other dimensions of rationality. Nevertheless, and reflecting the GARP's special status in economics, we focus on this measure as a proxy for rationality.

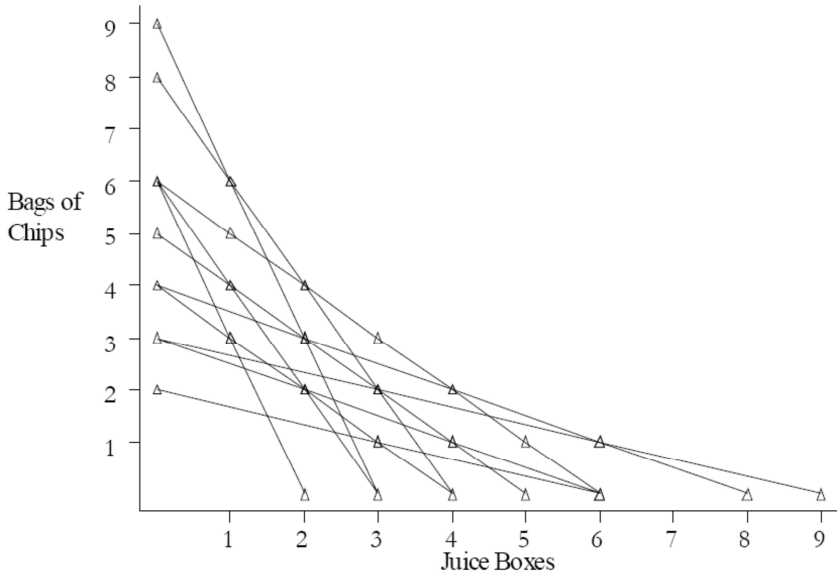


Figure 1: Gift Bundles and possible choice sets (Harbaugh et al., 2001)

Next, they induced a random subsample of the young shoppers, aged 6-18 years, to participate in the sportscards' market.[31] Randomization at this stage allows the analysts to remove the potential bias due to self-selection. Upon comparing the learning rates of "non-treated" shoppers to the treated ones, List and Millimet again find that market experience and rational choice directly related. The key result is that treated shoppers have significantly less GARP violations than the control group.[32] Thus, List and Millimet present robust empirical proof of endogenous rationality invited by participating in a real market.

[31] To induce the selected subsample to participate to the sportscards' market, they offered them a "parting gift" worth approximately $25 of sportscards and memorabilia. Then they informed the subjects about the approximate value of the gifts and stated the dealers at the show were interested in the goods. Finally, the experimenters stressed that the gifts could be sold, swapped, or taken home.

[32] As a significant number of subjects did not return to the experiment, List and Millimet possibly faced a bias due to non-random attrition, i.e. the possibility that some subjects had self-selected out of the experiment had different characteristics from the participating subjects. The Heckman Selection model however yielded no statistically meaningful evidence of non-random attrition, whilst the significance of the market experience term remained robust.

Should such findings have implications for policymakers? That depends. While Akerlof and Yellen (1985) find that small amounts of non-maximizing behavior have little impact on economic equilibria, and Gode and Sunder (1993) demonstrate that even zero-intelligence software reaches allocative efficiency at market level, the results with respect to endogenous rationality potentially have important implications for (economic) policy. For example, Goodhue et al. (1998) argue that liberalization policies in transition countries should take market "learning costs" into consideration when setting the target level of privatization. In addition, notwithstanding limited efficiency losses at the macro level, the distributional impacts of market experience and irrational choice can be a concern for policymakers, as it brings up problems of equality, rent allocation, competitiveness, and regional growth. This is certainly true in Ethiopia, where an actual policy change is intended to enhance competitiveness across the value chain of various primary commodities (see below).

Our experiment builds on List and Millimet (2008), and among other things allows us to test the external validity (generalizability) of their main findings. Rather than teenagers, we focus on adults (18 to 74 years old) with considerable experience in life (including market transactions, albeit in a non-competitive context). While local farmers mainly produce sesame, a cash crop, their "competitive market experience" is limited as local trade practices are often related to personal relations and interlinked transactions. The design resembles List and Millimet, but differs in several ways and makes a unique contribution. First, we vary the trading institution that forms the treatment condition, contributing to evidence that competitive market experience *per se* improves rationality. Second, we implement a treatment that should be of first-order salience to respondents. Subjects are asked to engage in transactions involving their main source of income, sesame, and that involve an institutional innovation that should be salient to them in light of policies with respect to the Ethiopian Commodity Exchange (ECX). Apart from the novelty of the context, other differences have to do with the income level (our

respondents are poor, with an average income level of 1-2 US$ a day for the farmers and 6-7 US$ for the brokers), culture and policy relevance. In fact the treatment and its implied opportunities were very significant to the participants—we ensured that the income from participating in the experiment would exceed the local opportunity cost of their time. These differences broaden the scope of the research beyond merely assessing the external validity of List and Millimet's results, upgrading it to a distinctive field experiment with unique features and relevant policy implications.

Since the concept was introduced by Varian (1982), GARP violations have been studied in different contexts. Among others, Sippel (1997), and Andreoni and Miller (2002), analyzed GARP violations by college students. Harbaugh et al. (2001) "exported" the experiment outside "campus" by studying GARP violations on kids, whilst List and Millimet (2008) set their experiment in the field—a sportscards fair. To the best of our knowledge, however, this is the first paper to document the effect of market experience on rationality in a developing country context.

4.3 Context and background

To test rationality effects from market experience amongst "real agents" we organize an experiment involving sesame smallholder farmers and brokers from the village Baeker, near the town of Humera. At the border with Eritrea and Sudan, in the Ethiopian lowlands, Humera has experienced a period of stable economic growth since sesame was introduced as a cash crop in the late 1990s. Smallholder farmers, however, have benefitted little from this innovation, locked up by credit constraints and ineffective trade practices. The farmers used for this study were selected from the village of Baeker, separated from the main town by 57 km of dirt road, and living in a rather isolated environment. They typically deliver their produce to the village cooperative, which then sells it on their behalf, or directly sell the sesame locally. Local traders sometimes work in interlinked markets, serving also as moneylenders. Smallholder farmers are

therefore restricted in their selling choices, and tend to trade with the same trader over time.

Two types of traders exist: first, and *not* the subject of this paper, there is a group of traders that lives off trading margins—the difference between purchase and sale prices; second, there are so-called "brokers"—who mediate in the exchange of sesame. Such brokers often have peasant origins and limited formal education. Unlike other traders, they normally receive a fixed commission from each trading partner (usually 2 Birr, or USD 0.16 per quintal), and live off a percentage on quantities traded. Brokers assist the flow of various tons of sesame per year, and therefore could be considered more experienced traders than smallholder farmers. However, they do not work in a conventional competitive market environment. Instead, to outperform their colleagues, long-standing relations, trust, and reputation are important. They spend time and money to "court" sellers and buyers: sticking banknotes into pockets, offering beers and motor rides, going to weddings, and so on. Besides, as mentioned, they sometimes engage in money-lending activities. Bargaining over prices is therefore much less important for this end of the sesame value chain. All traders that participated in the experiment belong to this category, and all are members of the only brokers' cooperative of Humera.

Sesame trading in Ethiopia will soon be in a state of flux. A major institutional innovation has been introduced by the central government—the Ethiopian Commodity Exchange (ECX). Brokers, and others, might see changes in their position in the value chain during the next few years. The philosophy behind the ECX is that a central and transparent market for key export crops will lower transaction costs in the value chain, and will address issues about asymmetric information. The idea is to link farmers directly to international markets, and the mechanism chosen is an open outcry double auction trade floor, in the capital city Addis Ababa. This study does not aim to assess the consequences of a change in the market "environment" on the livelihood or well-being of agents. This would

be quite difficult to predict.[33] Rather, it looks at the effects of the pending institutional change on the behavior of key agents. Like Plott (1994) and Roth and Ockenfels (2002) do for other markets, we test-bed the hypothesis that institutional innovations can affect economic decisions in a market that will soon be reshaped by a major institutional overhaul. At the village level, we introduce key elements of the ECX – the competitive market environment characterized by public bidding – and allow a random sample of farmers and brokers to engage in competitive selling and buying of their main cash crop.

4.4 Experimental design and data

We randomly selected 68 smallholder farmers from Baeker – which had already agreed to sell sesame collectively for a given price through their cooperative – to participate in the field experiment. We also invited all members of the only local broker association to participate in the experiment (22 individuals)—all brokers accepted. In other words, there is no selection bias due to self-selection in the first stage of the experiment. After filling out a brief survey, including age, level of formal education attained, hectares of land owned, amount of money borrowed from informal moneylenders, and a self-evaluation parameter expressing their "market experience", farmers and brokers where kindly asked to select their preferred gift—the standard approach to measuring GARP violations. They were shown 8 separate sheets of paper on which 6 to 10

[33] Note that we study the effect of market experience on behavior in an unrelated choice experiment. The potential range of settings where behavior may be affected is large, which is one of the reasons why predicting the "net impact" of the ECX on well-being of farmers and brokers (and others) is difficult. For example, it is not clear that more rational behavior will make brokers better off given the nature of the other markets they face. If some fraction of total trading goes through ECX in the future while the rest is implemented through traditional, relational transactions, then the overall implications may be mixed for the individual. Other, non-competitive markets may become less functional as a result of increased "rationality" caused by the ECX.

distinct gift possibilities were depicted. Gifts included raw coffee beans, sugar and mango juice. All items were familiar, similar in price, and commonly consumed by the farmers and brokers. By showing drawings of possible gifts, rather than giving a budget and relative prices, it was made certain that the subjects did not need to 'do the math to stay within a budget constraint' (Harbaugh et al., 2001: p.1545). To ensure comprehension, several trial runs were done. Moreover, it was clearly stated that each agent would receive only one of the eight selected gifts. A transparent lottery process was used to determine which choice would be selected. Table 1 shows the relative prices that determined the composition of the gift bundles in each choice set (an example of the choice sheets shown to participants can be found in the Appendix, Figure A1).[34] GARP violations were counted with the GARP software hosted on the EconPort digital library (see Cox and Swarthout, 2006).

After the GARP experiment, brokers and farmers were randomly divided into four groups—two treatment groups (one for farmers and one for brokers), and two controls groups. Treatment groups were asked to participate in a trading session mimicking the new sesame auction process at the ECX, the following Sunday. Control groups, instead, were invited to simply re-do the same GARP game after 7 days. The randomness of the partition is confirmed by the descriptive statistics of Table 2.1 and 2.2.

[34] In choice set 2, for example, with a budget of 4 units the subjects could receive: 1. two bags of coffee (200g); 2. one bag of coffee (100g) and two bags of sugar (1000g); 3. one bag of coffee (100g) and one bottle of mango juice (300ml); 4. four bags of sugar (2000g); 5. two bags of sugar (1000g) and one bottle of mango juice (300ml); or 6. two bottles of mango juice (600ml).

Table 1: Choice sets, budgets and relative prices

Choice set	Available budget	Price			Choices
		Coffee	Sugar	Mango Juice	
(1)	4	2	2	2	6
(2)	4	2	1	2	6
(3)	3	1	1	2	6
(4)	4	1	2	2	6
(5)	3	1	2	1	6
(6)	3	2	1	1	6
(7)	4	2	2	1	6
(8)	3	1	1	1	10

Notes: One unit represents 1 bag of 100g of raw coffee beans, one unit of sugar represents 1 bag of 500g of sugar, and one unit of mango juice represents 1 bottle of 300 ml of mango juice.

Table 2.1: Descriptive statistics (brokers)

Variable	Group	Observations	Mean	Std. Dev.	Min.	Max.	Pr>|t|
$GARP_0$	control	10	12.2	7.54	0	24	0.925
	treatment	12	11.8	10.07	0	28	
Education	control	10	5.6	3.84	0	10	0.865
	treatment	12	5.8	2.48	2	10	
Experience	control	10	4.5	0.70	3	5	0.249
	treatment	12	4.0	0.90	2	5	
Age	control	10	35.3	5.42	29	48	0.947
	treatment	12	35.1	8.81	28	59	

Notes: See Appendix for variable definitions.

Table 2.2: Descriptive statistics (farmers)

Variable	Group	Observations	Mean	Std. Dev.	Min.	Max.	Pr>F
$GARP_0$	control	21	11.1	5.94	0	20	0.720
	treatment	26	12.8	5.27	1	21	
	drop-out	21	11.7	6.37	1	22	
Education	control	21	3.5	3.09	0	9	0.313
	treatment	26	3.9	3.46	0	10	
	drop-out	21	3.6	3.09	0	10	
Experience	control	21	3.7	1.35	1	5	0.407
	treatment	26	3.8	0.98	2	5	
	drop-out	21	3.6	1.16	1	5	
Age	control	21	39.4	14.42	18	74	0.861
	treatment	26	46.6	14.16	20	68	
	drop-out	21	41.4	13.99	18	70	

Notes: See Appendix for variable definitions.

As is evident from the top rows of Tables 2.1 and 2.2, the average number of GARP violations during the baseline round ($GARP_0$) did not vary significantly across subgroups. The same is true for any other variable reflecting "personal characteristics," suggesting the random assignment into treatment was successful. In the fourth column we report the results of simple mean comparison tests for treated and control groups. Since there was considerable attrition among the farmers (see below), we also compare participant farmers and drop outs. There are also no significant differences (details not reported but available on request). Importantly, GARP violation levels also did not vary significantly across "types"—farmers or brokers (p=0.964). On the other hand, and not unexpected, we document some significant differences between brokers and farmers (see Figure A2 in the Appendix). Education, age and experience are all significantly different (at a 5% level). Brokers tend to have attained a slightly higher average education level (+2.05 years) and self-declare higher trading experience (+0.55). Farmers instead are on average older than brokers (7.61 years), and present a wider age range.

Next, we turn to the details of the treatment. The market treatment was inspired by Smith (1976), and consisted of an open outcry double auction similar to the trading floor of the ECX. Farmers and brokers were told that a given quantity of sesame sold previously by their cooperative had to be re-bargained that day, and that market participants could make extra-profits in accordance with the selling/buying prices that eventuated in the auction. The cost associated with the auction where thus modest (we only paid for the difference between the "market prices" and the formerly agreed price). The sesame had been sold collectively by the cooperative (circa 500 members), not by the farmers themselves. Farmers and brokers faced normal incentives to bargain, as their payoff depended on deviations (trading margins) from threshold values. The trading margin for

farmers was simply the bargained price minus 1.540 Birr (which was the price struck earlier by the cooperative).[35] The participating farmers were only allowed to sell 1 quintal per round, and were paid at the end of the experiment only the sum of the margins over the 6 rounds. The rest, as promised, would be paid through their cooperative. The participating brokers were told to buy 2 quintals per round, at the lowest price possible. For each round they would receive the difference between the buying price, and the threshold value of 1.565 Birr. Participating brokers were also offered a 300 Birr award, to cut opportunity costs, and had their bus trip fully organized and paid, to reduce transaction costs.

During this "fictitious real market" 144 quintals of sesame were sold, moving 222,437 Birr (US$ 17,794) to a modest cost for the experiment's finances. The average selling price was 1,545 Birr per quintal (US$ 123). Farmers sold on average 5,5 bags of 1 quintal of sesame, earning 8,555 Birr (US$ 684). Of this, only 26 Birr (US$ 2) per quintal was paid by the experimenters. Treated brokers earned 244 Birr (US$ 19) on average. No feedback was given to participants before, between or after the "market experiment" rounds. We therefore view learning as market-induced. After the treatment, brokers and farmers were asked to re-perform the GARP choice experiment, and again were handed the selected gift. Two days later the "control" group re-performed the GARP test. This ended the data collection process.[36]

[35] In addition, farmers were offered a 20 Birr fee to reward cooperation.

[36] Note there was a 5-day delay between the two rounds of GARP violations for the treated group and a 7-day delay for the control group. Insofar as there was learning in the first round of the experiment combined with a gradual "decay" of this knowledge over time, it is possible that these results conflate the effect of the treatment and more limited decay of knowledge due to the shorter time lag. However, we view the latter effect as relatively unimportant.

There is no relationship between the goods used to measure rationality violations (coffee, sugar and juice) and the good used in the market experiment (sesame). This approach follows Slonim (1999) and List and Millimet (2008), and aims to minimize the so-called "Learning by Rules of Thumb" (LRT) effect, enabling clean identification of the "Learning to be More Rational" (LMR) effect (Slonim, 1999). In artefactual field experiments it is important to discriminate between LRT and LMR if participants may learn to act more rational by applying a set of "rules of thumb" valid only for that particular context. It may be wrong to claim that applying such rules also produces a behavioral response in another – non-experimental – environment. Measuring the behavioral effects of exogenously induced market experience in a distinct market therefore represents a 'particularly demanding test of the impact of market experience on learning' (List and Millimet, 2008: p.2).

The core of our analysis is a set of regression models where we seek to explain (differences in) GARP violations by personal characteristics and the market treatment. We follow List and Millimet and will present the results of a series of models, nested in the following general equation:

$$GARP_{it} = \alpha + \beta Treat_{it} + \gamma X'_i + \pi[\phi(\theta_{it}\Phi(\theta_{it})] + \eta_{it} \quad (1)$$

where $GARP_{it}$ represents the number of GARP violations for subject $i = 1, \dots, N$ in round $t = 0{,}1$. $Treat_{it}$ is a dummy representing whether or not a subject participated in the market session; X'_i is a vector of personal characteristics including type (farmer or broker), age, education and self-assessed market experience level; ϕ represents the standard normal density function; Φ is the cumulative density function; $\theta_{it} = \zeta z_{it}$, where $(\Pr(GARP_{it}\ is\ observed) = \Phi(\zeta z_{it})$; and $\eta_{it} = \varepsilon_{it} + \mu_i$ is the error term composed by the idiosyncratic shock and the individual-specific terms. Imposing $\pi = 0$ and $\sigma_\mu^2 = 0$ (where σ_μ^2 is the variance of the random effects), equation (1) reduces to an OLS model. Imposing $\pi = 0$ reduces (1) to a GLS random effects (GLS-re) model, or a fixed effects (FE) model. Imposing $\sigma_\mu^2 = 0$ reduces (1) to the standard

Heckman selection model. Finally, $[\phi(\theta_{it}\Phi(\theta_{it})]$ is the inverse Mills' ratio, used to test and control for non-random attrition. In fact, while there was no attrition for the sample of brokers, 21 farmers failed to show up after the first round of GARP experiments (restricting the sample to 47 farmers for the panel). The Heckman model allows exploring whether non-random attrition of farmers might bias the results. We exclude farm size from vector X'_i and include it in the selection equation to ensure that the model is nonparametrically identified.[37]

Furthermore, we estimate some additional models to probe the robustness of our findings.[38] First, and exploiting the count nature of GARP violations data, we estimate a Poisson regression model, based on the assumption that the number of rationality violations is drawn from a Poisson distribution with parameter λ_{it}, so that:

$$\Pr(GARP_{i,t} = g_{it}) = \frac{\exp\{\lambda_{it}\}\lambda_{it}^{gt}}{g!_{it}} \quad (2)$$

where $g_{it} = 0,1,2,\ldots$, and $\ln(\lambda_{it} = \alpha + \beta Treat_{it} + \gamma X'_i)$ as above. Second, to account for the fact that there are many more outcomes with zero rationality violations than predicted by the Poisson model (see Figure 2 and Figure A2 in the Appendix), we also estimate a zero-inflated Poisson (ZIP), which extends (2). Finally, since random assignment may not necessarily create a credible counterfactual in case of small sample size, we also estimate a propensity score matching (PSM) model to estimate the "treatment effect on the treated".

[37] Farm size arguably captures an opportunity cost of time and thereby explains participation in the 2nd stage. In none of the models we have estimated do we find that farm size is (directly) correlated with behavior in the choice experiment.

[38] This empirical strategy closely follows List and Millimet and deviates in only a single aspect. Since our analysis does not suffer from non-compliance (or partial compliance) we do not estimate an instrumental variable model.

The small size of my sample implies the analysis has low power. To be precise and using my data for an ex-post assessment of the power: the pooled panel (brokers and farmers) has a power of only 0.7.[39] This implies treatment effects will have to be large to be picked up. Fortunately we were able to measure GARP violations and treatment participation without any error. Small sample size also casts doubts on the representativeness of the sample. However, we focus on two narrowly defined activities (sesame production and trade) in a specific region characterized by fairly homogeneous conditions. As always, the generalizability of field experimental data to other settings is hazardous, but we believe the current analysis nicely complements the data collected in North America by List and Millimet.

4.5 Results

We used a three-goods GARP violations test to determine the consistency of choices. Compared to two-good tests, this approach invites more frequent GARP violations. Indeed, compared to List and Millimet (2008) we find that a greater share of the farmers and brokers revealed at least one GARP violation during the first round.

> ***Result 1:*** *98.5% of the farmers and 81.8% of the brokers revealed at least one GARP violation in the first round of the experiment.*

Upon computing sample means and standard deviations we find that the difference in GARP violations between farmers and brokers disappears. Of course this may also reflect the low power of the test due to the small sample size.

[39] Power calculations were performed using STATA. Comparable results were achieved using the GPower 3.1 software.

> ***Result 2:*** *farmers and brokers did not show significant differences in the number of GARP violation levels in the first round of the experiment (t=-0.05; p=0.964).*

Importantly, we find no evidence that the 21 drop-out farmers are systematically different than their participating peers (recall that attrition cannot bias the results for brokers as there was zero attrition in that sample). For example, a simple t-test detects no significant difference in the number of $GARP_0$ violations for returning and drop-out farmers (P-value = 0.388). To probe further, we have also estimated probit models predicting the return of farmers, using the number of GARP violations in the first round as an explanatory variable. As it should be, this initial number of GARP violations does not enter significantly, neither when regressed alone (P-value = 0.385), nor as part of a multivariate model that includes a vector with "personal characteristics" (indeed; no variable was significant at a 5% level). Early analysis thus causes me to reject the hypothesis of non-random self-selection in the 2nd part of the experiment. We return to this in a more formal analysis below.

On average subjects exhibited 11.9 GARP violations during the first round. This number fell in the second round, both for the treated and control group. For the control group, this could suggest a learning effect from playing the experiment for the second time, but the reduction is not statistically significant (at 5%) and therefore may be attributed to random variation. For the treated group, instead, we pick up a significant treatment effect. During the second round the pooled average decreased to 10.4 for the non-treated and 6.8 for the treated subjects (Table 3). The difference between the reductions in GARP violations is statistically significant, implying that the pooled data picks up a treatment effect, despite the low power caused by the small sample. Subjects who have been exposed to the auction make significantly less rationality violations than their peers in the control group (Figure 3).

Table 3: Average GARP violations over time, per treatment and type

Type	Group	Observations	GARP violations		
			t=0	t=1	Change
Farmers	treatment	26	12.81	7.69	-5.12**
	control	21	11.10	9.95	-1.15
	drop-out	21	11.67		
Brokers	treatment	12	11.83	4.42	-7.41**
	control	10	12.20	11.20	-1.00

Notes: Confidence: *** ↔ 99%, ** ↔ 95%, * ↔ 90%.

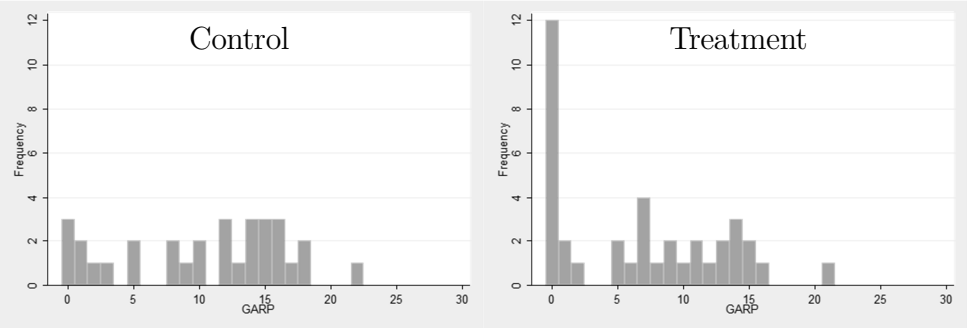


Figure 2: GARP violations of control and treatment groups at time 1

Next we report my main results; the estimation outcomes for the various regression models outlined above. These results are summarized in Table 4. The Treatment variable enters with a negative sign and is significant at the 5% level. This is true for random and fixed effects models (a Hausman test suggests the random effects model is preferred), the Poisson models, the Heckman selection model, and the propensity score matching model. Note that, consistent with the discussion above, there is no evidence to suspect that my analysis suffers from non-random attrition—the coefficient on the inverse Mills ratio is statistically insignificant. Taken together, we interpret this as robust evidence that exposure to a market treatment invites more rational behavior (as measured by the number of GARP violations). Interestingly, none of the personal characteristics significantly affects the number of GARP violations.

Table 4: GARP violations decrease after exogenous market experience

	GARP violations						
	OLS	OLS	GLS-re	Poisson	ZIP	Heckman	PSM
	All	All	All	All	Brokers	Farmers	All
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treatment	-4.913***	-4.913***	-5.298***	-0.553***	-0.625***	-4.077***	-4.13**
	(1.224)	(1.130)	(1.084)	(0.152)	(0.149)	(1.351)	(1.74)
Farmer	0.596	0.596	0.478	0.060			
	(1.236)	(1.603)	(1.425)	(0.159)			
Age	0.024	0.024	0.027	0.002	-0.004	0.074	
	(0.048)	(0.053)	(0.054)	(0.005)	(0.007)	(0.073)	
Education	-0.018	-0.018	-0.030	-0.002	-0.016	0.538*	
	(0.193)	(0.233)	(0.219)	(0.022)	(0.016)	(0.295)	
Experience	0.358	0.358	0.265	0.034	-0.061	0.079	
	(0.493)	(0.583)	(0.560)	(0.055)	(0.087)	(0.555)	
Mills Lambda						2.500	
						(7.738)	
Clustered s.e.	N	90	N	N	N	N	N
Observations	159	159	159	159	44	136	57
R^2	0.103	0.103	0.102				0.103

Notes: Standard errors in parenthesis. In (3) a Hausman test suggests that the random effects model is preferred to fixed effects (most efficient specification). The Vuong test for the Zero-Inflated-Poisson (5) resulted significant at the $p < 0.05$ level. The Heckman selection model (6) uses land size (ha), age, education and experience as exclusion restrictions in the first-stage selection equation. Propensity score matching (PSM) imposes the common support (12 observations dropped) and is estimated via probit; the propensity score includes type, age, education, experience and GARP violations at time 0; the standard errors of the PSM are obtained by bootstrap, using Epanechnikov Kernel matching. Confidence: *** ↔ 99%, ** ↔ 95%, * ↔ 90%.

> ***Result 3:*** *market experience and rational choice behavior are directly related: engaging in a competitive market reduced pooled GARP violations by 4.91 (OLS), or ¾ of a Standard Deviation.*

The magnitude of the treatment coefficient is larger than in the List and Millimet study. We speculate this may be due to two factors: first, our market treatment was more intense and salient—a full day of trading, in which US$ 17,794 worth of sesame was exchanged;[40] and second, the

[40] List and Millimet find that the treatment effect varies with the intensity of the treatment: those youths who fully complied with their treatment (i.e. started trading sports cards memorabilia beyond the parting gift provided by List and Millimet) had larger rationality gains than youths who only complied partially (sold their cards and

endline measurement was immediately after the treatment (rather than up to 7 months later). There is no reason to believe that the effect is due to discussions about the first round of the experiment by the respondents during the treatment. We carefully monitored the interaction of respondents during the treatment, and there is also no evidence to suggest that people talked about the choice experiment in the previous week.

Next, we report the results of a non-parametric permutation test to probe the robustness of the results. To obtain consistent standard errors we follow Bloom et al. (2011) and compute the Wei-Lachin test statistic:

$$T = \sum_{i=1}^{N} \sum_{j=1}^{N} Z_i(1 - Z_j)U_{ij} \tag{3}$$

Where N is the total number of respondents, subscripts i and j refer to specific respondents $i \neq j$; $Z_i = 1$ denotes that respondent i was assigned to the market treatment (so that $Z_i = 0$ indicates assignment to the control group), and the following holds:

$$U_{ij} = \begin{cases} +1 \; if \; GARP_i > GARP_j \\ -1 \; if \; GARP_i < GARP_j \\ 0 \; if \; GARP_i = GARP_j \end{cases} \tag{4}$$

This approach allows us to evaluate whether the difference in the number of GARP violations between the treated and control group respondents at $t = 1$ is greater than may be expected based on chance: under the null hypothesis of no treatment effect the treatment outcomes should not be systematically larger than the control outcomes. However, the associated p-value for post-market treatment is 0.0067, implying we can reject the null

never entered a sports cards show again). We speculate that the salience of the market treatment may have a similar effect as the intensity of trading.

hypothesis of no effect. The non-parametric findings are thus consistent with the parametric estimates.[41]

We proceed by considering farmers and brokers separately. To formally test whether there are statistically different models for farmers and brokers we interacted the farmer dummy with all regressors and did an F-tests. No interaction term entered significantly. The results from regressing the number of GARP violations on the treatment dummy and personal characteristics are summarized in Table 5. They are consistent with the findings derived from the pooled dataset.

> ***Result 4:*** *Both farmers and brokers separately reveal significant reductions in GARP violations after the market experience. Subjects revealing at least one GARP violation drop to 73.1% and 58.3%, respectively.*

The treatment coefficient is greater for traders than for farmers. Does this imply that traders learned more from participating in the experiment than the farmers? To further assess whether there is a difference in "learning rates" between farmers and traders, we ran an additional OLS regression for treated and non-treated subjects separately, controlling for type, age, education and experience (results not shown but available on request). The farmer type dummy entered significantly for the treated sample, but not for the control group. This is perhaps suggestive of a difference in learning rates across types, but in light of the low power due to small sample size we treat the lack of a significant effect for the control group as tentative and indicative only. In addition, while both brokers and farmers had to bargain on a competitive market, they played different roles in the market experiment (buying and selling sesame, respectively) and were confined by

[41] We have also computed the Wei-Lachin test statistic for the baseline dataset. Consistent with the earlier parametric results, here we cannot reject the null hypothesis (p=0.6521). Similar results are obtained when estimating a log-rank model.

Table 5: Market experience reduces GARP violations for farmers and brokers separately

	GARP violations			
	OLS Farmers	GLS-re Farmers	OLS Brokers	GLS-re Brokers
	(1)	(2)	(3)	(4)
Treatment	-4.052***	-4.406***	-6.675***	-7.083***
	(1.268)	(1.259)	(2.191)	(2.049)
Age	0.049	0.051	0.049	0.049
	(0.052)	(0.053)	(0.134)	(0.198)
Education	0.318	0.292	-0.632	-0.632
	(0.216)	(0.242)	(0.551)	(0.470)
Experience	-0.241	-0.300	2.148	2.116
	(0.578)	(0.561)	(1.358)	(1.689)
Clustered s.e.	68	N	22	N
Observations	115	115	44	44
R^2	0.084	0.084	0.289	0.288

Notes: Robust standard Errors are in parenthesis. Confidence: *** ↔ 99%, ** ↔ 95%, * ↔ 90%.

different "rules." It is of course possible that any behavioral difference for the two groups is caused by these factors.

Finally, we test whether better "bargainers" experienced a greater drop in GARP violations. We find tentative support for this hypothesis. In a model that explains changes in GARP violations, we insert the subset of brokers and farmers earning above median profits as a separate regressor. The associated coefficients are of the expected sign, but not robustly significant. Specifically, due to small sample size we don't detect a significant effect for brokers, but we do for farmers. This suggests some weak support for the hypothesis that subjects more actively engaged in the competitive market setting are characterized by higher learning rates.

4.6 Conclusions

In this study, we explore whether market experience affects the rationality of choices made by a sample of sesame farmers and brokers. Following earlier work by List and Millimet (2008), we provide new evidence of such endogenous rationality. Markets are not only neutral institutions to efficiently allocate resources among participants. They also improve the

rationality of the decision-making process of participants, introducing the possibility of dynamic efficiency gains associated with market expansion in developing countries. Compared to the earlier study by List and Millimet, our study was carried out in a radically different context (a different country and continent, culture and respondent pool, and institutions), with very different subjects, and with a treatment that was much more salient to the participants (involving sesame trading, which is at the core of their livelihood strategy, and introducing a trading institution that arguably will reshape the sesame market in the coming years). The fact that the results are quite consistent across studies provides support for the external validity of the notion of endogenous rationality.

Our first result is that behavior of the vast majority of the subjects in the baseline study (prior to the treatment) is not consistent with assumptions regarding the straw man *Homo Economicus*. The main result is that our subjects respond to a single-day trading session, where they were engaged in competitive market behavior, by reducing the number of GARP violations by three- fourths of a standard deviation. Somewhat surprisingly, given the difference in trading experiences across these groups, farmers and brokers performed similarly on the baseline choice consistency test. We conjecture that brokers seemingly do not outperform farmers – as observed in the domain of sports card trading, for example – by the fact that sesame trading in the Ethiopian countryside does not take place in a setting resembling a competitive market. Personal relations matter more than responding quickly to price margins.

Our data do not allow us to examine the (evolution of) efficiency of the double cry auction market (this requires the provision of induced values to farmers and brokers, resulting in well-defined upward-sloping supply curves and downward-sloping demand curves). However, it is possible to say something about the distribution of rents across our groups of farmers and brokers. Figure 4 provides the bargained price over the six trading rounds,

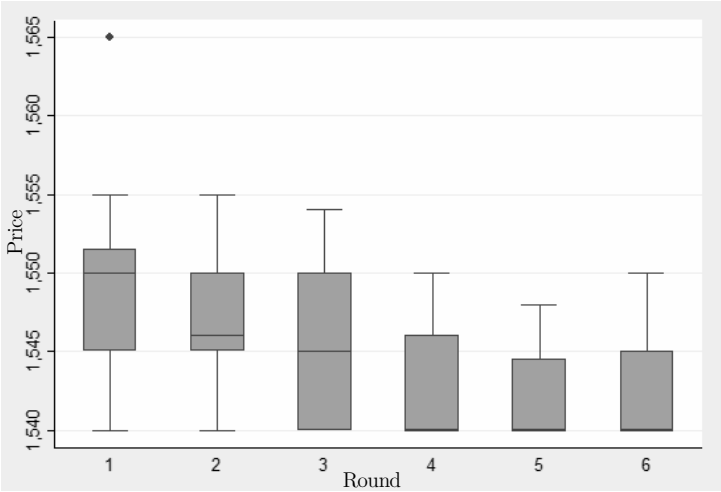


Figure 4: Trading rounds and bargained prices

and after three rounds, the median price has been bargained down to the reservation price of the farmers (1,540 birr). This suggests that, over time, buyers were able to secure most of the rents associated with the trading.

In an era of rapid market liberalizations, an expanding role of the market in the life of people might lead to efficiency gains, reducing deadweight losses due to noncompetitive exchanges. While Akerlof and Yellen (1985), among others, found that deviations from rational behavior may have little impact on general economic equilibria, there may be significant effects at the micro level. In a competitive environment, different levels of rationality across groups in society, may affect the distribution of wealth. This phenomenon could have an impact also at a micro as well as regional level, affecting regional growth rates. For example, while the ECX was initiated to improve the performance of the commodity value chain, an unexpected side effect may be a shift in the distribution of rents from one group of actors to another in response to differential ability to respond to incentives and margins. Obviously, these statements should be interpreted as speculative at this point; since market-induced rationality may affect many spheres of decision making, including ones not explored in this study, it is difficult to predict how market exposure will affect well-being.

Differentiated endogenous rationality poses new challenges to policy makers in terms of economic policy and reforms, requiring more attention to the dynamic effects of partial equilibria and rent distribution. In general, if the (trading) context determines the rationality of choice, affecting the behavior of agents, then the dynamic effects of policies aimed at accelerating or decelerating market expansion are very difficult to assess, ex ante. If so, the welfare effects of many policies aimed at stimulating development may be difficult to gauge.

Appendix

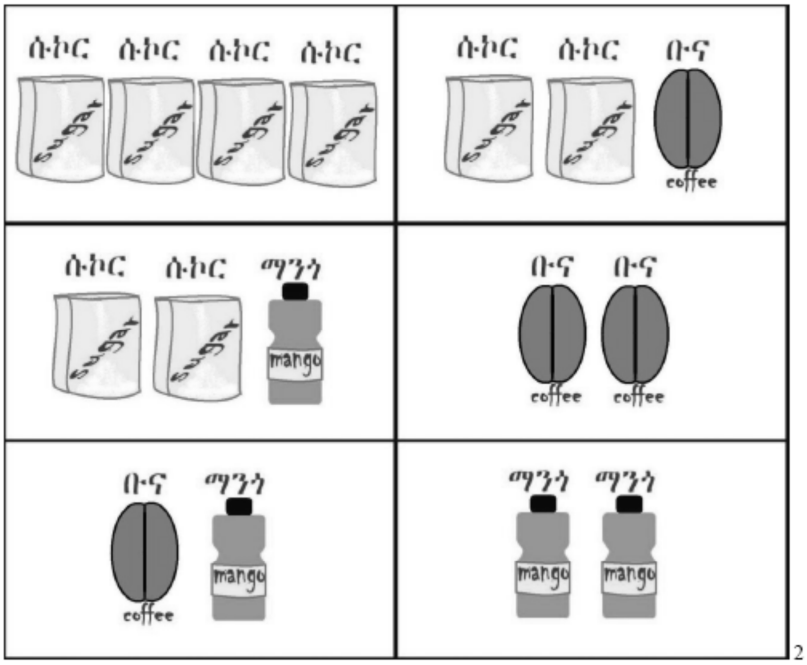
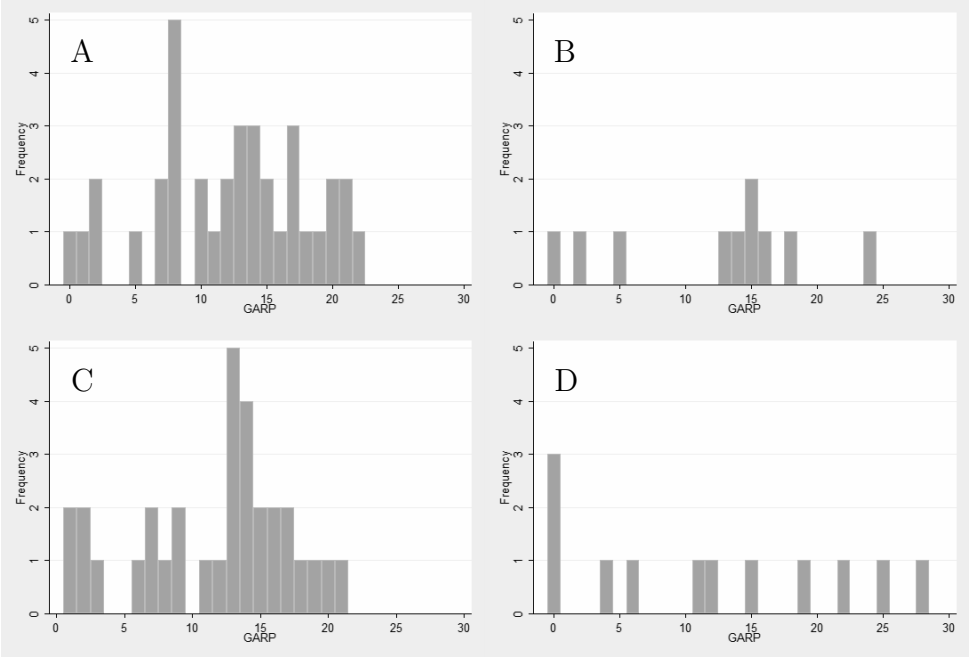


Figure A1: Choice set example (sheet 2)



A: Control farmers B: Control brokers; C: Farmers to be treated; D: Brokers to be treated

Figure A2**:** GARP violation levels across types and treatment selection at T=0

Variable definitions:

GARP violations. The number of GARP violations that responded i made in a simple choice experiment with 3 goods.

Education. The number of completed years of education of respondent i.

Age. Age of respondent i in years, rounded down to the last birthday.

Experience. Individual level self-evaluation index expressing the "market experience" of respondent i on a scale from 1 (no experience) to 5 (most experienced).

Land. Land owned by farmer in Hectares (ha). Ownership here does not imply property rights/land titles.

Farmer. Individual level dummy variable taking value of unity if the i-th respondent was a farmer, 0 if he was a broker.

CHAPTER 5
Formal Insurance and the Dynamics of Social Capital

Experimental Evidence from Uganda

Abstract

We explore how the introduction of formal insurance affects the within-village dynamics of social capital in south-western Uganda. Consistent with existing evidence, our data suggests formal insurance crowds-out social capital—proxied by contributions in a public goods game. However, it is not those adopting the formal insurance who reduce their contributions. Instead, social capital erodes because of the uninsured. This is consistent with "weapons of the weak" theories, emphasizing social embeddedness. As informal sharing networks start to unravel, those unable to benefit from formal insurance fear they will lose out, and use the public goods game to signal their dismay.

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5.1 Introduction

Institutions are constraints devised by humans to shape human interaction (North, 1990). This broad definition captures both formal and informal institutions, where the former are often treated as exogenous constraints, enforced by an outside party (possibly "the state"), and the latter are endogenous constraints—self-enforcing rules representing the sub-game perfect equilibrium of a repeated game (Greif, 1993; Aoki, 2001a, 2001b). While recent empirical work has identified institutions as a key determinant of economic performance (Rodrik et al., 2004), much remains unknown about how institutions evolve over time, or about the interaction between different (types of) institutions.

In this paper we ask how formal institutions affect informal ones. This topic is gaining importance as, globally, systems of formal institutions are expanding. For example, global value chains are penetrating further into societies heretofore oriented towards subsistence activities; the formalization of land rights increasingly affects customary institutional tenure arrangements; and the expanding reach of the state and formal court system is altering informal judicial institutions. The current wave of experimentation with (index) insurance products in environments characterized by informal sharing arrangements represents another example. While some theoretical work exists to analyze how (exogenous) changes in formal institutions affect the equilibrium of repeated games (for example, Aoki 2001b), empirical work in this domain remains very scarce. Roland (2004) describes the interaction between slow- and fast-moving institutions and argues that, depending on the context, specific institutions may be complements or substitutes. Institutional innovations in one domain may therefore crowd out, or solidify, existing institutional arrangements.[42]

[42] For recent experimental work along these lines, refer to Chandrasekhar et al. (2014).

In this paper we seek to enhance our understanding of the impact of a specific formal institutional innovation on local cooperation ("social capital") using experimental methods. We focus on the provision of insurance—a domain relatively well-studied by economists. Rural producers in developing countries are exposed to various shocks, and typically are better off when pooling their risks (in particular when the co-variation of risks across individuals is modest or absent). Since, until recently, prohibitive transaction costs typically precluded the writing of formal insurance contracts, rural producers by and large depended on informal insurance arrangements to secure their livelihoods (Townsend, 1994; Udry, 1994; De Weerdt and Fafchamps, 2011). Such sharing could take various forms, including redistribution within friendship or kinship networks (Fafchamps and Gubert, 2007; Alger and Weibull, 2010), or transfers in patron-client relationships (Richards, 1996). In recent years, however, various agencies have experimented with the provision of formal insurance for rural households, based on written contracts and possibly outside enforcement. This development was facilitated by improvements in communication technology, lowering transaction costs, but also by the creation of "new" insurance products such as index insurance.

Insofar as formal and informal insurance are "substitutes", one might expect that expansion of formal insurance possibilities will crowd out informal insurance arrangements. A small literature seeks to empirically test this hypothesis, providing some support for it. Dercon and Krishnan (2003) find that public transfers in the form of food aid crowd out informal sharing in rural Ethiopia.[43] Landmann et al. (2012) use data from the Philippines to show that formal insurance lowers voluntary transfers among members in social networks. Klohn and Strupat (2013) examine the link between the provision of formal health insurance and informal

[43] Also see Bahre (2011), who examines the relation between formal financial arrangements and personal networks in post-apartheid South Africa, finding that increasing redistribution created frictions within networks.

transfers in Ghana, and find that formal insurance reduces both the probability of making transfers as well as amounts transferred. Lin et al. (2014) confirm the crowding out thesis in a laboratory setting.[44]

The main objective of this paper is to empirically explore how the introduction of formal health insurance affects within-village social capital dynamics in a sample of Ugandan villages. We seek to extend evidence reported by Klohn and Strupat (2013), the only other paper considering the implications of formal health insurance for informal institutions. To this end, we first compare the behavior of villagers with and without access to formal health insurance (i.e. an intention-to-treat effect at the village level). We proceed by disentangling behavior of adopters and non-adopters. Unlike existing work, we consider a voluntary insurance program with imperfect uptake—allowing us to probe *intra*-village implications. Moreover, we do not use informal transfers as our measure of social capital. Instead, our main dependent variable is based on behavior in public good (PG) games. The PG game captures the ability of communities to coordinate on first-best outcomes, and represents a well-known measure of social capital at the village level (e.g. Fearon et al., 2009). Following Klohn and Strupat (2013) and Morten (2013), one may hypothesize that formally insured village members depend less on their fellow villagers to sustain their livelihoods and, as a consequence, invest less in social relations.[45] If so, the result will be erosion of social capital, translating into lower contributions to the local public good.

[44] A related literature concerns the effects of migration (remittances) as a mechanism to provide insurance. For example, Morten (2013) establishes that temporary migration decreases informal risk sharing.

[45] Of course there are other reasons why the introduction of formal insurance (or storage) possibilities may cause adopters to opt out of informal institutions or networks. Klohn and Strupat (2013) discuss that formal institutions may crowd out altruistic behavior (see Bowles, 2008), or may reduce (the bite of) social sanctions associated with exit (see Grimm et al., 2013).

In areas with access to formal insurance, we find that uptake is far from uniform, and skewed towards the sub-sample of wealthier villagers. We also find that average contributions to the public good are indeed lower in areas with a formal insurance system in place. These results corroborate predictions of the game theoretical paradigm (outlined below), as well as the empirical findings mentioned above. Next, we zoom in on within-village social capital dynamics, documenting patterns in the data that are harder to explain. We find that lower aggregate contributions to the public good in areas with access to the formal insurance are *not* due to the withdrawal of insurance adopters. Instead, reduced average contributions are explained by declining contributions of non-adopters. We speculate this behavior serves as a signal of their displeasure of being left behind, after the wealthy threaten to opt out of informal sharing networks. If formal insurance is only available to a (wealthy) sub-sample of the population, who could then choose to exit informal insurance networks, the villagers left behind may lose. They would unambiguously be worse off if their informal insurance options deteriorate while they cannot benefit from formal insurance.

The interpretation about reduced contributions as a signal, or warning, is consistent with insights of the sociologist Mark Granovetter (1985), who emphasizes the importance of "social embeddedness" (i.e. social interaction beyond economic exchange), and supports insights of the anthropologist James Scott (1985), who studied the consequences of mechanization for social relations in a rice producing village in Malaysia. Scott coined the phrase "weapons of the weak" to describe how the poor resisted their degradation in the village hierarchy and culture. We return to these models below.

This paper is organized as follows. In section 5.2 we outline and illustrate the conventional economic perspective on the evolution of informal institutions (in response to changes in the broader environment). We then complement this perspective with the "weapons of the weak" argument advanced by Scott (1985). In section 5.3 we provide context regarding the

case study, and describe the insurance intervention as implemented by a local NGO. In section 5.4 we summarize our data, outline our identification strategy, and formulate four research questions. Section 5.5 contains our empirical results, and the conclusions ensue.

5.2 Formal and informal institutions

We first outline a simple economic perspective on the interaction between formal and informal institutions, which focuses on consequences in a specific domain of interest (typically related to some form of exchange). This perspective lends itself naturally to game-theoretic analysis, and economists have developed a coherent framework in which informal institutions are seen as the equilibrium outcomes of repeated interactions—outcomes that are persistent over time, shaping expectations and gradually evolving into norms of appropriate behavior. See, for example, Aoki (2001b) for a treatment of institutions as endogenous and self-enforcing equilibrium outcomes of a repeated game.

Economists have studied the evolution of informal insurance arrangements as alternative insurance opportunities emerge. Informal insurance via gifts and transfers occur within networks of family members (or friends) because of altruism, and may involve support to deal with persistent shocks (chronic illness or disability—see De Weerdt and Fafchamps, 2011). Informal insurance may also be motivated by expected reciprocity among individuals (or households) in a context of repeated interaction (Kimball, 1988; Coate and Ravallion, 1993; Ligon et al., 2002; De Weerdt and Fafchamps, 2011). The theoretical literature has emphasized that such non-altruistic sharing arrangements should be self-enforcing; individuals are willing to help others facing a temporary shock because of the credible promise of reciprocity in the future (so that participation constraints automatically limit the extent of risk sharing that is possible). The voluntary participation constraint for individual i may be written as:

$$u_{it}(c_{it} - h_{it}) - u_{it}\left(c_{it} + T_{ij} - h_{it}\right)_{it} \leq E_{t|hit,hjt}\left[\sum_{s=1}^{\infty} \beta^s \{u_{it}(c_{it+s} + T_{ijt+s} - h_{it+s}) - u_{it}(c_{it+s} - h_{it+s})\}\right] + A_{ij} \quad (1)$$

where u_{it} denotes concave utility of individual i at time t; c_{it} denotes consumption; $T_{ij}$is a positive or negative transfer from i to j;[46] h denotes the value of a health shock; E is the expectations operator; β is the (common) discount factor; and A_{ij} is a measure of altruism, or the utility obtained by individual i from helping individual j. The left hand side of (1) captures the temptation from reneging on a sharing obligation, or the immediate gain in utility from not transferring the transfer T_{ij} to individual j. This short-term benefit should be balanced against the loss of foregoing the (expected) potential benefits from sharing in the future (in all periods s that follow).

How does the emergence of formal insurance affect the participation constraint? An individual who is expelled from the insurance network now has the option to adopt formal insurance – paying a fixed fee τ every period and receiving payment F_{is} in case of a health shock. This implies we can rewrite (1) as:

$$u_{it}(c_{it} - h_{it}) - u_{it}\left(c_{it} + T_{ijt} - h_{it}\right)_{it} \leq E_{t|hit,hjt}\left[\sum_{s=1}^{\infty} \beta^s \{u_{it}(c_{it+s} + T_{ijt+s} - h_{it+s}) - u_{it}(c_{it+s} - \tau + F_{is} - h_{it+s})\}\right] + A_{ij} \quad (2)$$

Since an actuarially fair insurance product improves the autarky outcome of adopters in the future by facilitating consumption smoothing, it decreases the right-hand side of (2) with respect to (1). This implies the set of self-enforcing informal insurance transfers decreases (which may be inconsequential for sufficiently large values of A, or if the self-enforcement constraint does not "bind"). Intuitively, since individuals have access to a substitute insurance product tomorrow (other than transfers from peers), it is more tempting to renege on obligations today and opt out of the

[46] T_{ij} represents a specific transfer from a menu, depending on the realization of idiosyncratic health shocks to individuals i and j. Here we consider the case of $T_{ij} < 0$ in the present period.

network, so that only a menu of small transfers can now be supported as an equilibrium. As a result, insurance options for non-adopters deteriorate.

The main message is that substitute insurance mechanisms lower the value attached to informal sharing arrangements (for those able to adopt the substitute). Substitutes could be formal insurance, as above, but alternatives exist. For example, Ligon et al. (2002) focus on the implications of self-insurance via storage, which strictly increases the value of autarky. As above, individuals who were previously (almost) indifferent between participating in the sharing network and autarky will now renege when they have the option to store, and subsequently opt out of the network. While their welfare improves as a result, the utility of individuals remaining in the network decreases unambiguously. They are worse off because the network shrinks and loses part of its ability to absorb shocks. Ligon et al. (2002) demonstrate that introducing the possibility of storage may even reduce overall welfare. Migration (remittances) may provide yet an alternative substitute mechanism to protect households from health shocks, and Morten (2013) demonstrates that migration tends to decrease insurance provided via informal networks.

The economic perspective thus proposes that the expansion of formal institutions, insofar as they provide a substitute for informal institutions, may undermine these informal institutions. The economic system shifts from one sub-game perfect equilibrium to another, reflecting the new choice sets for economic agents. In particular, "adopters" of the formal institution may opt out of pre-existing arrangements. In the process, distributional issues emerge and net welfare may decrease as a result. Is this a complete characterization of the evolution of societies?

A broader perspective emerges if we recognize that economic agents are social creatures that also interact in other domains than the economic one. In the words of sociologist Granovetter (1985), economic transactions are "socially embedded". Aoki writes that 'the economic transaction domain is embedded in a social exchange domain in which the same members

repeatedly interact socially and invest in, and enjoy returns from, social capital' (2001a: p.98). This perspective can be worked out formally using game theory, by linking multiple games and expanding the payoff structure. For example, Aoki (2001a) demonstrates that linking an economic exchange and social interaction game may expand the set of equilibria that can be supported in the exchange game.

There are alternative approaches to study social embeddedness. For example, the anthropologist Scott (1985) uses a descriptive approach to study the consequences of mechanization in rice farming in a Malay village—a process inviting consolidation of farms (for example, by inviting landowners to start reneging on long-term tenure arrangements) and dramatically reducing demand for hired labor. The result was "proletarianization" of small farmers and landless laborers—the creation of an underclass of society members whose well-being was increasingly inconsequential for upper strata of society (and whose interests were increasingly ignored). This is the unraveling of social networks caused by selective exit of the privileged (that is, the adopters), more or less along the lines discussed above. However, the story does not stop here. Scott demonstrates that "losers" in one sphere of interaction (for example, the labor market) are fully aware of their situation, resent it, and seek to remind the adopters of their historical responsibilities and social obligations by retaliation in other spheres of interaction—including everyday social life.

Scott (1985) investigates such strategies in detail. Peasants recognize their limited (economic, political and symbolic) power, and typically prefer non-rebellious and non-revolutionary acts of resistance. Typical "weapons of the weak" consist of low-key recalcitrance, foot dragging, dissimulation, false compliance, pilfering, feigned ignorance, chicanery, slander, arson, and acts of minor sabotage. Resistance strategies range from gossip and character assassination to strikes (such as when machines break down and landowners suddenly need labor to harvest their fields) and boycotts of

social activities such as weddings and political rallies organized by those well-off. More in general, resistance signals a reduced overall tendency to cooperate. This indicates a deterioration of local levels of social capital, undermining the ability of communities to coordinate on first-best outcomes.

Do these insights extend from the context of Malaysia to Africa, and from the context of mechanization to insurance? In what follows we test whether formal insurance affects voluntary contributions of villagers to the local public good—distinguishing between those adopting insurance and those not adopting insurance, faced with a potentially shrinking informal network. To empirically probe this issue we organized behavioral games in a sample of rural Ugandan communities.

5.3 The case study: health insurance in Uganda

Approximately half the population in Uganda is below 14 years of age, and this population is expected to double in size in the next twenty years (UNESA, 2012). To help sustain strong post-war economic recovery during the last decade, Uganda is increasingly focusing on health interventions and the accumulation of human capital (World Bank, 2011).

We focus on Kitagata sub-county, located in the South-Western Sub-region of Uganda. A local not-for-profit organization called Save for Health Uganda (SHU) has recently implemented a health micro-insurance project across four parishes in Kitagata, with the ultimate mid-term goal of covering the entire sub-county.[47] In what follows, we refer to households from these parishes as having "access" to formal health insurance—in these parishes households actively decided whether to adopt insurance, or not. Each village in the covered parishes is encouraged and facilitated to create

[47] Since then, Kitagata Sub-county has been administratively divided into two sub-counties: Kitagata and Kasaana. SHU however keeps working in both sub-counties with a single program.

a so-called Community Health Financing (CHF) scheme, which provides health insurance to member families. Members receive an insurance card, allowing access to all services provided by the contracted facilities.[48] These services include transport to the hospital in case of delivery, antenatal care, outpatient services, admission services and surgery.

Although there was slight variation with respect to the starting date of village level schemes, SHU required villages within the same parish to begin simultaneously, ensuring relatively homogeneous uptake rates. When we collected our data, in August 2012, all the randomly selected villages from areas with access to the insurance had a running scheme, to which approximately 53% of the families had subscribed. The great majority of the villagers in access areas (some 95%) indicated to be familiar with CHF schemes, while in parishes without access to the formal insurance around 56% of the people interviewed had heard about the schemes, but were aware that it was not yet available to them (yet).[49] On average, member households were expected to pay just above 26,000 USh (or $10) to cover the yearly insurance premium, with some variation depending on village and family size. Premiums were not sufficient to cover insurance costs, which are subsidized by international donors and sponsors. Nonetheless, only 18% of the participating families had been able to pay the full yearly premium to that date, with average payment rates hovering around 34%.[50] Families that have not fully paid the premium do not yet qualify for compensation; so many families currently have one foot in the formal system but also need to continue investing in informal arrangements.

[48] One private hospital and two public healthcare facilities.

[49] Notably, SHU had already undertaken informational and sensitization meetings in 2 parishes in no access areas, at the time of our fieldwork and, while the scheme was neither active nor running, the insurance was scheduled to start there in the near future.

[50] Dekker and Wilms (2010) find that participants to a similar private health-insurance scheme in five rural and two urban communities in Uganda face comparable difficulties in paying the premium: only 37% of participating households were able to pay.

We believe there is considerable demand for formal health insurance schemes, but that paying for the premium remains a challenge for many families. The latter is confirmed by many open-ended exit interviews. While we did not collect hard evidence on the pre-existence of informal social arrangements of mutual insurance, anecdotes complement the (qualitative) literature on informal arrangements in Uganda (e.g. Taylor et al., 1996; McDonald et al., 1999), stressing the importance of relatives, neighbors and friends in providing financial support in times of hardship.[51] We interpret this context as one of being in institutional flux, in which a considerable share of the local population appears "in limbo" between alternative insurance modalities, as one in which "signaling" by discontent fellow villagers may be effective.

5.4 Experimental identification, data, and design

The health insurance scheme was implemented according to a pipeline approach: while the ultimate aim is to establish a running CHF in every parish, logistical constraints forced the implementing agency to gradually roll out their intervention. Some parishes are therefore treated earlier than others, and we refer to households living in parishes where the intervention had not taken place yet as not having access to formal insurance. Strictly speaking, the selection of (early) access parishes was arbitrary and did not follow an explicit randomization procedure, but not surprisingly access and no-access households are very similar across almost all dimensions we measured. Abundant anecdotal evidence suggests access parishes are comparable to no-access parishes, and that there are more differences between villages within the same parish, than between parishes. Moreover,

[51] An open-ended questionnaire highlighted that people help each other in times of illness, contributing to hospital bills and helping the family financially though donations of food and money, or by transporting sick persons to the hospital. In the words of one respondent, 'here the story is that people help each other [...] when they are sick, and in case of unlucky events.'

villages with access are not significantly different from their counterparts in terms of total number of households, distance and time from a major road, and from the Sub-County headquarters (see Table 1).[52]

We randomly selected 21 villages from three access parishes, and another 23 villages from five neighboring parishes (in what follows: no-access villages). Four of these parishes are located in the same sub-county as the access parishes (Kitagata), and one parish containing five villages is located in neighboring Mitooma sub-county, bordering the largest parish with an active insurance scheme. In each village we first constructed a census of all households, and then randomly selected 10 households per village. After an extensive household survey, we randomly selected one adult family member to participate in our lab-in-field experiments.[53] We have no relation with the NGO offering the insurance product, and to avoid gift exchange and demand effects we did not emphasize the insurance program in our sessions.

Table 1 summarizes the household data, split out between the access and no-access households. We first establish that access and no-access respondents are indistinguishable according to most socio-economic dimensions—the two groups appear balanced. This is consistent with information given to us by the implementing NGO—they did not use a specific targeting rule when prioritizing villages to enroll in the program.

[52] Both distance (Km) and time (minutes) where measured following the route most commonly used by locals. We used a motorbike taxi as reference for time, as it represents the most commonly used motorized vehicle by locals, as well as the means of transport typically used by SHU extension agents.

[53] Enumerators made a list of each eligible family member before randomly selecting one name through a transparent ballot. Households were informed that the selected participant would be the only accepted household representative in the experiment, and that failure to comply with the rule would result in the exclusion of the household from the study. 44,5% of selected participants where female (see Table 1) and compliance averaged 93%. The plausible non-random attrition bias is discussed in section 5.

Table 1: Group means of socio-economic variables

Variable	No-access		Access		Diff.	Std. Err.
	N	Mean	N	Mean		
Village size (households)	23	65.13	21	58.05	-7.08	4.27
Time to main road	23	6.13	21	7.24	1.11	1.56
Distance from main road	23	2.33	21	2.47	0.14	0.66
Time to sub-county town	23	15.70	21	14.67	-1.03	3.18
Distance from sub-county town	23	5.50	21	4.31	-1.19	0.89
Female participant	230	0.47	210	0.42	-0.05	0.05
Single	216	0.05	207	0.03	-0.02	0.02
Married/Engaged	216	0.79	207	0.79	0.00	0.04
Widowed	216	0.15	207	0.16	0.01	0.02
Male household head	230	0.75	210	0.76	-0.01	0.04
Age household head	216	46.61	199	50.23	3.61**	1.54
Education household head	230	5.31	209	4.95	-0.36	0.41
Household size	230	6.13	210	6.05	-0.08	0.23
Mothers in house	230	0.74	210	0.79	0.05	0.41
Watch TV weekly	230	0.87	210	0.88	0.01	0.03
Read newspaper weekly	230	0.24	210	0.24	0.00	0.04
Radio	230	0.91	210	0.91	0.00	0.03
Phone	230	0.84	210	0.78	-0.06	0.04
Bicycle	230	0.45	210	0.42	-0.03	0.05
Motorbike	230	0.15	210	0.11	-0.04	0.03
Television	230	0.04	210	0.04	0.00	0.02
Car	230	0.04	210	0.04	0.00	0.02
Generator	230	0.02	210	0.03	0.01	0.01
Wealth factor	230	0.04	210	-0.05	-0.09	0.08
House features index	230	1.40	210	1.35	0.05	0.08
Common assets index	230	2.20	210	2.11	0.05	0.08

Notes: Confidence: *** ↔ 99%, ** ↔ 95%, * ↔ 90%.

Age of the household head is the only variable that differs, but this difference is small and presumably caused by chance. A key variable for our purposes is the so-called wealth factor. This variable is constructed by factoring out a wealth index from a series of dummy variables related to asset ownership (that is, phone, radio, television, generator, bicycle, motorbike and car) and house features (iron roofing, brick walls and cement floors) locally perceived as primary indicators of wealth. The average asset index score is not significantly different in access and no-

access villages. Also, for the full range of socio-economic variables, bias-corrected variance estimates within parishes are greater than those across parishes.

In terms of analysis, we first seek to explain variation in adoption of health insurance in access villages. Exit surveys identified lack of financial resources as the most frequent reason for opting-out of the insurance scheme. We therefore expect wealthy households to be more likely to join the scheme, creating the sort of within-village divide described by Scott (1985)—non-random adoption of the new technology based on income or wealth. On the other hand, since health status is also likely to vary with wealth, demand for insurance may also be greater among the non-wealthy. Ultimately the distribution of effective demand for formal insurance across social groups is an empirical question and using the sub-sample of treated parishes (villages), we estimate the following probit models:

$$\Pr(Insured_i = 1 | Wealth_i) = \alpha + \beta_1 Wealth_i + \beta_1 Wealth_i^2 + \varepsilon_i \quad (3)$$

$$\Pr(Insured_i = 1 | Poor_i) = \alpha + \beta Poor_i + \gamma X'_i + \varepsilon_i \quad (4)$$

where $Insured_i$ is a dummy for insurance uptake by household i; $Wealth_i$ is a factor of wealth (based on a range of assets, discussed above), $Poor_i$ is a poverty dummy, and X'_i is a vector of individual and household characteristics. The poverty dummy takes a value of one when a household does not own more than one of the three most common assets (phone, radio, bicycle) and not more than one improved house features (iron roof, brick wall, cement floor). In words, we explain adoption status by variables measuring wealth or poverty. Our first research question reads as follows:

> **RQ1**: *Are wealthy households more likely to pick up health insurance than poor ones?*

Next, we are interested in explaining the effect of (non-random) adoption of formal insurance on village-level social capital. To construct a measure of social capital, we follow Fearon et al (2009) and organized a lab-in-field experiment, or a standard public goods (PG) game with five participants.

A PG game captures the ability of groups of respondents to overcome free riding incentives. It likely also picks up altruism and trust (as respondents want to avoid outcomes where they feel others have taken advantage of them), as well as the extent to which norms of sharing and cooperation are internalized by villagers. These issues speak to different dimensions of (cognitive) social capital.

We randomly divided our 10 participants from each village into two groups of 5 individuals, carefully explained the rules of the game, and played a trial round. We gave five tokens to each participant, who could then anonymously contribute any integer amount into the common pot. After participants made their contributions, we doubled the amount in the pot and split this amount equally across all group members. As is well-known, the joint surplus is maximized when all participants contribute the full amount to the pot, but the privately optimal contribution (the Nash equilibrium) is to give nothing. After the trial round, participants were informed that they would play the game an unknown number of times (up to a maximum of 5), and that their pay-out at the end of the game would be based on a randomly selected round (determined by a simple lottery).[54] Payoffs for all participants were based on that same round, and averaged around USD 2.20—or about 2/3 of daily rural household incomes in the Western region, as estimated by the Ugandan Bureau of Statistics.[55] On average, participants without access to formal insurance shared 2.89 tokens, against the 2.60 tokens shared by adopters, and 2.39 by non-adopters with access. Similarly, only 25.5% of the participants with access

[54] In the absence of group reshuffling, game dynamics of reputation, retaliation and learning are expected to drive contribution decisions in repeated games (Rand et al., 2009). Our intent, however, is to proxy real life dynamics. For this reason our analysis is based solely on the choices performed during the first round of each game version. Successive iterations were performed so that defection is not necessarily the only Nash equilibrium present: players may cooperate on an equilibrium path, provided they are sufficiently interested in future outcomes (Dal Bó, 2005). For the same reason, the exact number of repetitions was not revealed to participants.

[55] http://www.ubos.org/UNHS0910/chapter7.Average%20Monthly%20Household%20Income.html

made a "high contribution" to the common pot (4 or 5 tokens) against 34.8% of those without access (see Figure 1).

If formal insurance crowds out informal insurance and reduces cooperation at the local level, then we would expect aggregate (or average) contributions to the common pot to be lower for respondents with access to the formal insurance scheme. Thus, we seek to pick up something resembling an intention-to-treat effect (as did Klohn and Strupat, 2013) and estimate the following two models:

$$Shared_i = \alpha + \beta Access_i + \pi[\phi(\theta_i \Phi(\theta_i)] + \varepsilon_i \quad (5)$$

$$\Pr(ShareHi_i | Insurance_i = 1) = \alpha + \beta Access_i + \varepsilon_i \quad (6)$$

where $Shared_i$ represents the number of tokens shared in the common pot, $Access_i$ is a dummy taking value 1 for respondents living in access villages, ϕ represents the standard normal density function; Φ is the cumulative density function, and $\theta_i = \zeta z_i$, where $\Pr(Shared_i \text{ is observed}) = \Phi(\zeta z_i)$—a standard Heckman Selection Model that can control for plausible non-random attrition in the experimental sample. In (5), β measures an intention to treat effect. In (6), $ShareHi_i$ is a dummy for having shared a high amount (that is: 4 or 5 tokens), so this model is estimated using a Probit specification. In words, by estimating (5) and (6) we ask:

> **RQ 2**: *Is the possibility to access a formal insurance associated with lower average contributions to the public good in a PG game?*

We now analyze contributions to the common pot more closely. The conventional economic perspective on the evolution of informal insurance suggests that formal insurance provides a substitute mechanism for the wealthy, who could subsequently choose to opt out of existing informal arrangements and cease cooperation with fellow villagers. The "weapons of

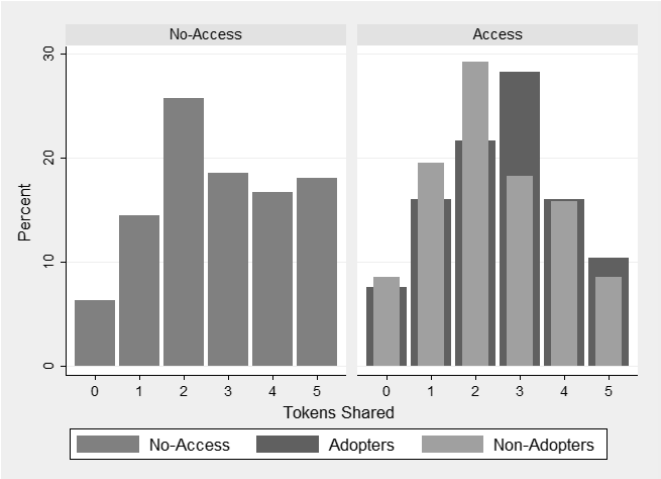

Figure 1: Sharing decisions of participants with and without access

the weak" thesis proposes that disgruntled non-adopters signal their dismay at the newly-created dichotomy in the village, which threatens their ability to pool future risks. In other words, both adopters and non-adopters may reduce their contributions to the public good. We estimate the following 'naïve' models, which resemble (5) and (6) above but distinguish between adopters and non-adopters:

$$Shared_i = \alpha + \beta_1 Adopter_i + \beta_2 Nonadopter_i + \varepsilon_i \quad (7)$$

$$\Pr(ShareHi_i = 1 | Adopter_i, Nonadopter_i) = \alpha + \beta_1 Adopter_i + \beta_2 Nonadopter_i + \varepsilon_i \quad (8)$$

To probe differences in behavior between adopters and non-adopters, A is a dummy taking value 1 for actual adopters (among those with access), and NA is a dummy taking value 1 for non-adopters (among those with access; respondents with no-access are always the omitted category). In words, our third research question is:

> ***RQ3**: Assuming that the introduction of formal insurance is associated with reduced aggregate contributions to the common pot in a PG game, who is responsible for these lower contributions: adopters, non-adopters, or both?*

While it is easy to explain variation in common pool contributions by adoption status, it is likely that adoption status is correlated with other

relevant variables driving contributions. In that case, correlations obtained in our regression framework might be spurious. As a robustness test we therefore do a slightly more elaborate analysis. We first explain adoption status (based on observations in access parishes only) and, based on the resulting regression coefficients of (4), predict the probability of non-adoption for all households (including in the no access villages). This regression analysis enables us to compare public good contributions for (predicted) adopters and non-adopters across villages with and without access to insurance. We are interested in establishing whether predicted non-adopters without access behave similarly to predicted non-adopters with access. We estimate:

$$\Pr(ShareHi_i|\ \Pr(NA)_i) = \alpha + \beta\ \Pr(NA)_i + \varepsilon_i \tag{9}$$

$$\begin{aligned}\Pr(ShareHi_i|\ \Pr(NA)_i) = {} & \alpha + \beta_1\ \Pr(NA)_i + \beta_2 \mathrm{Access}_i \\ & + \beta_3[Pr(NA)_i \times Access_i] + \varepsilon_i\end{aligned} \tag{10}$$

where $0 \leq \Pr(NA)_i \leq 1$, is the predicted probability of non-adoption, and $\Pr(\mathrm{NA})_i \times Access_i$ is the interaction term.

To complement the analysis we also estimate a propensity score matching (PSM) model, matching respondents from access and no-access areas. To probe whether contributions by predicted non-adopters are similar, or not, we then compare contributions of non-adopters, with credibly similar respondents in no access-areas, based on nearest neighbor matching.

> **RQ4**: *Do predicted non-adopters with access to formal insurance behave the same as predicted non-adopters without access—is behavior of non-adopters driven by population characteristics or by insurance?*

5.5 Results

We first test the prediction that wealthier households are more likely to purchase formal health insurance than relatively poor households. In Table 2, we focus on those parishes where insurance was offered, and explain

Table 2: Wealth and the adoption of health insurance

	Insurance Adoption			
	(1)	(2)	(3)	(4)
Wealth	0.146**	0.140**		
	(0.061)	(0.061)		
Wealth^2	-0.056**	-0.035		
	(0.023)	(0.030)		
Poor			-0.226***	-0.197**
			(0.082)	(0.095)
Additional Controls	N	N	N	Y
Observations	210	208	210	185
Correct predictions at P=0.5	63%	63%	62%	68%

Notes: Robust standard errors in parentheses; probit marginal effects in (4). Wealth Factor based on vector of household level asset dummies. Model (2) removes the highest outlier from the sample. Probit marginal effects for Model (3); Poverty Dummy takes value of unity when household owns not more than one of the three most common assets (phone, radio, bicycle) and not more than one improved house feature (iron roof, brick wall, cement floor). Confidence: *** ↔ 99%, ** ↔ 95%, * ↔ 90%. Additional controls include Age household head, Male household head, Education household head, Single, Household size, Female participant, Mothers in house, Watch TV weekly, Read newspaper weekly.

variation in adoption across households. Columns (1) and (2) illustrate a positive correlation between wealth and adoption. This relation may possibly be non-linear (column 1), but a linear relation ensues when we omit two outlier from the sample (see column 2). The finding that relatively wealthy households are more likely to adopt formal insurance is confirmed in columns (3) and (4), where we replace the wealth variables with a poverty dummy. We report probit marginal effects, so poor households are 20% less likely to adopt health insurance than non-poor households in access villages.[56]

We next explain how the introduction or formal insurance affects play in the PG game. We first use the number of tokens shared as the dependent variable, and examine how this quantity varies with access status. Our access dummy captures whether or not a respondent's household resides in

[56] This finding is confirmed by abundant anecdotal evidence. Many respondents cite 'financial problems' as the main reason for not purchasing the insurance. They claim that their income is too low in comparison to the fee, and would have joined the scheme 'if only they had more money.' Others state that they would join if there was a 'reduction on the fee that SHU asks from them', or stress that they 'joined but paid half of the money', and thus could not benefit from the insurance coverage.

a village that has an active CHF scheme, regardless of its own adoption status. Results are reported in Table 3. Column (1) shows the results of the Heckman selection model, which controls for possible non-random attrition. Of 440 households interviewed, only 409 correct household representatives showed up for the afternoon PG game session. The other 31 participants were substituted by a random villager that was not a member of any of the interviewed households. While some observed characteristics affect the likelihood of showing up (that is, if the household head is male, or if the randomly selected respondent is female), the coefficient on the inverse Mills ratio is statistically insignificant. In other words, there is no detectable self-selection bias into the experiment, and it is therefore safe to estimate the model using OLS and Probit. Respondents with access to the formal insurance share on average fewer tokens than their counterparts, consistent with findings by Klohn and Strupat (2013). Using a Poisson model yields similarly significant results (details available on request).

In columns (2-4) we attempt to go further and identify who is responsible for the lower PG contributions—adopters or non-adopters. While one may expect that adopters will reduce their contribution to the PG—as they have less use for social capital in the future—our empirical results are different. Lower public good contributions are driven by relatively lower contributions of non-adopters. On average, non-adopters with access to the formal insurance contribute about 0.4 tokens less than respondents without access, or over ¼ of a standard deviation. In column (3) we cluster standard errors at the village level, which only slightly increases the standard errors. In column (4) we distinguish between households that have paid their premium (eligible to benefit from the insurance) and those that have only paid in part. Again, non-participants drive the reduction in contributions.

Table 3: Sharing in the public goods game

	Shared				ShareHi=1		
	Heckman	OLS	OLS	OLS	Probit	Probit	Probit
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Access	-0.307**				-0.093**		
	(0.148)				(0.045)		
Adopter		-0.188	-0.188			0.082	
		(0.170)	(0.185)			(0.057)	
Full payment				0.008			0.049
				(0.352)			(0.132)
Partial payment				-0.234			-0.112*
				(0.192)			(0.054)
Nonadopter		-0.402**	-0.402*	-0.402*		-0.101**	-0.101**
		(0.186)	(0.226)	(0.226)		(0.046)	(0.046)
Mills Lambda	-0.798						
	(0.819)						
Cluster robust s.e.	N	N	44	44	44	44	44
Observations	440	409	409	409	409	409	409
Pseudo R^2 and R^2		0.012	0.012	0.013	0.008	0.009	0.013

Notes: Robust standard errors in parentheses; probit marginal effects in (5-7). Two-stage Heckman Selection Model is used in (1) to assess non-random attrition in the experimental sample. Excluded instruments in the selection stage include: Insurance adopter, Poverty dummy, Male household head, Education household head, Household size, Female participant, Watch TV weekly, Read newspaper weekly. Confidence: *** ↔ 99%, ** ↔ 95%, * ↔ 90%.

Similarly, when we try to explain the incidence of high contributions (that is, the contribution of 4 or 5 tokens), the finding that there are fewer high contributors in access areas (column 5) is driven by non-adopters (columns 6 and 7). They are 33% less likely to be high contributors, again representing about ¼ of a standard deviation.

Other factors may explain both adoption and contribution rates. Table 2, for instance, shows that non-adoption is primarily explained by wealth—poor households are significantly less likely to adopt insurance. If poorer households systematically contribute less to the common pot, the correlations in Table 3 may be spurious. To attenuate this concern we next use information about the propensity to be a non-adopter (based on the regression results in column (4) in Table 2). This enables us to compare "predicted non-adopters" in access areas to "predicted non-adopters" in no-access areas.

The results of the follow-up step are reported in Table 4. In column (1) we zoom in on access areas, and demonstrate that predicted non-adopters are less likely to share a high fraction of their tokens in the PG game. These results, of course, echo those in Table 3. However, when estimating the same model for the sub-sample of respondents with no-access, we find that the sign of the coefficient of interest changes–from negative to positive (column 2). Predicted non-adopters without access are *more* likely to contribute 4 or 5 tokens to the common pot. This may reflect the greater importance attached to social capital by relatively poor households. Upon pooling the data and including an interaction term (predicted non-adoption probability multiplied by insurance access), we can capture these results in one specification. According to results in columns (3) and (5), predicted non-adopters in no-access areas are more likely to contribute more to the common pot, but this is not true for predicted non-adopters living in parishes with an active formal insurance scheme—the interaction effect dominates the level effect of predicted non-adoption. Results in (3) are robust to clustering at the parish level (8) instead of the village level (44), using wild bootstrap inference to correct for the small number of clusters (Cameron et al., 2008).

As an alternative approach, in columns (4) and (6) we use Propensity Score Matching to compare non-adopters to credibly similar counterparts in no-access areas, thus excluding the 106 adopters from the analysis. The average treatment effect obtained through nearest neighbor matching is consistent with previous findings. Matching is based on the same variables of column (2), Table 4. A logit model is used to calculate propensity scores. Balancing properties are satisfied for all matching variables, in each of the 5 blocks of equal score range. Also, the mean propensity score in each block is not different for non-adopters (in this setting, the 'treated') and no-access participants ('controls'). The predicted scores range between 0.044 and 0.688 for non-treated and between 0.045 and 0.602 for controls, so the common support region ranges between 0.045 and 0.602 (suggesting

Table 4: Predicted non-adoption and sharing in the public goods game

	ShareHi=1				Shared	
	Probit Access	Probit No-access	Probit Pooled	PSM PSM	OLS Pooled	PSM Pooled
	(1)	(2)	(3)	(4)	(5)	(6)
Pr(NA)	-0.381**	0.328*	0.323*		0.837	
	(0.193)	(0.189)	(0.184)		(0.569)	
Access			0.259**		0.451	
			(0.115)		(0.373)	
Access x Pr(NA)			-0.710***		-1.344*	
			(0.267)		(0.729)	
Nonadopter				-0.105		-0.402**
				(0.058)		(0.186)
Cluster Robust s.e.	21	23	44	N	44	N
Observations	185	184	369	185	369	303
Pseudo R^2 and R^2	0.013	0.009	0.011	0.013	0.006	

Notes: Cluster robust standard errors in parentheses; probit marginal effects in (1-3). Average treatment effect on the treated (ATT) using nearest neighbor matching with random draw. The propensity score in (4) and (6) is calculated on: Poverty dummy, Age household head, Male household head, Education household head, Single, Household size, Female participant, Mothers in house, Watch TV weekly, Read newspaper weekly (see Table 2, column 4); the same variables are used to predict Non-adoption in columns (1-3) and (5). Clustering at the Parish level in (3), and correcting for the small number of clusters following Cameron et al. (2008), yields a P-value of 0.020 for the interaction term, significant at the 5% level. Confidence: *** ↔ 99%, ** ↔ 95%, * ↔ 90%.

adequate overlap between groups). Restricting the analysis to the common support region (N=265, 70 treated and 195 controls) yields an ATT of -0.063 in (4), insignificant, and -0.349 in (6), significant at the 10% level. Kernel matching with bootstrapped standard errors yields similar results to Table 4.

We interpret these findings as tentative evidence that the results in Table 3 are not driven by spurious correlation. Non-adopters do not contribute less in public good games because they are poor, but because of the interaction between poverty and formal insurance availability. This is fully consistent with the perspective on insurance-related arrangements embedded in a broader social structure. Those villagers left behind after the relatively wealthy have adopted insurance signal their discontent by foregoing cooperation in other domains of social interaction.

5.6 Conclusions

Social capital is a key determinant of the ability of communities to overcome social dilemmas, and fostering social capital has become a policy goal in and of itself. For example, Fearon et al. (2010) demonstrate that outside innovations may facilitate investments in social capital. However, not all interventions have such benign effects. Specifically, interventions which benefit a subsample of the population may weaken mutual dependencies and erode social capital. Our study provides evidence to support this latter interpretation—our data are consistent with a broad perspective on social interactions, where reduced inter-dependencies in one domain (informal insurance) invite behavioral changes in other domains (public good contributions).

Formal insurance provides a substitute for informal insurance, crowding out social capital. Anecdotally, we find evidence of such dynamics in our open-ended questionnaire. Respondents lament that, since the introduction of the formal insurance, some of the informal risk sharing mechanisms have deteriorated. A non-adopter states that, in contrast to insurance holders, when she fell sick she had to 'totally cater for herself.' Another respondent claims that someone 'had joined SHU, and therefore refused to help a neighbor who had a sick son.' In the words of other respondents, the insurance scheme 'spoils care for those who can't pay for the scheme,' and 'if you are not a member you are not catered for.' But the deterioration of mutual assistance works both ways. Various respondents emphasize that (non-adopting) neighbors refuse to take insurance holders to the hospital (even if transport costs are typically not part of the insurance package). Another respondent mentions the case of a pregnant woman whom 'people refused to help, because she is in the insurance.' Apparently the non-adopters are not passively undergoing their plight—they take action and signal their discontent by ceasing cooperation in other domains, in an effort to "discipline" adopters.

Indeed, one important new finding of our study is that the erosion of social capital is not primarily caused by adopting community members – those respondents who may opt out of the insurance network. Instead, the deterioration of social capital in our field experiments is fully explained by a behavioral response of non-adopters.

In light of the widely-recognized importance of social capital (and informal institutions more generally) as a determinant of economic outcomes, we speculate that a "weapons of the weak perspective" on outside interventions may be relevant. Indeed, there may be cases where welfare analysis (based on CGE models or otherwise) should be augmented to explicitly consider social embeddedness—private cost-benefit ratios associated with specific innovations may fail to accurately predict patterns of adoption. Similarly, policy makers should not underestimate the power of the weapons of the weak in shaping policy outcomes—welfare externalities might be far reaching and tough to predict.

Appendix

Variable definitions:

Female participant. Takes value of 1 if selected game participant is female, 0 otherwise.
Single. Marital status household head single = 1, 0 otherwise
Married/Engaged. Marital status household head married or engaged = 1, 0 otherwise.
Widowed. Marital status household head widowed = 1, 0 otherwise
Age household head. Age of the household head in years, rounded down to the last birthday.
Male household head. Takes value 1 if household head is male, 0 otherwise
Education household head: takes value 1 if household head has completed primary school.
Household size: number of people that usually eat at least one daily meal together with the household head, acknowledging its authority and living with the rest of the household.
Mothers in house: takes value of 1 if the household comprises at least one pregnant woman or mother taking care of a child at present time, 0 otherwise
Watch TV weekly: takes value of 1 if the household head declares to watch TV at least once a week, 0 otherwise.
Read newspaper weekly: takes value of 1 if the household head declares to read the newspaper at least once a week, 0 otherwise
Radio. Takes value of 1 if the household possesses any type of radio, 0 otherwise
Phone. Takes value of 1 if the household possesses any type of phone, 0 otherwise
Bicycle. Takes value of 1 if the household possesses any type of bicycle, 0 otherwise
Motorbike. Takes value of 1 if the household possesses any type of motorbike, 0 otherwise.
Television. Takes value of 1 if the household possesses any type of television, 0 otherwise.
Car. Takes value of 1 if the household possesses any type of car, 0 otherwise
Generator. Takes value of 1 if the household possesses any type of electricity generator, 0 otherwise.
Wealth. A principal factor obtained by factor analysis following Sahn and Stifel (2003), from the abovementioned asset list (radio to generator) possessed by the household
House features index. Is the sum of three dummy variables representing improved house features, i.e. iron roof, brick wall, cement floor
Common assets index. Is the sum of three dummy variables representing the three most common assets, i.e. radio, phone and bicycle
Poor. Takes value of 1 if the household scores 1 or less in the House Features Index and scores 1 or less in the Common assets index, 0 otherwise
Adopter. Takes value 1 if a household is residing in a parish (village) with an active and running CHF insurance scheme, and adopted the insurance; 0 otherwise
Non-adopter. Takes value 1 if a household is residing in a parish (village) with an active and running CHF insurance scheme, and knowingly renounced participation; 0 otherwise.
Full payment. Takes value of 1 if a household that adopted the CHF insurance scheme has fully paid the due premium, 0 otherwise.
Partial payment. Takes value of 1 if a household that adopted the CHF insurance scheme has only partially paid the due premium, 0 otherwise.

Access. The sum of Adopter and Non-adopter. It takes value 1 if the household has access to the formal insurance, regardless of its adoption status; 0 otherwise .
Shared. Is the number of tokens contributed to the common pot of the public goods game by a given participant, minimum is 0 and maximum is 5.
ShareHi. Takes value 1 if the participant has contributed 4 or 5 tokens to the common pot of the PG game—i.e. one standard deviation or more above the average –, 0 otherwise.
Pr(NA). The probability of adoption as predicted for respondents from access areas, based on the individual and household level characteristics of column (4), Table 2.

CHAPTER 6
Statutory Law and Customary Change

a Lab-in-Field Experiment in Ethiopia

Abstract

Through a lab-in-field experiment with villagers and real customary judges in Ethiopia, we test the hypothesis that customary courts strategically adapt arbitration outcomes if they face increased competition by the formal law. We show that introducing a costly legal fallback reduces arbitration biases and draws the decisions of customary judges significantly closer to the formal law. At the same time, agents disfavored by the custom do not take advantage of their increased bargaining power. Our results suggest that local customary dispute resolution institutions may have a role to play in shifting preexisting customs towards a desired outcome. In areas where formal legal institutions have limited outreach, most effects of increased competition between formal law and customary legal institutions may rise from changes in the latter, rather than from plaintiffs seeking justice under the rule of law.

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6.1 Introduction

Formal laws play a marginal role in governing the lives of many African citizens, particularly those residing in rural areas (Chirayath et al., 2006). Instead, customary legal systems provide prompt, accessible and culturally coherent justice services (Wojkowska, 2006). Customary courts oversee and enforce customs and informal rules of behavior, typically taking into account local egalitarian and redistributive norms (Platteau, 2000). Their adherence to minimum standards of justice and human rights remains nevertheless disputed. Customary courts may persistently discriminate against the underprivileged, entrenching mechanisms that perpetuate local power structures (e.g. Ordioni, 2005; Asfaw and Satterfield, 2010; Pimentel, 2010). Local gender biases, for instance, may affect the distributional decisions of customary dispute resolution institutions (Asfaw and Satterfield, 2010). Understanding the effects of increased competitiveness of formal law in predominantly customary institutional environments is therefore central to achieving fair and functional legal systems—a primary driver of economic development (Acemoglu et al., 2001; Rodrik et al., 2004). Yet, data on extrajudicial and customary disputes are rarely available (Landeo et al., 2007), and the interaction between customary legal institutions and formal law has been subjected to little rigorous empirical analysis so far.

The work of Sandefur and Siddiqi (2013) in Liberia is a notable exception. They find that the demand for mediation by paralegals trained in formal law are greater for plaintiffs disadvantaged by the customary system, and that direct access to the formal law results in strong socioeconomic gains for the underprivileged. Increased competition of formal law may nonetheless also foster indirect changes to the norms enforced by customary dispute resolution institutions. These indirect effects are particularly salient if agents face strong disincentives to appeal to formal legal institutions—e.g. if customary norms are backed by credible social sanctioning against defection and appeal to alternative forums. If those

disadvantaged by the customary system fear that overruling its decisions may be costly, thus complying with preexisting customs even when the law is individually preferred, much of the direct socioeconomic gains may be dissipated. Aldashev et al. (2012a, 2012b) provide clear theoretical predictions on the evolution of customary legal outcomes induced by the introduction or empowerment of formal laws. If customary authorities fear jurisdictional and reputational erosion,[57] they may strategically adapt arbitration outcomes in response to the introduction of a competing formal law.

In this paper we empirically investigate the hypothesis by Aldashev et al. (2012a, 2012b). Through a lab-in-field experiment in rural Ethiopia – where controversies are habitually settled through customary courts – we study the effects of introducing a costly legal fallback, on the arbitration decisions of local customary judges and the behavior of plaintiffs. In West Gojjam, we randomly select 60 customary judges, known among the local Amhara people as *Shimagelle*, to rule over controversies born from an ultimatum game with outside option, played by 532 villagers. For a random subsample, we allow participants to further appeal the arbitration through a costly fixed law. While some studies have looked at the influence of extraneous factors on formal judicial rulings (e.g. Danziger et al., 2011), the relative scarcity of naturally occurring data on customary rulings has limited their analysis. By bringing the lab into the field (see List, 2007), this work is the first to bridge this gap, studying the arbitration decisions of real Ethiopian customary judges.

In line with previous literature, we find evidence of significant arbitration bias against female participants, and in favor of plaintiffs known by the

[57] Jurisdiction erodes as plaintiffs begin to use formal courts instead of customary forums. In so far as customary judges face a positive utility in ruling over a controversy, jurisdictional erosion will reduce their utility. Moreover, customary judges may face an intrinsic disutility in seeing their decision overruled. Reputation thus erodes when plaintiffs reject the customary arbitration decision and appeal to formal legislation.

customary judge and advantaged by the egalitarian custom. Our main finding is that introducing a legal fallback reduces such biases, and that customary arbitration outcomes are drawn significantly closer to the formal law. Furthermore, we find that agents disfavored by the custom do not take direct advantage of the increased bargaining power offered by the legal fallback. In equilibrium, only a fraction of them make direct use of the formal law. These results complement the work of Sandefur and Siddiqi (2013), and highlight the importance of indirect customary responses to the increased competitiveness of the formal law. The "threat of law" may induce significant gains for those disfavored by the custom, even if they do not actively seek justice under the rule of law.

The rest of the paper is organized as follows. Section 6.2 discusses the literature on formal law and customary institutions. Section 6.3 provides a brief account of the Ethiopian institutional context and legal system. Section 6.4 outlines the experimental design. Section 6.5 discusses the empirical strategy. Section 6.6 illustrates the results, and Section 6.7 concludes.

6.2 Customs, legal institutions, and the law

Legal institutions encompass both formal and informal structures, and are central to enforcing the "rules of the game" that govern everyday life (North, 1990). Formal legal institutions typically preside over written constraints, such as statutory laws and constitutions—prescribed and enforced by exogenous legislative authorities. Customary legal institutions, instead, oversee the 'codes of conduct, norms of behavior, and conventions' that take form in a particular social setting (North, 1990: 36). Yet, formal and informal legal systems are not necessarily mutually exclusive, and often coexist and overlap to a considerable extent. Legal pluralism is thus prevalent in numerous countries and regions worldwide, including large

portions of sub-Saharan Africa (Merry, 1988; Bennett, 2006; Tamanaha, 2008).[58]

The norms upheld by customary legal institution typically represent fairness standards intended at maintaining peace and social cohesion, but may also result in systematic discrimination against certain disadvantaged categories. In either case, they may have substantial consequences on investment decisions and long-run growth (Platteau, 2009; Baland et al., 2011).[59] However, replacing undesired customary norms with the rule of law has proven to be a complex and daunting exercise (e.g. Andre and Platteau, 1998; Kuyu, 2005; Sacco, 2008). Formal legal institutions will not successfully replace incompatible or unwanted customary norms, unless they become a "focal point" of convergence in the expectations of agents (Basu, 2000; Aoki, 2001b; Greif, 2006). In the presence of preexisting customary focal points, the fear of social punishment inhibits the consolidation of formal laws that contradict such customs. In fact, norms influence behavior not only through internal incentives (e.g. guilt aversion, or a taste for moral virtuousness), but also through external ones (Polinsky and Shavell, 2007). People may be willing to punish non-compliers even if the punishment is costly and doesn't yield direct private benefits (Fehr and Gächter, 2000). As a result, agents find it harder to deviate from norm-compliant behavior even when the law is individually preferred. This is especially true for rural communities, where social pressure and sanctioning are more pervasive, and the cost of social exclusion is greater (e.g. Crook, 2004; Gedzi, 2012).

[58] In what follows, we use the terms "customary" and "informal" as synonyms. "Statutory law" and "formal law" are also used interchangeably.

[59] Customary norms may also add to the uncertainty over property rights, in turn affecting the investment decisions of individuals. Goldstein and Udry (2008), for instance, find that competing claims and higher insecurity of tenure over specific plots cultivated by a given individual correspond to lower intensity of investments on those plots.

While certainly a source of institutional "stickiness", customary legal institutions need not to necessarily discourage institutional change (see Boettke et al., 2008). Instead, they are often crucial to the functioning of formal institutions—such as the legal system (Platteau, 2000; Aoki, 2001b). Their interaction with statutory law has thus surfaced as a pivotal issue to effectively reach the objectives sought by legislators (Richman, 2012). Several theoretical studies have examined this interaction (Greif and Laitin, 2004; Helmke and Levitsky, 2004; Dixit, 2007; Dhillon and Rigolini, 2011; Aldashev et al., 2012a, 2012b). Typically, these studies focus on the behavior of agents, not institutions. Formal laws alter the net benefit of adhesion to prevailing norms – affecting the bargaining power and fallback position of economic agents – such that established patterns of behavior may evolve. Studies empirically investigating the empowerment of formal laws are somewhat less prevalent. Banerjee et al. (2002), for instance, study the effects of a land tenancy reform in West Bengal. They find that the increased bargaining power that the tenants acquire, once a legal fallback is introduced, has positive effects on productivity, whilst arbitrary evictions by landlords all but disappear.[60]

Sandefur and Siddiqi (2013) propose a "forum shopping" model in which plaintiffs choose between the customary and formal systems based on rational tradeoffs. If plaintiffs face high entry costs to the formal legal system, they will bring their cases to customary forums even when these are systematically biased against them. After providing evidence of such barriers to access in Liberia, they investigate the outcomes of a randomized intervention that increases the competition between formal and customary law by offering complimentary mediation and advocacy services through community paralegals trained in the formal law. They find that female and

[60] The theoretical model underpinning these findings, however, assumes that the legal innovation replaces a sort of institutional vacuum, in which "landlords wielded a lot of power within the village and were therefore able to intimidate tenants" (Banerjee et al., 2002: p. 242).

ethnic minority plaintiffs – facing poor odds in the customary system – are more likely to adopt and to be satisfied with the paralegal service, and that the program increases average household wellbeing. Nonetheless, less than 10% of the recorded cases where brought to the attention of paralegals. The relatively low pick-up of complimentary paralegal services, even among self-selected paralegal clients,[61] may be an indication of social sanctioning dynamics that create strong disincentives to appeal to any authority alternative to customary ones. Under such constraints much of the socioeconomic gains of increased outreach may be dissipated, unless increased competition of formal law produces indirect changes in customary arbitrations.

Aldashev et al. (2012a, 2012b) provide clear theoretical predictions on the evolution of customary legal outcomes induced by the introduction or empowerment of formal laws. Central to their argument is that not only agents, but also customary institutions respond to incentive structures. In fact, if customary judges insist on imposing custom-compliant outcomes once the legal fallback is introduced, they do not only increase the likelihood of appeal to the formal law by unsatisfied plaintiffs, but also the distance between the expected outcome and the custom. Provided the formal law is not too radical – i.e. it is not excessively costly for customary judges to deviate from the preferred arbitration outcome – increasing its competitiveness will thus shift the conflicting custom in the direction intended by the legislator. The law, therefore, does not only provide a direct alternative to customary dispute resolution institutions; it also changes the custom—indirectly improving the welfare of the disadvantaged sections of the population (Aldashev et al., 2012a, 2012b). In this paper we empirically investigate this hypothesis by observing the arbitration

[61] Their sample stems from a sub-category of villagers that had self-selected into contacting the paralegals before the baseline. It represents a "snapshot of potential paralegal clients, and therefore not representative of Liberians as a whole" (Sandefur and Siddiqi, 2013: p. 28).

outcomes stemming from real customary judges in rural Ethiopia, half of which face the risk of seeing their decisions overruled by a fixed law. To this end, we formulate the following research questions:

> **RQ1:** *Are post-arbitration payouts to agents disfavored by custom downwardly biased?*
>
> **RQ2:** *Are biases against agents disfavored by the custom reduced by the legal fallback?*
>
> **RQ3:** *Does increased competition by the formal law draw customary arbitration decisions closer to the law itself?*
>
> **RQ4:** *Do disfavored agents take advantage of the increased bargaining power created by the legal fallback?*

6.3 Legal institutions in Ethiopia

Ethiopia is home to more than sixty customary legal systems (Donovan and Assefa, 2003). Statutory law was first introduced in the 1950s, and customary institutions remain very vibrant. The formal legal system is far from penetrating and, since the mid-nineties, the government is committed to recognize and preserve local customary dispute resolution authorities. Most Ethiopian ethnic groups have their own customary systems for dispute settlement and conflict resolution. Famous examples include the *Shimagelle* system of the Amhara, the *Gadaa* system of the Oromo and *Sharia* courts of Muslim communities. In this paper, we investigate Amhara's *Shimagelle* system, which contains elements of customary law practices that are very much in use across the whole country. This traditional institution can best be viewed in line with principles of arbitration, where the arbitrators are mostly religious leaders and village elders who review existing evidence and arguments from both sides and issue a verdict to settle the case based on customary norms. The fact that customary judges are local elders and religious leaders ensures that arbitration outcomes are strongly embedded into community dynamics.

This in turn favors the enforcement of deliberations, but also provides strong disincentives to use alternative forums, such as formal courts.

Typically, the adjudication proceeds focusing on narrowing of differences through negotiations, rather than through adversarial procedures. The verdict may vary depending on the nature and gravity of the dispute, ranging from a simple apology for petty disputes, to blood money for homicide crimes. At the end of the dispute resolution, the restoration of prior relationships is marked through customary rituals or ceremonies to which both parties take part. Once the arbitrators have held their verdict, they closely monitor its enforcement. Nominally, arbitrators lack the coercive powers of the formal law to ensure compliance; however, they rely effectively on the presence of social pressure and sanctions to enforce their decisions. A party failing to abide by the outcomes will be considered as insulting the arbitrators and will be shunned by the community. Social sanctions – including ostracism by neighbors and friends – and loss of reputation soon follow.

Shimagelle are expected to provide their services without an explicit fee. Yet, there are several individualistic as well as altruistic reasons for the engagement of customary judges in dispute settlement. First, they are motivated by social recognition. Among the Amhara, there is a tradition of holding a special funeral ceremony for those who are believed to have played an important role for their community. Elders often serve their community hoping that they will be mourned accordingly. Second, in such strictly hierarchal communities, arbitration is an age-ascribed role, that provides a mix of status and responsibility. Third, arbitrators are expected to ensure the welfare of both parties and, through that, the wellbeing of the community. Fourth, the vast majority of *Shimagelle* serve some sort of religious function too, and being recognized as a *Shimagelle* is a signal of piousness and righteousness. While accounting for religious scriptures, local norms and customs, deliberations will therefore reflect the need of

customary judges to maintain their reputation, reaffirm their social and moral standing, and minimize potential jurisdictional erosion.

The official status of Ethiopia's customary courts has not been unequivocal in the legal history of the country. Both the Imperial and *Derg* regimes opted for a centralized legal approach that did not embrace legal pluralism. Only since 1995 does the state recognize customary legal systems (Gopal and Salim, 1998).[62] The jurisdiction of customary courts is accepted as long as both parties to the dispute give their consent to be heard at customary forums, and the verdicts thereof are in conformity with human rights provisions. The Constitution preserves the mandate to adjudicate criminal matters solely through the formal law, but customary courts enjoy *de facto* wider jurisdictions—spanning from petty offences, land tenure and inheritance issues, to violent crimes and homicide. In many respects, Ethiopian customary institutions stand out as more functional and powerful than statutory law. The political stalemate that followed the country's 2005 general election, for example, was ultimately settled through the arbitration of selected *Shimagelle*.

The relative dominance of Ethiopian customary institutions is in fact related to the problems encountered by the formal legal system. First instance courts are available in every Woreda (district), averaging approximately 100,000 people per tribunal (Guttman et al., 2004). However, as is true for much of Africa, the formal legal system in Ethiopia is regularly reproached as dysfunctional and inaccessible to ordinary people (Gowak, 2008; Asfaw and Satterfield, 2010). Moreover, the formal legal system is limited by the overload generated by the lack of physical resources, personnel, infrastructure and inadequate information systems (B. Baker, 2013): access to formal justice can be a daunting task for the average Ethiopian. Statutory law is often perceived as costly to access and

[62] The provisions of the Constitution embracing legal pluralism are provided under Articles 34(5) and 78(5).

punitive – rather than conciliatory – and regularly fails to deliver proper redress to aggrieved parties (Sandefur and Siddiqi, 2013). In contrast, customary courts provide prompt and accessible dispute resolution mechanisms. However, their capacity to produce unbiased sentencing is contested. Asfaw and Satterfield (2010) study land and property dispute settlements by *Shimagelle* in the Zeghie Peninsula, in the Amhara region. They find that the formal justice system is both inaccessible and dysfunctional, while customary arbitration outcomes are largely unfavorable to women. This in turn reinforces gender inequality and entrenches local power relations.

6.4 Experimental design

6.4.1 Sample and setting

We develop a multi-stage laboratory experiment involving 532 villagers and 60 real local *Shimagelle*. Participants belong to 18 *Kebele* (municipalities) of West Gojjam, in the Amhara region of Ethiopia. At each of the fifteen game-sessions, 4 customary judges were randomly selected from a pool of well-established local *Shimagelle*. The villagers belong to a list of randomly selected farmers responding to an agriculture-productivity related survey, administered in the same area in the previous year. Respondents are the main income earners (household heads), 92% are male, they average 43 years of age, and have slightly more than two completed years of formal education. Households are comprised of about 6 family members, and almost the entire sample is orthodox Christian. 82% of respondents take part in at least one informal safety net mechanism. Table 1 reports balance statistics for individual and experimental characteristics by treatment. No variable differs significantly at the 5% level, indicating a successful treatment randomization. The slight difference in sample size across

Table 1: Balance and summary statistics for the experimental samples

Variable	Customary only (N=252)	Customary + Law (N=280)	Diff.	Std. Err.
Male (%)	93.7	89.6	4.1	(2.40)*
Age	43.1	43.9	-0.8	(0.97)
Married (%)	92.5	91.1	1.4	(2.40)
Orthodox (%)	98.8	97.1	1.7	(1.24)
Education	2.6	2.3	0.3	(0.16)
Household size	6.4	6.2	0.2	(0.18)
Non-farm income	0.2	0.2	0.0	(0.03)
Informal safety nets (%)	81.3	84.3	3.0	(3.33)
First risk game (selected ball)	26.4	24.0	2.4	(2.31)
Second risk game (invested %)	46.4	46.6	0.2	(0.22)
Private Endowment	118.9	125.8	-6.7	(4.88)
Joint venture endowment	276.5	289.8	-13.3	(10.22)

Notes: Group means; t-test. Confidence: $^{***} \leftrightarrow 99\%$, $^{**} \leftrightarrow 95\%$, $^{*} \leftrightarrow 90\%$.

treatments is due to the randomization at game-session level. This ensured that villagers and judges participating in the same game-session were all instructed on the same treatment, minimizing potential spill-overs and confusion.[63]

The experiment was comprised of four stages: (1) private investment; (2) joint venture; (3) ultimatum game with arbitration by a customary judge as outside option; and (4) the application of the formal law treatment.

6.4.2 Private investments and joint venture

The first two stages only involved villagers (from here onwards called agents), not the judges. In our setting, ultimatum offer rejections are a necessary premise to arbitration by customary judges. We make use of previous findings in economic literature to increase the likelihood of litigation in each anonymous pair, without biasing the randomness of

[63] We randomly selected participants from a list of 612 farmers that had responded to an agriculture-productivity related survey the previous year. Sampling halted once at least 250 villagers had participated in each treatment. Random re-sampling ensured insignificant attrition bias: participants and non-participants show no significant differences (see Appendix A1).

relative endowment assignments. Agents were randomly allotted an endowment of either 80 or 120 tokens,[64] and made two individual risky investment decisions that could increase or decrease the endowment. By having each agent make private investment choices, we made use of the so-called "earned endowment effect": people exhibit more self-interested behavior in bargaining and sharing games when relative wealth is earned in some way rather than obtained through a pure windfall gain (e.g. Gantner et al., 2001; Cherry et al., 2002; Frohlich et al., 2004; Oxoby and Spraggon, 2008). The first risk game combined the design of Eckel and Grossman (2002) with that of Holt and Laury (2002).[65] The resulting endowment was then used in the second game, which followed Gneezy and Potters (1997). Agents decided how much of their endowment to invest in a lottery with 50% probability of doubling and 50% probability of halving the invested amount. To further strengthen a sense of ownership with respect to the endowment (see Kahneman et al., 1990), at the end of this stage agents were given a sleek and colorful endowment card, reporting private earnings.

The second stage of the experiment involved paired anonymous decisions, and took place the following day. Agents were assigned to a randomly selected anonymous partner with higher or lower private endowment. They merged their individual endowments and jointly decided about the same risky investments mentioned above.[66] On average "higher investors" (i.e. participants contributing a higher share in the joint venture) contributed two thirds of the joint capital, while "lower investors" contributed the

[64] Each token was worth 0.5 Birr, resulting in endowments of USD 2.2 and 3.3 respectively, or around twice the average daily income in the area.

[65] Agents selected one out of eight balls, with exponentially increasing value from 1 to 128, as their winning prize. They then drew one ball from a bag containing all eight balls: if the extracted ball was worth at least as much as the selected ball, they won the value on the selected ball; if the extracted ball was worth less than the selected ball, they would lose the amount specified on the extracted ball (see Appendix Table A2).

[66] The rationale of repeating choices in a paired setting follows that of intra-household bargaining literature (e.g. Bateman and Munro, 2005; Carlsson et al., 2012; He et al., 2012).

remaining third.[67] In other words, one agent typically invested twice as many stakes as their partner.[68] We expect such endowment heterogeneity to lower ultimatum offers in the next stage (Cherry et al., 2005). Particularly, we expect high endowment agents to prefer offers proportional to investment shares, and low endowment individuals to prefer egalitarian redistributions (Rutström and Williams, 2000). Pairs could bargain on their investment choices for up to eighteen rounds, through oral messages collected and delivered by experimenters. On average, 4.5 counteroffers were made across the two games, with a maximum of fourteen bargaining rounds before reaching an investment agreement. Once the joint investments were completed, the next task was to split the final outcome through an ultimatum game—the source of our experimentally induced litigations.

6.4.3 Ultimatum and arbitration

In the third stage, pairs play an ultimatum game with outside option (see Güth et al., 1982; Schmitt, 2004). One agent was randomly selected to become an ultimatum sender, the other one becoming the receiver. The sender was asked to make a split offer, which the receiver could either accept or reject. In case the offer was rejected, a randomly assigned local *Shimagelle* would mediate a resolution to the litigation. The customary judge independently studied the game history of each player and made an independent arbitration verdict. The verdict overruled the ultimatum offer,

[67] "Higher investors" had an average private endowment of 165 tokens, "lower investors" 80.

[68] By design the risk propensity of each agent should not significantly influence the likelihood of being paired with a higher or a lower endowed partner (because of the random assignment of initial endowments). We test such assumption through a Pearson's Chi-squared test and find no evidence of significant correlation, with p=0.611 and p=0.303 for the first and second game respectively. Individual risk preferences are thus not correlated with the relative size of the investment in the joint venture.

and imposed a new division of the joint venture capital.[69] Before the ultimatum decision, experimenters exposed the name of the assigned customary judge, and stressed that in case of arbitration the judge would be informed about the players' names. While joint venture partners remained anonymous to each other throughout the game, the arbitration process was thus non-anonymous (although strictly confidential).

The non-anonymity of arbitration ensured that decisions of agents and customary judges were rooted into local reputational dynamics. We expect the experiment to reproduce disincentives to deviate from norm-compliant behavior, closely related to those faced by agents in their daily life.[70] Similarly, we expect systematic biases in arbitration decisions to reflect the customary favoritisms present in our setting (e.g. biases against women and in favor of well-known plaintiffs). On top of this, random relative endowment heterogeneity offers an experimentally generated source of discrimination. In the absence of a legal fallback, we expect arbitration decisions to mimic local egalitarian norms (Henrich et al., 2006), exogenously disfavoring those who invested a higher share of the capital.

6.4.4 The legal fallback

For about half of the game sessions, the game ended with the decision of the customary judge. The remaining sessions also included a fourth stage, consisting in the application of the formal law treatment. Agents could reject the arbitration outcome by appealing to a costly fixed law. At the cost of 10% of the final joint capital – a fictional measure of the costs

[69] Throughout the first and second stage of the experiment, customary judges where trained on understanding the game process and implications. The training only halted once each customary judge was able to individually explain the game procedure without external assistance.

[70] Henrich et al. (2006) show that ultimatum bargaining behavior mirrors local egalitarian and redistributive norms. They observe that laboratory behavior is consistent with economic patterns of everyday life in several small-scale societies, and that community characteristics explain experimental patterns better than individual level variation.

related to formal legal litigation – the law divided the joint venture capital according to initial investment shares. This rule, known as liquidating dividend policy, dates back to sixteenth century maritime trade expeditions, if not Hellenistic and Roman merchants (Benrud, 2009). It reflects a standard practice for joint-venture dissolution in several national legal systems, including that of Ethiopia.[71] It represents an alternative idea of fairness that may appeal higher investors that feel entitled to a higher portion of the joint capital. But it is also in sharp contrast with the concepts of distributive justice and egalitarianism that typically characterize rural communities (see Platteau, 2000). In other words, it allows us to study the effect of introducing a proportional split rule – also known as "liberal egalitarianism" (Cappelen et al., 2003) – in a context of "strictly egalitarian" norms (with biases), enforced by the customary judge.[72]

6.5 Empirical strategy

We investigate the research questions outlined at the end of section 2. First, we verify the presence of systematic bias against customarily disfavored agents and in favor of privileged ones, without a legal fallback (RQ1). Second, we look at these potential biases in the presence of a costly fixed law, expecting them to attenuate (RQ2). Third, we make use of the strictly egalitarian discrimination against (exogenously determined) higher investors, to verify that the competing formal law draws the arbitration

[71] The provisions of the Ethiopian Commercial Code referring to the dissolution of partnerships and joint ventures are provided under Articles 258(1) to 279(3).

[72] Strict egalitarianism requires that 'all inequalities should be equalized' (Cappelen et al., 2003: 818). In our context, it means that each partner in the joint venture would receive an equal share of the joint capital (in the absence of a legal fallback, this was the case in over 50% of the arbitrations). Instead, liberal egalitarianism accepts inequality as long as it stems from choices under individual control. In our context, it is closely represented by the fixed law, in which each partner receives a portion of joint capital proportional to their initial investment share.

decisions of customary judges closer to the law itself (RQ3). And fourth, we investigate the behavioral changes of agents, in terms of ultimatum offers and rejection probability, with and without legal fallback (RQ4).

In the absence of the legal fallback, we expect customary judges to discriminate against less "powerful" agents (e.g. women), and in favor of more "embedded" agents (e.g. known plaintiffs). Also, we expect these biases to decrease once a non-discriminatory legal alternative is introduced. We test RQ1 and RQ2 by observing the arbitration outcomes relative to individual characteristics of agents, through the following regressions estimated under the two separate treatments:

$$Payout_i = \alpha + \beta Female_i + \gamma Age_i + \zeta Education_i + \theta HighInvest_i + \kappa Known_i + \varepsilon_i \quad (1)$$

$$Payout_i = \alpha + \beta Female_i + \gamma Age_i + \zeta Education_i + \theta HighInvest_i + \kappa Known_i + \lambda(Known_i \times HighInvest_i) + \varepsilon_i \quad (2)$$

Where $Payout_i$ refers to the post-arbitration payout of individual i, as a fraction of the overall mean payout, $Known_i$ is a dummy taking value of 1 if the agent is known to the customary judge assigned to the arbitration, $Female_i$, Age_i and $Education_i$ indicate respectively the gender, age, and education level of the respondent, and $HighInvest_i$ takes value of 1 if the agent is the higher investor in the joint venture. Standard errors are clustered at the customary judge level. In the absence of a legal fallback, we expect biases in favor of known plaintiffs ($\kappa > 0$), especially if they are favored by the strictly egalitarian custom (i.e. known lower investors), and against women ($\beta < 0$). Once the formal law is introduced, we expect such biases to attenuate.

The experimental design exogenously imposes customary discrimination on half the agents—the higher investors in the joint venture. We proceed to assess the robustness of the previous finding by testing whether the exogenously imposed customary discrimination is reduced by the introduction of the legal fallback (RQ3). Assuming that the legal fallback is a credible threat in the eyes of arbitrators, the optimal strategy in our

experiment would be to reallocate to lower investors the "legal cost" that higher investors would lose by applying the law, therefore redistributing in the direction of the strict egalitarian norm, but not beyond reserve utility of higher investors. We estimate a specification with only the law dummy as a regressor (OLS), and gradually include other individual level variables to reach the following full specification:

$$Adev_{is} = \alpha + \beta Law_s + \gamma HighInvest_{is} + \lambda(Law_s \times HighInvest_{is}) + \zeta JVineq_{is} + \theta Udev_{is} + \iota X'_{is} + \varepsilon_{is} \quad (3)$$

where $Adev_{is}$ represents the deviation of each arbitration i from a proportional split in percentage points (i.e. the rule of law) for each arbitration,[73] Law_s is a dummy taking value of 1 if the session included the formal law option, $HighInvest_{is}$ is a dummy taking value of 1 if the ultimatum game sender is the higher investor in the joint venture, $JVineq_{is}$ is the deviation of joint venture investment shares from 50-50 in percentage points, $Udev_{is}$ represents the ultimatum offer deviation from a proportional split in percentage points, and X'_{is} is a vector of other individual and game characteristics. Other notations have the same meaning as in (1). We expect $\beta < 0$.

Finally, we look at the effect of introducing the legal fallback on the behavior of agents. Higher investors should benefit from the introduction of a law that imposes splits according to initial investment shares (RQ4). Whether they are willing to use the increased bargaining power deriving from the legal fallback depends, however, on the expected social cost of such action. Higher investors may forego the benefits of formal legislation in the presence of reputational concerns or expected social sanctioning. We measure such shift through the following fully specified equation:

[73] We regress arbitrations (129), rather than participant (258), as otherwise the same ruling would appear twice, for each individual in the pair.

$$Udev_{is} = \alpha + \beta Law_s + \gamma HighInvest_{is} + \lambda(Law_s \times HighInvest_{is}) + \zeta JVineq_{is} + \iota X'_{is} + \varepsilon_{is} \quad (4)$$

where $Udev_{is}$ represents again the deviation of the ultimatum offer of sender i (N=266) form a proportional split in percentage points, and all other notations have the same meaning as before. If social sanctioning or the threat of it is not stringent, we expect $\beta < 0$ and particularly $\lambda < 0$. Yet, a sufficiently high social cost of deviation from the norm may reverse these expectations. In our setting, the identity of everyone's' joint venture partner is kept anonymous, but the identity of agents is revealed to customary judges if the ultimatum offer is rejected—actions are observed by customary judges. If agents have reputational concerns particularly at heart, they may wish to signal their distaste for the law, and indicate their intention not to make use of it by making more strictly egalitarian ultimatum offers once the law is introduced. These concerns would only be expected from agents that stand to gain from the law, i.e. higher investors, and not from those that would lose from it. In such case, $\beta = 0$ and $\lambda > 0$.

As additional control to RQ4, we investigate the probability of ultimatum offer rejection. If agents favored by the custom anticipate a shift in arbitration outcomes once the law is introduced, their willingness to accept an ultimatum offer may increase. On the other hand, it is plausible that the legal fallback increases the prevalence of disputes, particularly if agents previously disfavored by the custom take advantage of their increased bargaining power (H4). We estimate a linear probability model with only the law dummy as a regressor, and gradually include other variables to reach the following full specification:

$$Dispute_{is} = \alpha + \beta Law_s + \gamma HighInvest_{is} + \lambda(Law_s \times HighInvest_{is}) + \theta Udev_{is} + \zeta JVineq_{is} + \iota X'_{is} + \varepsilon_{is} \quad (5)$$

where $Dispute_{is}$ is a dummy taking value of 1 if the joint venture i resulted in an arbitration,[74] and other notations have the same meaning as in (1) and (2). We expect $\gamma < 0$ and $\lambda > 0$ once the interaction term is introduced, and $\beta \neq 0$, where the sign of the coefficients depends on the dominating effect.

6.6 Results

In total our experimental setup induced 129 pairwise disputes arising from rejected ultimatum offers, involving 258 out of 532 villagers. 144 agents entered a controversy in the treatment with only the customary arbitration, the remaining 114 being from the one with the additional possibility of appealing to a fixed law. First, we test the assumption that customary courts may disfavor less powerful agents (RQ1) (Asfaw and Satterfield, 2010; Sandefur and Siddiqi, 2013), and that biases against them may be reduced by the introduction of a formal legal fallback (RQ2) (Aldashev et al., 2012a, 2012b). We regress the 258 payouts resulting from arbitration by a customary judge, on the individual characteristics of agents (Table 2). In the absence of law, we find that post-arbitration payouts are more than 31% lower for female players (Table 2, column 1). Also, arbitration outcomes are significantly higher for known lower investors—favored by the strictly egalitarian customary norm (Table 2, column 2).

> ***Result 1:*** *Arbitration outcomes result in lower payout to women and higher payout to known plaintiffs with lower initial investment shares.*

[74] Again, regressing at the joint venture level (266), instead of the individual level (532), avoids double counting outcomes.

Table 2: Skewed sentencing against the underprivileged

	Payout: relative to mean payout (=1)			
	Customary only		Customary + Law	
	(1)	(2)	(3)	(4)
Female	-0.313**	-0.289**	0.146	0.142
	(0.138)	(0.126)	(0.119)	(0.122)
Age	0.008	0.008	0.003	0.003
	(0.005)	(0.005)	(0.004)	(0.004)
Education	-0.002	-0.001	-0.007	-0.008
	(0.022)	(0.021)	(0.016)	(0.017)
Higher investor	0.234***	0.701	0.557***	0.516***
	(0.053)	(0.424)	(0.055)	(0.132)
Known	-0.062	0.169**	-0.009	-0.038
	(0.172)	(0.079)	(0.073)	(0.055)
Higher investor × Known		-0.539		0.063
		(0.454)		(0.202)
Observations	144	144	114	114
R^2	0.098	0.132	0.348	0.349

Notes: OLS, standard errors clustered at the judge level (in parentheses). Confidence: *** ↔ 99%, ** ↔ 95%, * ↔ 90%.

Most importantly, these statistical differences disappear once the legal fallback is introduced. The post-arbitration payouts for agents participating to the treatment with the legal fallback are not lower for women and not higher for known plaintiffs (Table 2, columns 3 and 4).

> ***Result 2:*** *Arbitration biases disappear once the legal fallback is introduced.*

Result 1 had been documented by previous literature both within and outside Ethiopia (Asfaw and Satterfield, 2010; Sandefur and Siddiqi, 2013). Result 2, instead, represents a novel finding. It indicates that customary dispute resolution institutions are susceptible to increases in competitiveness of formal laws. In response to the introduction of a legal fallback, customary judges change their verdicts, reducing discrimination against agents disfavored by the custom. On the other hand, their capacity to enforce strict egalitarian norms is weakened by the liberal egalitarian law—payouts of lower investors are on average almost 56% lower, instead of 23%.

Result 2 is all the more salient as in our experiment agents disfavored by customary outcomes make limited direct use of the formal law. In fact, only 12 plaintiffs ultimately appealed to the rule of law. In other words, over 91% of the agents settled for a share of capital below that enforceable through the rule of law. Aldashev et al. (2012a, 2012b) predict that, at least partially, this is the consequence of customary change in the direction of the law. As they put it, 'the "magnet" effect of the law is triggered by the preoccupation of village elders to maintain their authority and to retain enough potential claimants within the purview of their informal jurisdiction' (Aldashev et al., 2012b: 193). Next, we explicitly test if the legal fallback draws the decisions of customary judges significantly closer to the formal law (RQ3). To verify this, we take the deviation of arbitration outcomes from a split proportional to investment shares (i.e. the law), as a dependent variable. We find that arbitration outcomes are on average 10 percentage points closer to initial investment shares when the formal law is introduced (Table 3, column 1). This "magnet effect" is robust to introducing experimental and individual controls (Table 3, columns 2 to 4). Importantly, arbitration outcomes are still significantly different from the proportional split rule imposed by the law (p=0.000): on average, lower investors receive 36% of the joint capital (instead of 45%), against an initial investment of only 33%.

> ***Result 3:*** *Customary judges (partially) adapt arbitration outcomes in response to the formal law.*

Next, we proceed to investigate the behavior of agents across the two treatments (RQ4). Higher investors acquire bargaining power once the legal fallback is introduced, as the law grants them a portion of joint venture capital equivalent to the initial investment share. As a result, we could expect higher investors to make less egalitarian ultimatum offers. Yet, if they fear reputational loss and social sanctioning, they may not be willing to use their improved bargaining position, especially if their actions are observable. In our experiment, joint venture partners are unknown to

Table 3: Law shifts arbitration outcomes closer to the law

	Arbitration: deviation from proportional split			
	(1)	(2)	(3)	(4)
Law treatment	-0.102***	-0.107***	-0.105***	-0.104***
	(0.012)	(0.015)	(0.015)	(0.017)
Higher investor sender		0.047**	0.047**	0.043*
		(0.020)	(0.020)	(0.022)
Law × Higher investor sender		0.011	0.011	0.011
		(2.103)	(2.103)	(2.329)
Ultimatum offer deviation from law		0.316***	0.316***	0.274***
		(0.097)	(0.097)	(0.094)
Joint venture inequality			-0.025	-0.081
			(0.149)	(0.126)
Additional controls	No	No	No	Yes
Observations	129	129	129	129
R^2	0.475	0.560	0.560	0.591

Notes: OLS, standard errors clustered at the judge level (in parentheses). Additional controls in (4): Age, Education, Female, Joint venture capital, Joint venture profit, Number of counteroffers, Win risk game 1, Win risk game 2. Confidence: *** ↔ 99%, ** ↔ 95%, * ↔ 90%.

each other, but the arbitration process is non-anonymous, and customary judges play the role of community "observers" of the actions of senders. In the presence of egalitarian customary norms that counter the law, those who stand to benefit from the law may wish to signal their intention not to make use of it, by making more egalitarian ultimatum offers once the law is introduced. Table 4 shows that the legal fallback does not significantly change the deviation of ultimatum offers from the law (Table 4, column 1). In fact, higher investors seem to make more egalitarian offers in the presence of it (Table 4, columns 2 to 4). Higher investors make ultimatum offers that are on average 10 percentage points closer to initial investment shares compared to lower investors, but once the law is introduced they increase the relative distance from the law by 3 percentage points. Likewise, the legal fallback increases the probability that higher investors make an equal split offer from 13.3% to 27.8% (p=0.039).

Table 4. Higher investor senders do not take advantage of the law

	Ultimatum: deviation from proportional split			
	(1)	(2)	(3)	(4)
Law treatment	-0.007	-0.022	-0.011	-0.008
	(0.011)	(0.014)	(0.008)	(0.010)
Higher investor sender		-0.097***	-0.103***	-0.099***
		(0.012)	(0.012)	(0.012)
Law × Higher investor sender		0.039**	0.037**	0.032*
		(0.019)	(0.018)	(0.018)
Joint venture inequality			0.783***	0.793***
			(0.099)	(0.095)
Additional controls	No	No	No	Yes
Observations	266	266	266	266
R^2	0.002	0.208	0.333	0.352

Notes: OLS, standard errors clustered at the judge level (in parentheses). Additional controls in (4): Age, Education, Female, Joint venture capital, Joint venture profit, Number of counteroffers, Win risk game 1, Win risk game 2. Confidence: *** ↔ 99%, ** ↔ 95%, * ↔ 90%.

Finally, we look at the behavior of ultimatum receivers. If ultimatum receivers anticipate the shift in arbitration outcomes, their willingness to accept such offers may change even if senders do not take direct advantage of the legal fallback. Particularly, lower investors may not want to frustrate higher investors by refusing partially redistributive offers, motivating them to apply the costly law. We find that receivers reject significantly less ultimatum offers in the presence of the formal law: 40.7% against 57.1%. This result is driven entirely by reduced rejection rates of lower investors (-28 percentage points), previously favored by the pro-egalitarian custom, with no significant variation in rejection rates attributable to higher investor receivers (Table 5, columns 1 and 2). This result is robust to controlling for the investment share inequality between the higher and lower investor, and to other potential confounds (Table 5, columns 3 and 4).

> ***Result 4:*** *The formal law does not alter ultimatum offers. Disfavored senders (higher investors) actually make offers less advantageous to themselves. The likelihood of offer rejection is reduced, but only for customarily favored agents (lower investors).*

Table 5: The law reduces rejections by lower investor receivers

	Ultimatum: rejections (=1)			
	(1)	(2)	(3)	(4)
Law treatment	-0.164**	-0.280***	-0.258***	-0.263***
	(0.064)	(0.086)	(0.080)	(0.081)
Higher investor receiver		-0.023	-0.006	0.003
		(0.078)	(0.076)	(0.080)
Law × Higher investor receiver		0.260**	0.267**	0.261**
		(0.119)	(0.118)	(0.120)
Joint venture inequality			0.022***	0.021***
			(0.008)	(0.008)
Additional Controls	No	No	No	Yes
Observations	266	266	266	266
R^2	0.027	0.056	0.085	0.106

Notes: LPM, standard errors clustered at the judge level (in parentheses). Additional controls in (4): Age, Education, Female, Joint venture capital, Joint venture profit, Number of counteroffers, Win risk game 1, Win risk game 2. Confidence: *** ↔ 99%, ** ↔ 95%, * ↔ 90%.

6.7 Conclusions

This paper builds upon the work of Sandefur and Siddiqi (2013), and explicitly investigates the indirect effects that increased competitiveness of formal law may have on customary arbitration outcomes. Similar to them, we find that customary arbitration outcomes systematically discriminate against women, and favor well-embedded agents. We contribute to the literature on the interaction between customary institutions and formal law, by showing that not only agents, but also customary judges may respond to incentive structures. We do so by observing the ultimatum game decision of local villagers in rural Ethiopia, as well as the arbitration choices of real local customary judges, ruling over controversies born from the ultimatum game itself.

Introducing a costly legal fallback reduces arbitration biases and draws the decisions of customary judges significantly closer to the formal law. Agents disfavored by the custom, instead, do not take advantage of their increased bargaining power. If the formal legislation does not depart too radically from the custom (Aldashev et al., 2012a), customary dispute resolution institutions may have a role to play in shifting preexisting norms towards

the desired outcome: in areas where formal legal institutions have limited outreach, most socioeconomic gains of increased competition between statutory law and customary institutions may rise from changes in the latter, and not from plaintiffs seeking justice under the rule of law. On the other hand, formal legislation may limit the redistributive functions of customary legal institutions that enforce local pro-egalitarian norms. This is especially true when, as in our setting, the formal law provides an alternative rule of fairness (liberal egalitarianism vs. strict egalitarianism). Legislators should not overlook the potential contribution of customary legal institutions in changing the custom.

Appendix

Table A1: Attrition analysis

Variable	Non-participants (N=80)	Participants (N=532)	Diff.	Std. Err.
Male (%)	92.5	91.5	1.0	(3.32)
Age	41.8	43.5	-1.7	(1.36)
Married (%)	90.0	91.7	-1.7	(3.43)
Orthodox (%)	96.3	97.9	-1.6	(1.79)
Education	2.4	2.4	-0.0	(0.22)
Household size	6.2	6.3	-0.1	(0.25)
Non-farm income	0.2	0.2	0.0	(0.05)
Informal safety nets (%)	80.0	82.9	2.9	(4.56)

Notes: Confidence: *** ↔ 99%, ** ↔ 95%, * ↔ 90%.

Table A2: First risk game lottery choices

Selected ball value	Winning probability	Highest possible loss	Standard deviation	Expected value	N
1	8/8	0	0.0	1.0	9
2	7/8	1	1.0	1.6	15
4	6/8	2	2.4	2.6	39
8	5/8	4	5.1	4.1	130
16	4/8	8	10.1	6.1	143
32	3/8	16	19.0	8.1	125
64	2/8	32	33.6	8.1	51
128	1/8	64	52.3	0.1	20

Notes: Winning values are in tokens. Each lottery comprises 8 balls with value $B_n = 2^{n-1}$. Agents select the desired winning ball value B_s. If the ball extracted $B_e \geq B_s$, players win B_s, if $B_e < B_s$, they lose B_e.

Table A3: Experimental procedure

Stage 0	
0.1	Each participant is assigned a random endowment of either 80 or 120 tokens.
Stage 1	
1.1	Each participant is brought to a private location within the premises of the experimental field by an enumerator. Enumerators explain the first risk game to their participant, handing in the endowment of either 80 or 120 tokens.
1.2	The enumerator explains the rule of the risk game until the respondent is able to convey them back correctly. Then a trial round is performed.
1.3	After the trial round, the respondent is asked to explain the outcome of the 'investment'. If the explanation is correct, the actual game can take place, otherwise the enumerator explains the rules again until full understanding.
1.4	Once the first risk game is completed, the enumerator states the new endowment and proceeds with the second risk game. Steps 1.2 and 1.3 are thus repeated.

1.5 Once the second risk game is completed, the enumerator states the final private endowment of the participant. Each enumerator writes the amount on a 'private endowment card', signs it and hands it to the participant. The enumerator explains that that card represents the tokens obtained by the participant during the experiment, and states the value of the endowment in local currency. Once the participant has understood the value of the card, the enumerator seals it into a closed envelop.

1.6 At the end of the first stage, participants are reminded that their choices and earning are anonymous and that their participation to the next stages is tied to the redelivery of their sealed envelope the next day. Participants can go home.

Stage 2

2.1 The next day, each participant is randomly paired with another participant with higher or lower endowment. Participants within each pair are not known to each other and are brought to separate private locations by different enumerators. Once there, enumerators explain the rules of the joint-venture stage, separately to both participants.

2.2 Participants are told that they will merge their private endowment with an anonymous partner, and perform the same risk games as the previous time. This time, however, they will need to compromise on the investment choice. Enumerators state the value of the joint endowment and remind each participant about their private share within the joint venture.

2.3 Pairs bargain over the same risk games of step 1.1 to 1.4. For each risk game, enumerators record and deliver messages between the joint-venture partners, to reach a consensus over the investment choice. If a consensus is not reached by bargaining round 9 of each game, the enumerator may take the average of the two proposals, rounded down to the nearest available option, as investment choice.

2.4 After the two risk games are completed, the enumerator states the final joint-venture endowment of the pair. Each enumerator writes the amount on two 'joint endowment cards', signs them and hands them in to each participant in the pair. The enumerator explains that that card represents the total tokens obtained by the pair during the experiment.

Stage 3

3.1 Each enumerator randomly assigns one participant to be the sender, and one to be the receiver in an ultimatum game. Enumerators explain the rules of the game, and state that the game will determine which portion of the joint endowment will be theirs to take home. They state that if the receiver does not accept the offer of the sender, the litigation will be sent to a local customary judge.

3.2 Prior to making the ultimatum offer, both participants are informed about the name of the customary judge that would rule over the controversy in case the ultimatum offer is rejected. They are asked to state whether they know that customary judge, and are told that in case an arbitrage is required, the judge will be informed of their name and game history.

3.3 Throughout Stage 2, four customary judges have been instructed on the rules of the game. Each judge is asked to explain the rules of the two risk games, as well as the joint venture stage. If the explanation is not correct, the enumerator explains the rules again until full understanding. Before presiding an ultimatum game arbitrage, customary

judges are asked to sign an informed consent and confidentiality notice, requiring them to maintain the anonymity of game participants.

3.4 If the ultimatum offer is accepted by the receiver, the game terminates. Otherwise, the selected customary judge receives a game information sheet containing the names and game history of both participants, and independently reaches a final verdict.

Stage 4 (only for the 'Customary + Law' treatment)

4.1 Participants to the '+ Law' treatment are informed of the legal fallback during step 3.1. Similarly, judges are informed during step 3.3. Enumerators explain that the arbitration verdict can be overruled by a costly fixed law: at the expense of 10% of the joint endowment, the law imposes a split according to initial investment shares.

4.2 After the customary judge has emitted the verdict, each participant to the '+ Law' treatment is asked whether they accept or reject the verdict. If at least one pair member rejects the verdict, the costly fixed law is applied.

Variable definitions:

Female. A dummy variable taking value of 1 if the respondent is female, 0 otherwise.
Age. Age of respondent i in years, rounded down to the last birthday.
Married. A dummy variable taking value of 1 if the respondent is currently married, 0 if otherwise.
Orthodox. A dummy variable taking value of 1 if the respondent is Orthodox Christian, 0 if otherwise.
Education. A variable indicating the number of completed years of education of respondent i.
Household size. The number of people sharing the same roof and sharing the same pot.
Non-farm income. The estimated percentage of yearly household income not deriving from farm activities.
Informal safety nets. A dummy variable taking value of 1 if the participant belongs to at least one informal institution between Debo, Eqqub, and Iddir. Debo is a local labor exchange arrangement; Eqqub is a rotating savings and credit association, and Iddir is a funeral association functioning as informal insurance arrangement. 0 otherwise.
First risk game. The value of the selected ball in the first risk game.
Second risk game. The fraction of endowment chosen for investment in the second risk game.
Private Endowment. The value (tokens) of the endowment possessed by participant i at the end of the "private investment" stage.
Joint venture endowment. The value (tokens) of the endowment jointly possessed by each pair of joint venture participants at the end of the "joint venture" stage.
Sender. The participant that makes an ultimatum offer to the receiver, with respect to how to split the joint endowment.
Receiver. The participant that can accept or reject the ultimatum offer made by the sender
Law treatment. A dummy variable taking value of 1 if the game session included both the customary judge arbitration and the proportional law as outside options to the ultimatum game.
Higher investor. A dummy taking value of 1 if respondent i has a higher than 50% share of the joint venture capital.
Arbitration offer deviation from law (Adev). The deviation of the arbitration decision from a proportional split (the law), in percentage terms with respect to joint the endowment.
Ultimatum offer deviation from law (Udev). The deviation of the ultimatum offer made by the sender from a proportional split (the law), in percentage terms with respect to joint the endowment.
Joint venture inequality. The ratio between the private endowment of the lower investor and the private endowment of the higher investor in each joint venture.

CHAPTER 7
Synthesis

7.1 General discussion

In 1942 Hicks wrote that economics had made 'better progress in the application of scientific methods to the study of human conduct than has been made by other human sciences', and that 'the study of economics can therefore take us a considerable way towards a general understanding of human society, that is, of men's behavior to one another' (1942: p.2). Since then the economic science has made unimaginable progress, particularly with respect to economic modeling and statistical inference. However, it has made relatively less progress in unraveling the dynamics of human behavior, perhaps considering it for too long 'the province of the psychologist' (Friedman, 1962: p.13).

Yet, 'as the complexity of the environment increases, or its speed of change, we need to know more and more about the mechanisms and processes that economic man uses to relate himself to that environment' (Simon, 1959: p.279). Therefore economists have more recently returned to the behavioral underpinnings of individual decision making, primarily as tool to derive implications at a broader level. In fact, 'while the economic approach to behavior builds on a theory of individual choice, it is not mainly concerned with individuals' (Becker, 1993: p.402). Studying changes in individual decision making may reveal a great deal about how individuals respond to shocks, but it also contributes to narrow a gap in current economic scholarship—helping to understand the dynamics that undergird human interaction, and thus economic development.

The shocks discussed in this thesis do not necessarily refer to intrinsically negative "traumatic events". Rather, they represent unforeseen exogenous changes in the context in which individuals and institutions are embedded. They reflect both the complexity and speed of change of the environment in which individuals and institutions operate. Ethiopian sesame farmers, for instance, may not be as capable as professional traders in calculating expected utility (see List and Haigh, 2009). If exposed to competitive markets, however, they may change their behavior to fit the new circumstances (Chapter 4). At the same time, if the new institutional environment persists, they may lose some of the skills that were an important asset in previous interlinked markets but less so in competitive ones—such as the capacity to build long-term relationships with buyers and brokers. Similarly, within-village informal rules of cooperative behavior may be undermined by something as "simple" as the introduction of a formal health insurance (Chapter 5).

These responses are not passive processes of adaptation. Instead, they strongly support the idea that 'actors do not behave or decide as atoms outside a social context' (Granovetter, 1985: p.487). People respond to shocks through "rational" – although not necessarily conscious – changes in their behavior and decisions. This process of "creative destruction" is best exemplified through the long-lasting change in preferences for parochial altruism in response to conflict exposure (Chapter 3). While these changes result from direct experiences, they mirror the behavior that co-evolutionary models predict would be "promoted" by conflict over the very long run. Another example is provided by the response of non-adopters of a formal health insurance (Chapter 5). Those who do not insure their families respond to this inequality-increasing innovation with the only "weapons of the weak" at hand—by reducing cooperation in other domains. At the same time, they rationalize these forms of "cautious resistance" through narratives 'bemoaning the decreasing cooperation between villagers' (Scott, 1985: p.188), and attributing the root of the problem to those who have adopted the innovation.

Every chapter contributes to the understanding of the micro-foundations of either preferences, institutions, or both. Nonetheless, the two chapters representing the beginning and end of the zoom-out process represented by this thesis perhaps best illustrate how field experimental evidence can contribute to economic theory. The first one introduces the concept of "fetal origins" to the domain of other-regarding preferences, finding that the latter are being shaped right from the womb (Chapter 2). The last one, instead, studies the decision making of informal institutions, finding that institutions too may "respond" to incentives (Chapter 6). Whether expected or unexpected, these findings emphasize the need to continuously underpin theoretical predictions with empirical evidence; not only as a confirmatory tool, but especially to examine off-path behavior. The next sections review the main lessons learned from each chapter, discussing the resulting policy recommendations, and the implications for future research.

7.2 The fetal origins of preferences

In Chapter 2 we looked at the impact of prenatal trauma on the social preferences of children born during an armed conflict. We learned that prenatal hormonal distress – proxied by the 2D:4D digit ratio – reduce the child's probability of contribution to a public good. In other words, being exposed to traumatic events in the womb may shape later-life preferences away from cooperation. This finding represents the missing empirical link in an otherwise well-established causal chain across different disciplines. We already knew that violent conflict increases the likelihood of PTSD. Also, we knew that trauma and PTSD affect the hormonal releases transmitted to children during pregnancy—and that these have long lasting psychophysical effects on the fetus. Finally, markers of prenatal hormonal exposure were already known to correlate with other-regarding preferences. To the best of our knowledge, however, no study before had attempted to cover the entire causal chain.

What are the underlying policy implications? First, interventions in post-conflict settings are still mostly concerned with those who experienced war violence, but other groups are often neglected. The post-conflict recovery programs that I have witnessed in northern Uganda, for instance, typically focused on recovery from postnatal traumatization, helping victims regain their confidence and build positive relationships with their peers. Recognizing the fetal origins of preferences means more attention should be given to those who suffered from trauma before being born, and to women that endure pregnancy in highly traumatizing environments. As a result, the time horizon of post-conflict interventions should be extended, and target beneficiaries should be reconsidered. Second, policymakers should take into account the heterogeneity of the responses to conflict. Prenatal epigenetic effects on the preferences for cooperation seem to follow a distinct path from postnatal conflict exposure (see Chapter 3). Acknowledging this difference may help improve the effectiveness of post-conflict recovery programs.

From a research perspective, the notion that preferences may be shaped right from the womb implies that economic models on the intergenerational transmission of preferences should acknowledge an additional path of preference transmission, beyond the standard nature-nurture duality. Also, economists concerned with early life circumstances should not underestimate the direct impact of trauma on preferences. Yet, given that these results touch upon some of the basic underpinnings of economic theory, they need to be confirmed in different settings—to verify their generalizability outside the Ugandan context. Moreover, it would be important to test the stability of prenatal effects throughout life, preferably through panels of data eliciting the preferences of children at different points in time. Further research should also present more comprehensive evidence about the fetal origins of preferences, including primary determinants of savings and investments behavior such as risk and time preferences. Finally, the interaction between the prenatal and postnatal effects of conflict on behavior needs to be investigated in greater detail.

7.3 Conflict and preferences

In Chapter 3 we looked at aggressiveness and willingness to compete against in- and out-groups. We learned that conflict exposure increases aggressiveness and willingness to compete towards the out-group, but not the in-group. These effects are in line with coevolutionary theories, highlighting the role of conflict in promoting parochial altruism. Taken together, the results of Chapter 2 and 3 contribute to the growing literature about the consequences of conflict for preferences and institutions. The temporary shock represented by conflict may induce long lasting behavioral changes on those who experience its violence, affecting local institutional and social equilibria—fundamental drivers of long-run economic performance that 'govern whether a society recovers, stagnates, or plunges back into war' (Blattman and Miguel, 2010: p.8).

From the point of view of policymakers, the findings of Chapter 3 are most intriguing. Post-conflict recovery programs tend to see those that witness war violence as victims of 'development in reverse' (Collier et al., 2003: p.13). In fact, conflict remains a decisive factor generating and intensifying the problems of global poverty and underdevelopment. Yet, conflict may also contain the sprout of inclusive and dynamic societal transformations. First, it may foster in-group altruism and cooperation. A notable example of this rests in the unprecedented expansion of taxation – and the creation of the welfare state – experienced by Europe after the Second World War. Second, it may reduce the pressure of traditional redistributive norms that favor strict egalitarianism and disfavor competition (see Platteau, 2000; Di Falco and Bulte, 2011). Emblematic of this is the common choice among ex-child soldiers in countries like Sierra Leone, Liberia and DRC to enter the "perfectly competitive" sector of motorcycle taxis. Policymakers should not overlook the significance of these autonomous responses. Instead, they should make use of these transformations to improve the process of demobilization of combatants, and to speed up reconstruction efforts.

A clear limitation of most empirical studies on the consequences of conflict is that they rely on the assumption that selection into victimization is not driving the results. Working with children born shortly before or during a conflict reduces this concern, but it does not eliminate it. Future research in this field should focus on case studies for which panels of data have been collected prior to the intensification of war violence. Moreover, it should look into the intergenerational effects of conflict on preferences. These changes matter in the long-run only insofar as the preferences of the generations that follow the cessation of violence do not return to a "steady state". While this is not likely to be the case, direct evidence in favor or against this is still missing. Finally, within the theoretical framework of parochial altruism different studies have reported divergent results depending on whether the conflict sparked inter- or intra-community violence (Voors et al., 2012; Cassar et al., 2013; Rohner et al., 2013; Gilligan et al., 2014). A more comprehensive concept of in- and out-groups should be debated and defined, to limit ex-post rationalizations.

7.4 Endogenous rationality

In Chapter 4 we looked at the role of market experience in promoting rational choice. We learned that exogenous exposure to competitive markets fosters more rational behavior—proxied by fewer GARP violations in a simple choice experiment. This sustains the theory of "endogenous rationality". Markets are not only neutral institutions to efficiently allocate resources. They also improve the rationality of the decision-making process of participants, introducing the possibility of dynamic efficiency gains associated with market expansion in developing countries. On the other hand, differentiated "competitive market experience" levels could affect partial equilibria and regional rent allocation.

This is all the more salient in light of the increasing expansion of market institutions. If the process of globalization is not neutral, the penetration of international companies and markets in relatively remote areas of the

world may result in hard to gauge "externalities" at the local level. This may turn out to be a blessing for some, and a curse for others. Policymakers should consider differential "learning costs" when designing policies aimed at stimulating market developments.

Traditional microeconomic theories typically rest on the assumption that human decision making is in some way approximated by rational choice, which is in turn determined by stable and clearly defined preferences. To this, the concept of "bounded rationality" introduces information sets, cognitive limitations, and the idea that agents may weigh the costs and benefits of different levels of cognitive effort when making a choice (see Simon, 1957; Rubinstein, 1998; Gigerenzer and Selten, 2002). Yet, 'when competence at decision making can be improved only at a cost, competence becomes endogenous' (Conlisk, 2005: p.486). If market experience reduces the perceived "costs" associated with decision making – such that rational choice may become relatively easier to attain – treating rationality as exogenous may be untenable. Moreover, endogenous rationality may affect the endogenous formation of preferences in the longer run—even if the institutional shock that generated it is temporary. When developing models of human behavior in rapidly changing institutional environments, economists should not overlook the implications of both endogenous rationality and endogenous preference formation.

7.5 Formal institutions vs. informal institutions

In Chapters 5 and 6 we looked at how informal institutions respond to shocks, in the form of exogenous formal alternatives. The notion that institutions "respond" is perhaps hardest to comprehend, and is clearly intended as figurative. Yet, the process of adaptation of informal institutions can be analyzed through the behavioral lens of individual decision making. In a way, that is what studies exploring social capital do when using individual public good contributions to proxy the former (e.g. Fearon et al., 2009). In Chapter 5 we did exactly that. We learned that

formal insurance institutions may crowd-out informal social capital and cooperation. In contrast with some theoretical predictions, however, we find that social capital erodes because the non-adopters, and not those insured, lower their contributions.

This finding highlights the need for greater communication between policymakers and researchers. In particular, it shows how seemingly harmless development interventions aiming to address a clear market failure – the absence of a functioning insurance market – may have unforeseen consequences for targeted communities. What would happen to social capital and informal sharing norms if the insurance program would suddenly terminate? Would they bounce back to the pre-program levels, or would they remain stably lower? The transient nature of development interventions makes these questions even more relevant. Many development programs I have witnessed only intended to target a group of beneficiaries for a relatively short time before moving on. The intervention object of study, for example, had an intended lifespan of five years. Donors should consider the unintended social implications of their interventions, following-up on targeted communities even after projects have ended.

From a research perspective, economic models should embrace the multidimensionality and embeddedness of social interactions. However, more empirical and experimental evidence is needed, especially with respect to long-run responses of informal institutions to the increasing penetration of formal alternatives. Moreover, these dynamics are likely to be at play beyond the domain of insurance. We need to understand the extent to which these results can be generalized to other institutional arrangements, such as microfinance, old-age care, and any other "extension" effort by formal institutions to replace informal ones (see Chapter 6 for an example). Once more evidence is collected economists should work towards a generalized theory about the interaction between formal and informal institutions.

7.6 Institutions "respond" to incentives

In Chapter 6 we departed from the standard debate about the interdependence of formal and informal institutions. We did not investigate the direct effects of increased penetration of formal legal institutions, but rather their spill-over on the rulings of customary legal authorities. We learned that customary judges respond to the "threat of law" by changing their dispute-resolution decisions in the direction of the law itself. Those disadvantaged by the customary system, instead, do not take direct advantage of their increased bargaining power, perhaps in fear of social sanctioning.

These responses are in line with the theoretical framework developed by Aldashev et al. (2012a, 2012b), in which customary judges are averse to the jurisdictional and reputational erosion deriving from plaintiffs appealing against their rulings. In other words, informal institutions change for the very reason that they do not "want" to be substituted. This finding has clear policy implications: informal dispute-resolution institutions may have a role to play in shifting the latter towards a desired outcome. In fact, while customary authorities may intrinsically value custom-compliant rulings, they do not necessarily face the social sanctioning associated with deviating from it. In areas where formal legal institutions have limited outreach, policymakers typically see informal legal institutions as change-inhibiting. As a result, they may delegitimize, if not outlaw, these institutions, expecting that formal laws would replace local customs and norms. Instead, policymakers should aim at increasing the integration between state law and other normative orders. One example of this is that of the *Katarungang* Pambarangay, in the Philippines. There, the traditional dispute-resolution institutions have been integrated in the formal legal system as "first instance" courts. Judges are still selected by disputants among the eligible residents of each village, but dispute settlements may be appealed to the higher level municipal court (see Silliman, 1985).

Future research on the penetration of formal law into predominantly customary settings should focus on dynamic outcomes. Moreover, greater attention should be devoted to the extent to which "moderately progressive" formal laws may have a greater "magnet effect" on customs than "radically progressive" ones (see Aldashev et al., 2012a, 2012b). Finally, it would be important to complement lab-in-field evidence with naturally occurring data—ideally through a natural field experiment.

7.7 Final remarks

This thesis started with the intention to investigate the endogenous formation of preferences and institutions. Through the lens of individual choices, it looked at endogenous responses to shocks at both the individual and community levels. Yet, 'coming to an end always means that one is making a cut, leaving many questions not asked and others not answered' (Balogh and Treumann, 2013: p.399). There is still a lot to be understood about how preferences and institutions evolve and interact. This thesis has helped scratch the surface, exploring some of the most pressing questions, and raising several new ones. One question that is not addressed, but weaves through all the findings, is: if preferences and institutions are endogenous and continuously reshaped, what can still be considered exogenous? Perhaps economic theory should increasingly move towards a "dynamic systems" approach. Until then, field experimental methods are likely to remain a fundamental tool to explore the complexity of human interactions, and should be employed in as diverse settings as possible. Even though all the evidence in this thesis comes from Sub-Saharan Africa, I believe that similar dynamics may be unraveling in very different environments too—hence the references to the recent history of China. This needs to be confirmed. To this end, I will be glad to share any of the questionnaires, experimental protocols, and datasets used for this thesis, and I strongly encourage other researchers to make use of them.

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Summary

In this thesis I investigate the formation of preferences and institutions. Although these concepts are central to economic theory, there is still a lot to be understood about how preferences and institutions change in response to shocks. I present field-experimental evidence from Sub-Saharan Africa, gradually zooming out through different levels of responses to shocks. In Chapter 1 I introduce the concepts of preferences and institutions, presenting an overview of the methodologies and research questions guiding the core chapters.

In Chapter 2 I look at the fetal origins of preferences for cooperation. I study the effect of prenatal trauma on the cooperation of those born during the Lord's Resistance Army insurgency in northern Uganda. I find that a rise in the relative length of the index finger with respect to the ring finger – a marker for prenatal hormonal shock – reduces the child's probability of contribution to the public good. I interpret this as evidence that prenatal trauma may affect later-life individual preferences, and that the nine months in utero may be more important than previously thought.

In Chapter 3 I look at the preferences for competition towards in- and out-groups, in relation to conflict exposure. I study aggressiveness and willingness to compete among youth in Sierra Leone, using the group dynamics generated by a local football tournament to separate in- and out-group behavior. I find that football players that experienced more intense exposure to violence are more likely to get a foul card during a game. Also, I isolate competitiveness from aggressiveness in the lab, and find that conflict exposure increases the willingness to compete towards the out-group—not the in-group. I conjecture that violent conflict is not only a destructive process, but that it may also trigger autonomous transformations in believes and preferences.

In Chapter 4 I look at the endogeneity of rational choice among adults. I study the relationship between market exposure and rationality in rural Ethiopia, through a laboratory experiment involving sesame brokers and farmers. Following a randomly assigned trading session in a competitive auction, I find that farmers and brokers selected for the treatment behave more rationally than their peers in the control group. Markets are thus not only neutral institutions; they change the way people make decisions. I speculate that, in the presence of endogenous rationality, a rapid market expansion may offer dynamic efficiency gains, but that it may also affect the distribution of rents and wealth at the local and regional levels.

In Chapter 5 I look at the relationship between formal and informal institutions. I study the dynamics of social capital – proxied by contributions to a public goods game – in response to the introduction of a formal insurance scheme in southwestern Uganda. I find that formal insurance crowds-out social capital, but that it is not those adopting the formal insurance who reduce their contributions (as predicted by theory). Instead, social capital erodes because of the uninsured. I argue that this is consistent with "weapons of the weak" theories, emphasizing social embeddedness. Those who fear to lose from this inequality-increasing innovation respond with the only "weapons" at hand—by reducing cooperation in other domains.

In Chapter 6 I look at how the penetration of formal law affects customary legal institutions. I study the effects of introducing a formal legal alternative on the arbitration decisions of real customary judges in Ethiopia. I find that introducing a legal fallback reduces arbitration biases and draws the decisions of customary judges significantly closer to the formal law. At the same time, agents disfavored by the custom do not take advantage of their increased bargaining power. I argue that most effects of increased competition between formal law and customary legal institutions may rise from changes in the latter, rather than from plaintiffs seeking justice under the rule of law. Chapter 7 offers a discussion and synthesis.

Samenvatting

In deze dissertatie onderzoek ik de vorming van voorkeuren en instituties. Hoewel deze concepten centraal staan in economische theorieën, is er nog steeds veel onbekend over hoe voorkeuren en instituties veranderen als reactie op schokken. Ik presenteer resultaten uit veldexperimenten uitgevoerd in Sub-Sahara Afrika, waarmee er telkens op een hoger niveau naar de respons op schokken gekeken wordt. In Hoofdstuk 1 introduceer ik de concepten voorkeuren en instituties, en geef ik een overzicht van de onderzoeksmethoden en -vragen die de kernhoofdstukken vormgeven.

In Hoofdstuk 2 onderzoek ik de foetale oorsprong van preferenties voor samenwerking. Ik bestudeer het effect van prenatale trauma's op het samenwerkingsvermogen van personen die zijn geboren tijdens de opstand van de Verzetsleger van de Heer in het noorden van Oeganda. Ik vind dat een stijging van de relatieve lengte van de wijsvinger met betrekking tot de ringvinger – een kenmerk voor prenatale hormonale schokken – de waarschijnlijkheid dat een kind bijdraagt aan het 'publieke goed' vermindert. Ik interpreteer dit als een aanwijzing dat prenatale trauma's voorkeuren in iemands latere leven kunnen beïnvloeden, en dat de negen maanden in de baarmoeder mogelijkerwijs belangrijker zijn dan oorspronkelijk gedacht.

In Hoofdstuk 3 kijk ik naar de voorkeuren voor concurrentie jegens leden van de groep en buitenstaanders, in relatie tot blootstelling aan conflict. Ik bestudeer agressiviteit en de bereidheid om te concurreren onder jongeren in Sierra Leone, met behulp van groepsdynamiek gegenereerd door lokale voetbalwedstrijden, om zo gedrag jegens leden van de groep te onderscheiden van gedrag jegens buitenstaanders. De resultaten laten zien dat voetbalspelers die intensere blootstelling aan conflict hebben ervaren, meer kans hebben om een rode of gele kaart te krijgen tijdens een wedstrijd. Daarnaast isoleer ik competitiedrang van agressie waaruit blijkt dat blootstelling aan conflict de bereidheid om te concurreren buiten de groep – niet binnen de groep – vergroot. Ik vermoed dat gewelddadig conflict niet alleen een destructief proces is, maar dat het ook kan leiden tot autonome veranderingen in overtuigingen en voorkeuren.

In Hoofdstuk 4 kijk ik naar de endogeniteit van rationele keuzes onder volwassenen. Ik bestudeer de relatie tussen blootstelling aan de markt en rationaliteit in ruraal Ethiopië, door middel van een lab-experiment met sesamzaadmakelaars en -boeren. Na een willekeurig toegewezen handelssessie in een competitieve veiling, vind ik dat boeren en makelaars die geselecteerd zijn voor de interventie zich rationeler gedragen dan hun gelijken in de controlegroep. Markten zijn dus niet alleen neutrale instituties; ze veranderen de manier waarop mensen hun keuzen maken. Ik vermoed dat door endogene rationaliteit een snelle uitbreiding van de markt dynamische efficiëntie kan vergroten, maar dat het ook de verdeling van economische baten en welvaart op het lokale en regionale niveau kan beïnvloeden.

In Hoofdstuk 5 bekijk ik de relatie tussen formele en informele instituties. Ik bestudeer de dynamiek van sociaal kapitaal - waarbij de bijdragen in een publiekegoederenspel als maatstaf hiervoor dienen - als reactie op de introductie van een formeel verzekeringsstelsel in het zuidwesten van Oeganda. Ik vind dat het stelsel sociaal kapitaal verdringt, maar dat het niet diegenen zijn die de formele verzekering aannemen, die hun bijdragen verminderen (zoals wordt voorspeld door de theorie). Integendeel, sociaal kapitaal wordt aangetast door de onverzekerden. Degenen die bang zijn om achteruit te gaan door deze ongelijkheid-vergrotende innovatie gebruiken het enige "wapen" dat zij hebben: het verminderen van de samenwerking op andere gebieden.

In Hoofdstuk 6 bekijk ik hoe het binnendringen van formeel recht de instituties rondom gewoonterecht beïnvloedt. Ik bestudeer de effecten van het introduceren van een formeel en legaal alternatief op de arbitrale beslissingen van rechters binnen het gewoonterecht in Ethiopië. Ik vind dat het introduceren van een legaal alternatief arbitrale afwijkingen vermindert en dat de beslissingen van rechters binnen het gewoonterecht aanzienlijk dichter bij het formele recht komen. Tegelijkertijd maken degenen die kansarm zijn in het gewoonterecht geen gebruik van hun sterkere onderhandelingspositie. Ik betoog dat de meeste effecten van toegenomen concurrentie tussen formeel recht en gewoonterecht ontstaan door veranderingen in het laatstgenoemde, in plaats van door procespartijen die gerechtigheid zoeken middels de rechtsstaat. Hoofdstuk 7 biedt een discussie en synthese.

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Completed Training and Supervision Plan

Wageningen School of Social Sciences

Name of the activity	Department/Institute	Year	ECTS
A) Project related competences			
Advanced Econometrics (AEP 60306)	Wageningen University	2011	6
Spatial Econometrics - Theory and Practice	WASS	2011	1.5
Game Theory with Applications	WASS	2011	1.5
Advanced Microeconomics (ECH 32306)	Wageningen University	2012	6
Panel Data Econometrics - Microeconomics	NAKE	2012	3
Behavioral and Experimental Economics	WASS	2012	1.5
ArcGIS 10	GeoDesk WUR	2013	1.5
B) General research related competences			
Research Proposal	WASS	2011	4
Teaching Assistant DEC-20806	Wageningen University	2011	1
Teaching Assistant DEC-20306	Wageningen University	2012	1
Teaching Assistant RDS-22306	Wageningen University	2013	2
C) Career related competences/personal development			
Regular PhD meetings	DEC	2011-2014	4
Reviewer/Discussant at PhD Day	WASS	2012	1
'Violent Conflict and Competitiveness: Experimental Evidence from the Football Field in Sierra Leone'	CSAE (Oxford)	2012	1
'Formal Insurance and the Dynamics of Social Capital: Experimental Evidence from Uganda'	NCDE (Helsinki)	2014	1
'Can Formal Law Induce Customary Change? A Lab-in-Field Experiment in Ethiopia'	Gorman Workshop in Economics, Oxford University	2014	1
'Does Prenatal Trauma Reduce Cooperation? Evidence from a Public Goods Experiment in Post-conflict Uganda'	Effects of Early Interventions on Child Health and Education, Surrey University	2014	1
Recognized Studentship - Hilary Term	Oxford University	2014	2
D) Advanced courses previously attained at WUR			
Advanced Macroeconomics (ENR 30806)	Wageningen University	2008	-
Qualitative Data Analysis: Procedures and Strategies (YRM 60806)	Wageningen University	2008	-
Total			**40.0**

*One credit according to ECTS is on average equivalent to 28 hours of study load

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