Institutions, Violent Conflict, Windfall Gains and Economic Development in Africa

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This research was conducted under the auspices of the Wageningen School of Social Science (WASS).

Institutions, Violent Conflict, Windfall Gains and Economic Development in Africa

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Thesis

submitted in fulfillment of the requirements for the degree of doctor
at Wageningen University
by the authority of the Rector Magnificus
Prof. dr. ir. M.J. Kropff,
in the presence of the
Thesis Committee appointment by the Academic Board
to be defended in public
on Wednesday November 16, 2011
at 4 p.m. in the Aula.

Maarten Jan Voors

Institutions, Violent Conflict, Windfall Gains and Economic Development in Africa
166 pages

Thesis, Wageningen University, Wageningen, NL (2011)
With references, with summaries in Dutch and English

ISBN 978-94-6173-102-9

Dedicated to my parents

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Chapter 1

Introduction

About three decades ago, in 1978, the first World Development Report, was written in an effort to provide an in-depth analysis of development and make policy recommendations aimed to call governments and non-governmental agencies worldwide to action (see Yusuf, 2009). Since then, 33 World Development Reports have been written, covering a diverse set of topics deemed important for development: ranging from the role of agriculture, governance, the environment, health, poverty and equity to the role of geography, climate change and civil war.

In the course of the past decades, the fortunes of developing economies have meandered, and the impact of foreign policy and development assistance has been subject to a heated debate (Easterly 2009). The first World Development Report could not have come at a more crucial time. The mid-1970s marked the start of what was later referred to as *Africa's growth tragedy*[1] – the stagnation and decline of living standards for nearly a billion people over the three subsequent decades. In the post WWII years, many African countries saw development take off, but after 1975 their fortunes changed and across the board incomes steadily declined: average living standards across Africa dropped by about 20 percent (see Figure 1). The African growth collapse is seen as one of the greatest economic disasters of the late twentieth century (Easterly and Levine 1997).

To understand the causes of Africa's recession several, candidate explanations have been advanced: ranging from geographical impediments to a crisis in policy. Yet, while these explanations have provided useful insights and perspectives, they fall short to explain the heterogeneity in the growth path of most African nations; some countries only suffered a weak recession, while others saw living standards decline dramatically. For example, the economy of Botswana grew steadily over the past decades, yet why has the economy of Liberia predominantly declined? And why has economic growth in Rwanda been so high following the end of the civil war while growth in neighboring Burundi is still sluggish? Why has Cote d'Ivoire plunged back into fighting and has Ghana escaped civil war since Independence?

As will be argued, the answers to these questions are inherently complex as a multitude of factors interact and depend on time and country.

[1] Whenever the term Africa is used, I refer to sub-Saharan Africa.

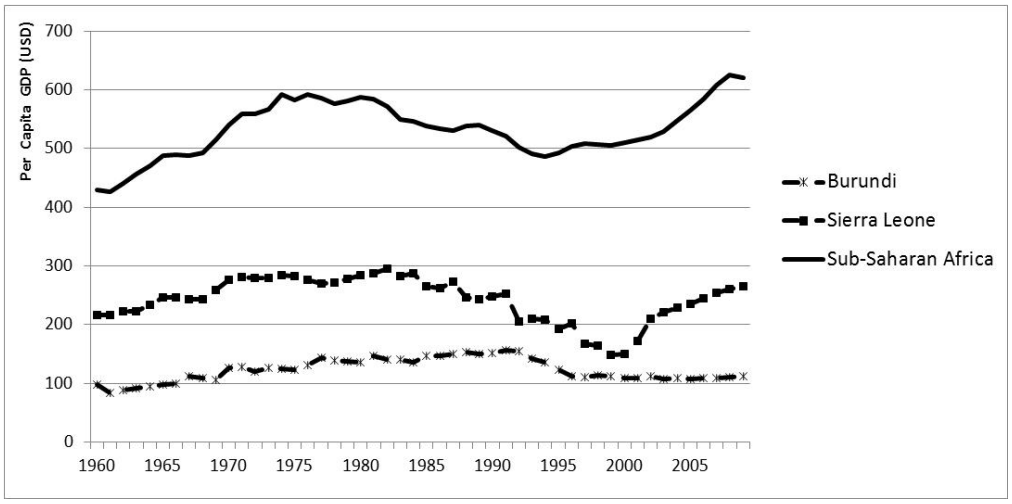

Figure 1. Per capita GDP for Burundi, Sierra Leone and sub-Saharan Africa average[2]

Figure 1 shows the development of per capita income during 1960 – 2005 for Africa and for two individual countries, which are the main focus of this thesis: Burundi and Sierra Leone. While different in many accounts - Burundi is a resource poor, densely populated country in East Africa and Sierra Leone, a resource rich, land abundant country on Africa's West coast – there are parallels in their history. Both have been theatres of excessive violence in civil war, know widespread poverty and rank at the bottom of almost any human development indicator. Economically, Burundi and Sierra Leone knew steady increases in per capita incomes between 1950 and the late-1970s. Yet, in the early 1980s economic growth in Sierra Leone slowed due to a decline in diamond mining and increased corruption. In the decade that followed economic activity declined even further, and in the early 1990s the country became engulfed in a brutal civil war that lasted until 2002. By that time the economy was in shambles, having reached a historical low. Across the East, Burundi followed a similar path of development with steady, albeit lower, economic growth until the early 1990s, when ethnic tensions exploded and a 10-year civil war between the countries main ethnic groups (Hutu and Tutsi) broke out. Currently, a decade after the civil wars ended, economic growth in both countries is slowly taking root and improvements on many development indicators can be seen.

This recent positive income trend is in line with development elsewhere in the continent (Figure 1). Many African countries started to recover in the mid-1990s and there is reason to believe that this trend will continue. For one, the prices of important commodities, such as oil, minerals and agricultural produce have risen. In part this growth is accounted for by an increased demand from the rapidly growing economies in

[2] Authors calculations based on World Bank Indicators 2011

Asia (mainly China and India). Not only demand has increased. Asian investments in resource extractive activities (such as oil and minerals) and public goods (roads, dams, telecommunication, etc) have shot up dramatically also. At the same time, many civil wars have ended: at the moment for the first time in 50 years, more African countries are at peace than at war (Nkurunziza 2008).

Indeed, from a quick glance at Figure 1, incomes in Africa seem to be improving. Yet the picture is not as positive as it would seem: while growth has gone up, the fraction of people living below the poverty line has gone up too (Ivanic and Martin 2008). As many African households (especially in urban centers) are net buyers of food, increased food prices have put a heavy weight on many people's budget. This trend is likely to continue in the future. One estimate shows that food production will have to rise by 70% in order to keep pace with population growth (Economist 2011). It is uncomforting to note that increased poverty levels provide an exacerbating risk for the outbreak of civil war (Collier and Sambanis 2005, Fearon and Latin 2005). Testimony of the fragile state of many African countries is the outbreak of recent violence in Cote d'Iviore, the divide of North and South Sudan and the constant re-emergence of violence in Uganda and the DRC.

Many development practitioners and academics have attempted to explain (under) development in Africa and the role of demographics, natural resources, institutions, civil wars, etc therein. Testimony are the wide range of topics covered in the World Development Report pages. These themes signal the state of thinking about development and show that the answers proposed have changed over time. The first decades of development thinking were motivated by the Washington Consensus, where a central role was reserved for the functioning of markets. This view on development attracted criticism from people pointing to the pivotal role institutions play in development. As a response, in the past decade development thinking has moved to a post-Washington Consensus stage where the main emphasis is on institutions and governance (Rodrik *et al.* 2004).

Academic literature too has increasingly pointed to the quality of institutions as the main driver of (under) development across the world (Acemoglu, *et al.* 2005). At the same time, it is recognized that institutions themselves endogenous to economic development and a growing body of literature seeks to understand the factors shape them (Williamson 2000, Rodrik *et al.* 2004). Yet, even though aid transfers have become conditional on improvements in institutions, such as the reduction of corruption and

improving the freedom of speech and political expression, how to improve institutions and promote development remains an open question. This is alarming, as the structural factors related to development: food prices, climate change, resource rents, civil wars, etc are expected to become more problematic (APR 2011). At the same time, the role of aid in development has been a polemical debate between its proponents and its adversaries, arguing about the proper scope of international development assistance. These views range from grand transformational approaches based on a blueprint development programs (such as the Millennium Development Goals initiative advanced by Jeffrey Sachs) to incremental approaches looking at piece meal development possibilities (see Easterly 2006). Summarizing the literature on aid and development, Easterly (2009) concludes that little has been learned from the past six decades of foreign assistance: the same ideas are recycled, blue print development is still attractive, and the research on which development policy works best is shaky in terms of its identification strategies, sampling frames and indicators. One conclusion is that more and better research is needed. Especially, what is needed is a better understanding of the micro foundations of the impacts of, for example, civil war, aid and institutions on growth in order to explain the heterogeneity in country responses. Many questions are as yet unanswered. How do institutions respond to fluxes in incomes? Are all types of institutions affected in a similar manner? Does civil war affect factors of production and institutions? Do they fall and by how much? Do they recover and how fast? Do people change their behavior in a way that is conducive to development or detrimental? How can development assistance play a role in mitigating the negative effects of conflict?

1.1 Objectives and research questions

The overarching objective motivating the chapters presented in this thesis is to improve the understanding of (some of) the interrelationships between institutions, windfall gains, civil war and economic development. It is not my objective to provide a comprehensive treatise linking all four concepts, rather the chapters consist of independent academic papers, each intended as stand-alone contributions to the literature. More formally the research questions break down to:

1. How do windfall gains alter the quality of institutions (chapter 3)?
2. How does violent conflict shape institutions and behavior (chapter 4 and 5)?

3. To what extent have grievances over social institutions, that are thought to have contributed to the onset of civil war in Sierra Leone, persisted in the post-war period (chapter 6)?
4. To what extent can behavioral experiments be used to study social institutions (chapter 7 and 8)?

1.2 Outline

The chapters in this thesis are organized as follows. In chapter 2, I discuss the main concepts of this thesis – economic development, civil war, windfall gains and institutions – and show where the individual chapters contribute to the literature on these topics.

In chapters 3, I focus on the impact of windfall gains resulting from productivity shocks, on the quality of institutions at the macro (cross country) level. In the chapter, I revisit the debate on income and institutions for a panel of African countries and investigate whether a virtuous cycle exists. Do incomes and institutions move together in a way that they positively reinforce each other? I find evidence of the contrary. Income booms tend to decease institutional quality. Further analysis reveals that if governments can bring corruption levels under sufficient control, the detrimental effects of windfall gains may be avoided.

In the next series of chapters, chapter 4-8, I switch to the micro level and use a novel dataset collected in Burundi and Sierra Leone. In chapter 4 and 5, I look at the impact of the civil war in Burundi on institutions and behavior. In chapter 4, I look at the impact of violence on a set of institutional variables including tenure rights security, the rule of law and the strength of social capital. Chapter 5, I extend the analysis and look at the impact of conflict on behavior, using a series of behavioral experiments to measure social, risk and time preferences. The analysis poses a challenging question. If war effects human behavior and institutions and these effects still observable several years after the conflict has ended, it would demonstrate that conflict shocks can have long-lasting effects on growth enhancing factors.

In chapter 6, I focus on Sierra Leone and investigate grievance motivations for the onset of civil war. The chapter investigates whether these social grievances, over farm labor and marriage, that are thought to have contributed to the outbreak of the violence, have persisted in the post-war period in the Eastern and Southern part of the country, the cradle of the war.

The final empirical chapters, 7 and 8, are methodological contributions. Both chapters focus on a series of behavioral experiments aimed at measuring social institutions, and provide some insights into the external validly of these experiments in the field. Chapter 7, investigates the relationship between public spiritedness, as measured in a public goods game, and conservation. In similar fashion, in chapter 8, I look at whether behavior in a public good game is predictive of behavior in a village public good aid project in chapter 8. Finally, chapter 9 provides a discussion of the main findings of this thesis, reviews its limitations and offers some recommendations for future research.

Chapter 2

Institutions, Violent Conflict, Windfall Gains and Economic Development

In this chapter, I provide an overview of the literature on institutions, violent conflict, windfall gains and economic development and identify where the subsequent chapters aim to contribute the literature. Section 2.1, introduces the research on economic development and argues that institutions have taken center stage in explanations on (under) development in Africa. In section 2.2, I discuss the role of institutions on the growth process in more detail and focus specifically on the role of windfall gains and civil war as factors shaping institutions (the focus of chapters 3-6). Finally, section 2.3 discusses methodological issues on the measurement of institutions (to which chapters 7 and 8 contribute).

2.1 Origins of Economic Development

Understanding the origins of the (unequal) distribution of per capita income, both between and within countriesm has attracted a vast body of literature. Early contributions were inspired by Harrod (1939), Domar (1946) and Solow (1956), who laid the foundations of growth theory. Solow showed that varying rates of savings and population growth jointly determine the steady state level of per capita income for a given country. The model produces simple and empirically testable hypotheses. At first glance the model predictions seem to fit the data, countries with higher saving rates and/or lower population growth rates are richer. Yet, while the model does well in predicting the direction of income differences, it assumes that the factors driving these differences are exogenous. In response, several modified models have been advanced, taking into account the determinants of saving, population growth, and worldwide technological change.[3] These models are increasingly able to explain income differences between countries. Still, when holding these models to an empirical test, the conventional factors of growth (labor, physical and human capital accumulation) do not fully explain the growth experience of many countries, especially those in Africa (Barro 1991, Easterly

[3] See for example Mankiw *et al.* (1992) for the augmented Solow model with human capital accumulation, and Romer (1986) and Lucas (1988) for endogenous growth models.

and Levine 1997). In search of an explanation, in the past two decades a heated debate has taken center stage on the pages of books and economics journals pointing to a wide range of factors important for explaining growth, and its failure. The factors that have been advanced are many and they fall into several strands of thought: culture, trade policies, resource endowments, disease environment, windfall gains, civil war and institutions. While studies have highlighted the direct impact of these factors on economic development, they too show that an important channel runs via their impact on institutions (Acemoglu *et al.* 2001, Easterly and Levine 2003, Rodrik *et al.* 2004). In his widely read book, Collier (2007) states that it is mainly institutional failures that cause the poverty traps of the poorest people in the world. In recent years, a consensus seems to be emerging that institutions trump all other explanations in explaining income differences between countries (Rodrik *et al.* 2004).

In this thesis, I place central emphasis on institutions.[4] Figure 1, depicts the direct and indirect channels by which institutions, violent conflict and windfall gains impact on economic development.

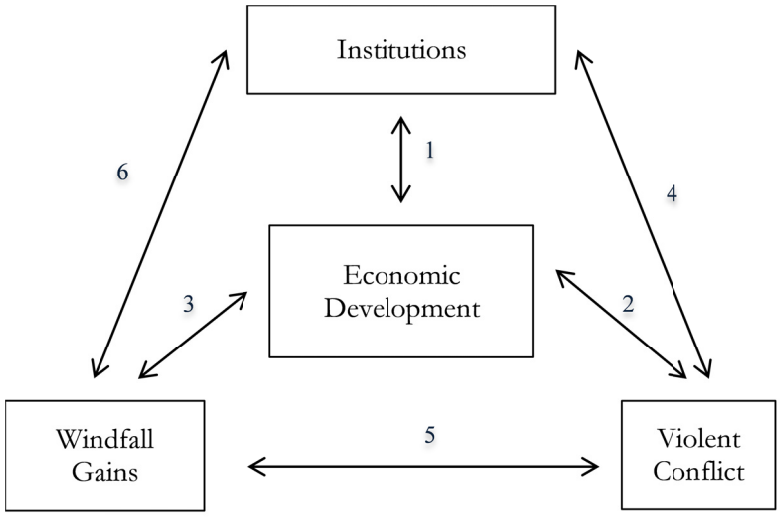

Figure 1. Institutions, Violent Conflict, Windfall Gains, and Economic Development

[4] The focus of this thesis is limited as it assessing how civil war and windfall gains affect the quality of institutions. This does not imply that geography, trade and culture are of lesser importance to development. For example, research into how geographical challenges, such as the depletion of soils, unfavorable weather conditions and diseases hinder growth via its impacts on low agricultural productivity, a heavy disease burden and low levels of trade concentration has been a lively field of research (see Bloom and Sachs 1998, Sachs 2005 amongst others). Also, deposits of natural resources, such as diamonds, minerals and forests are a key geographical factor contributing to growth as they provide windfall gains into the coffers of governments (see below). Empirical evidence also suggests that countries with open trade regimes have higher income levels (Dollar 1992, Sachs and Warner 1995). Culture based explanations focus, for example on the conducive effects of the protestant reformation on the development of a virtue of hard work (see Weber 1930, Nunn 2008b).

2.2 Institutions in the Growth Process

There is a growing body of literature that demonstrates a link between institutions and income (arrow 1, see Lipset 1960, Rodrik *et al.* 2004). North (1990) emphasized how the quality of institutions affects private investments and subsequently economic performance. Rodrik *et al.* (2004) shows, more systematically, how property rights protection and the rule of law positively affect economic development. Acemoglu and Johnson (2005) highlight how limiting expropriation by the government and powerful elite, as well as contract enforcement, matters for growth. Similarly, at the micro level several authors show how increased property rights protection induces income-enhancing investments in a range of African countries (see Besley 1995 amongst others). Switching to another institutional dimension, Bardhan (2005) and Acemoglu *et al.* (2008) show that political rights matter for development also.

Relatedly, authors from various disciplines have emphasized the role of social institutions (or social capital) in the growth process. Putnam (1993) pioneering work showed that membership of social organizations explains regional differences in incomes in Italy. This inspired follow-up work that shows that such bridging social capital as well as bonding social capital (such as pro-social preferences) is growth enhancing. Easterly *et al.* (2006) for example, show how social cohesion and trust affect growth both directly and indirectly.[5]

In sum, institutions matter for development. Unfortunately, much less is unknown about the main determinants of institutional quality. Partly this is due to the fact that the term *institutions* is used as a generic and imprecise phrase covering a range of governance and policy priorities. Some dimensions, such as culture and social norms, are considered reasonably permanent and change at a slow rate (Glaeser *et al.* 2004). Other dimensions appear to be more like stock variables in a state of constant flux with dynamics governed by policy choices (Rodrik *et al.* 2004). Williamson (2000) refers to the former interpretation as the institutional environment (where changes over time are measured on a time scale spanning many decades), and the latter as institutional arrangements or governance (where changes are measured on a 1-10 years scale).

Empirical studies have highlighted both the persistent and immediate impacts of institutions. At one end of the extreme, several authors have shown how historical events have (partly) shaped quality of current institutions. For example, in a seminal paper

[5] see also Miguel et al. 2004 and Kumar and Matsusaka 2009, for macro level investigations and Narayan and Pritchett 1999, Fafchamps forthcoming and di Falco and Bulte 2010 for micro level analyses of social capital and development in Africa.

Acemoglu *et al.* (2001) point to the long-term effect of colonial institutions. The authors show how early extractive institutions (created shaped by a detrimental disease environment for settlers) undermine current institutional performance and subsequently growth. In similar fashion, others have pointed to the identity of the colonizer (Grier, 1999; Bertocchi and Canova, 2002) and the post-colonial institutions (Englebert 2000a,b). Relatedly, Nunn (2008a) argues that slave trade created a culture of violence in host countries, which damaged growth-enhancing institutions (property rights and rule of law). Dell shows how a system of slave labor enforced by the Spanish colonists in Bolivia and Peru (see Dell 2010) undermined public good provision and explains lower income and nutrition levels today.

At the other extreme, a host of empirical contributions have shown how institutions are shaped in the short run by natural resource rents (Leite and Weidmann 1999, Ross 2001), development aid (Brautigäm and Knack 2004, Rajan and Subramanian 2007, Djankov *et al.* 2008) and civil wars (Bellows and Miguel 2009, Blattman 2009).

In this thesis, I "unbundle" institutions and distinguish between several dimensions of the institutional framework governing both vertical (interactions between people and the government, such as economic and political institutions) and horizontal dimensions (interactions between people, such as cooperation and trust). As such institutions are defined loosely as a system of rules, beliefs, norms, and organizations that together conduce a regularity of (social) behavior (see Greif 2006).

How do shocks, such as those resulting from windfall gains and violent conflict, impact on institutions (arrow 4 and 6)? One useful theoretical insight comes from a model by Acemoglu and Robinson (2008, see also Dalgaard and Olsson 2008). In their model dominant groups within a society decide which institutions best represent their interests. The resulting institutions can then be viewed as a compromise outcome—an equilibrium that emerges as from the competition between social groups. The nature of interaction, combined with the strength and interests of the groups, determines how the institutional framework evolves. Income or other types of shocks might alter the incentives of social groups and the balance of power between them, inviting processes of change.

2.2.1 Windfall Gains, Economic Development and Institutions

The impact of windfall gains stemming from natural resource rents, foreign aid or other sudden fluxes on income, has received a lot of attention in the literature (arrow 3,

Dalgaard and Olsson 2008).[6] In theory, increased natural resource exports or aid transfers endowments should spur growth. Empirically, however, authors have pointed to a *resource curse* rather than a resource *blessing*. Motivations for these findings have differed and point to Dutch disease type problems, increased corruption, higher probabilities of conflict and instability (see Sachs and Warner 1999, Isham *et al.* 2005). These findings are however not uncontested (see Brunnschweiler and Bulte 2008a,b, and van der Ploeg and Poelhekke 2008). Similarly, the direct impacts of aid on growth have yielded little conclusive evidence (e.g., Boone 1996, Burnside and Dollar 2000, Easterly *et al.* 2004, Rajan and Subramanian 2007).

Yet, the absence of an aggregate growth effect does not imply that windfall gains are neutral to development (see Dalgaard and Olsson 2008). Several studies have assessed the channels through which windfall gains affects growth and have looked at the impact on the promotion of peace (arrow 5, see de Ree and Nillesen 2009, Brunnschweiler and Bulte 2008b) and changes in institutional quality (arrow 6, see Mehlum *et al.* 2006, Djankov *et al.* 2008).

How do windfall gains impact on institutions (arrow 6)? On the one hand, demand for better institutions may rise as countries and their citizens become wealthier, putting pressure on corruption (Rodrik and Subramanian 2003). On the other hand, negative income shocks may invite corruption if it reduces government revenues (and hence salaries) thereby increasing incentives of civil servants to engage in corruption (see Mauro 2004 amongst others) or by reducing the government's ability to monitor corruption (di Tella and Schargrodsky 2003). Positive income shocks may reduce institutional quality as the political elite is caught in the redistribution frenzy, or are tempted to create unproductive public sector employment to prolong their reign (Robinson *et al.* 2006). A virtuous circle between income shocks and institutional quality is hence not so obvious.

In chapter 3, I look at whether such virtuous cycles of development and institutional improvements exist. For a host of African countries, I look at the impact of (rainfall induced) agricultural productivity growth on institutional quality (more specifically: corruption). I find that evidence to the contrary: positive income shocks tend to invite more corruption, not less. Closer inspection reveals that this result is limited to the most corrupt countries. Conversely, countries with a sufficiently low level of

[6] See Glaeser *et al.* (2004) on institution and growth, Easterly (2009) on development assistance and van der Ploeg (2011) for a overview of the literature on natural resources.

corruption can escape the detrimental effect of income booms on corruption and may actually experience a virtuous cycle of development.

2.2.2 Civil War, Economic Development and Institutions

Research on the impact of civil war on development (arrow 2) has largely been ignored in economics until about a decade ago (Blattman and Miguel 2010). Since then, it has attracted more and more attention with contributions at both at the macro and micro level.

At first instance there is no reason to assume that conflict has any long-term impact on growth. The Solow growth model predicts that if capital stocks are destroyed, after the cessation of violence, they will rebound so that per-capita incomes eventually return to their steady state. Indeed, empirical studies seem to confirm the model prediction (see Davis and Weinstein 2002, Brakman *et al.* 2004, Miguel and Roland 2005). However, the actual speed of recovery following conflict is divergent (e.g. Chen *et al.* 2008, Cerra and Saxena 2008) as conflict may affect factors shaping the growth process. For example, if conflict increases uncertainty, it may cause capital flight (e.g. the reduction of private investments) or it may worsen the fiscal balance of government (by decreased tax revenues and or increased and redirected spending) undermining growth. Also conflict may affect the development of human capital (though death, disease, reduces access to education, mental stress, etc), institutions (such as property rights, democracy and the rule of law) and social organization (Bellows and Miguel 2009, Blattman 2009). If wars contribute to the erosion of social capital or raise levels of impatience, adverse growth and level effects could eventuate. The opposite could happen when war invites institutional improvements or alters preferences in such a way that savings and investments are stimulated. Subsequently, war could affect long run growth by changing the scale parameter ("A") in the production function. Whether a country returns to its steady state level and pre-war income levels is hence not obvious. This thesis examines the impact of civil war on institutions and behavior (Chapter 4 and 5).

How could a shock resulting from civil war affect institutions (arrow 4)? War impacts on individuals and communities in a plethora of ways: either directly though death, injury or loss of significant others, displacement, destruction of property or indirectly through diminished access to health and education and changes in preferences, institutional quality, social cohesion, cooperation and collective action. Empirical macro level studies confirm the detrimental impacts of violence (see Collier 2003, Fearon and

Latin 2003). Yet, research into understanding how war affects these dimensions and how long these effects persist is unfortunately still minimal (Blattman and Miguel 2010). Recently academics from various disciplines (notably economics and political science) have made serious strides to address this disparity. In many post-conflict counties, micro data is being collected increasingly enabling researchers to investigate these issues. Most empirical studies use cross-section data and use plausible random variation in exposure to violence to assess its impact on postwar outcomes. The results are somewhat surprising. For example, Bellows and Miguel (2009) show that civil war *increases* political and social behavior under victims in Sierra Leone. Blattman (2009) provides similar results for ex-combatants in Uganda.[7] Interestingly, these findings resonate in psychological literature that shows that (temporary) shocks can have persistent effects on someone's outlook on life as well as his subjective valuation of goods and services (Punamaki *et al.* 1997, Tedeschi and Calhoun 2004). Social scientists have long argued that violence can carry the seed of societal reform, spurring the expansion of capitalism and promoting economic growth (for a theoretical contribution see Acemoglu and Robinson 2006, Tilly 1975 discusses state formation in Europe). For example, in his book, 'Civil War is Not a Stupid Thing', Cramer (2006) discusses the paradox of violence: it destroys but is also the root of social creativity and renewal.

These insights form the starting point of chapter 4 and 5 where I present work from data collected in Burundi. In chapter 4, I look at the impact of exposure to civil war on a set of institutional variables. In line with macro literature (Acemoglu and Johnson 2005), I specify three types of institutions: property rights security, social capital and the rule of law. I find that exposure to civil war at both the household and community level increases the demand for property right security and social capital. At the same time, I find weak evidence that it negatively impacts on the rule of law (notably the trust in police and justice).

In chapter 5, I extend the analysis and look at the impact of conflict on behavior measured in a series of experiments measuring social, risk and time preferences. I find that conflict affects behavior: individuals exposed to violence display more altruistic behavior towards their neighbors, are more risk seeking, and have higher discount rates.

[7] These results are not limited to Africa alone. Several authors have begun to use behavioral experiments to assess the impact of conflict on social preferences and find evidence that conflict breeds parochial altruism (see Gilligan *et al.* 2011 for evidence on Nepal, Gneezy and Fessler (2011) for evidence on Israel and Bauer *et al.* 2011 for evidence on Georgia).

Taken together the results from chapter 4 and 5 are consistent with the view that conflicts induce a shift in preferences and the demand for institutions pushing communities to new institutional equilibrium. If so, these results seem to suggest that large adverse shocks can thus alter savings and investments decisions, and potentially have long-run consequences – even if the shocks themselves are temporary.

Another domain in the civil war literature concerns the outbreak of violence. How do civil wars start? A persistent finding in the empirical literature is the negative correlation between incomes and civil war (Collier and Hoeffler 2002, Fearon and Latin 2003). There is a growing theoretical and empirical literature on both the macro and micro foundations of conflict (for an excellent review see Blattman and Miguel 2010). Common explanations link the outbreak of conflict to economic conditions or greed based explanations (arrow 2, see Collier and Sambanis 2005) and grievances over ethnic and religious divisions and political repression (partly captured by arrow 4).[8]

The brunt of the work on conflict is based on cross-country regressions. While useful in their own right, they naturally gloss over local and individual factors that explain why people choose to engage in fighting. Several micro papers offer empirical tests for the greed-grievance model. For example, Barron *et al.* (2009) find support for greed-based explanations as they document a positive correlation between violence and income inequality and unemployment in Indonesia. Along similar lines Dube and Vergas (2007) find positive correlations between drops in coffee export prices and the intensity of civil war, arguing that prices impact on the opportunity costs of participating in rebel movements. Looking at the determinants of participation in the war in Sierra Leone, Humphreys and Weinstein (2008) find support for both greed and grievance hypotheses.

Grievances may be deep rooted and persistent. In chapter 6, I look at grievances thought to have been a root cause behind the civil war in Sierra Leone. I use a new household and village survey, as well as data from regional local courts, to empirically test whether intra-peasant tensions over marriage and farm labor have persisted in the post war period in the Eastern and Southern part of the country, the cradle of the war.

[8] Note that other factors may play a role too, such as the impact of foreign assistance (de Ree and Nillesen 2009) or changes in temperature see Burke et al 2009, (but see Buhaug 2010 for evidence on the contrary).

2.3 Measuring institutions

While a promising body of literature on windfall gains, civil war, institutions and economic development is emerging, the empirical evidence leaves much to be desired. Part of the problem lies in the availability of good quality data.

Especially at the micro level, data is often scant and patchy, as there is little synergy in data collection efforts between countries. Often sampling problems and inconsistencies in surveys make data sets incomparable across sites. As a result comparability and interpretation is challenging. Empirical data from post-conflict countries is even more limited (Blattman and Miguel 2010). Subsequently, many questions related to the coevolution of institutions, shocks and economic development remain under researched. This thesis aims to contribute to filling this void. For example, for the work presented in chapters 4-9, we collected detailed data in Burundi and Sierra Leone. As part of our data collection we included modules on the civil war exposure and institutions. Taken together these data form a unique survey dataset on civil war and post war development.[9] In addition to surveys, we implemented behavioral experiments[10] to measure a range of preferences. For the analysis presented in chapter 5, we implemented a series of behavioral experiments to analyze the impact of war exposure on social, risk and time preferences. In Sierra Leone, we collected survey and experimental data to analyze relation between public spiritedness, community aid projects and conservation behavior. In chapter 7 and 8, I test the external validity of one type of behavioral experiment: a public good games. Chapter 7 analyzed whether public spiritedness, as measured in a public good game, is predictive of conservation behavior. I find that individuals behaving in a pro-social manner in a public goods game do not automatically display more pro-conservation behavior in the field. In chapter 8, I present a similar analysis and look at whether behavior in a public goods game is predictive of behavior in choices over voluntary contributions to a real public good: a village aid project. Again, I find that it is not. This evidence raises questions on the external validity of behavioral experiments and cautions that the lessons learned in these experiments need not necessarily extend to other domains.

[9] See the individual chapters for more details on the data used.

[10] In this thesis I use behavioral experiments and Artefactual Field Experiments (as in Harrison and List 2004) interchangeably.

Chapter 3

Income Shocks and Corruption in Africa:

Does a Virtuous Cycle exist?

Abstract: Empirical evidence suggests that governance quality is a key driver of economic growth and that, in turn, higher incomes might have a positive causal effect on the quality of governance. Such complementarity could invite virtuous cycles of development. Using a measure of corruption as our proxy for the quality of governance, and rainfall as an instrument for income, we explore this issue and find evidence to the contrary. For a panel of African countries, positive income shocks *on average* tend to invite extra corruption. Closer inspection, however, reveals that this result can be attributed to the most corrupt countries. Conversely, countries with a sufficiently low level of corruption can escape the detrimental effect of income booms on corruption and may actually experience a virtuous cycle of development.

Publication Information: Voors, M.J., Bulte, E.H. and R. Damania, 2011. Higher Incomes and Corruption in Africa: Does a Virtuous Cycle exist?, Journal of African Economies, 20 (3): 395-418

3.1 Introduction

The relation between institutional quality, broadly defined, and income (growth) is a hotly contested issue in economics. Institutions, income and various other economic measures such as human capital tend to move together over time as societies develop, and unraveling the underlying causal relationships – what drives what? – has proven extremely challenging. Two complementary perspectives exist, summarized and discussed by Paldam and Gundlach (2008). Institutions may be a key driver of economic outcomes (Lipset 1960), and countries will improve their institutions after they become richer (Glaeser *et al.* 2004). Both views have influential supporters, and point to persuasive evidence to back their positions. Obviously, both views may be correct simultaneously. If so, positive feedback effects between income and institutional quality – i.e., institutions and income as complements in the development process – could result in a 'virtuous cycle' of economic and institutional development (Treisman 2000). Unfortunately, much is still unknown about the main determinants of institutional quality and its links with economic growth.

The main objective of this paper is to analyze the short-term causal effect of exogenous income shocks on corruption, using a sample of countries in sub-Saharan Africa. Our focus on exogenous (rainfall-induced) income shocks is arguably restrictive, as is our focus on *short-term* corruption responses. By honing in on exogenous shocks we ignore the "demand" for lower levels of corruption by producers that might eventuate in response to a "developing economy." The short-term perspective implies that (sufficiently) slow responses to income shocks are not considered. Nevertheless, our approach allows for a clean evaluation of the scope for virtuous cycles of economic and governance development to be kick-started by exogenous forces. We hope this is relevant for evaluations of various types of shocks, including certain forms of development aid, or windfall gains due to price shocks on commodity markets.

We find evidence of a positive effect, on average, of income shocks on corruption. Closer inspection reveals, however, that it is the relatively corrupt countries that are more likely to experience a worsening of corruption following an income boom, though countries with relatively low levels of corruption may enjoy a virtuous cycle of development. This finding suggests the need to place corruption control high on the development agenda if temporary income booms are to be sustained.

The plan of the paper is as follows. In section 2 we briefly summarize the main issues and positions in the causality debate, and clarify our contribution. Section 3

presents our data and empirical strategy. In section 4 we present our panel regression results: both 'naïve' OLS regressions and more sophisticated 2SLS models suggest a positive effect of income on corruption. We also document the results of an extensive robustness analysis. Finally, we place our findings in perspective in section 5.

3.2 Institutions, Corruption, and Income

It is widely held that "getting institutions right" is a prerequisite for economic development. Some dimensions of the institutional framework are more amenable to "fixing" than others. This especially applies to various governance variables which arguably are as much "outcomes" – influenced by deeper seated institutional variables, policy choices and overall development – as they are constraints or "rules of the game." Williamson (2000) proposes that governance variables can evolve relatively quickly, and suggests measuring changes on a 1-10 years scale. Abundant evidence suggests governance variables are significantly associated with income, in both an economic and statistical sense (e.g. Easterly and Levine 2003, Rodrik *et al.* 2004, see also Temple and Johnson 1998).

One prominent governance variable, and the focus of this analysis, is corruption. Corruption is typically defined as the misuse of public office for private gains and includes, but is not limited to, bribery. Available estimates suggest corruption is a highly relevant issue. According to Rose-Ackerman (2004), worldwide bribery involves some $1 trillion per year, or some 3% of global income. Bribes are transfers, but imply real economic costs due to their distortive effects. Mauro (1995), Li *et al.* (2000), Wei (2000), and Rock and Bonnett (2004) document how corruption may adversely affect (foreign) investment and economic growth.[11] As corruption reflects the underlying institutional environment, Svensson (2007) speculates that the different forms of corruption are likely correlated.

Does income also affect corruption? Sometimes it is suggested that income and corruption control are complements in development (see below). If so, higher incomes improve corruption levels, and simultaneously reduced corruption tends to raise incomes. This implies scope for virtuous development cycles to eventuate. While there is uncertainty about the pace of change of governance variables (including corruption)

[11] See Krueger 1974 as well as Murphy *et al.* 1991, 1993, for theoretical treatments, including arguments based on the propping up of inefficient firms and the allocation of talent and resources away from their most productive use.

over time, evidence suggests such variables can change rather quickly and indeed respond to incentives (e.g. Tavares 2003, Djankov *et al.* 2008, Dalgaard en Olsen 2008).

We exploit the variability of governance (corruption) data to perform an annual panel analysis, involving 29 sub-Saharan African countries over 20-year period for which data are available. Rather than instrumenting for institutions in an income regression, we instrument for income in a regression that aims to explain changes in corruption. Using (lagged) rainfall measures as instrumental variables for income, we evaluate whether income shocks set in motion a process of change with respect to this key governance variable.[12]

Income shocks can affect corruption directly and indirectly. On the positive side, demand for better institutions rises as countries and their citizens become wealthier, thus reducing corruption (Rodrik and Subramanian, 2003). Direct effects may occur if adverse income shocks reduce government revenues, lowering wages and increasing incentives of civil servants to engage in corruption (Becker and Stigler 1974, van Rijckeghem and Weder 2001, Aidt 2003, Mauro 2004), or reducing the government's ability to monitor corruption (di Tella and Schargrodsky 2003). Similarly, adverse income shocks may impact on priority setting in policy formulation and implementation, perhaps because such shocks enhance the risk of civil conflict (Miguel *et al.* 2004, Ciccone 2008), thus impacting on institutions indirectly (as in Hodler 2006).

Not all impacts of higher incomes need be benign. In addition to effects at the demand side, income booms can play out via the supply side. Extra income in the hands of individuals can translate into (incentives for) enhanced corruption and bribing. Extra income in government coffers can trigger rent seeking or clientalism and nepotism. For instance, per capita income levels in Gabon have been amongst the highest in sub-Saharan Africa (over $4000 versus $580 for SSA), yet governance problems remain. Theoretical work by Tornell and Lane (1999) describes interacting interest groups feeding off the government budget (treating it as a common pool), and demonstrates that windfall gains from resource rents can slow down growth. It is easy to imagine how governance would deteriorate in the redistribution frenzy. Robinson *et al.* (2006) highlight perverse incentives for incumbent policy makers who are tempted to create unproductive public sector employment to prolong their reign. Similarly, and also

[12] The so-called "modernization theory" argues the existence of a causal relationship from high incomes to democracy (e.g. Barro 1999), though this is disputed by others (Acemoglu *et al.* 2005, Brückner and Ciccone 2010). Moreover, the nature of the relation from political regimes to economic performance is disputed (e.g., Przeworski and Limongi 1993, Barro 1996, Durham 1999, Fosu 2008).

focusing on the incumbent elite, Dalgaard and Olsson (2008) as well as Fors and Olsson (2008) analyze how institutional change responds to incentives, and demonstrate that institutional erosion can be a rational response to changing economic conditions, including specific income shocks.

Empirical work on the effect of income on institutions is scarce. Results of La Porta *et al.* (1999) and Treisman (2000) emphasize the importance of deep-seated factors such as religious traditions, overall development, design of the legal system and legal culture, a sustained democracy, and (albeit less robust) openness to trade. Focusing on corruption, Treisman (2000: 401) concludes: "The distant past appears as important as – or more important than – current policy. … The only slightly more hopeful finding is that, even though corruption hinders growth, countries can at times grow their way out of corruption. If other factors lead to vigorous economic development, corruption is likely to decrease." This view is consistent with results reported by Husted (1999), Paldam (2002) and others, who also find that low incomes tend to encourage corruption. We explore the robustness of this hypothesis in more detail herein.

Two other streams of literature are somewhat related to our work. Political scientists and economists have analyzed how natural resource rents impact on institutions (e.g., Karl 1997, Leite and Weidman 1999, Ross 2001, Bulte *et al.* 2005, Brunnschweiler and Bulte 2008a,b), and how foreign development aid impacts on institutional proxies in receiving countries (e.g., Tavares 2003, Brautigam and Knack 2004, Rajan and Subramanian 2007, Djankov *et al.* 2008, Nillesen and Voors 2008). Both literatures produce ambiguous evidence. The 'resource rent' literature typically implicates a specific subset of resources – 'point' resources, clustered in space and potential 'prey' for an elite. In recent years, aid transfers are often conditional on efforts to improve governance. The question of how exogenous and unconditional income shocks for a wide swath of the population affects governance remains unresolved.

3.3 Data and Model

The approach taken in this paper seeks to instrument for income in a regression explaining corruption, using rainfall as an instrument. The empirical focus is on sub-Saharan Africa – a region dependent where agriculture is heavily reliant on the level of rainfall.[13] Unlike many other arid regions in the world (e.g. the Middle East, Pakistan and

[13] Of course not all countries are equally reliant on agriculture. However, rainfall may also indirectly impact other sectors via primary production (through various linkages). For this reason we focus on total income rather than income in the primary sector only.

Central Asia), sub-Saharan Africa has a relatively low hydrological storage capacity and limited irrigation distribution system. Hence, the fortunes of agriculture vary with the idiosyncrasies of rainfall. This, combined with a relatively under-developed secondary and tertiary sector, implies that precipitation levels have an influence on economic performance. In our model we use (current and lagged) rainfall as instruments for African income levels (see also, Paxson 1992, Miguel *et al.* 2004, Ciccone 2008, and Brückner and Ciccone 2010).

Our panel approach enables us to control for time-invariant country-specific effects, eliminating an important source of omitted variable bias, a potential problem in regressions of institutional quality. There is an obvious trade-off. One may ask whether relatively high frequency data are appropriate for testing relationships governed by slow dynamics (for a similar tradeoff, see e.g. Forbes 2000). Can the short-run variability in income carry the identification load for issues with long-run dimensions such as institutions? This is an open question, but a crucial one for economists interested in reforming institutions or making contingent statements on the relation between economic outcomes and institutions – at what time scales do we find evidence of institutional change in response to economic incentives?[14] The time step in our panel (one year as well as two years) is the lower bound, according to Williamson (2000), for the frequency interval during which governance structures are updated. Note that the "virtuous cycle" perspective is based on the implicit assumption that institutional change will occur relatively rapidly. The regressions reported below shed further light on this issue and suggest that short run impacts from income to institutions indeed do appear to be visible.

3.3.1 The Data

Table 1 presents summary statistics for our main variables (see appendix for a full description of our data and its sources). The data are for 29 countries in sub-Saharan Africa from 1985-2004 (see appendix for complete list).[15] The rainfall data is from Miguel *et al.* (2004) and Brückner and Ciccone (2010), and is based on the Global Precipitation Climatology Project (GPCP) database. The GPCP data are computed from

[14] Brautigam and Knack (2004) do not use annual variation in the governance data. Instead, they explain the changes in governance quality over the sample period. The reasons are that institutional response may only occur with a time lag (something we test and try to control for), and the absence of a suitable annual instrument for aid (something that does not affect us as we have an annual instrument for income).

[15] Due to data availability of our institutions (governance) variables, our sample size is smaller than Miguel *et al.* (2004) who analyzed 42 sub-Saharan African countries.

local rainfall station gauge measures and corroborated against satellite data of cloud cover and precipitation. These measures are generally regarded as the most reliable estimates of rainfall. There is considerable variation in rainfall in sub-Saharan Africa – both across countries and over time.

Corruption data are taken from the ICRG governance indicators—a subjective and ordinal index published by the Political Risk Services Group (PRSG), a company rating information on political risks for overseas investors and lenders.[16] While there are several competing measures of institutional strength (see Svensson 2007 for an overview), the ICRG data are particularly useful for our purposes because of their extensive coverage of Africa over a relatively long time span.[17] The corruption variable is measured on a 6 point scale, with higher scores indicating more corruption. According to PRSG, the index captures the likelihood that high government officials will demand special payments and the extent to which illegal payments are expected throughout government tiers. Corruption indicators are available on a monthly base, and we use the January rating for all countries in our set, though similar results eventuate when using annual averages.

Table 1: Summary statistics, 1985-2004

Variable	Obs	Mean	Std. Dev.
Corruption (ICRG)	570	2.50	1.06
GDP per capita	576	600.39	946.14
GDP per capita (ln)	576	5.86	0.87
Rainfall (mm)	569	1042.44	497.76
Aid per GDP	578	13.59	13.04
Agriculture value added per worker	557	332.94	276.94
Population (mln)	580	17.00	21.70
Regime type	580	0.32	0.47
Primary exports per GDP	547	11.13	23.04

Our income variable is per capita GDP in constant 2000 dollars, taken from World Development Indicators (2007). Consistent with the considerable fluctuations in

[16] Most empirical work – and certainly the work involving cross-country analysis – on corruption is based on corruption perceptions; a subjective measure. Olken (2009) demonstrated that there may be a sizable gap between corruption perceptions and reality. He compared corruption perceptions of Indonesian villagers to an objective measure of "missing expenditures" (the difference between claimed costs and costs as measured by a team of experts), in the context of a road building project (see also Olken 2007). While perceived and actual corruption levels are correlated, subjective assessments convey limited information about actual corruption levels. The elasticity of a respondent reporting any likelihood of corruption with respect to missing expenditures is only 0.08. This is important to keep in mind when interpreting the results.

[17] Many studies use the World Bank data on governance. Similar to the ICRG data, the World Bank governance data are based on subjective assessments by (foreign) investors. However, contrary to the ICRG data, for the World Bank indicators respondents are instructed to report assessments based on their experiences *over the past few years*. Hence, the World Bank corruption indicator for, say, 2008 would reflect experiences during 2006 and 2007, rendering these data less suitable for our purposes.

rainfall, we find large annual fluctuations in per capita income (indeed: the standard deviation of this variable is more than 150% of its mean). As control variables we include population levels, to capture potential accountability constraints (i.e., as governments of smaller countries may be held accountable more easily, see Bräutigam and Knack 2004); aid as a percentage of GDP, to control for any conditionality effects on institutions; a measure of regime type (a dummy being unity if country was democratic in a certain year and zero if autocratic); and the ratio of primary exports relative to GDP to capture possible resource-curse type effects.

3.3.2 Estimation Framework

We start by estimating the impact of income on the change in corruption in a basic panel specification:

$$\Delta C_{it} = \alpha_i + \beta_0 Y_{it-2} + \beta_1 C_{it-1} + \beta_2 X_{it-2} + \lambda_t + \varepsilon_{it}, \quad (1)$$

where ΔC_{it} is the change in corruption of country *i* between period *t* and *t-1*, and ε_{it} is a random error term. In all regressions we include country fixed effects (α_i) to capture time-invariant characteristics of countries associated with income[18] and time dummies (λ_t), and the standard errors are robust to both arbitrary heteroskedasticity and autocorrelation (HAC). We also include the initial level governance quality (C_{it-1}) to capture regression-to-the-mean effects, and persistence in corruption. In some regressions we include a series of (lagged) time-variant controls (X_{it-2}). Regressing changes in corruption on levels of income, as in (1), amounts to estimating a transformed standard 'levels on levels' specification, where $C_{it} = \alpha_i + \beta_0 Y_{it-2} + \tilde{\beta}_1 C_{it-1} + \beta_2 X_{it-2} + \lambda_t + \varepsilon_{it}$, with $\tilde{\beta}_1 = 1 + \beta_1$. Since we use January ratings for governance quality, we take the two year lag of our income variable. This should effectively ensure that institutional change follows income shocks.

To control for potential reverse causality as well as any bias resulting from omitted variables affecting both income and corruption, we instrument for income levels using lagged rain fall levels. Given our specification in (1), our 1st stage regression is:

$$Y_{it-2} = \alpha_i + \beta_0 R_{it-2} + \beta_1 R_{it-3} + \beta_2 X_{it-2} + \lambda_t + \varepsilon_{it} \quad (2)$$

where, our (two-year) lagged per capita income measure (Y_{it-2}) is explained by rainfall in both the same year R_{it-2} and in the previous year R_{it-3}. Our benchmark estimation

[18] A standard Hausman test rejects a random effects model at the 1% level: Chi(2) = 42.67***

procedure departs from Miguel *et al.* (2004), who regressed income *growth* on rainfall *growth*. Instead it follows the approach proposed by Brückner and Ciccone (2010), who demonstrated that using rainfall growth rather than levels may cloud the transitory nature of rainfall shocks—conflating positive or negative shocks with mean reversion processes. For example, high rainfall growth between $t-1$ and t may be due to a *positive* rainfall shock at t or due to mean reversion following a *negative* shock at $t-1$. In light of our interpretation this is problematic – does rainfall growth imply a (current) positive income shock or a (lagged) negative shock? Hence, our preferred specification is based on regressing growth on levels, but we also estimate the "growth-on-growth" model.

3.4 Regression Results

Tables 2a and 2b contain our first set of results. Table 2a presents OLS and IV-2SLS estimates of the impact of (lagged) income on corruption, and Table 2b summarizes the matching first stage results of the 2SLS estimates in Table 2a.[19]

Column (1) shows the unconditional regression of (lagged) income on changes in corruption, while controlling for country fixed effects and time dummies. In column (2) we add a measure of lagged corruption, and in column (3) we add a vector of common controls. All OLS results demonstrate a significant positive association between income and corruption – higher incomes are associated with "more corruption." A one standard deviation increase in income translates into a 0.21 standard deviation increase in corruption.[20] However, and as usual, endogeneity concerns preclude the drawing of causal inferences.

In columns (4) - (7) we present 2SLS results. They also reveal a strong and robust positive effect of income levels on corruption.[21] A one standard deviation increase in income translates into a 1.01 standard deviation change in corruption score. These results suggest that income and institutions are not complements, and that a virtuous cycle of development does not exist for African countries.[22] The significant negative

[19] We have taken logs of our main explanatory variables, and find this does not influence the qualitative nature of the results. We have also tested for the presence of outliers in our data (using the procedure developed by Hadi 2004), and found no evidence of outliers in our rainfall and income data.

[20] Calculated as ΔStDev C = (StDev Y x β(Y)) / StDev C, here (0.258*0.87)/1.06=0.21

[21] The 2SLS point estimate on per capita income is much larger than the OLS estimate. This is a common finding in cross-country studies, usually attributed to measurement error (biasing the OLS estimates towards zero). At the same time it is likely that $Cov(C_{it-1}, \varepsilon_{it}) \geq 0$ resulting in an upwards bias of our 2SLS results, and the coefficients should therefore be viewed as upper bounds (see Pande and Urdy 2005).

[22] The same applies to alternative governance proxies. For example, we have also looked at the impacts on income shocks on Bureaucratic Quality, and find no evidence of a virtuous cycle in development (results not reported, available on request).

coefficient associated with the lagged governance variable may be interpreted as a convergence effect. When omitting this variable the income coefficient remains significant and grows in size (not shown). Yet, the presence of a lagged dependent variable may bias our results as C_{it-1} is correlated with ε_{is} for $s < t$. If so, the fixed effects estimator in columns (2)-(7) may not be consistent. In column (8) we use an alternative estimator, the system GMM estimator of Arellano and Bond (1991). The coefficients are similar to our OLS results and the model passes the usual identification tests, such as the AR(2) and Hansen test (see p-values at bottom row of the Table).

Panel 2b provides the first stage results. Consistent with Miguel *et al.* (2004), Ciccone (2008), and Brückner and Ciccone (2010) we find that rainfall is an important determinant of income in African countries, and a suitable instrumental variable for income. Obviously, rainfall can be used to identify exogenous variation in income—standard instrument validity tests are all satisfied.[23] However, the partial F statistics are low (there is more to African economies than rainfed farming) indicating that our instruments are only weakly correlated with per capita income when using the 2SLS estimator. To address potential bias, we re-run our analysis using the Limited Information Maximum Likelihood (LIML) estimator in column (6).[24] While the standard errors are somewhat inflated, we again find a positive effect of income shocks on corruption.

A second method to assess whether the 2SLS estimates are valid follows Chernozhukov and Hansen (2008), who develop a protocol to construct confidence intervals which does not rely on asymptotic approximations.[25] These confidence regions are robust to non i.i.d. errors and have good power properties. The robust weak-instrument 95% confidence interval is reported at the bottom of Panel 2A (column 7). While this interval is not informative on the upper bound of the lagged income coefficient, β_0 [26], it does provide a sharp lower bound. Since the lower bound is positive, negative values for β_0 in the corruption equation are ruled out, confirming the positive impact of income on corruption.

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[23] We report Hansen J p-values for instrument validity at the bottom of the regression tables.

[24] See Hahn *et al.* (2004), who show that the finite sample properties of LIML may be superior to 2SLS and outperforms 2SLS in case of weak instruments.

[25] As asymptotic confidence intervals may be invalid when instruments are weakly correlated with endogenous regressors.

[26] For computational reasons this regression is based on one instrument. The upper limit of the weak-instrument confidence intervals is equal to the limit we for the set of values we consider for the 2SLS estimate [-2, 4] with equally spaced steps of 0.001. Chernozhukov and Hansen (2008) find the same in several of their examples.

Table 2: Rainfall, Income and Corruption, 1985-2004 (yearly panel)

Panel 2a. OLS and 2SLS regressions Income and Corruption

Dependent variable:Δ corruption	(1) OLS	(2) OLS	(3) OLS	(4) 2SLS	(5) 2SLS	(6) LIML	(7) 2SLS	(8) sys-GMM
GDP per capita$_{t-2}$ (ln)	0.258***	0.388***	0.331**	1.228*	2.445**	1.301^{+}	1.672*	0.212*
	(0.086)	(0.094)	(0.146)	(0.765)	(1.228)	(0.837)	(0.991)	(0.114)
Corruption$_{t-1}$		-0.235***	-0.248***	-0.278***	-0.278***	-0.282***	-0.301***	-0.051
		(0.054)	(0.048)	(0.065)	(0.044)	(0.068)	(0.061)	(0.111)
Aid per GDP$_{t-2}$ (ln)			-0.018		-0.125			
			(0.035)		(0.095)			
Population $_{t-2}$ (ln)			1.010		2.084			
			(0.692)		(1.926)			
Regime type$_{t-2}$			-0.010		0.016			
			(0.057)		(0.072)			
Primary exports per GDP$_{t-2}$			0.016**		0.009			
			(0.007)		(0.033)			
Weak-instrument inverval							(1.04, 4)	
R squared	0.09	0.20	0.21					
Hansen test (p-value)				0.26	0.28	0.26		0.98
AR(2) test (p-value)								0.50
Fixed effects	yes	yes	yes	yes	yes	yes	yes	..
Time dummies	yes	yes	yes	yes	yes	yes	yes	yes
Observations	541	541	431	536	419	536	537	536

Panel 2b. First Stage Results Rainfall and Income (yearly panel)

Dependent variable: GDP per capita$_{t-2}$ (ln)				
Rainfallt-2 (ln)	0.117**	0.075*	0.117**	
	(0.059)	(0.042)	(0.059)	
Rainfallt-3 (ln)	0.133**	0.104**	0.133**	0.155**
	(0.056)	(0.043)	(0.056)	(0.068)
Corruption$_{t-1}$	-0.043***	0.009	-0.043***	-0.046***
	(0.014)	(0.010)	(0.014)	(0.017)
Aid per GDP$_{t-2}$ (ln)		-0.059***		
		(0.013)		
Population $_{t-2}$ (ln)		1.438***		
		(0.211)		
Regime type$_{t-2}$		0.002		
		(0.018)		
Primary exports per GDP$_{t-2}$		0.020***		
		(0.006)		
R squared	0.95	0.98	0.95	0.95
Partial F stat	6.59***	4.68***	6.59***	13.45***
First stage regression for column	(4)	(5)	(6)	(7)

HAC robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1, $^{+}$ significant at 12%. Hansen J statistic tests for joint instrument validity, null hypothesis is that the instruments are valid, i.e., uncorrelated with the error term, and that the excluded instruments are correctly excluded from the second stage equation. The Asymptotic Interval reports the 95-% confidence interval constructed using the usual asymptotic approximation. The weak-instrument interval is constructed based on Chernozhukov and Hansen (2008) and refers to the 95-% confidence interval constructed using the weak-instruments robust statistic. We use a weak instruments interval [-2, 4] with equally spaced steps of 0.001. Column (8) uses system GMM estimator of Arellano and Bond (1991) with robust standard errors, instruments are double lag income and lagged rainfall.

3.4.1 Robustness analysis

In Tables 3a and 3b we provide several robustness analyses. First, in columns (1) - (3) we use a two-year panel instead of a one-year time step, and present results for the OLS specification, the 2SLS specification and the system GMM model. A two-year step may attenuate the concern that governance measures may not capture the impact of income shocks within a one-year period. We find that both the first and second stage results are qualitatively the same as before, and note that the 2nd stage income coefficients are considerably larger than before (perhaps capturing that measurement error is introduced when 'averaging' rainfall observations). Most importantly, we again show that (lagged) income has a positive effect on corruption.

We replicated the analysis using three-year steps and four-year steps. The 2nd stage results are similar, and confirm the view that there is no virtuous income-institutions cycle in Africa. However, and unsurprisingly, lumping multiple years together implies that quite a bit of the variation of our rainfall variable disappears. The correlation between income levels and rainfall decreases, and the partial F-statistics fall (further).[27]

The remaining columns provide additional robustness analysis. In column (4) and (5) we replace our per capita income variable with (log) agriculture value added per worker, arguably more closely related to rainfall. The agricultural income statistics are of lower quality than the regular income data, and as a result we had to omit Tanzania, Sierra Leone and Liberia from the dataset. While statistical significance has been compromised (perhaps due to the drop in the number of observations), we find that the main results go through. The OLS results indicate that agricultural income enters statistically significant at the 10% level (column 4), and the 2SLS estimate yields an effect significant at the 11% level (column 5).

[27] Results are not reported, available on request.

Table 3 Robustness, 1985-2004

Panel 3a. OLS, GMM and 2SLS Regressions

Dependent variable : Δ corruption	(1) OLS two-year data	(2) 2SLS two-year data	(3) sys-GMM two-year data	(4) OLS year data	(5) 2SLS year data	(6) 2SLS year data	(7) 2SLS year data
GDP per capita$_{t-2}$ (ln)	0.690***	4.791***	0.167^{+}				1.885**
	(0.193)	(1.738)	(0.110)				(0.931)
Agriculture v.a. per worker$_{t-2}$ (ln)				0.194*	2.570^{+}		
				(0.105)	(1.616)		
Economic growth$_{t}$						1.133	
						(2.478)	
Economic growth$_{t-1}$						0.468	
						(2.911)	
Corruption$_{t-1}$	-0.489***	-0.607***	-0.133	-0.208***	-0.247***	-0.212***	-0.217***
	(0.059)	(0.117)	(0.106)	(0.037)	(0.056)	(0.039)	(0.034)
Hansen test (p-value)		0.49	0.86		0.61	027	0.51
AR(2) test (p-value)			0.71				
R squared	0.22			0.20			
Fixed effects	yes	yes		yes	yes	yes	yes
Time dummies	yes	yes	yes	yes	yes	yes	yes
Observations	229	227	255	462	457	492	387

Panel 3b. First Stage Results

Dependent variable:	GDP per capita$_{t-2}$ (ln)	Agriculture v.a. per worker$_{t-2}$ (ln)	Economic growth$_{t}$	Economic growth$_{t-1}$	GDP per capita$_{t-2}$ (ln)
Rainfall$_{t-2}$ (ln)	0.266***	0099**			0.021
	(0.095)	(0.049)			(0.044)
Rainfall$_{t-3}$ (ln)	0.0342	0.042			0.106**
	(0.0833)	(0.049)			(0.045)
Growth in rainfall$_{t}$			0.041**	0.040*	
			(0.001)	(0.021)	
Growth in rainfall$_{t-1}$			0.001	0.042***	
			(0.019)	(0.016)	
Corruption$_{t-1}$	-0.0175	-0.015***	-0.002	-0.002	-0.020*
	(0.022)	(0.009)	(0.006)	(0.006)	(0.012)
R squared	0.95	0.96	0.17	0.16	0.96
Partial F/ Kleibergen-Paap Wald rk F stat	3.68***	2.77**	2.89	2.89	2.74*
Stock-Yogo critical values 25%			5.45	5.45	
First stage regression for column	(2)	(5)	(6)	(6)	(7)

HAC robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1, $^{+}$ p<15. Hansen J statistic tests for joint instrument validity, null hypothesis is that the instruments are valid, i.e., uncorrelated with the error term, and that the excluded instruments are correctly excluded from the second stage equation. Kleibergen-Paap Wald rk F statistic for weak instruments is an adaption of Gragg Donald F statistic as errors are assumed to be the non i.i.d. and robust to HAC. The degrees-of-freedom for the rk statistic is ((N-L)/L1)*((N-1)/N)*(N_clust-1)/N_clust), where N is the number of observations, L is the number of instruments and L1 is the number of excluded instruments. Stock -Yogo (2005) critical values are for 25% maximal LIML size. Column (3) uses system GMM estimator of Arellano and Bond (1991) with robust standard errors, instruments are double lag income and lagged rainfall. Dependent variable in column (7) is a dummy variable, taking unity if a country became more corrupt relative to the previous year.

Table 4: Subsamples, 1985-2004

Panel 4a OLS regressions Income and Corruption

	(1) OLS income < mean	(2) OLS income > mean	(3) OLS interaction term	(4) 2SLS interaction term
GDP per capita$_{t-2}$ (ln)	0.375***	0.387*	-0.568***	-0.379^{+}
	(0.123)	(0.223)	(0.031)	(0.267)
Corruption$_{t-1}$ x GDP per capita$_{t-2}$ (ln)			0.163***	0.157***
			(0.002)	(0.009)
Corruption$_{t-1}$	-0.247***	-0.213***	-0.956***	-0.937***
	(0.035)	(0.049)	(0.011)	(0.031)
Hansen test (p-value)				0.57
R squared	0.21	0.30	0.95	0.94
Fixed effects	yes	yes	yes	yes
Time dummies	yes	yes	yes	yes
Observations	319	222	541	536

Panel 4b. First Stage Results Rainfall and Income

Dependent variable:	GDP per capita$_{t-2}$ (ln)
Rainfall$_{t-2}$ (ln)	0.096*
	(0.054)
Rainfall$_{t-3}$ (ln)	0.078
	(0.055)
Corruption$_{t-1}$	-0.099***
	(0.027)
Corruption$_{t-1}$ x GDP per capita$_{t-2}$ (ln)	0.030***
	(0.005)
R squared	0.97
Partial F stat	3.31
First stage regression for column	(4)

HAC robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1, $^{+}$ p<15. Hansen J statistic tests for joint instrument validity, null hypothesis is that the instruments are valid, i.e., uncorrelated with the error term, and that the excluded instruments are correctly excluded from the second stage equation.

One concern plaguing our specification in equation (1) is that per capita income levels are a non-stationary process. A stochastic trend in our data may invite spurious correlation.[28] To address this issue we estimate a model based on income growth rather than levels in column (6) — essentially following the specification by Miguel *et al.* (2004). Again we do not find evidence of a virtuous cycle (the coefficient is again positive, albeit no longer significant). In light of the discussion above, however, this is not surprising. If we use rainfall growth to explain income growth we mix up positive rain shocks and recovery to normal levels after negative shocks, conflating positive and negative (lagged) income shocks, thus obscuring the second stage results.

[28] Incomes in SSA on average have decreased up to the mid-1990s and have tended to increase thereafter. Yet, at the individual country level, incomes paths show great divergence, attenuating concerns about non-stationarity. A unit root test (Levin-Lin-Chu test) clearly rejects the null hypothesis of non-stationarity of per capita income in our sample.

In column (7) we assess the impact of income shocks on a transition to increased corruption. Our dependent variable is now a dummy variable, taking on unity if a country is perceived as more corrupt than in the previous year, and zero otherwise. The results are consistent, and again show that positive income shocks increase the probability that a country becomes more corrupt.[29]

Does the negative effect of income on governance apply to all African countries in our sample, or to just a sub-set of them? To tentatively explore this issue we first divide the sample in two subsamples—distinguishing between countries with incomes above and below the mean per capita income level. We redo the analysis, and report OLS results for the yearly panel in columns (1)-(2) of Table 4 (results for the two-year panel are qualitatively similar). The erosive impact of income on governance obtains for both sub-samples; apparently initial income levels have little influence over how corruption is affected by an income shock.[30]

Next, in column (3) and (4) where we interact corruption and income, we find that this interaction term enters significantly (at the 1% level) and is positive. Hence, countries with higher initial corruption levels are less immune to the adverse impacts of income than countries with lower initial corruption levels. Specifically, our main result, that positive income shocks increase corruption, materializes only for countries with high initial levels of corruption. Conversely, countries with a low level of corruption (lower than the threshold value 0.568/0.163 = 3.5) are able to escape the detrimental impacts on corruption resulting from an increase in income. Specifically, of our 570 country-year observations, 125 have a corruption value above our threshold value of 3.5. In total, 12 countries retain an average corruption level above 3.5 throughout the 1985-2005 period. Ironically, perhaps, we are left with the result that countries with the worst governance scores – needing improvement most – will suffer the worst negative impacts of positive income shocks.[31] At the same time, this result sends a clear message to policy makers to place corruption control high on the development agenda: if countries manage to bring corruption levels down sufficiently, income shocks no longer increase corruption.[32]

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[29] We estimate this model using an ordinary 2SLS procedure (see Hyslop, 1999).

[30] We also re-estimated our model with a quadratic term and found that it was not significant. We hence abstain from claiming that initial income levels impact on the income-corruption nexus.

[31] Our main result is a positive *average* effect of income shocks on corruption. The mean corruption level for Africa is 2.5 for the study period. We ran separate regressions for a sub-sample of below mean corruption levels and found no significant effect of income on corruption, again indicating that our result can be attributed to countries with high levels of corruption (results not shown).

[32] We sought to analyze the robustness of our results to using another measure of corruption. Unfortunately, the coverage of alternative corruption measures is restricted, especially in terms of the time

3.5 Discussion and Conclusion

We have tried to shed light on one of the ongoing controversies dividing the literature on institutions (governance) and income. Many studies have demonstrated positive correlations between per capita income and a range of measures of institutional quality – but what does this mean? While earlier work has sought to instrument for institutions when explaining incomes, we have chosen the opposite route of instrumenting for income when explaining governance quality. Our focus was on a relatively variable governance factor: the perceived level of corruption. The results indicate that there is no mutually reinforcing relationship – or *virtuous cycle* – between income (growth) and reduced corruption in the short run in Africa. Instead, we find evidence of a positive effect of income shocks on corruption, especially when initial corruption is high.

Our results echo those obtained in various parallel literatures. There is some evidence, albeit controversial, that institutions deteriorate with inflows of resource rents (e.g., Dalgaard and Olsson 2008; Brunnschweiler and Bulte 2008a,b). Moreover, others have found that higher incomes do not translate into more democracy.[33] Improvements in income need not, therefore, be a driver of change in institutional quality, and hence there is no clear evidence of a virtuous and self-reinforcing development cycle of institutional improvements and higher incomes.

Instead, we are left with the possibility of an institutions-induced poverty trap. Especially for the most corrupt African countries it appears as if positive income shocks may invite rent seeking, patronage, and nepotism. At the same time, in countries which manage to bring corruption levels under control, income booms will no longer increase corruption, but may actually lower it. In light of the evidence that institutions matter for long-term growth and development, this result implies that positive income shocks could be short-lived, unless institutional reform is a conscious choice of policy makers. Our results therefore support the need for ongoing investment in governance and institutional improvements as a prerequisite to sustaining growth.

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period for which data are available. For example, Transparency International data are available for the sub-period 1998-2004 only. Though the TI data are strongly correlated with the ICRG data (it is about 0.75 – see Svensson 2007), when restricting the analysis to this period, the results are not robust, and are sensitive to an outlier—Ghana. During the sub-period, Ghana experienced an increase in income combined with a dramatic fall in perceived corruption. Omitting Ghana from the sample, the ICRG data yield estimates that are consistent with the main analysis, but the TI data fail to produce any significant relationship between income and corruption. Details are available from the authors on request.

[33] See Acemoglu *et al.* (2008). For an analysis of democracy and Africa in a set of African countries using a similar instrumentation strategy as in this paper, see Brückner and Ciccone (2010).

Appendix

A. Countries included in the analysis

Angola, Botswana, Burkina Faso, Cameroon, Dem. Rep. Congo, Cote d'Ivoire, Ethiopia, Gabon, The Gambia, Ghana, Guinea, Guinea-Bissau, Kenya, Liberia, Madagascar, Malawi, Mali, Mozambique, Namibia, Niger, Nigeria, Senegal, Sierra Leone, Sudan, Tanzania, Togo, Uganda, Zambia, Zimbabwe.

B. Data definitions

- Rainfall data are from Miguel *et al.* (2004) and Brückner and Ciccone (2010), and based on the Global Precipitation Climatology Project (GPCP) database, available at http://precip.gsfc.nasa.gov.
- Corruption data are January ratings of ICRG indicators: corruption is measured on a 6 point scale, with 6 (high corruption) and 0 (low corruption) (item rescaled) Corruption index is concerned with corruption control within the political system and measures control over '*actual or potential corruption in the form of excessive patronage, nepotism, job reservations, 'favor-for-favors', secret party funding, and suspiciously close ties between politics and business.*' Higher scores indicate that government officials are more likely to demand special payments.
- GDP per capita in constant 2000 US dollars are from World Development Indicators (WDI) 2007.
- Agriculture value added per worker in constant 2000 US dollars are from WDI 2007.
- Aid per GDP is Official Development Assistance (ODA) over GDP. ODA data from OECD statistics table 2a available at: http://www.oecd.org/. GDP data are in constant 2000 US dollars from WDI 2007.
- Population data are from WDI 2007.
- Primary exports share of total primary (mineral and natural) exports relative to GDP in constant 2000 US dollars from WDI 2007
- Regime type is based on scores for political institutions of the Polity IV project, available at http://www.systemicpeace.org/polity/polity4.htm.

Chapter 4

Conflict and the Evolution of Institutions:

Unbundling Institutions at the Local Level in Burundi

Abstract: How does violent conflict affect institutional quality? We use new data from Burundi to explore this issue. Using fixed effects and instrumental variable models we conclude that conflict has an institutional legacy, implying that – in contrast to predictions of simple neoclassical growth models driven by capital accumulation – conflict may have permanent effects on income and development. Our data suggest the impact of conflict on institutional quality may be positive or negative, depending on the institutional measure. Conflict exposure seems to foster social capital and promote tenure security, but appears to undermine the quality of local political institutions and governance.

Publication Information: Voors, M.J. and Bulte, E.H., 2011. Conflict and the Evolution of Institutions: Unbundling Institutions at the Local Level in Burundi, Working Paper.

4.1 Introduction

The distribution of per capita income across and within countries is very unequal, and has prompted a vast amount of research into the determinants of growth and welfare. A prominent literature argues the 'institutional framework' is such a key determinant (see Rodrik *et al.* 2004). This institutional perspective on (under)development has received impetus from a recent series of cross-country studies (e.g., Acemoglu *et al.* 2001, Easterly and Levine 2003, Rodrik *et al.* 2004). The 'geography-as-destiny' perspective on development gave way to a view emphasizing the indirect role of geography and natural endowments, as a factor shaping the institutional framework.[34] While the operational guidance offered by cross-country results is sometimes judged to be "extremely meager" (Rodrik *et al.* 2004), institutional reform for development has taken center stage.

An interesting literature examines the determinants of institutional quality, emphasizing the persistent nature of institutions. For example, some analyses identify pre-colonial factors (such as land abundance – see Fenske 2010) as key determinants, and others point to experiences during the colonial era. Examples include settlement decisions by colonial powers (Acemoglu *et al.* 2001), slave raiding (Nunn 2008), or forced labor systems (Dell 2010). However, institutions are not completely invariant with respect to post-colonial events—institutions evolve in response to economic pressures. While a complete theory of "endogenous institutions" is lacking, theory suggests (*de facto*) institutional quality co-evolves with economic incentives for social groups (e.g., Congdon Fors and Olsson 2007, Acemoglu and Robinson 2008). Perhaps dominant groups within society decide which institutions best represent their interests. Alternatively, institutions may represent a compromise outcome—an equilibrium that emerges as competing social groups pull in opposite directions, exerting pressure in an effort to shape institutions to their advantage. The nature of such interaction, combined with the strength and interests of the groups, then determines how the institutional framework evolves.

This paper seeks to connect and contribute to two literatures. First, building on the literature on "endogenous institutions" we explore how institutions evolve in response to shocks (see also Brautigam and Knack 2004, Djankov *et al.* 2008). Second, we seek to contribute to the literature on the economic consequences of civil war. More than two thirds of African nations have experienced civil strife during the past three decades, and the consequences of such violence has recently become an important focus of research (see Blattman and Miguel 2009 for one review). The neoclassical growth

[34] For treatments on the role of institutions in African development, refer to Bhattacharyya (2009) and Fenske (2010).

model predicts that post-war economies should rapidly accumulate capital and experience rapid growth. However, the evidence regarding the speed of post-conflict recovery is subject of debate (e.g. Chen *et al.* 2008, Cerra and Saxena 2008, Miguel and Roland 2010). Conflict could have permanent consequences—casting countries off their growth path towards a new steady state income—if it affects human capital (Bundervoet *et al.* 2009), preferences (Voors *et al.* 2011) or institutions (Bellow and Miguel 2009).

The main objective of this paper is to analyze whether conflict shocks affect the quality of institutions. We study the evolution of institutions using a new dataset from Burundi that we collected ourselves.[35] We adopt a micro perspective and seek to "*unbundle*" local institutions, distinguishing between three dimensions of the institutional framework: (*i*) tenure rights security (our closest substitute for property rights security in the macro literature on institutions and growth), (*ii*) a measure of appreciation of local political institutions and rule of law, and (*iii*) the strength of social capital (trust and cooperation).[36] Our micro focus implies exploring the role of conflict as a determinant of *within*-country differences in institutional quality and conflict intensity. We thus seek to exploit the synergies between research based on micro data and questions posed by the broader institutions and growth literature (Pande and Udry 2005).

Conflicts and institutions are naturally linked if conflict breeds "institutional disequilibrium" (Lin 1995). It shakes up social relations, creates opportunities for innovation and redistribution, and raises awareness. Violence can carry the seed of societal reform. For example, Cramer (2006) points to historical events to support his claim that violence can "*produce institutional changes, amendments to the rule of the game. In retrospect, many changes that come to be seen as progressive have their origins in social conflicts that have taken a violent turn. Herein lies a paradox of violence and war: violence destroys but is also often associated with social creativity*" (Cramer 2006, 279).

Our data suggest that conflict shocks affect institutions, potentially setting the stage for long-lasting effects on income and growth. In terms of scope as well as results, our paper is closest to Bellows and Miguel (2009), who document that exposure to violence promotes collective action, trust, and political awareness and participation in Sierra Leone. They conclude, "civil war experiences are transformative." Our results for

[35] The data collection was a collaborative effort between the Institut de Statistiques et d'Etudes Economiqeus du Burundi (ISTEEBU), Antwerp University, Brussels University and Wageningen University, and was implemented under the flag of MICROCON – an EU funded project focusing on household analysis of violent conflict in various regions of the world.

[36] A few *macro* studies aim to "unbundle" institutions at the cross-country level (La Porta *et al* 1999, Bardhan 2005, Acemoglu and Johnson 2005).

Burundi are consistent with such a conclusion—we also document positive associations between conflict and various measures of social capital.[37] Obviously it is comforting to note that the accumulation of evidence across (African) settings creates a consistent picture of the effect of war on institutions. However, and interestingly, the effects of conflict appear to vary across institutional proxies and are not unambiguously positive. While conflict in Burundi provided an impetus to the informal titling of land—arguably improving tenure security—other evidence suggests it erodes the quality of local political institutions and rule of law.

We also document that these "institutional effects" are unlikely to be driven by income effects of war—brought about by NGOs, or otherwise. Consistent with Bellows and Miguel (2009) we find no evidence of lasting effects of conflict on material standards. There are no significant differences in terms of assets or income between conflict and no-conflict villages. We tentatively argue that transformed preferences are a more likely candidate explanation for institutional effects (see Voors et al 2011 for a detailed analysis of risk, time and social preferences in post-conflict Burundi).

This paper is organized as follows. In section 2 we provide background to the Burundi case. In section 3 we introduce our data and empirical strategy. We base our analysis on fixed effects OLS models (comparing neighbors with different levels of exposure to violence within the same village, or exploring variation in violence across villages in the same province), and also estimate a series of 2SLS models (using geographical variables as instrumental variables for local conflict intensity to account for potential endogeneity and omitted variable issues). Section 4 contains our results, and in section 5 we present a brief exploration of the direct economic effects of conflict. Section 6 contains additional evidence to support our identification strategy, and section 7 concludes.

4.2 Conflict in Burundi

Burundi has known several periods of civil war between the nation's two main groups: Hutu (85% of the population) and Tutsi (14%). Under Belgian colonial rule, Tutsi dominated the public administration and most Hutu were barred from (active) political participation. The process of Hutu exclusion intensified following independence. Ngaruko and Nkurunziza (2000) describe how Tutsi from one region – Bururi province

[37] See also Blattman (2009), who reports a positive effect of conflict on political participation and awareness in Uganda, which he explains by arguing that violence augments the value placed on political expression.

– dominated the state and managed the nation for private gains. This process prompted Hutu insurrections (mainly in 1965, 1972, 1988 and 1993), which typically triggered drastic responses from the Tutsi-dominated army. In 1993, following the assassination of Burundi's first Hutu president Ndadaye, Hutu rebel groups massacred thousands of Tutsi. The army responded with large-scale attacks "…*making no distinction between communities which had been involved in violence against Tutsi and those that were not.*" (HRW 1998: 15). In a period of weeks some 30.000 - 50.000 people were slain. In the following years violence by rebels and the army raged across the country, killing over 300.000, predominantly Hutu. Many more have been injured or displaced. About 1.2 million Burundians were internally displaced or refugees in neighboring countries, so 20% of the country's population was uprooted (Krueger and Krueger 2007).

Burundi violence can be divided into two types—selective and indiscriminate violence. The former type targets individuals or communities selectively. The army, for example, singled out individuals who could threaten the Tutsi government. This included Hutu with higher levels of education (Krueger and Krueger 2007). Selective violence probably also occurred within communities, where the breakdown of the rule of law provided an opportunity to 'settle scores,' take measures to erase outstanding debts, or reshuffle valued property (e.g., André and Platteau 1998 for evidence in Rwanda).[38]

Other parts of violence appear indiscriminate. In the absence of information to distinguish rebels from the general population, armies often resorted to unselective violence. Other motivations include a desire for extermination or revenge, and plundering. Sometimes there is a perceived need to demonstrate power as part of the "tactics of fear" to control a population (Lyall 2009, see Uvin (1999) for evidence on Burundi). Our data, presented in more detail below, support the view that our measures of exposure to conflict are exogenous to virtually all household and village characteristics. However, local intensities of conflict vary with respect to geography, regions closer to neighboring countries Congo and Rwanda saw more violence as these areas were comparatively more frequented by rebels, regions in the south saw less violence. We use a communities' longitude and latitude as instruments for local conflict intensity, and exploit this information in a two-stage modeling strategy, outlined below.

Even though violence and intimidation have not disappeared from Burundi, the country has now known several years of relative peace. Efforts to quell the violence caught root at the turn of the century, and in 2003 an internationally-brokered peace

[38] Similarly, Deininger (2003) reports distance from infrastructure, asset inequality, appropriable wealth (cash crops) and low levels of human capital as determinants of rebel attacks in Uganda.

agreement was signed between the government and all-but-one of the rebel groups. This paved the way for a transition process that led to an integrated defense force, establishment of a new constitution, and elections resulting in a majority Hutu government. However, Burundi is still amongst the poorest in the world. Most of the population is employed in agriculture, of which the majority is at least partly dependent on subsistence farming. Arguably, the success of future transformations from subsistence farming to commercial agriculture or manufacturing will depend on the quality of extant institutions.

4.3 Data and empirical strategy

First we introduce our data. We mainly draw from the new and extensive Burundi Priority Household Survey (BPHS) and Community Survey (BCS). Local enumerators, in collaboration with ISTEEBU and MICROCON researchers, collected most of the data in August and September 2007. The BPHS contains data from 872 households in 100 *collines* (communities). Following up on a first wave of data collection by the World Bank in 1998, we recorded information on socio-economic and farm characteristics. We also collected information on institutional quality. Unfortunately, since such data were not recorded in 1998 we cannot construct a panel to study institutional change. Instead, we have to resort to a cross-section identification strategy (see below), where we can benefit from lagged variables from key explanatory variables. The BCS collected community-level data, for which we interviewed several (typically: three) community leaders (e.g., local administrators) in all communities. We collected data in 13 of 16 Burundi provinces (see Appendix 1).[39]

Table 1 summarizes our main variables. Panel A shows summary statistics for our local institutional data. We sometimes aggregate these household responses to arrive at community level variables.[40] Our first institutional variable is household ownership of informal "land titles," which is a proxy for tenure security at the household level. It is measured as the fraction of plots to which a household possesses full "titles." In our case study, land titles comprise an *acte de notoriété*, a document of paper signifying ownership rights, possibly recorded after a land transaction. These papers are accredited by the *chef de commune*, the administrative level encompassing several *collines*. These land rights are enforced both formally, through mediation by the *Commission National de Terre et*

[39] In the remaining provinces data could not be collected due to intense rebel activity in 1998.

[40] The small number of observations per community arguably implies that we measure institutional quality with some error introducing attenuation bias (i.e. regression results will be biased towards zero).

autre Biens, and informally, through local elder councils called *Bashingatahe*. Tentative analysis (not shown, but available on request) indeed suggests that informal titles improve tenure security—costly investments to reduce soil erosion and improve soil fertility are more likely to be undertaken on titled than on untitled plots.[41] On average, 42% of the plots in our sample is titled.

Next, we have two proxies for social capital. The exact definition of social capital is subject to debate, but it is often treated as a characteristic of communities, and described in terms of trust, and norms or networks (e.g. Bowles and Gintis 2002). Social capital may affect economic outcomes by lowering transaction costs or by enabling collective action (say, providing local public goods). For our first indicator of social capital we asked respondents whether they agreed with the statement '*Do most of the people in your colline help each other out when help is needed?*'. On average 86% of households answered affirmatively. We also asked a World Value Survey type of trust question to gauge whether villages trusted their fellow villagers, or not. We asking respondents to rate their level of trust in their household, extended kin and fellow colline members. We created a dummy variable to indicate households that recorded high trust levels on all three trust questions.[42] On average, trust in kin and community members was rather high, with an average score of 0.86.

Our final institutional dimension is the quality of rule of law. The BPHS asked respondents to rate the quality of three community-level institutions on a six point scale: (*i*) local justice authority (e.g., mediating in land conflicts), (*ii*) the army, and (*iii*) police. These three sub-indices were added to construct one overall index. In addition we asked respondents to express their appreciation of the quality of local political leadership (typically the *Chef de Colline*, the community chief). We treat this subjective assessment as a measure of the quality of local political institutions.

[41] Similar tentative analysis do not support the idea that titling enhances land sales, or that titled land acts as collateral to facilitate access to credit (details not shown, but again available on request).
[42] Our results are robust to omitting trust in household members and only considering stated trust in extended kin and fellow *colline* members, or indeed considering just trust in *colline* members

Table 1: Descriptive Statistics

	obs household level	obs community level	mean	st. dev.	min	max
Panel A: Institutional variables						
Fraction of plots with land titles [2]	846	100	0.42	0.47	0	1
Cooperation [2]	870	100	0.86	0.35	0	1
Trust [2]	871	100	0.86	0.35	0	1
Rule of Law [2]	859	100	12.08	3.38	3	18
Local Institutions [2]	856	100	4.17	1.28	1	6
Panel B: Conflict variable						
Victimization household level [2]	871	100	0.51	0.50	0	1
Panel C: Household variables						
Household head education level (1998) [1]	871	100	1.38	0.58	1	5
Household head education level (2007) [2]	853	100	1.54	2.42	0	15
Household head is literate (2007) [2]	860	100	0.49	0.50	0	1
Household head age (years, 1998) [1]	869	100	43.04	16.06	14	99
Household head age (years, 2007) [2]	869	100	49.83	14.73	12	99
Household head is male (1998) [1]	871	100	0.22	0.42	0	1
Household head is male (2007) [2]	871	100	0.71	0.45	0	1
Household per capita total expenditure (BIF, 1998) [1]	871	100	7084.15	4881.88	175.95	44363.52
Household per capita total expenditure (BIF, 2007) [2]	871	100	14325.48	16135.18	0	156860.2
Livestock farmer (1993) [4]	248	35	0.472	0.500	0	1
Perceived poverty status (2007) [2]	871	100	0.16	0.18	1	6
Cash-crop farmer (2007) [2]	871	100	0.61	0.49	0	1
Cash-crop farmer (1993) [4]	871	100	0.47	0.50	0	1
Bought land (2007) [2]	846	100	0.28	0.45	0	1
Invested in soil improvement (2007) [2]	871	100	0.75	0.43	0	1
Soil quality (2007) [2]	871	100	2.62	0.58	1	3
The war affected my life (2007) [2]	871	100	0.83	0.38	0	1
Sick months (2007) [2]	871	100	10.88	37.58	0	495
Do you have access to credit? (2007) [2]	871	100	0.12	0.32	0	1
NGO project (2007) [2]	871	100	0.36	0.48	0	1
Panel D: Community control variables						
Distance to market (2007) [3]		100	2.95	0.81	1	4.9
Land inequality (2007) [2]		100	0.26	0.21	0	0.56
Population density (log, 1990) [5]		99	5.38	0.51	3.71	6.27
Votes for Ndadaye in 1993 (%) [6]		98	62.59	18.41	3.16	93.51
Fraction livestock farmer (1993) [4]		35	0.46	0.20	0.13	1
Fraction cash-crop farmer (1993) [4]		35	0.33	0.31	0	1
Ethnic homogeneity (1993) [4]		31	82.87	16.89	30	99
Socio-economic homogeneity (1993) [2]		99	1.57	0.86	1	5
Latitude [7]		100	-3.22	0.43	-4.07	-2.35
Longitude [7]		100	29.94	0.32	29.10	30.80

Data sources: (1) Burundi Priority Household Survey 1998, (2) Burundi Priority Household Survey 2007, (3) Burundi Community Survey 2007, (4) Burundi Experiments Exit Survey 2009, (5) Monographies Communales Burundi. Ministre de la Planification du Développement et de la Reconstruction Nationale, Bujumbura, 2006, (6) Sinunguruza T. 2001. Les Elections au Burundi. Tout Savoir et Tirer des Leçons de Juin, (7) http://www.tageo.com. USD 1 = 1169.55 BIF (17 July 2008)

Panel B summarizes our data on local conflict intensity. To measure individual exposure to violence, the BPHS asked respondents about a range of war experiences, including death of family members, theft, ambush, forced labor, intimidation, property theft and destruction of household members. We use this information to construct a so-called victimization dummy indicating if a household experienced any of these experiences. About 51% of the households in our sample experienced such events. We also aggregate these individual indices to arrive at community-level violence measures.[43]

Household-level controls (X_i) and community-level controls (C_j), are summarized in Panels C and D, respectively. Household controls include age (years); gender (dummy for male-headed households); education (number of years of education of household head); and wealth (total per capita expenditures). For a follow-up analysis in section 6, where we probe the appropriateness of our identification strategy, we also use lagged control variables and a pre-war (1993) variable, capturing whether the household owned livestock or not. Community variables are distance to an agricultural market (measured in time intervals walking distance); and a Gini variable measuring land inequality. We also have used recall techniques to collect pre-war data on a range of community characteristics (for various subsamples of villages—see section 6). These include the share of community members that owned livestock, the share of the population that voted for the first Hutu president, population density, a measure of pre-war income and measures of ethnic and social heterogeneity. See Appendix A for variable definitions and summary statistics.

Next turn to our empirical strategy. We first consider correlations between our measures of local conflict and institutions at the community level:

$$I_{jk} = \alpha + \delta_1 V_{jk} + \delta_2 C_{jk} + \gamma_k + \varepsilon_{jk}, \quad (1)$$

where I_{jk} refers to our institutional variables, i.e., property rights security, social capital and rule of law of community j, where j=1,…, 100, V_{jk} refers to our victimization index, and C_{jk} refers to a vector of community control variables. We include province fixed effects, γ_k, where k=1,…,13, so that we are exploiting variation in exposure to violence across neighboring villages in the same province for sharp identification.

Second, we switch to the individual level and compare households that experienced conflict to those who did not:

[43] In addition we use data from our BCS, which recorded the local number of civilians killed and injured from confrontations between army and rebels, or one-sided violence during 1993-2003. In addition the robust to using conflict data from the ACLED database of PRIO (Raleigh and Hegre 2005). Results are qualitatively similar and available on request.

$$(2) \qquad I_{ij} = \alpha_j + \delta_1 V_{ij} + \delta_2 X_{ij} + \varepsilon_{ij},$$

where α_j refers to community level fixed effects (j=1, …,100), so that we are isolating variation in exposure to violence across fellow-villagers within the same village, and V_{ij} is the household victimization index. X_{ij} is a vector of household controls.

Third, to explore whether the consequences of violence may be felt throughout the community even if only a sub-set of individuals directly experienced acts of violence, we also look at the effect of village level violence on individual institutional quality:

$$(3) \qquad I_{ijk} = \alpha + \delta_1 V_{jk} + \delta_2 X_{ijk} + \delta_3 C_{jk} + \gamma_k + \varepsilon_{ijk},$$

Fourth, we explore whether conflict affects incomes directly. We replace our dependent variable in equation (3) by a set of income proxies (total per capita expenditure, livestock ownership and a measure of perceived poverty). This allows us to see whether any impacts of violence on institutional quality are due to income effects.

A key issue for inference is the potential non-random nature of violence during the civil war. An identifying assumption in our analyses until now is that exposure to conflict within and across villages is (almost) random, conditional on observable characteristics (vectors *X* and *C*). We use community longitude and latitude as instrumental variables for conflict exposure in a 2SLS framework to attenuate remaining endogeneity and omitted variable concerns. The use of these instruments is motivated by historical accounts of the war. The war started in the Northern provinces where several rebel groups formed, using the Congolese border and Kibira forest for shelter.[44] The fighting moved down to the nation's capital, Bujumbura, as both rebels and the army fought over its control, but soon spread to North-Eastern provinces as well, and eventually ravaged communities throughout the country (see United Nations 1996, Bundervoet *et al.* 2009, and Chétien en Mururi 2000 for detailed accounts). However, a gradient in the intensity of violence is evident – fighting was more common in the Northern and Eastern regions of the country. The ensuing exogenous variation in conflict intensity suggests latitude and longitude are potentially suitable instruments. This is confirmed by various test statistics reported below (instrument relevance and overidentification restrictions). We return to identification in section 6.

[44] Initially the *Conseil Nationale pour la Défense de la Démocratie*, but later others followed.

4.4 Results

We present our results on the relationship between conflict and institutions in Tables 2, 3 and 4. We report OLS as well as IV results, and present specifications with various vectors of controls. As mentioned, we use village or province fixed effects, depending on the nature of the dependent variable, so we compare victims and non-victims in one village (or attacked and non-attacked villages in one province).[45]

Table 2 contains the results for tenure security. Across all OLS specifications of equations (1)-(3), we record a statistically significant and positive correlation between land titling and conflict intensity at the community level (column 1 and 4) as well as at the household level (columns 2 and 3).[46] Column 5 presents the outcomes for a 2SLS model where we instrument for conflict intensity using the geographical variables (coefficients of excluded instruments provided in bottom panel). The results are robust across these specifications, and suggest greater security—in terms of informal titling—for individuals or communities that suffered from attacks. In pre-war Burundi, land title ownership was limited to cities and land in the countryside was mostly inherited. Yet, after the war land markets started to develop (MEATTP 2006), and subsequently the demand for titles grew. Lin (1995) describes how land markets develop in response to factor imbalances, and it is easy to imagine that large-scale killings in early stages of the war (over 200.000 deaths in 1993 alone) distorted factor balances to a large extent. In addition, people sought to acquire land closer to roads and community centers to benefit from greater security provided by the army (MEATTP 2006). In later years people sought to secure land ownership where ownership could be contested by those who fled but could return (ICG 2003). In many cases returning refugees were unable to exercise their claim on previously owned land. Using the so-called "return package" provided by UNHCR, many households resorted to buying land with accompanying title deeds (UNHCR 2007).[47] Column (6) provides the associated beta-coefficients of the estimation in column (5). Scaling the variables such that coefficients are directly comparable suggests that exposure to violence is the most important determinant of tenure security.

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[45] The relation between conflict and institutions may be non-linear. We have estimated several models with interaction terms or squared terms (to capture diminishing returns), and consistently find that such terms do not enter our results significantly (results not shown, but available on request).

[46] Note that the coefficient on violence increases if controls are included in column 3 Following the reasoning of Bellows and Miguel (2009) this suggest that it is unlikely that omitted variable bias explains away the conflict effect (see also Altonji *et al.* 2005).

[47] UNHCR has coordinated and facilitated the return of refugees to Burundi since 2003. As an incentive people are provided with logistical and financial support, consisting of a cash grant of 50.000 BIF.

Table 2. Conflict and Economic Institutions

	(1) Tenure OLS	(2) Tenure OLS	(3) Tenure OLS	(4) Tenure OLS	(5) Tenure 2SLS	(6) Tenure 2SLS
Victimization household level		0.0621**	0.0619**			
		(0.0303)	(0.0306)			
Victimization village level	0.354**			0.305**	1.225***	0.665
	(0.167)			(0.133)	(0.468)	
Household head education level	0.0803**		-0.00794	0.00642	0.0101	0.050
	(0.0330)		(0.00566)	(0.00781)	(0.00860)	
Household head age	0.00279		-0.000961	-0.0000904	0.000915	0.027
	(0.00504)		(0.000938)	(0.00110)	(0.00127)	
Household head is male	-0.274		-0.00785	-0.0485	-0.0570	-0.054
	(0.166)		(0.0304)	(0.0325)	(0.0393)	
Household per capita total expenditure (BIF)	0.000002		0.000001	0.000001	0.000001	0.026
	(0.000004)		(0.000001)	(0.000001)	(0.000001)	
Land inequality	0.118			0.207	0.473*	0.096
	(0.238)			(0.205)	(0.283)	
Distance to market	0.0257			0.0212	0.00664	0.011
	(0.0344)			(0.0327)	(0.0392)	
Constant	0.402	0.618***	0.663***	0.486***	0.172	
	(0.307)	(0.168)	(0.179)	(0.146)	(0.230)	
Fixed Effect	Province	Village	Village	Province	Province	Province
N	100	846	827	827	827	827
adj. R-sq	0.463	0.440	0.451	0.252	0.117	
Latitude					0.314**	
					(0.147)	
Longitude					0.526***	
					(0.188)	
Partial F-stat					5.53***	
Hansen J					0.68	

Robust standard errors in parentheses clustered at village level for Column (4)-(5). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Column (6) reports beta coefficients of column (5).

Table 3. Conflict and Social Capital

	(1) Cooperation OLS	(2) Cooperation Probit	(3) Cooperation Probit	(4) Cooperation Probit	(5) Cooperation IV-Probit	(6) Trust IV-Probit	(7) Cooperation IV-Probit
Victimization household level		0.274**	0.280**				
		(0.139)	(0.142)				
Victimization village level	0.136**			0.712***	1.864**	1.220+	1.864
	(0.0574)			(0.255)	(0.893)	(0.865)	
Household head education level	0.0197		0.0494*	0.0551**	0.0589**	-0.00653	0.059
	(0.0145)		(0.0300)	(0.0241)	(0.0241)	(0.0247)	
Household head age	0.000424		0.00119	0.00114	0.00235	0.00261	0.002
	(0.00294)		(0.00454)	(0.00388)	(0.00390)	(0.00396)	
Household head is male	-0.108		0.0742	-0.00762	-0.0261	0.132	-0.026
	(0.0767)		(0.145)	(0.116)	(0.123)	(0.134)	
Household per capita total expenditure (BIF)	-0.000001		2.53e-08	-0.000001	-0.000001	-0.000004	0.000
	(0.000002)		(0.000003)	(0.000004)	(0.000004)	(0.000003)	
Land inequality	0.0546			0.319	0.599	0.884	0.599
	(0.0876)			(0.603)	(0.680)	(0.668)	
Distance to market	0.00559			0.0194	-0.0121	-0.0940	-0.012
	(0.0174)			(0.0708)	(0.0868)	(0.0782)	
Constant	0.806***	1.003*	0.858	0.537	0.192	0.813	
	(0.195)	(0.568)	(0.624)	(0.442)	(0.558)	(0.560)	
Fixed Effect	Province	Village	Village	Province	Province	Province	Province
N	100	577	555	850	850	851	851
adj. R-sq	0.133	0.07	0.08	0.06			
Latitude					0.299**	0.294**	
					(0.145)	(0.147)	
Longitude					0.536***	0.540***	
					(0.183)	(0.181)	
Partial F-stat					5.74***	5.50***	
Wald test P-value					0.19	0.21	

Robust standard errors in parentheses clustered at village level for Column (4)-(6). + $p < 0.15$, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Column (7) reports marginal effects of column (5).

In Table 3, we present how conflict affects our proxies of social capital. We focus on cooperation, and treat our trust variable as a robustness check.[48] Across the various estimations we document a positive effect of violence on social capital. This is true for the OLS, probit and 2SLS models. It appears as if both trust and cooperation are positively affected by conflict exposure (even if the 2SLS results for cooperation are more significant than those for trust). We believe these results echo the findings of Bellows and Miguel (2009) for Sierra Leone, and to some extent also those of Blattman (2009) for Uganda. They are also consistent with the Burundi evidence on conflict and pro-social preferences in sharing experiments, presented by (Voors *et al.* 2011). Column (7) reports marginal effects of the estimation in Column (5), indicating that exposure to violence is the most important driver of social capital.

However, the impact of conflict on institutions is not always positive. In Table 4 we present evidence for conflict and our proxies of political institutions. We find that exposure to violence tend to be associated with a lower appreciation of the rule of law (people's subjective assessment of the police, justice and the army) and local political leadership. However, and unlike before, we find the variables enter with negative signs and that coefficients are only significant in a few specifications.[49] In light of the evidence on violence and political participation presented by Bellows and Miguel (2009) and Blattman (2009) this creates an interesting puzzle. Do communities affected by violence inherit worse leadership, so that individuals have a greater incentive to become engaged in an effort to ameliorate the situation? Or does greater popular participation adversely affect the quality of local governance? Unfortunately our data does not permit us to explore this further.

Interestingly, we find a negative and significant correlation between the quality of political institutions and informal land titles (ρ = –0.2**). Perhaps people respond to poor leadership by demanding greater land security (ICG 2003).

[48] We also experimented with another prominent measures of social capital–association membership. A strong and positive association exists between conflict exposure and membership, further cementing the social capital findings. These results are significant at the 5% level, or better.

[49] Specifically, the 2SLS models are significant and the OLS models are not. This may be because instrumental variables techniques address measurement error (arguably a relevant concern for us, given the nature of our variables and size of our sample). However, 2SLS may also overestimate the true effect if the included instruments are positively correlated with omitted variables that have the same sign as the endogenous institutional variables in the income regression.

Table 4. Conflict and Political Institutions

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Rule of law	Rule of law	Rule of law	Rule of law	Rule of law	Local institutions	Rule of law
	OLS	OLS	OLS	OLS	2SLS	2SLS	2SLS
Victimization household level		-0.263	-0.250				
		(0.267)	(0.272)				
Victimization village level	-0.904			-0.402	-3.811*	-1.393**	-0.293
	(0.803)			(0.623)	(2.124)	(0.710)	
Household head education level	-0.250		-0.0175	-0.0475	-0.0627	-0.00881	0.046
	(0.152)		(0.0534)	(0.0495)	(0.0556)	(0.0240)	
Household head age	0.00417		0.00525	0.00478	0.00111	0.00296	0.004
	(0.0316)		(0.00877)	(0.00830)	(0.00882)	(0.00277)	
Household head is male	-0.394		-0.0784	-0.0449	0.0233	0.00267	0.003
	(0.914)		(0.273)	(0.292)	(0.297)	(0.122)	
Household per capita total expenditure (BIF)	0.000003		0.00001	0.00001	0.00001	0.000001	-0.029
	(0.00002)		(0.00001)	(0.00001)	(0.00001)	(0.000002)	
Land inequality	1.951*			2.144	1.180	0.601	0.033
	(1.066)			(1.310)	(1.426)	(0.539)	
Distance to market	-0.294			-0.263	-0.190	0.0149	-0.045
	(0.178)			(0.171)	(0.207)	(0.0750)	
Constant	13.63***	10.87***	10.55***	12.09***	13.14***	4.096***	
	(1.862)	(0.899)	(0.974)	(1.033)	(1.180)	(0.466)	
Fixed Effect	Province	Village	Village	Province	Province	Province	Province
N	100	859	839	839	839	846	839
adj. R-sq	0.186	0.070	0.062	0.032	-0.007	0.024	
Latitude					0.301**	0.296**	
					(0.147)	(0.148)	
Longitude					0.544***	0.535***	
					(0.185)	(0.185)	
Partial F-stat					5.74***	5.50***	
Hansen J					0.92	0.15	

Robust standard errors in parentheses clustered at village level for Column (4)-(6).), * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Column (7) reports beta coefficients of column (5).

4.5 Post war outcomes

We now investigate whether exposure to conflict is correlated with post-war outcomes. If war-affected communities have higher incomes, perhaps income is an omitted variable driving both the incidence of conflict and institutional quality? Table 5 reports coefficients from OLS regressions where we "explain" a series of post-war outcome variables by victimization and other variables. To economize on space we only report the coefficients of interests, but in all models we included a full set of controls and fixed effects (see bottom of Table 5).

In Panel A we investigate whether conflict is related to post-war income levels (proxied by total per capita expenditure and perceived poverty levels). We find no such effect, which is consonant with recent findings elsewhere. For example, Bellows and Miguel (2009) find no lasting effects of the civil war in Sierra Leone on income levels.[50] Of course a relatively short period of time has elapsed since the cessation of violence and our measurement of income. If conflict affects institutions and if institutional quality matters for development, then we would expect the incomes of conflict and non-conflict communities to diverge in the future. This could be tested in the future, as additional income data come available.

In Table 3, we present how conflict affects our proxies of social capital. We focus on cooperation, and treat our trust variable as a robustness check.[51] Across the various estimations we document a positive effect of violence on social capital. This is true for the OLS, probit and 2SLS models. It appears as if both trust and cooperation are positively affected by conflict exposure (even if the 2SLS results for cooperation are more significant than those for trust). We believe these results echo the findings of Bellows and Miguel (2009) for Sierra Leone, and to some extent also those of Blattman (2009) for Uganda. They are also consistent with the Burundi evidence on conflict and pro-social preferences in sharing experiments, presented by (Voors *et al.* 2011). Column (7) reports marginal effects of the estimation in Column (5), indicating that exposure to violence is the most important driver of social capital.

[50] Similarly, at the macro level few persistent effects on income have been found (see Davis and Weinstein 2002 for empirical evidence on Japan, Brakman *et al.* 2004 for Germany, and Miguel and Roland 2005 for Vietnam). However, also see Bundervoet *et al.* (2009) for conflicting evidence.

[51] We also experimented with another prominent measures of social capital–association membership. A strong and positive association exists between conflict exposure and membership, further cementing the social capital findings. These results are significant at the 5% level, or better.

Table 5. Post war outcome variables

Dependent variables	Victimization Village Level
Panel A: Welfare Variables	
Total per capita expenditure	-1242.25
	(1596.75)
Perceived poverty status ([0-10]	0.133
	(0.0932)
Panel B: Farm productivity Variables	
Livestock farmer	0.0243
	(0.159)
Cash-crop farmer	0.170*
	(0.100)
Respondent bought land	0. 657+
	(0.102)
Invested in soil improvement	0.00870
	(0.255)
Soil quality	-0.0336
	(0.208)
Panel C: Human capital	
Respondent is literate	-0.182
	(0.275)
The war affected my life	0.529*
	(0.284)
Sick months	10.50
	(8.229)
Panel D: Access to credit and NGO project	
Do you have access to credit?	-0.378
	(0.379)
NGO project	0.908
	(0.852)
Fixed effects	Province

Robust standard errors in parentheses clustered at village level + $p < 0.15$*, $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$ Table reports coefficients from separate household level OLS regressions. The regressions include age, literacy, gender, per capita land size, distance to markets, land inequality and province fixed effects.

In Panel B we consider several farm productivity indicators, and find that war exposure is positively associated with the probability of whether a household produces cash crops or had bought land.52 Arguably these two variables are both correlated with our tenure security, analyzed above ($\rho = -0.13$** and $\rho = -0.03$* respectively). Conflict prompted people to secure land ownership (ICG 2003) and adjust their crop portfolio's towards more profitable crops.

Another likely channel through which conflict may have affected long term prospective may be through the impacts on human capital (Panel C). While we record no direct effects on literacy and health outcomes (but see Bundervoet *et al.* 2009), we do find that war affected the outlook of people on life. This view is consistent with perspectives

[52] see Nillesen and Verwimp (2010) for a more elaborate discussion of the impact of the Burundi conflict on agricultural portfolio choice.

in the political science literature. For example, Blattman (2009) reports that war exposure produced "changes in personal goals, perspectives or self-regard" (p. 243) under former abductees of Uganda's Lord's Liberation Army. Similarly, the psychological literature documents that victims display remarkable levels of resilience to traumatic experiences and describes how this can permanently alter someone's outlook on life (see Tedeschi and Calhoun 2004). Taken together, especially when combined with evidence provided by Voors *et al.* (2011) on conflict exposure and changes in preferences, we tentatively speculate that conflict-induced shifts in preferences, attitudes and perceptions are one of the channels via which institutional change eventuates.

Finally, in Panel D we investigate whether the incidence of war attracted NGO activities. If such selective targeting by aid agencies took place, our estimates could be spurious (for example, if NGO programs promote institutional development by improving cooperation or reducing land ownership contestations in post-conflict villages). However, we find no evidence of such selective placement of NGO activities, and conclude it is unlikely that NGO targeting explains our results.

4.6 Exogeneity and Selection Bias

While our instrumental variable strategy, based on exogenous geographical information, should purge most endogeneity concerns, it may be the case that latitude and longitude are correlated with certain omitted variables—or even with the quality of local institutions—if ethnic or economic variables vary across the Burundi landscape along a predictable gradient. If so, our instruments may be correlated with the error terms of our 2nd stage equations (our institutions models 1-3). Note that the Hansen J test has low power, and verifying whether the exclusion restriction holds is notoriously difficult. To further explore whether households and communities were indeed targeted randomly (conditional on geographic location), we investigate which household and community characteristics are related to attacks. For example, if rebel sympathies systematically vary from one region to another, or if economic outcomes vary along a geographical gradient, driving both institutional development and patterns of conflict, then our IV strategy may be biased.

To address this potential concern we regress our victimization variable on lagged household and community characteristics. Table 5 summarizes our results. In Columns 1 and 2 we explain victimization at the household level, and columns 3 and 4 present village-level results. The difference between column 1 and 2 is that the latter contains

pre-war data collected for a subsample of villages (data on livelihoods and ethnical composition).[53] Hence, column 2 contains extra variables but is based on fewer observations. Similarly, village-level results in column 3 are based on the large sample, and column 4 focuses on the subsample of villages for which pre-war data are available. All models include fixed effects.

Across all specifications we find no evidence of selective targeting of individuals or villages. None of the (pre-war) household or village characteristics enters significantly. This further supports our identification strategy.

Another source of potential bias stems from potential non-random attrition. If a non-random subsample of villagers was killed during the conflict, or migrated and did not return, then this could bias our results. We analyzed attrition between the start of the war in 1993 and 2007, and by comparing larger samples between 1998 and 2007, and find that (*i*) attrition was surprisingly low in our sample (only 14% between 1993 and 2007), and (*ii*) violence was not very targeted. To investigate whether attrition was non-random we estimated a series of probit models of attrition on a vector of lagged control variables. The only (statistically) weak evidence of attrition in our sample is for gender and age. We conclude attrition bias is not likely to affect our results. To save space these results are not shown here, but they are available on request and also appear in Table 2 of Voors *et al.* (2011).

[53] These data are available for a subset of 35 communities for which additional data was collected in a third wave of data collection, in 2009 (see Voors *et al.* 2011).

Table 6. Identification

	(1) Victimization Household Level Probit	(2) Victimization Household Level Probit	(3) Victimization Village Level OLS	(4) Victimization Village Level OLS
Panel B: Household variables				
Household head education level(1998)	0.144	0.186		
	(0.0974)	(0.185)		
Household head age (1998)	0.00736	-0.002		
	(0.0151)	(0.029)		
Household head age^2 (1998)	-0.000169	-0.0001		
	(0.000143)	(0.0003)		
Household head is male (1998)	-0.0802	0.328		
	(0.127)	(0.265)		
Total per capita expenditure (1998)	-0.00000233	-0.00002		
	(0.0000128)	(0.00002)		
Livestock farmer (1993)		0.212		
		(0.215)		
Cash-crop farmer (1993)		0.255		
		(0.221)		
Ethnicity		-0.036		
		(0.318)		
Panel B: Community variables				
Average education level(1998)			-0.0802	
			(0.107)	
Average household head age (1998)			0.0478	
			(0.0385)	
Average household head age^2 (1998)			-0.000606	
			(0.000435)	
Fraction male (in 1998)			0.228	
			(0.163)	
Average total per capita expenditure (1998)			0.0133	
			(0.0234)	
Population density in 1990 (log)			-0.251	
			(0.157)	
Socio-economic homogeneity (1998)			0.0191	
			(0.0226)	
Votes for Ndadaye			-0.144	
			(0.234)	
Fraction livestock farmers (1993)				0.180
				(0.285)
Fraction cash-crop farmers (1993)				0.043
				(0.226)
Ethnic homogeneity (1993)				-0.0003
				(0.0026)
Constant	0.618	0.155	-10.35*	0.487
	(0.633)	(1.127)	(5.575)	(0.719)
Fixed effects	Village	Village	Province	Province
N	811	227	97	31
pseudo R^2/ adj. R^2	0.18	0.15	0.50	0.21

Robust standard errors in parentheses * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

4.7 Conclusions

Institutions are key determinants of growth and development, but our understanding of how "endogenous institutions" evolve over time, in response to shocks or otherwise, is badly developed. In this paper we seek to contribute to our understanding by using micro data to understand how exposure to conflict affects local institutional quality. For this purpose we "unbundle" local institutions and distinguish between a measure of tenure security, proxies for social capital, and a measure of local political institutions. Our main result is that violence affects institutions, so that conflict can have persistent (or at least: long-lasting) effects on growth and development.

To the extent that the scope of this paper overlaps with earlier work (specifically: Bellows and Miguel for Sierra Leone, and Blattman for Uganda), our results are qualitatively consistent with the existing evidence. These emerging patterns, suggests that we are not picking up noise or documenting anomalies. However, the scope of this project also extends earlier work, and includes unbundled features (tenure security and subjective appreciation of local political institutions) that have heretofore not been analyzed. The overall picture is mixed, and our second main result is that the impact of conflict appears to vary, depending on the institutional variable.

Our data lend no support for the hypothesis that institutional change is invited by income shocks resulting from conflict (possibly induced by NGO intervention). Rather, it appears as if conflict has altered the preferences of villagers, affecting the demand for institutional reform (and possibly the supply of effort to reform institutions). The latter perspective is consistent with recent evidence on the positive impact of conflict on (pro-social) preferences, gathered in a variety of contexts—Voors et al (2011) in Burundi, Bauer et al (2011) in Sierra Leone and Georgia, Gilligan *et al.* (2011) in Nepal and Gneezy and Fessler (2011) in Israel. Further research into the consequences of conflict on preferences and institutions – and implications for post-conflict recovery and growth – appears a promising field of research for the future.

Data Appendix

Institutions variables

- *Fraction of plots titled.* Household level variable for the *i*-th household is created by taking the fraction of plots for which the household owns titles (*propriétaire avec titre).* Alternatively, in regressions using community level title ownership, the variable comprises a community average of the individual responses (BPHS 2007).
- *Cooperation*. Household level variable for the *i*-th household is a dummy taking unity if responded positive to the question '*Do most of the people in your colline help each other out when help is needed?*, zero else. Alternatively, in regressions using community level title ownership, the variable comprises a community average of the individual responses (BPHS 2007).
- *Trust*. The trust variable in the *j*-th community is a dummy variable taking unity if the *i* -th households' rating of their level of trust in member of colline was above 3 on a 1 (no confidence)-6 (a lot of confidence) point scale (BPHS 2007).
- *Rule of law*. The rule of law index in the *j*-th community is based on the *i* -th households' rating of their level of confidence in (1) police, (2) justice authority, (3) army. The indices were scaled on a 1 (no confidence)-6 (a lot of confidence) point scale. The sub-indices scores are added. Alternatively, in regressions using community level title ownership, the variable comprises a community average of the individual responses (BPHS 2007).
- *Local institutions*. The political institutional quality index in the *j*-th community is based on the *i* -th households' rating of their level of confidence in local political authorities. The indices were scaled on a 1 (no confidence)-6 (a lot of confidence) point scale. (BPHS 2007).

Household and community variables

- *Victimization Household Level* is a dummy variable taking unity if any household member was exposes to death of household member, forced labor, torture, ambush, theft of assets, crops and money (BPHS 2007). *Victimization Community Level* is created by averaging household level victimization dummies (BPHS 2007).
- *Household head age* is measured in years (BPHS 1998 and 2007).

- *Household head is male* is a dummy variable taking unity if head of household is male, zero else. In community level regressions, variable household dummy's are averaged over number of households per community (BPHS 1998 and 2007).
- *Household head education level in 2007* is the years of education (BPHS 2007).
- *Household head education level in 1998* is the level of education ranging in five levels ranging no education- university (BPHS 1998).
- *Household head is literate in 2007* is the years is a dummy variable taking unity if head of household is literate, zero elsewhere (BPHS 2007).
- *Per capita total expenditure* is the aggregated income of per capita expenditure for all goods purchased over a 15 day period valued at local market prices and divided by the adult equivalents of household i (BPHS 1998 and 2007).
- *Cash-crop farmer in 2007* is a dummy taking unity if household produces cash crops (coffee, tea and tobacco) in 2007, zero elsewhere. (BEES 2009)
- *Livestock farmer in 1993* is in household level regressions variables is dummy taking unity if household owned livestock in 1993, zero elsewhere. In community level regression, variable for j-th community is created by averaging over all community households (BEES 2009)
- *Perceived level of poverty status in 2007*: assessment of poverty status on a 6 point scale 1 (very poor)-10 (very rich) (BPHS 2007).
- *Bought land*. Dummy variable taking unity if household owns land which has been purchased, zero else (BPHS 2007).
- *Access to credit*. Dummy variable taking unity if household currently has access credit from an 'Association de Credit Cummunautaire' (COOPEC) (BPHS 2007).
- *NGO project*. Dummy variable taking unity if agricultural project was present in community between 1993-2007 (BCS).
- *Invested in soil improvement*. Dummy variable taking unity if household invested in soil improvements (BPHS 2007).
- *Soil fertility*. Assessment of soil fertility, measured on a 1(fertile)-3(not fertile)point scale (BPHS 2007).
- *The war affected my life*. Dummy taking unity if responded positive to the question '*Do you feel the war affected my life?*, zero else. (BPHS 2007).
- *Sick months*. Household variable indicating the months household members have been very sick between 1998 and 2007 (BPHS 2007).

- *Distance to market.* Distance to main agricultural market where food and non-food items are traded, measured in time intervals of 15 minutes, where $t = 1,...,5$ (BCS).
- *Land inequality.* Variable based on household land holdings. Community level Gini coefficient is created by $G = 1 - 2\int_0^1 L(X)dX$ (BPHS).
- *Population density in 1990* is number of people in community per square kilometer (RNPH 2008).
- *Percentage of votes for Ndadaye*: percentage of votes in favor of Ndadaye at the commune level during the presidential elections in 1993
- *Ethnic homogeneity*: percentage of Hutu population in community (BEES 2009).
- *Social homogeneity*: measure of perceived within village socio-economic inequality rated on a five-point scale 1 (no inequality)-5 (high degree of inequality). (BCS 1998 and 2007).
- *Latitude.* Community latitude coordinate. Tageo 2011
- *Longitude.* Community longitude coordinate. Tageo 2011

Data sources

- BPHS. Burundi Priority Household Survey 1998 and 2007
- BCS. Burundi Community Survey 1998 and 2007
- BEES. Burundi Experiments Exit Survey 2009
- FAO. Valuation Des Recoltes, Des Approvisionnements Alimentaires et de la Situation Nutritionelle, 2006
- MCB 2006. Monographies Communales Burundi. Ministre de la Planification du Développement et de la Reconstruction Nationale, Bujumbura, 2006
- Sinunguruza T. 2001. Les Elections au Burundi. Tout Savoir et Tirer des Leçons de Juin
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Chapter 5

Violent Conflict and Behavior: a Field Experiment in Burundi

Abstract: We use a series of field experiments in rural Burundi to examine the impact of exposure to conflict on social-, risk- and time preferences. We find that conflict affects behavior: individuals exposed to violence display more altruistic behavior towards their neighbors, are more risk seeking, and have higher discount rates. Large adverse shocks can thus alter savings and investments decisions, and potentially have long-run consequences – even if the shocks themselves are temporary.

Publication Information: Voors, M.J., E.E.M. Nillesen, P. Verwimp, E.H. Bulte, B.W. Lensink and D.P. van Soest, 2011. Violent Conflict and Behavior: a Field Experiment in Burundi, American Economic Review, In Press

5.1 Introduction

Civil wars are sometimes referred to as "development in reverse." They are typically associated with the destruction of physical capital, and temporary drops in income. This has been documented, for example, for Japan by Davis and Weinstein (2002), for Germany by Brakman *et al.* (2004), and for Vietnam by Miguel and Roland (forthcoming). A simple Solow-style growth model predicts that, following the cessation of violence, capital stocks rebound so that per-capita income eventually returns to its steady state. The actual speed of recovery following conflict is subject of debate (e.g. Chen *et al.* 2008, Cerra and Saxena 2008). However, when conflict affects institutions, social organization, or other preferences it is not obvious that societies will bounce back to pre-war income levels. If wars contribute to the erosion of social capital or raise levels of impatience, adverse growth and level effects could eventuate. The opposite could happen when war invites institutional improvements or alters preferences in such a way that savings are encouraged.

A number of African countries have experienced remarkable post-war recovery after civil war. Examples include Mozambique, Angola, Rwanda, and Uganda. Undoubtedly, this is partly due to generous aid flows that typically follow the cessation of violence. However, other mechanisms may be at play too. Heterodox social scientists have long argued that violence can carry the seed of societal reform, spurring the expansion of capitalism and promoting economic growth. For example, Cramer (2006) points to historical events to support his claim that violence can "*produce institutional changes, amendments to the rule of the game. In retrospect, many changes that come to be seen as progressive have their origins in social conflicts that have taken a violent turn. Herein lies a paradox of violence and war: violence destroys but is also often associated with social creativity.*" (Cramer 2006:279). A small literature is now emerging that appears consistent with this perspective. Blattman (2009) uses data from northern Uganda and links past abduction by rebels to increased political engagement of victims. Bellows and Miguel (2009) report positive correlations between violence and political and social behavior in Sierra Leone.

One possible interpretation consistent with this evidence is that exposure to conflict induces a shift in preferences. This explanation implies a challenge for orthodox economic theory. Economists regard preferences as exogenous and fixed in their straw man model of *Homo economicus* (at least in the short-term).[54] The notion of endogenous,

[54] In the (very) long run, evolutionary processes can change the distribution of preferences, as suggested by Netzer (2009) in case of risk and time preferences, and by Choi and Bowles (2007) for social preferences, even if preferences are still hard-wired and fixed for individuals.

or context-dependent, preferences gnaws at the foundations of standard welfare theory. Interestingly, there is little opposition against the concept of "malleable preferences" in other social sciences. Indeed, in psychology it is widely accepted that large (temporary) shocks can have persistent effects on someone's outlook on life (Carmil and Breznitz 1991, Punamäki *et al.* 1997, Tedeschi and Calhoun 2004), or in economist terms, on someone's preferences. Since such preferences are fundamental determinants of consumption, saving and investment behavior — the drivers of economic growth — the notion of endogenous preferences has far-reaching consequences for how we should think about development. In particular, the scope for vicious and virtuous development cycles may be radically altered.

The main objective of this paper is to examine the causal effect of exposure to violence on behavior in a series of economic experiments, in which payoffs vary between choices across three dimensions: timing, riskiness, and social consequences. Key questions are: do victims of conflict behave more pro-socially, do they have a higher propensity to save and invest in the future, and are they more prone to taking risks? We try to answer these questions by pulling together survey and new experimental data from Burundi. First, we collected detailed information on the (local) history of violence in a set of Burundian communities, and on a range of household and community variables. We then conducted a series of field experiments, implementing games to determine risk -, time -, and social preferences in an incentive–compatible fashion. While such preferences have been measured in a variety of contexts, this study is the first to apply experimental methods in a post-conflict environment to gauge the effect of violence on human decision-making.

Our results strongly suggest that exposure to violence affects behavior – possibly via altering preferences. We find that individuals who have either experienced violence themselves, or who live in communities that have been violently attacked, display more altruistic behavior, are more risk seeking, and act less patiently. Results are robust across several specifications, and are obtained for both experimental data as well as observational data collected in the survey (information about social capital, crop choice and expenditures on farm improvement). We believe these findings shed new light on post-war recovery processes by speaking against overly pessimistic views on the destructive long-term consequences of civil war.

Identifying whether or not preferences are endogenous poses two problems for the analyst. The first concern regards the potential endogeneity of the shock due to

selection bias and non-random attrition. Although our tests indicate that attrition is unlikely to be substantial, we probe the robustness of our findings using various additional analyses, including an instrumental variable approach. Second, we cannot directly observe preferences. Instead, we observe behavior (in an experiment), and can try to make inferences about the underlying preferences. While the literature commonly interprets experimental play as reflecting underlying preferences (e.g., Henrich *et al.* 2001, Tanaka *et al.* 2010), behavior and preferences are not identical concepts. Behavior is affected by many factors, including the (social) context and beliefs about the behaviors of others (which may not be invariant with respect to the history of violence either). We present these competing hypotheses, and point out that our quasi-experimental data do not allow us to cleanly separate the relative importance of the various mechanisms linking violence to shifts in behavior.

This paper is organized as follows. In section 2 we discuss the background to the conflict. In section 3 and 4 we describe our data and research design. Section 5 discusses our identification strategy, including the way in which we address endogeneity issues. In section 6 we present our main experimental results, including a robustness analysis, and aim to interpret them in the context of economic thinking. Section 7 concludes.

5.2 Background to the Conflict in Burundi

Since independence, Burundi has been the stage of nearly three decades of civil war between the country's two main ethnic groups; Hutu (85% of the population) and Tutsi (14%). At the outbreak of the most recent episode of violence in 1993, following the assassination of the country's first Hutu president, Melchior Ndadaye, Hutu groups (mostly farmers) targeted Tutsi in retaliation throughout the country, killing thousands of Tutsi within weeks. In turn, the Tutsi-dominated army responded with indiscriminate and large-scale attacks on Hutu, "…*making no distinction between communities which had been involved in violence against Tutsi and those that were not*" (Human Rights Watch 1998: 15).

This started a civil war. In the Northern provinces, benefiting from the proximity of the Congolese border and the Kibira forest for shelter, several rebel groups formed (initially the Conseil Nationale pour la Défense de la Démocratie, but later others followed). Much of the war was concentrated near the nation's capital, Bujumbura, as both rebels and the army fought over its control, but the conflict soon spread to the North-Eastern provinces, and later to Central and Southern Burundi (for information

about the evolution of the war, refer to United Nations 1996, Bundervoet *et al.* 2009 and Chrétien en Mukuri 2000). Eventually, rebel groups and the army ravaged communities throughout the country, but a gradient in the intensity of violence is evident – fighting was more intense near the capital.

Which factors explain patterns of violence in Burundi? Standard explanations of greed and grievance appear to have little explanatory power (see section 5 below). Instead, Uvin (1999) proposes an explanation based on fear. Traumatized by earlier waves of violence in the 1970s, Hutu were fearful of being victimized (again) and launched pre-emptive attacks on Tutsi army barracks or Tutsi civilians. The Tutsi-dominated army responded by killing Hutu indiscriminately. In response, attacks by Hutu militia became "*increasingly brutal and random, affecting all of the country and causing profound fear among Tutsi as well as Hutu bystanders*" (Uvin 1999: 262). Chaos and anarchy erupted, and the civilian population paid a high prize. Human Rights Watch refers to most of the victims of violence in Burundi as "proxy targets" (Human Rights Watch 1998): while the army and Hutu militias fought for control, direct clashes between the rival fractions were rare; instead most of the violence was unleashed on civilians.

Violence was largely indiscriminate because of the army's inability to identify rebels, but also by a desire for extermination, "revenge by proxy", plundering, and a perceived need to demonstrate power as part of the tactics of fear to control a population (Uvin 1999, Krueger and Krueger 2007; refer to a broader discussion of such tactics to Kalyvas 2006). This type of violence is near-exogenous to household characteristics and local economic conditions, hitting communities and civilians regardless of social status, education or income. Both army and rebels have arbitrarily and sweepingly raided communities throughout the country.[55] People were also attacked while fleeing or during the creation of 'regroupment camps,' between 1996-2001. In areas suspected of rebel activity, the army led a brutal campaign to cut rebels of supplies, support and shelter. Civilians were evicted from their homes and forcibly brought together in camps. As an incident report from Human Rights Watch states: "*[…S]oldiers would … order people to gather at a specific site. They killed anyone who refused.*" (Human Rights Watch 1998: 29). At its apex, an estimated total of 220,000 people lived in these camps.

[55] Unlike the situation in neighboring Rwanda, where conditions of anarchy enabled villagers to settle scores and target specific fellow villagers (e.g. André and Platteau 1998), there is little or no evidence of such selective killings in Burundi (Uvin 1999).

Burundi has only recently started to recover from this violence, which left over 300,000 Burundians dead and displaced 1.2 million people (Ngaruko and Nkurunziza 2000).

5.3 Research Design and Data

We conducted our series of experiments in March-April 2009, using a sample of 300 household heads from 35 communities in rural Burundi. These communities and households were drawn from a set of 100 communities that were earlier visited in 1998 and 2007 to collect survey data. The 2007 survey included variables on local history of conflict, social capital and a range of household and community variables (including proxies for social, risk and time preferences).[56] We randomly selected 35 communities and revisited all respondents of the earlier survey, inviting them to participate in a series of experiments. Of the 35 communities, 24 experienced violence in the period 1993-2003, and 11 were not exposed to violence.[57]

The key variable of interest is our community measure of conflict victimization measured as the share of war-related deaths (1993-2003) to the total population, resulting from confrontations between the army and rebels or one-sided violence by either group.[58] Such attacks resulted in the death of up to 15% of the communities' inhabitants. We also create a household level victimization index, based on the experience of death, theft, ambush, forced labor and torture of household members. The index is additive and, given its arbitrary scale, normalized to have mean 0 and standard deviation 1 (Table 1, Panel B). A detailed description of our variables and their sources is provided in an on-line appendix.

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[56] Burundi Priority Survey 2007. The data collection was a collaborative effort between the Institut de Statistiques et d'Etudes Economiques du Burundi (ISTEEBU), Antwerp University, Vrije Universiteit Brussel and Wageningen University, and was implemented under the flag of MICROCON – an EU funded project focusing on household analysis of violent conflict in various regions of the world. A first wave of data was collected by the World Bank halfway through the war (in 1998) in 391 communities. For a total of 1400 households in 100 communities we have panel data (1998-2007) regarding many important household characteristics as well as information on the development and consequences of armed conflict. The surveys and their sampling designs are described in an on-line appendix.

[57] The end of the conflict was officially sealed in 2005 when a new Constitution, largely based on the Arusha Peace Agreement, was approved by referendum. Yet, the intensity of conflict in the last 2 years was negligible as compared to the intensity in the first 11 years. When constructing our variables on conflict and victimization, we focus on the incidents in the 1993-2003 period.

[58] Reports of violence are based on retrospective accounts by villagers (collected in village focus group meetings) and cross checked with published reports from ACLED database of PRIO (Raleigh *et al.* 2010).

Table 1 Descriptive Statistics

Variable	obs household level	obs community level	mean	std. dev.	min	max
Panel A: Preferences						
Social preferences (0-100, 2009)	286	35	27.32	27.22	0	100
Risk preferences Gains (2009)	220	35	1.87	1.31	0	3
Risk preferences Losses (2009)	233	35	2.31	1.18	0	3
Discount rate (%, 2009)	273	35	40.16	41.43	0	100
Panel B: Conflict variables						
Relative number of dead in attacks (%, 1993-2003) (3)		35	1.99	4.09	0	15.63
Attack (1993-1998) (8)		35	0.69	0.47	0	1
Individual victimization index (1993-2003) (2)	287	35	0	1	-0.78	5.03
Physical attack (1993-2003) (2)	287	35	0.32	0.47	0	1
Non-physical attack (1993-2003) (2)	287	35	0.45	0.50	0	1
Panel C: Household variables						
Household head is literate (1998) (1)	285	35	0.40	0.49	0	1
Household head is literate (2009) (4)	287	35	1.60	0.49	1	2
Household head age (1998) (1)	283	35	42.28	15.30	16	99
Household head age (2009) (4)	286	35	45.96	15.11	18	90
Household head is male (1998) (1)	286	35	0.62	0.49	0	1
Household head is male (2009) (4)	287	35	0.79	0.41	0	1
Household head ethnic origin (2009) (4)	287	35	0.22	0.41	0	1
Total expenditures (1998) (1)	287	35	8.56	0.70	5.17	10.7
Total land holdings per capita (ha^2, 2007) (2)	281	35	5.54	5.13	0.10	34.25
Household head is livestock farmer (1993) (4)	287	35	0.41	0.49	0	1
Perceived trust level (1998) (2)	287	35	4.64	2.17	1	10
Social capital index (2007) (2)	282	35	0.02	1.04	-0.47	4.49
Investments farm buildings (FBU, 1998) (1)	241	35	5.57	1.20	1.08	8.52
Investments farm buildings (FBU, 2007) (2)	287	35	229.35	1022.67	0	12155.87
Share of cash crops in total production (1998) (1)	276	35	0.22	0.30	0	1
Share of cash crops in total production (2007) (2)	280	35	0.06	0.14	0	1
Severe draught (% yes, 2007-2009) (4)	277	35	0.33	0.47	0	1
Access rain (% yes 2007-2009) (4)	231	35	0.62	0.49	0	1
Manioc disease (% yes 2007-2009) (9)	287	35	0.40	0.49	0	1
Upcoming ceremony (% yes, 2009) (4)	231	35	0.39	0.49	0	1
Panel D: Community variables						
Land Gini coefficient (2007) (2)		35	0.29	0.20	0	0.54
Distance to market (2007) (3)		35	2.86	0.72	1.38	4.22
Conflict over land (% yes, 2007) (2)		35	0.25	0.15	0	0.6
Ethnic homogeneity (1993) (5)		31	82.87	16.89	30	99
Ethnic homogeneity (2009) (5)		35	86.68	15.57	30	100
Votes for Ndadaye in 1993 (%) (7)		34	64.51	17.68	9.94	93.51
Socio-economic homogeneity (1998) (2)		35	1.66	1.06	1	5
Socio-economic homogeneity (2007) (2)		35	1.57	0.95	1	5
Population density (log, 1990) (5)		35	5.43	0.51	4.20	6.11
Population density (log, 2008) (6)		35	5.76	0.47	4.50	6.49
Per capita total expenditure (log, 2007) (2)		35	9.25	0.47	8.09	10.40
Distance to Bujumbura (km, log)		35	4.49	0.39	3.68	5.12
Altitude (m, log) (5)		35	7.41	0.10	7.14	7.70

Data sources: (1) Burundi Priority Household Survey 1998, (2) Burundi Priority Household Survey 2007, (3) Burundi Community Survey 2007, (4) Burundi Experiments Exit Survey 2009, (5) Monographies Communales Burundi. Ministre de la Planification du Développement et de la Reconstruction Nationale, Bujumbura, 2006 (6) Recensement National de la Population et de l'Habitat 2008, (7) Sinunguruza T. 2001. Les Elections au Burundi. Tout Savoir et Tirer des Leçons de Juin, (8) Clionadh Raleigh *et al.* 2010, (9) FAO Valuation Des Recoltes, Des Approvisionnements Alimentaires et de la Situation Nutritionelle, 2006.

5.4 Experimental Games

For our experiments we adapted well-established experimental game protocols to implement social orientation, risk and time preference experiments. These experiments are discussed in more detail in an online appendix—to economize on space we only highlight key features here.

To measure social preferences we used a modified version of the social value orientation experiment devised by Liebrand (1984). Subjects were anonymously matched

to another participant from their community (their 'partner'), and made six choices between two own-other payoff combinations; A and B. These combinations differed not only in the proposed division of the sums of money but also in the total sums to be allocated. We conducted the experiment offering non-negative payments only.[59] A subject's social orientation is reflected by the ratio of the total amounts of money allocated to the partner and to himself. This ranges from totally selfish (if the subject always chose the allocation with the highest payment for himself) to totally altruistic (if he always chose the option with the highest payment for his partner). We rescaled the results such that social orientation is measured on a scale from 0 to 100, with 0 denoting purely selfish preferences, 100 identifying the subject to be maximizing his partner's payoff, and 50 identifying the social optimum (i.e., choosing allocations to maximize joint payoffs). On average we find a value of 27, indicating that most subjects are fairly individualistic (Table 1, Panel A).[60]

Risk preferences were measured using a game based on Harbaugh *et al.* (2002) where subjects could choose between playing a simple gamble and receiving a specific amount of money with certainty. Six choice cards were presented, each of which offered them the choice between A: receiving (or losing) an amount of money with certainty, and B: participating in a game where they could either gain (lose) 2000 FBU[61] with probability 0.3, or gain (lose) nothing with probability 0.7.[62] Hence, the expected absolute value of the gamble was always the same, and the amount of money received with certainty varied across choices (lower, equal to, and higher than the expected value of the gamble). The point at which a subject switches from the risky to the safe alternative allows us to determine the respondent's degree of risk aversion.

To measure time preferences, we presented subjects with a set of nine simple pairwise choices between two options: receiving an amount of money at some date in the

[59] Because we only offered choices with non-negative payoffs for each participant, our experiment does not confound altruism and competitive preferences – unlike the standard Liebrand (1984) design. Our design does conflate altruism and a preference for efficiency, though, in three of the six choices subjects are asked to make. Moreover, even though this game is non-strategic (it is very similar to a dictator game), subjects may base their decisions on their expectations regarding the decisions made by the anonymous village members they are matched with – as pointed out by a referee. Unfortunately, we did not inquire into the expectations with respect to the play of others, so we cannot exclude that differences in beliefs about the play of others may also drive part of the result.

[60] This experiment measures the weight placed on another subject's welfare, but it may be an imperfect predictor of cooperation in the field. For example, patience may matter too – pro-social behavior in a fishery requires foregoing higher returns today to increase the community's future returns (e.g., Fehr and Leibbrandt 2008).

[61] USD 1 = 1,210 FBU (20 May 2009), which is roughly equal to a full day's wage rate for unskilled labor.

[62] Our design differs slightly from Harbaugh *et al.* (2002) as we specifically use information from questions where the certainty equivalent is different from the expected value of the gamble.

near future, and receiving a larger sum at a later time. The two options to choose between were A: receive 1000 FBU the following day, and B: receive 1000(1 □□ *d*) FBU in two weeks plus one day, with *d* equal to 0.00, 0.01, 0.02, 0.05, 0.10, 0.40, 0.70, and 1.00. Subsequently, at the highest interest rate subjects earned an additional 1000 FBU by waiting two weeks. In the experiment subjects were asked to identify the smallest *d* for which they preferred B to A) – the earlier people switch from A to B the more patient they are. The fact that there has been no war-related violence in our study area for several years combined with the relatively short delay (two weeks only) implies we believe that our estimate of time preferences is not confounded by risk due to anticipated violence – people are unlikely to choose the near immediate pay-off because they fear being killed by violence in the next two weeks.

After the series of experiments, the average participant walked away with a sum that is the equivalent of five days of wages for unskilled labor in Burundi – a salient incentive.

5.5 Identification Strategy, Exogeneity and Selection Bias

The key assumption underlying our empirical approach is that violence across and within communities was exogenous with respect to individual preferences. However, any co-variation of preferences and exposure to violence may be due to (i) non-random (or targeted) violence or (ii) non-random attrition in the sample. We present evidence to suggest that violence was not very targeted, and that attrition bias is not likely to affect our results.

Regarding the first, there is ample evidence highlighting the brutal and indiscriminate nature of the Burundi conflict (Human Rights Watch 1998, Uvin 1999, Krueger and Krueger 2007). To statistically examine whether 'selection into violence' biases our results we follow an approach taken by Bellows and Miguel (2009), and first assess whether violence experienced by communities is associated with lagged community characteristics; see Table 2. Violence is measured as whether or not the village was attacked during 1993-2003 (column 1), and as the number of people dying in attacks in that period, expressed as a share of the total population (column 2). Our specification implicitly tests for two theories of violence: greed and grievance (see Collier 2003). The first is 'economic profit' – exploiting the opportunity to use violence for stealing the assets of others, including livestock. The second is that violence was driven by grievance, or perhaps ethnic considerations, as measured by ethnic homogeneity,

votes for the assassinated Hutu president Ndadaye, and socio-economic homogeneity. In these two columns we find no support for the hypothesis that victims have been selected because of either motivation. The only two (exogenous) variables correlated with the share of villagers killed are geographical variables – distance to Bujumbura, the nation's capital, and remoteness (proxied by altitude) – consistent with the pattern of violence in Burundi as sketched by Uvin (1999, see section 2). The F-tests on the joint significance of all non-geographical variables in columns 1 and 2 yield p-values greater than 0.30.

These analyses are based on a relatively small number of observations – because of missing variables only 30 villages are used in the regressions. Hence we potentially face the risk of incorrectly failing to reject the null of non-targeted violence. We tested the probability of a type II error in column 2 of Table 2. When focusing on the explanatory power of the non-geographical variables, we find that this probability is equal to 0.38 (or the power is 0.62).[63] Fortunately, we can raise the power of these regressions by applying a similar analysis to the larger dataset of 100 villages. The results are reported in column 3, and the probability of a type II error is now less than 0.14 (power is 0.86), which is below the 0.20 (above 0.80) threshold routinely assumed in empirical analysis.

As the 35 villages in which we ran our experiments were randomly drawn from the set of 100 villages we used in our 2007 survey and the regression results in columns 2 and 3 are qualitatively identical, we conclude that the probability of incorrectly maintaining the null of non-targeted violence is acceptably small. Hence, our data provide support for the anecdotal evidence that violence in the Burundi war was not very targeted. Still, in what follows we will use altitude and distance to Bujumbura as instruments for violence to attenuate any remaining endogeneity concerns and measurement error.

[63] We used G*Power3 software to conduct the power tests; see Faul *et al.* (2007).

Table 2 Exogeneity

Dependent variable:	(1) Attack during 1993-2003	(2) Percentage dead in attacks 1993-2003	(3) Percentage dead in attacks 1993-2003	(4) Physical attack on household member	(5) Non-physical attack on household member	(6) Present village in 1998 and 2009	(7) Present in village in 1993 and 1998
	Probit	OLS	OLS	Probit	Probit	Probit	Probit
Community variables							
Percentage literate household heads (in 1998)	-0.0282	-0.0561	-0.002				
	(0.0208)	(0.041)	(0.001)				
Average age household head (in 1998)	-0.0273	0.0625	-0.001				
	(0.0520)	(0.133)	(0.005)				
Percent male (in 1998)	-0.00170	-0.0587	0.003				
	(0.0248)	(0.0634)	(0.002)				
Percentage livestock farmers (in 1993)	0.0252	0.0524					
	(0.0218)	(0.0510)					
Density in 1990 (log)	-2.119	0.0317	0.081				
	(1.443)	(1.970)	(0.123)				
Ethnic homogeneity (in 1993)	0.0155	0.0200					
	(0.0235)	(0.0557)					
Socio-economic homogeneity (in 1998)	-0.295	-0.934	0.051				
	(0.392)	(0.872)	(0.033)				
Percentage of votes for Ndadaye (in 1993)	0.0227	0.0457					
	(0.0283)	(0.0575)					
Distance to Bujumbura (km, log)	-2.561*	-4.884^{+}	0.109				
	(1.536)	(3.039)	(0.189)				
Altitude (m, log)	-8.112^{+}	-16.09*	-0.748**				
	(5.420)	(8.252)	(0.303)				
Relative number of dead in attacks 1993-2003						0.0255	
						(0.0345)	
Number of attacks 1993-1998							-0.001
							(0.002)
Household variables							
Respondent is literate (in 1998)				-0.114	0.224	0.312	0.023
				(0.251)	(0.234)	(0.208)	(0.137)
Respondent age (in 1998)				-0.005	0.006	-0.002	0.012***
				(0.008)	(0.008)	(0.006)	(0.003)
Respondent is male (in 1998)				0.447	-0.248	-0.794***	-0.054
				(0.303)	(0.278)	(0.225)	(0.083)
Livestock farmer in 1993				0.228	0.0958	0.0660	-0.0003
				(0.230)	(0.214)	(0.226)	(0.084)
Respondent ethnicity				-0.498	-0.473		
				(0.310)	(0.296)		
Total expenditures (in 1998)				0.361	0.345	-0.193	
				(0.222)	(0.217)	(0.170)	
Perceived trust level (in 1998)				-0.001	0.028		
				(0.051)	(0.046)		
Share of cash crops in total production (in 1998)				0.176	-0.499	-0.097	
				(0.484)	(0.434)	(0.367)	
Expenditures farm improvement (in 1998)				0.0949	0.237	0.030	
				(0.171)	(0.161)	(0.098)	
Constant	83.37	142.5*	4.784*	-10.06	-4.199*	2.295	-0.993***
	(52.05)	(74.01)	(2.473)	(0.00)	(1.951)	(1.448)	(0.215)
FE	no	no	yes (province level)	yes (village level)	yes (village level)	yes (strate level)	yes (province level)
N	30	30	94	198	219	279	1766
adj. R^2	0.33	0.19	0.27	0.19	0.16	0.10	0.11

Standard errors in parentheses, $^{+}$ $p < 0.15$, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Included fixed effects are at lowest level possible. Column (1) and (2) use 30 observations instead of 35 due to missing 1993 ethnicity data. Column (3) uses 94 observations and not 100 due to missing data, dependent variable dead in attacks relative to number of households and not population due to missing population data. Column (1)–(6) use mainly BPHS and BSC 2009 data. Ethnic and 1993 livestock ownership data are from BEES 2009. Dependent variable in column (6) is a dummy, 1 if respondent was interviewed in both 1998 and 2007, zero else. Literacy is measured as years of education. Column (7) uses the ESD-SR 2002 data to assess attrition between 1993 and 1998. Dependent variable is a dummy, 1 if respondent was present in village in 1993 and 1998, zero else. Literacy, age and gender are measured in 2002; the dummy for livestock farmer in 1993 is based on recall in 2002, the number of attacks between 1993 and 1998 were drawn from the ACLED database and matched to the UNFPA data at the commune level. The proportion of people absent from the sample was 16%. Regression uses province fixed effects. Including village fixed effects reduces the number of observations but does not change the results.

In columns 4 and 5 of Table 2 we repeat the analysis of columns 1 and 2, but now at the household level. We have two proxies for household exposure to violence: (i) whether a physical attack happened to a household member, and (ii) whether household members were exposed to non-physical violence (including theft, forced labor, etc.). Neither variable is correlated with a range of household characteristics (income, gender, literacy, etc.), which is in line with anecdotal accounts of violence in Burundi (Human Rights Watch 1998, Krueger and Krueger 2007). The same applies to 1998 survey-based data related to the preferences we are interested in: (i) perceived trust levels (a measure of social capital as a proxy for social preferences), (ii) crop choice (proxy for risk preferences), and (iii) expenditures on farm improvements (proxy for time preferences– see section 6.2).[64] The regression models in columns (4) and (5) are adequately powered as the probability of a Type II error is less than 0.01 in both instances. While we do not find evidence that (non-)trusting individuals have a higher probability of falling victim to violence,[65] we obviously cannot rule out that certain non-cooperative individuals were targeted in the violence (note we lack pre-war evidence of the villagers' experimental play).

In column (6) and (7) we analyze potential non-random attrition between 1993 and 2009.[66] In column (6) we analyze whether a non-random subset of the 1998 population was absent when we invited them for the experiments in 2009. This is important as households that migrate in anticipation of violence may have different preferences than those households which stayed behind. We follow the approach taken by Fitzgerald *et al.* (1998) and estimate a probit model of 1998-2009 attrition on a range of 1998 household characteristics. All but one variable enter non-significantly: households headed by a male were less likely to be present in the 2009 sample (we return to this below). But if our 1998 data were drawn from a non-random subset of the 1993 population this may have consequences for the external validity of our findings. We use another dataset to explore the magnitude of non-random attrition between 1993 and 1998. In 2002, UNFPA-Burundi undertook a nationwide Demographic and Health

[64] We lack pre-war data for these variables. Instead we rely on early war data from our first survey wave, conducted in 1998.

[65] We find additional support for this claim when testing whether the vectors of individually insignificant variables in Table 2, are jointly insignificant. The p-values obtained (using F-tests for the OLS models and Likelihood Ratio tests for the probit models) are all larger than 0.10 – and many are greater than 0.30. These results are consistent with the results of the power tests presented, and indicate we cannot reject the null hypothesis (i.e. the vector of individually insignificant variables is also jointly insignificant).

[66] Attrition between our first survey in 1998 and the second wave in 2007 is below 14%, and below 1% between this second wave and our experiments in 2009. The 1998-2007 attrition level is modest given the length of the period and the circumstances of civil warfare.

Survey (ESD-SR). In the survey respondents were (amongst others) asked to list their entire migration history, starting in January 1993 (before the start of the civil war). This data hence allows us to explore sources of attrition (which was below 16%) over the 1993-1998 period. In column (7) of Table 2 we present the results of an attrition model in which we include a range of household controls and fixed effects. We find that only age appeared to matter for migration decisions for this sample; older people were more likely to have stayed behind than younger heads of households – and significantly so at the 1% level. We return to these issues when testing the robustness of our results in section 6.2.

5.6 Conflict, Behavior and Preferences

The descriptive statistics in Panel A of Table 1 suggest considerable heterogeneity in experimental behavior. In this section we investigate whether experimental behavior varies with exposure to conflict, and regress decisions made in the social, risk and time preference experiments on our measures of violence. We include several household and community characteristics as controls, and also include regional fixed effects.[67] We focus primarily on the relationship between *community* exposure to violence and *individual* preferences. The reason is that even if only a sub-set of individuals directly experienced acts of violence, the consequences may be felt throughout the community (Yehuda 2002). Our main measure of violence is the total number of dead during 1993-2003 relative to population size in the community. However, in some models we also include an index of individual exposure to violence. Throughout we cluster standard errors at the community level to account for intra-community correlation.

5.6.1 Conflict and Behavior

We explore the relationship between conflict and behavior in the experimental games in Tables 3, 4 and 5.[68] In Table 3 we report the results for our measure for pro-social behavior. Across all OLS specifications we record a statistically significant and positive correlation between altruistic behavior and conflict intensity at the community level (column 1) as well as at the household level (columns 2-5). This is in line with survey

[67] See the on-line appendix for the exact variable definitions.
[68] All regressions in Table (3)-(5) use OLS or 2SLS. As our dependent variables take only a limited number of values, we also estimate the models in columns (1)-(6) using an ordered probit specification. The results are qualitatively identical and available on request.

work by Bellows and Miguel (2009), who report an increase in social cohesion and political participation in response to violence.

Table 3 Conflict and Social Preferences

	(1) OLS	(2) OLS	(3) OLS	(4) OLS	(5) OLS	(6) 2SLS	(7) 2SLS
Percentage dead in attacks	1.073**	0.875*	1.688***	1.686***		2.892***	0.486***
	(0.489)	(0.460)	(0.464)	(0.523)		(0.958)	
Individual victimization index					2.940*		
					(1.745)		
Household variables							
Respondent is literate			9.550***	9.430***	6.645*	10.39***	0.190***
			(3.521)	(3.450)	(3.945)	(3.418)	
Respondent age			-0.207*	-0.201*	-0.330**	-0.202*	-0.111*
			(0.108)	(0.109)	(0.158)	(0.110)	
Respondent is male			4.204	4.662	10.97**	5.615*	0.099*
			(3.373)	(3.436)	(4.234)	(3.174)	
Total land holdings per capita			0.807***	0.730***	0.590	0.827***	0.152***
			(0.253)	(0.250)	(0.380)	(0.263)	
Community variables							
Land Gini coefficient			-16.47	-13.60		-20.66*	-0.150*
			(10.01)	(9.376)		(12.12)	
Distance to market			-6.012**	-6.020**		-7.633***	-0.198***
			(2.583)	(2.542)		(2.851)	
Conflict over land			-29.99**	-26.79**		-36.77***	-0.207***
			(11.65)	(10.74)		(13.32)	
Ethnic homogeneity			0.204*	0.232*		0.148	0.080
			(0.118)	(0.124)		(0.126)	
Socio-economic homogeneity			-2.387	-2.245		-2.244	-0.072
			(1.948)	(2.154)		(2.077)	
Population density			6.712	5.770		8.511	0.136*
			(4.173)	(5.395)		(6.074)	
Per capita total expenditure			1.995	2.610		0.947	0.016
			(3.046)	(3.845)		(4.483)	
Constant	23.688***	25.20***	-23.68	-24.86	4.33	-12.17	
	(2.569)	(2.296)	(42.45)	(42.63)	(34.16)	(44.99)	
FE	no	no	no	yes (stratum level)	yes (village level)	yes (stratum level)	yes (stratum level)
1998 household controls	no	no	no	no	yes	no	No
N	35	286	278	278	225	278	278
adj. R^2	0.099	0.018	0.144	0.140	0.212	0.166	0.166
First stage instruments							
Distance to Bujumbura (log)						-6.687***	
						(2.489)	
Altitude (log)						-21.988***	
						(9.211)	
Hansen J, p-value						0.51	
Partial F						7.30	

Dependent variable: Degree of altruism scale 0-100. Robust standard errors in parentheses clustered at community level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Sampling weights not included, results are qualitatively identical. Column (1): dependent variable is community average. Column (5): household 1998 controls included. Column (6): excluded instruments of first stage reported only. The beta coefficients of column (6) are reported in column (7). Estimations in column (7) do not include clustered standard errors.

Our results are robust to the inclusion of ethnicity fixed effects, as well as a series of other household and community controls and regional fixed effects (Table 3, columns 3 and 4 respectively).[69] For example, behavior in the experiment is more pro-social when

[69] Note that the coefficient on violence increases if controls are included. Following the reasoning of Bellows and Miguel (2009) this suggests that it is unlikely that omitted variable bias explains away the conflict effect (see also Altonji *et al.* 2005).

respondents are literate and own more land. We find weak evidence that social behavior is declining in age. Turning to the community level controls, social behavior is positively associated with ethnic homogeneity, and negatively associated with distance to the market and ongoing conflicts within the community over land. The effect of market integration is consistent with Henrich *et al.* (2001), and the land conflict and ethnicity outcomes make intuitive sense. We find that population density, average community income and land distribution are not correlated with social behavior.

Next, we employ a household level conflict variable, rather than a community-based measure (Table 3, column 5). We again find a positive correlation with social behavior, indicating that both individual and community level violence are associated with altruistic behavior. Note that here, where we use village level fixed effects, we essentially compare victims and non-victims within one village. Compared to the results presented in columns 1-4 we now also find that male respondents are slightly more inclined to be generous to (non-kin) fellow community members – all else equal.

OLS regression results for risk preferences are presented in Table 4. Throughout we observe a positive correlation between community level conflict intensity and risk seeking.[70] This result is obtained when focusing on the community as well as on the individual level (column 1 vs column 2), and is robust to including common controls and fixed effects (columns 3-4). In column (6) the dependent variable measures preferences over losses. One key insight from Kahneman and Tversky's (1979) seminal paper is that people value changes in gains and losses differently. Their work resonates in ours because we find that conflict induces risk seeking over gains (column 4) while it does not affect attitudes towards losses (column 6). Though work by economists on shocks and risk preferences has so far been limited, this result suggests it may be a viable area for future research. It is striking to observe that risk preferences are not (robustly) associated with any of the household or community-level controls.

[70] Andreoni and Sprenger (2010) document that separate utility functions govern the assessment of certain and uncertain payoffs. Their results indicate that experiments like ours conflate the curvature of the utility function and a so-called "certainty effect" as they find that subjects exhibit a strong preference for payoffs that are certain. We admit that we cannot distinguish between these two possibilities, but the conclusion remains the same—exposure to conflict makes people more prone to take risks as their evaluation of the payoffs is affected. Note that our measure of time preference is not affected as respondents are asked to choose between two equally (un)certain payoffs – delayed payment by a trusted organization (to be discussed next) – in which case expected utility theory applies according to Andreoni and Sprenger (2010).

Table 4 Conflict and Risk Preferences

	(1) gains OLS	(2) gains OLS	(3) gains OLS	(4) gains OLS	(5) gains OLS	(6) losses OLS	(7) gains 2SLS	(8) gains 2SLS
Percentage dead in attacks	0.078***	0.0651**	0.0527**	0.0634**		0.0196	0.0729*	0.258
	(0.024)	(0.0246)	(0.0246)	(0.0263)		(0.0204)	(0.0376)	
Individual victimization index					0.165+			
					(0.114)			
Household variable								
Respondent literate			-0.225	-0.227	-0.005	-0.151	-0.214	-0.083
			(0.179)	(0.183)	(0.274)	(0.182)	(0.170)	
Respondent age			0.005	0.006	0.001	-0.008	0.006	0.065
			(0.006)	(0.006)	(0.009)	(0.006)	(0.005)	
Respondent is male			-0.217	-0.186	-0.279	0.0537	-0.176	-0.067
			(0.188)	(0.188)	(0.256)	(0.141)	(0.165)	
Total land holdings per capita			-0.025	-0.0341*	-0.028	-0.010	-0.034*	-0.130*
			(0.018)	(0.018)	(0.025)	(0.019)	(0.017)	
Community variable								
Land Gini coefficient			-1.129	-1.026		0.0393	-1.092	-0.170*
			(0.745)	(0.725)		(0.731)	(0.740)	
Distance to market			0.124	0.137		0.122	0.125	0.070
			(0.129)	(0.122)		(0.141)	(0.129)	
Conflict over land			0.0205	0.254		0.510	0.167	0.019
			(0.960)	(0.918)		(0.877)	(1.045)	
Ethnic homogeneity			-0.0008	-0.0001		0.00161	-0.001	0.009
			(0.0064)	(0.0067)		(0.00877)	(0.006)	
Socio-economic homogeneity			-0.00605	0.0314		-0.0381	0.0313	0.022
			(0.115)	(0.117)		(0.136)	(0.111)	
Population density			0.326	0.418		0.430	0.441	0.149
			(0.225)	(0.332)		(0.292)	(0.333)	
Per capita total expenditure			-0.249*	-0.274		-0.0302	-0.290	-0.106
			(0.143)	(0.170)		(0.239)	(0.177)	
Constant	1.680***	1.716***	2.237	2.022	4.902**	0.00443	2.149	
	(0.131)	(0.145)	(1.986)	(2.116)	(1.869)	(2.607)	(2.045)	
FE	no	no	no	yes (stratum level)	yes (village level)	yes (stratum level)	yes (stratum level)	yes (stratum level)
1998 household controls	no	no	no	no	yes	no	no	No
N	35	220	213	213	175	228	213	213
adj. R^2	0.17	0.05	0.18	0.20	0.38	0.09	0.20	0.20
First stage instruments								
Distance to Bujumbura(log)							-5.499*	
							(2.525)	
Altitude (log)							-19.221**	
							(8.474)	
Hansen J, p-value							0.10	
Partial F							6.50	

Dependent variable ranges 0 (risk averse) - 3 (risk loving). Robust standard errors in parentheses clustered at community level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Sampling weights not included, results are qualitatively identical. Column (1): dependent variable is community average. Column (5): household 1998 controls included. Column (7): excluded instruments of first stage reported only. Column (8): beta coefficients of column (7). Estimations in column (8) do not include clustered standard errors.

Table 5 Conflict and Time Preferences

	(1) OLS	(2) OLS	(3) OLS	(4) OLS	(5) OLS	(6) 2SLS	(7) 2SLS
Percentage dead in attacks	0.543	0.666	1.197*	1.188**		2.337**	0.265**
	(0.519)	(0.467)	(0.624)	(0.575)		(1.058)	
Individual victimization index					2.826		
					(3.294)		
Household variables							
Respondent literate			-5.540	-5.051	-12.51*	-4.136	-0.050
			(5.594)	(5.610)	(6.463)	(5.405)	
Respondent age			-0.162	-0.174	-0.254	-0.180	-0.065
			(0.189)	(0.186)	(0.267)	(0.178)	
Respondent is male			-0.168	-1.225	-11.42	-0.382	-0.004
			(4.921)	(4.747)	(7.483)	(4.470)	
Total land holdings per capita			0.154	0.331	-0.516	0.423	0.053
			(0.442)	(0.439)	(0.633)	(0.408)	
Community variables							
Land Gini coefficient			-32.65	-39.88*		-47.18**	-0.230**
			(23.40)	(22.50)		(21.50)	
Distance to market			3.013	2.873		1.318	0.023
			(3.704)	(4.211)		(4.769)	
Conflict over land			-14.49	-22.92		-33.35	-0.124
			(24.33)	(22.57)		(22.80)	
Ethnic homogeneity			-0.432*	-0.496**		-0.582***	-0.207***
			(0.224)	(0.212)		(0.195)	
Socio-economic homogeneity			7.287	6.839		6.711	0.144
			(4.516)	(4.928)		(4.881)	
Population density			7.489	9.804		12.48	0.132
			(6.715)	(7.421)		(7.999)	
Per capita total expenditure			8.874	7.632		5.825	0.066
			(6.479)	(5.267)		(5.564)	
Constant	39.38***	38.49***	-44.73	-45.15	74.50	-29.89	
	(4.377)	(3.950)	(72.99)	(70.65)	(64.10)	(71.18)	
FE	no	no	no	yes (stratum level)	yes (village level)	yes (stratum level)	yes (stratum level)
1998 household controls	no	no	no	no	yes	no	no
N	35	273	266	266	213	266	266
adj. R^2	0.01	0.01	0.15	0.16	0.40	0.15	0.15
First stage instruments							
Distance to Bujumbura(log)						-6.605**	
						(2.519)	
Altitude (log)						-22.488**	
						(9.388)	
Hansen J, p-value						0.97	
Partial F						7.25	

Dependent variable: discount rate. Robust standard errors in parentheses clustered at community level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Column (1): dependent variable is community average. Sampling weights not included, results are qualitatively identical. Column (5): household 1998 controls included. Column (6): excluded instruments of first stage reported only. Column (7) contains beta coefficients of column (6). Estimations in column (7) do not include clustered standard errors.

Finally, in Table 5 (columns 1-5) we summarize the impact of conflict on intertemporal choices. The models suggest that exposure to conflict causes an increase in discount rates. While the evidence seems more mixed than for the other experiments, violence appears to make people less patient. It is interesting to note that time preferences are not associated with any of the household-level variables (including individual exposure to violence, when controlling for community attacks). In contrast, several of the community variables enter significantly. Communities with higher levels of ethnic homogeneity and/or more unequal land holdings display lower discount rates.

Tables 3-5 thus suggest that exposure to conflict is positively correlated with altruistic behavior, risk seeking behavior, and impatience.[71] But correlation does not equal causation. To attenuate potential endogeneity and omitted variables concerns in Tables 3-5 we re-run our regressions using 2SLS for each of the three types of preferences, and use distance to Bujumbura and altitude as instruments for our conflict measures.[72] Results are reported in columns 6 and 7 in Tables 3 and 5, and columns 7 and 8 in Table 4. Predicted violence is significant at the 5% level (or better) in the social preferences and time preferences regressions (Tables 3 and 5), and at the 10% level in the analysis of risk preferences (Table 4). The values of the coefficients are larger than when using OLS.[73] Also, our identifying assumption is that the distance and altitude variables only affect the distribution of violence, and do not impact on preferences otherwise. This assumption is contestable. Distance to the capital could proxy for distance to markets, in which case preferences likely depend on distances to Bujumbura (see Henrich *et al.* 2001). However, direct effects of proximity to the capital are likely minimal as most farmers operate at subsistence level, selling goods at local markets only (via which goods make their way to the capital). Such local markets are nearby in all communities in our sample (never over a 2 hour walk away), reducing concerns about a correlation between geography and preferences. Even export crops, such as coffee, are usually sold to local intermediates or washing stations. Econometrically, this is confirmed by the test statistics in the bottom panel of Tables 3-5 which indicate our excluded instruments are correlated with the conflict variable (see the high partial F values) and correctly excluded from our second stage regression (the p-value of the Hansen J statistic is well above 0.10).

Finally, to assess the magnitude of these effects, we report the coefficients of all significant variables in each of the Tables 3-5 after having standardized the explanatory

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[71] The inter-relation between these behaviors is not the focus of this paper. However, when using our full sample of 288 observations, we find altruism and patience are negatively correlated ($r = -0.12$, $p<0.04$). Evidence provided by the scarce literature on the correlation between pro-sociality and patience using observed behavior suggest that the correlation is weak. For example, Fehr and Leibbrandt (2008) find no correlation between patience and social preferences in their study of Brazilian fishermen. This is in line with our findings: we also find no correlation when restricting the sample to non-conflict villages ($r=-0.06$, $p<0.56$).

[72] We have also tried using village level violence as an instrument for household level victimization. These results are qualitatively identical and available on request. However, psychological literature suggests that the effect of violence on behavior does not depend on *direct* experience with violence (Yehuda 2002). If true, the exclusion restriction of this approach would be invalid.

[73] The finding that the 2SLS point estimate is somewhat larger than the OLS estimate is common in cross-country studies, usually attributed to measurement error (biasing the OLS estimates towards zero). Alternatively, IV results may produce overestimates of the true effect if included instruments are positively correlated with omitted variables that have the same sign as the endogenous conflict variables.

variables such that they have a mean of zero and a standard deviation of one. These so-called beta coefficients are reported in column (7) in Tables 3 and 5, and in column (8) of Table 4. Clearly the impact of conflict dominates all other impacts. For example, the beta coefficient of violence in the social preference regression is 0.49, more than twice as large as the beta coefficients of the other variables in the same column.

5.6.2 Robustness

We now report the outcomes of a series of robustness analyses. First, we explore whether differences in experimental play translate into systematic differences in behavior in "real life." We estimate several models and summarize our findings in Tables 6 and 7. In Table 6 we replace our experimental variables with survey-based social-, risk- and time preference proxies. In column (1) the dependent variable is a social capital index in the spirit of Narayan and Pritchett (1999). This index comprises a weighted (and normalized) scale of respondents' participation in community organizations and the degree of membership in 2007. Consistent with our experimental variables we find a positive correlation with exposure to violence. In addition, we find that many of the significant explanatory variables in this regression also showed up significantly in the regressions in Table 3. Literacy and per capita land holdings positively contribute to our measure of social capital, while conflict over land reduces it. Next, if conflict alters risk preferences we would expect an effect on investments and asset portfolio choice—skewing resources to more risky and profitable activities such as the production of cash crops (see Dercon 1996). In column (2) we find that households in regions exposed to greater levels of violence cultivate relatively more cash crops. (Recall that earlier we demonstrated that growing cash crops in 1998 did not invite subsequent conflict – see Table 2 columns 4-6). Again this result is consistent with the experimental evidence. Some of the other control variables show up significantly too (in contrast to Table 4). Portfolio choice of crops is more risky as population density increases, conflict over land decreases, and the distribution of land holdings is more equal. In addition, male respondents and those with larger land holdings tend to invest more in risky cash crops. Lastly, in column (3) we use a measure of long-term investments – the share of expenditures on farm improvements in 2007 – as our dependent variable. The assumption is that a greater share of durable investments reflects greater patience. Again we find our experimental results reflected in the survey data: households affected by greater levels of conflict invest less in their farms.

Next, to further assess the robustness of our findings we return to the potential bias introduced by non-random attrition into our sample. We follow the approach of Bellows and Miguel (2009) and probe the robustness of our findings for a sub-sample of respondents. This enables us to assess the possibility that our findings are due to changes in the composition of the population (rather than changing behavior of individual respondents). Specifically, Table 2, column (6) showed that men may be underrepresented in our post-conflict sample (some 40% of households were female headed in 2009). Hence, correlation between gender and preferences may bias our estimates in Tables 3-5. In columns (4)-(6) of Table 6 we re-estimate our 2SLS models on a restricted sample of *same gender* respondents present in both 1998 and 2009 – households where the gender of the household head did not change. We find that for social and risk preferences the results go through as before, with only minimal differences in the coefficients.

As a final robustness test we assess whether other types of shocks such as natural disasters (draught and excess rainfall), plant diseases and expenses on wedding ceremonies etc. affect behavior in a similar fashion as conflict. We inserted, one by one, these non-conflict shocks as explanatory variables in the regression models in column 3 of Tables 3-5 – while omitting the conflict measure. In Table 7 we present the coefficients on these non-conflict shocks. Interestingly, we now find few significant effects of such shocks on behavior in the experiments. Natural disasters and diseases do not produce the same traumatic responses as exposure to conflict—attenuating the risk that our results in Tables 3-5 are due to omitted variable effects. The one exception is a correlation between severe draughts and social preferences, suggesting that responses to economic shocks differ from conflict shocks.

Table 6 Conflict and Real World Behavior and Decisions

	(1) social capital	(2) share of cash crops in total production	(3) expenditures on farm improvements	(4) social subsample 1998 same gender households	(5) risk subsample 1998 same gender households	(6) time subsample 1998 same gender households
	OLS	Tobit	OLS	2SLS	2SLS	2SLS
Percentage dead in attacks	0.033**	0.008*	-23.75*	3.080**	0.0930*	0.424
	(0.016)	(0.005)	(12.810)	(1.294)	(0.0574)	(1.246)
Household variables						
Respondent is literate	0.374**	0.041	113.8	12.30***	-0.0957	-9.317
	(0.150)	(0.039)	(92.48)	(4.625)	(0.190)	(5.915)
Respondent age	-0.001	0.002	-1.019	-0.346**	0.00154	-0.249
	(0.004)	(0.001)	(3.598)	(0.160)	(0.00657)	(0.212)
Respondent is male	-0.111	0.079**	-35.47	-1.625	-0.267	7.504
	(0.153)	(0.039)	(78.75)	(5.255)	(0.215)	(6.984)
Total land holdings per capita	0.027*	0.011***	-7.243	0.935***	-0.0304	0.158
	(0.015)	(0.004)	(9.346)	(0.309)	(0.0197)	(0.428)
Community variables						
Land Gini coefficient	0.097	-0.465***	261.6	-16.85	-0.871	-57.04***
	(0.353)	(0.165)	(291.7)	(16.67)	(0.996)	(21.62)
Distance to market	-0.092	-0.003	79.32	-8.053**	0.0952	6.209
	(0.107)	(0.043)	(54.88)	(3.520)	(0.167)	(4.456)
Conflict over land	-0.897*	-0.530***	656.0	-33.90*	0.149	-40.05*
	(0.445)	(0.180)	(464.9)	(19.73)	(1.318)	(23.88)
Ethnic homogeneity	0.003	0.002	-4.985	0.165	0.000822	-0.599***
	(0.007)	(0.002)	(3.597)	(0.148)	(0.00725)	(0.210)
Socio-economic homogeneity	0.044	0.011	39.08	-1.354	0.130	7.491
	(0.124)	(0.038)	(53.49)	(2.698)	(0.158)	(5.159)
Population density	0.014	0.304***	-62.60	8.192	0.536	15.22*
	(0.186)	(0.103)	(159.5)	(6.769)	(0.374)	(8.582)
Per capita total expenditure	-0.271*	0.099	113.5	1.172	-0.331	5.042
	(0.139)	(0.068)	(78.08)	(6.197)	(0.254)	(5.455)
Constant	2.180	-3.094***	-635.3	-3.600	1.793	-45.52
	(1.863)	(0.670)	(1261.1)	(58.28)	(2.643)	(74.99)
FE	yes	yes	yes	yes	yes	yes
N	274	277	279	212	158	205
adj. R^2	0.09		0.06	0.14	0.18	0.23
Hansen J, p-value				0.60	0.14	0.75
Partial F				6.09	5.07	5.99

Robust standard errors in parentheses clustered at community level. * p < 0.10, ** p < 0.05, *** p < 0.01. Sampling weights not included, results are qualitatively identical. Column (2) contains an unconditional Tobit regression with fixed effects, potentially inducing some bias in our estimate; regression using random effects is qualitatively similar. Columns (4)-(6): first stage instruments distance to Bujumbura (log) and altitude (log), results not shown.

Table 7 Preferences and Non-Violent Events

	(1) Social	(2) Risk	(3) Time
Severe draught	11.644***	-0.019	2.905
	(2.965)	(0.204)	(4.160)
Excess rain	-1.674	-0.039	-1.183
	(4.325)	(0.213)	(7.214)
Manioc crop disease	9.451	0.156	5.653
	(6.464)	(0.265)	(7.145)
Upcoming ceremony			2.894
			(5.967)

Table summarizes coefficients of separate regressions including controls and fixed effects. Robust standard errors in parentheses, clustered at community level. + p < 0.15, * p < 0.10, ** p < 0.05, *** p < 0.01. Sampling weights not included, results are qualitatively identical.

5.6.3 Behavior and preferences

The evidence documented in the various tables is consistent with the idea that preferences of people are endogenous and respond to experiences or (changes in) the context. However, the nature of our data does not permit us to rule out alternative explanations. For example, there may be selection on unobservables: i.e. communities with greater ethnic or political cleavages may be easier targets because they are less able to defend themselves or, conversely, communities with fewer cleavages may be more likely to be targeted because of their potential support "for the other side". The latter story would imply a correlation between exposure to violence and social preferences, such as we find in our data, but of course not a causal effect. While we cannot rule out such hypotheses entirely, there is nothing in our data to support them either. For example, when we include proxies for such cleavages (such as "the percentage of votes in favor of president Ndadaye" and "ethnic homogeneity") we find they are not statistically significant and do not affect our coefficients of interest.[74]

Alternatively, behavioral differences may be due to learning effects – learning about own preferences or those of others, or about consequences of behaviors over a range of contexts. Or, if conflict affects the social context which in turn affects behavior in "real life," then our experiment may also pick up such effects (see e.g. Herrmann *et al.* 2008 for evidence on the impact of context and culture on cooperation and punishment in public good experiments). Our quasi-experimental approach implies imperfect control, and cleanly distinguishing preference shifts from learning effects is not possible. The benefit of our approach is that we are able to analyze the response to an event of first-order salience. Analysts routinely trade-off control versus relevance when conducting experiments (e.g. List 2007). However, if we are to speculate about the underlying mechanism, we believe our data do not generally support the "learning interpretation." There is no reason to assume that *a-priori* (i.e. without exposure to conflict) respondents' uninformed guesses about their true preferences would be systematically biased towards one direction or another, and also the variance in behavior

[74] Similarly, and following Blattman (2009), we have experimented with alternative proxies and controls to probe the robustness of our findings (results not shown but available on request). For example, it may be that NGOs have selectively targeted high-conflict communities and have promoted prosocial preferences there. However, controlling for NGO interventions does not change our results. Similarly, selective exposure to post-conflict radio shows emphasizing reconciliation may potentially bias our results, but controlling for radio ownership does not affect our results. We also tested whether conflict induced sorting by people migrating to regions where their ethnic group was overrepresented. Our coefficient of interest is not affected when we control for changes in the ethnic composition of villages ("sorting") between 1993 and 2009 (for our sub-sample of 35 communities for which we have been able to collect this information), and population size. This is important because sorting could be another mechanism to explain why conflict victims behave more prosocially towards their peers.

is not smaller among victims than among non-victims (and hence behavior does not really "converge" towards the new behavioral pattern either). Details of the formal tests are available on request.

Similarly, our data do not provide strong support for the alternative hypothesis that changes in behavior are due to conflict-induced changes in the social context (or structure). Contextual differences in violence and non-violence communities are captured (at least to some extent) by the community controls in Tables 3-5 (such as ethnic and socio-economic homogeneity, land inequality and income), and our measures of violence still show up significantly. It may also be the case that villagers in affected communities expect more violence in the future, so that their behavior in the experiments reflects *expectations* of future exposure to violence rather than exposure to conflict in the past. Our data allow us to test this idea too. In our 2007 survey we included questions about subjective security in the post-war era, and we find no evidence that victims are more or less optimistic about their security situation than non-victims.[75]

This brings us to the possibility that exposure to violence affects preferences. The economic literature suggests a mechanism for endogenous preferences: the neoclassical model based on optimizing agents pioneered by Becker and Mulligan (1997).[76] In their theory of the short-term adaptation of preferences, individuals can choose (at some cost) to increase their discount factor above their so-called "endowed level." For example, when expected future payoffs increase, the return to investments in a stock of "future-oriented capital" (i.e., raising the discount factor) goes up as well. Hence people make an effort to change their preferences and become more patient, thus increasing the level of their (net present value of) utility. Similar reasoning may be applied to endogenize social preferences and risk preferences. Individuals can (at some cost) deviate from their endowed levels of altruism and risk aversion. Altruism is typically modeled as other agents' welfare levels being arguments in the decision maker's utility function, with positive weights. Following the argument of Becker and Mulligan

[75] Results available on request.

[76] Alternatively, preference shifts may have a neurobiological basis. Even though the genetic code of individuals is fixed at birth, it is possible that trauma has long-lasting effects on behavior by influencing the *expression* of genes known to affect brain chemicals implicated in social behavior – a process called methylisation (cf. IJzendoorn *et al.* 2010). For example, trauma may influence the expression of the gene regulating transportation of serotonin (5-HT) (Caspi *et al.* 2003), which has been linked to pro-social behavior (e.g. Crockett *et al.* 2008), discounting (Schweighofer *et al.* 2008) and risk taking (Kuhnen and Chiao 2009). Similarly, Kosfeld *et al.* (2005) show that altruism is regulated by the hormone oxytocin (OT), and de Dreu *et al.* (2010) find that OT not only influences within-group trust, but also stimulates aggression against competing "outsiders" – and hence OT may be the driver of parochial altruism (cf. Choi and Bowles 2007). Whether methylisation is the underlying mechanism linking shocks to preference shifts is an intriguing avenue for future research.

(1997), when an individual's peers do relatively well, the rational response for that individual would be to *increase* the weight she attaches to their welfare levels, and hence become *more* altruistic – because this raises the value of her own utility too. This theoretical prediction seems at odds with the reality of conflict. A conflict shock leaves fellow villagers worse off, so their wellbeing should receive *less* weight in the utility function of a rational individual, rather than more (as documented in the experiments).[77]

Our findings appear not to be unique to the case of Burundi. Related literature has pointed to personal growth after trauma. In the political science literature, for example, Blattman (2009) notes that individuals abducted by the Lord's Resistance Army (in Uganda) are more politically active, and speculates this is due to "changes in personal goals, perspectives or self-regard" (p. 243; see also Bellows and Miguel 2009). In the psychological literature there is lots of discussion (with varying degrees of rigor) of how shocks can permanently alter someone's outlook on life (or the value of social networks; see Tedeschi and Calhoun 2004).

Closer to our paper, Gilligan *et al.* (2011) use behavioral experiments to document how exposure to conflict during Nepal's civil war contributes to a greater propensity to invest in trust-based transactions and to contribute to the public good. Similarly, Bauer *et al.* (2011) run a series of sharing experiments with a sample of young children from the Republic of Georgia, shortly after a random sub-sample of the Georgian population was exposed to bombardments during the war with Russia over South Ossetia. They look at both inter- and intragroup sharing, and find evidence supporting the theory that conflict breeds parochial altruism (as in Choi and Bowles 2007). We believe the results about intra-group altruism nicely complement our results. Interestingly, Bauer *et al.* (2011) look at young children shortly after the bombing, while we look at adults approximately a decade after the cessation of violence. Together, they suggest, violence affects the behavior of both kids and adults, and the effects are both immediate and persistent.

5.7 Conclusions

The literature on the consequences of civil wars has often emphasized its detrimental effects on households' ability to cope. According to this view, such civil wars may invite poverty traps. However, this pessimistic view on development has come under new

[77] Note that our results also extend to households who themselves have not been exposed to conflict, but saw the negative impact on their fellow villagers. These individuals, too, behave more pro-socially, which is inconsistent with a theory predicting that people should care more about their peers when their peers are relatively well off.

scrutiny from a few recent careful micro level studies suggesting that exposure to conflict is not necessarily detrimental for development and may contribute to social capital (see Bellows and Miguel 2009, Blattman 2009, Gilligan *et al.* 2011, Bauer *et al.* 2011). Yet, social preferences are only one of a set of preferences of interest to development economists, and possibly affected by conflict shocks. We aim to extend earlier work by (i) including risk and time preferences in our analysis, and (ii) gauging such preferences with a series of incentive-compatible field experiments (rather than via a survey approach).

In this paper we set out to investigate the impact of conflict on social, risks and time preferences and use data from a series of economic experiments using 300 respondents in 35 randomly selected communities in Burundi. We find that conflict is robustly correlated with behavior. Econometric analysis reveals that individuals in communities which were exposed to greater levels of violence display more altruistic behavior to their neighbors, are more risk seeking and have higher discount rates. While our data do not allow us to exactly identify the mechanism linking conflict shocks to behavioral change, we discuss four candidate explanations: selection effects, changes in beliefs, social structure and preferences. Future research could be designed to distinguish between these hypotheses so as to identify the mechanism linking trauma to behavioral change.

A key finding of this paper is that large temporary shocks may have long-term consequences: civil war violence that occurred between 1993 and 2003 has a clear impact on individual behavior in 2009. These consequences may even prove to be permanent if they invite preference shifts. Our evidence for Burundi suggests that the net effect on development is unclear. While exposure to violence encourages risk taking and increases the weight people attach to their fellow community members' welfare, arguably positive features for development (at least within certain bounds), it also seems to trigger impatience. As impatience discourages savings it could drag down investment levels also in the presence of imperfect capital markets (as obviously prevailing in Burundi). If so, the net effect on the ability of communities to rebound after conflict is ambiguous. Nevertheless, the results may partially explain the pattern of recovery observed in many post-conflict settings, and thereby provide new evidence against pessimistic views on the destructive legacies of civil war.

Appendix A: Sample design, implementation and experiments

I. Surveys

We use panel data from two waves of household and community surveys. The first wave of data was collected in 1998, the second in 2007. In 1998 the World Bank and the Burundi Institute of Statistics and Economic Studies (ISTEEBU) conducted a general-purpose household survey to assess living standards in rural Burundi, using a two-stage clustered and stratified sampling approach. Stratification was based on the four agro-ecological zones that are present in Burundi. The first stage comprised the random selection of 391 villages (called *sous-collines* in Burundi; the lowest administrative unit) from a list that featured all villages in Burundi. Villages were chosen with a probability proportional to their population. Subsequently, within each village 10 households were randomly selected from a list of all households residing in the village (Enquête Prioritaire 1998).

In 2007, we designed the Priority Survey as a follow-up to the 1998 survey. We randomly drew 100 of the 391 baseline sites with the purpose to track and re-survey all 1000 original (1998) rural households in these sites. We located and re-interviewed 874 out of the 1000 selected households. In addition we interviewed on average three village officials in our community questionnaire. They would collectively discuss and answer our questions.

Our Burundian partner ISTEEBU informed the village chief of our upcoming visit and our survey team met with him one day beforehand. Enumerators were instructed to conduct the interview with preferably the household head, or spouse if the household head was unavailable. The enumerators would obtain oral informed consent to conduct the interview, and emphasized that the respondent could opt out of the interview at all times. Non-response rates to the surveys were very low; less than 3% in 1998 and less than 1% in 2007.

II. Experiments

For the experiments we drew a stratified random sample from our list of villages (interviewed in 1998 and 2007). We based our stratification on the incidence of war-related violence at the village level. In our 2007 survey we obtained information from

the village officials about the incidence of violent attacks in their villages, between 1993 and 2003. We ended up with a sample of 36 villages: 25 experienced violence and 11 did not. In one of these villages we were unable to conclude the experiments. Of the 306 household heads that were invited to the experiment 26 did not show up. The most important reasons were a move to a different location (19%), illness (15%), other businesses (11.5%), unable to find them when the village official came round to their house (11.5%), or for reasons unknown (43%).

The experiments were conducted during March-April 2009 in collaboration with Ligue Iteka, a Burundian non-governmental human rights organization. The organization has a long history of operation as well as nationwide coverage in Burundi and has a solid reputation of being trustworthy amongst Burundians.

We organized an extensive training of our local experimenters and ran several pilot tests to ensure that our typical participant was able to understand our experiments without much effort. A day before the experiments, research coordinators contacted local government officials in each research site, and asked them to invite the household heads of the 2007 survey participants. The experiments started at approximately 9 a.m., and lasted about three hours. Each session started with a general introduction in which the participants were informed, among other things, that upon completion of the session they would receive a show-up fee of 2000 FBU plus or minus the amount of money they would gain or lose as a result of their decisions during the session. The games were implemented by three teams, each with one instructor and two research assistants. Subjects who had difficulty completing record sheets by themselves were helped by research assistants who carefully avoided giving specific instructions about how to answer.

As many of our participants had received little or no education, we followed a relatively simple design and our experimenters used clear and visual instructions to make it easier for illiterate subjects to understand the consequences of any decisions they made in the games.[78] In each session the social preference experiment was implemented first, followed by the risk and time preference experiments. After subjects completed all experiments we randomly determined which pairwise choice was to be paid in the risk and time preference experiment. The average experimental earnings for three games

[78] We tested comprehension by asking test questions before the start of each experiment. To enhance understanding we also limited the group size to 10 participants and if needed experiments were conducted in two groups. Also, instructors went through the experiments question by question. All experiments were conducted in the local language Kirundi.

were about 6000 FBU[79] (roughly 5 days wages for unskilled labor), including the 2000 FBU show-up fee. The subjects were not informed about their earnings in each experiment until all experiments had been completed.

A. Social Preference Experiment

To measure social preferences we used a modified version of the social value orientation experiment devised by Liebrand (1984). In this experiment, subjects (denoted as *i, j*) are presented with 6 pairwise choices between two own-other payoff combinations. The pairs of allocations lie on a circle in the positive quadrant, where the amount of money the decision maker allocates to himself (Si) is measured along the horizontal axis and the amount of money allocated to the other participant is measured along the vertical axis (Oi).[80] The radius of the circle is 250 FBU, so that $S^2+O^2 = (250)^2$. As a result, the total amount of money to be allocated (S_i+O_i) is not constant across combinations. A subject's *i*'s earnings are equal to the amount of money allocated to himself (the sum of S_i's over the six choices), plus the amount of money allocated to him by his partner *j* in the experiment (the sum of O_j over the six choices); see Table A1

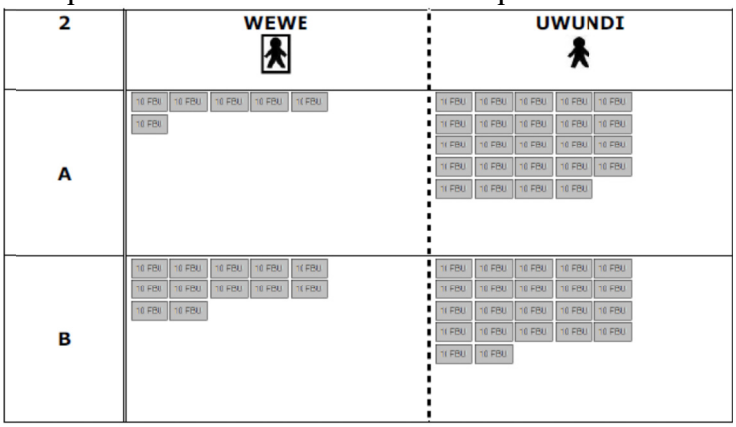
Table A1 Choice Pairs Social Preference Experiment

	A		B	
Question	Self	Other	Self	Other
1	0	250	60	240
2	60	240	120	220
3	120	220	180	180
4	180	180	220	120
5	220	120	240	60
6	240	60	250	0

[79] USD 1 = 1,210 FBU (20 May 2009), which is roughly equal to a full day's wage rate for unskilled labor.

[80] Originating in the social psychology literature this experiment is now frequently applied in the economics literature as well (Offerman *et al.*1996). In these versions subjects are offered 24 pair wise allocations, covering all four quadrants of the circle. Pretesting revealed that the cognitive burden of making all 24 choices was too large for our (largely illiterate) subject pool. We decided to just offer subjects pair-wise choices in the "first quadrant" of the social orientation circle—corresponding to positive amounts for both the giver and receiver. We thus reduced the cognitive burden imposed on our illiterate subjects at the cost of reduced precision with which pro-social preferences can be measured.

Example Record Sheet Social Preference Experiment



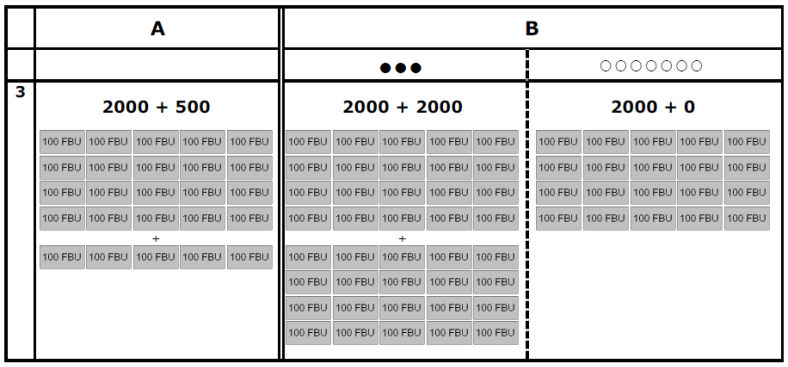
B. Risk Preference Experiment

Risk preferences were measured using a game based on Harbaugh *et al.* (2002). Subjects were presented with 6 choice cards, each of which presented them a choice between A: receiving (or losing) an amount of money with certainty (*y*, that varied between the 6 choice cards), and B: participating in a game where they may either gain (lose) 2000 FBU with probability 0.3, or gain (lose) nothing with probability 0.7. Hence, the expected absolute value of the gamble was always the same (600 FBU. This was an expected gain for three cards and an expected loss for the other three cards), whereas we varied the amount of money to be received with certainty (*y*); see Table A2. For both gains and losses the certain bid (*y*) was lower, equal to, and higher than the expected value of the gamble. As the certain payoff (*y*) in A increases, the gamble in B becomes less attractive. The point at which a subject switches from the risky to the safe alternative allows us to determine her degree of risk aversion.

Table A2 Choice Pairs Risk Preference Experiment

	Certain	Gamble	
Question	gain/loss	*p*	gain/loss
1	500	0.3	2000
2	600	0.3	2000
3	700	0.3	2000
1	-500	0.3	-2000
2	-600	0.3	-2000
3	-700	0.3	-2000

Example Record Sheet Risk Preference Experiment



The probabilities of the gamble (0.3 of winning/losing 2000 FBU, and 0.7 of receiving nothing) were represented visually using three black and seven white balls. To illustrate the chances of winning/losing money, the ten balls were put into a bag in the presence of all participants in the session. We would then shake the bag with the balls to convince the audience that the balls were all mixed. Next, we drew one ball from the bag about ten times – with replacement – to show the participants that the likelihood of drawing a black ball (implying winning/losing money) was less than half the likelihood of drawing a white ball (resulting in zero payoffs). The choice cards displayed the options both numerically and graphically with each change in money stock represented by an equivalent number of banknotes. Payoffs for this second experiment were not determined until after the third experiment had been completed. Then, payoffs were determined by first selecting which of the six cards was to be implemented. Six numbered balls were put into a bag to randomly select one card to be played for payment. Those subjects who had chosen the safe option A were informed about the amount of money *y*, as stated on that card, they were to receive (or had to pay). For those who had chosen the gamble, option B, the seven white balls and the three black balls were put into the bag again to determine whether they would receive (have to pay) 2000 FBU (when any of the three black balls were drawn), or whether they received nothing (if one of the seven white balls was drawn). Note that we made sure that at the end of the experiment all subjects had non-negative earnings because of the 2000 FBU show-up fee.

C. Time Preference Experiment

To measure time preferences, we follow Harrison *et al.* (2002) and presented subjects with a set of nine simple pairwise choices between two options: receiving an amount of money at some date in the near future, and receiving a larger sum at a later time. The

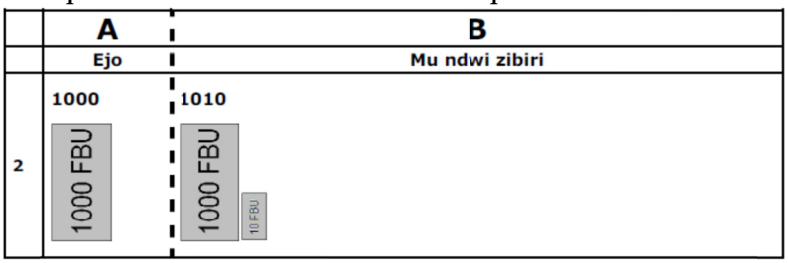
amounts of money were to be delivered by the same trustworthy local NGO, Ligue Iteka. However future money is always less certain than instantaneous money. Consequently, we provided subjects with a choice between two future options – receiving money tomorrow, or in 15 days – rather than one "instant" versus one future income option. The two options to choose from were A: receive 1000 BFU in one day, and B: receive 1000(1 + *d*) FBU in two weeks plus one day, with *d* equal to 0.00, 0.01, 0.02, 0.05, 0.10, 0.40, 0.70, and 1.00; see Table A3. Thus, at the highest interest rate subjects earned an additional 1000 FBU by waiting two weeks. In the experiment subjects were asked to identify their switching point from preferring A to preferring B. Increasing the interest rate *d* over the nine decisions allows us to observe the point at which a subject switches from preferring 1000 FBU tomorrow to preferring 1000(1 + *d*) FBU in two weeks plus one day. The switching point serves as a measure of the subject's discount rate; the earlier people switch from A to B the more patient they are.

Table A3 Choice Pairs Time Preference Experiment

Question	Tomorrow	In two weeks and one day	d
1	1000	1000	0
2	1000	1010	0.01
3	1000	1020	0.02
4	1000	1050	0.05
5	1000	1100	0.1
6	1000	1400	0.4
7	1000	1700	0.7
8	1000	2000	1

Participants were not shown (d).

Example Record Sheet Time Preference Experiment



After subjects completed all questions (and after having determined the payoffs for the social orientation game and the risk preference game), we randomly determined which pairwise choice was to be paid in the time preference experiment. To do so, we put 9

numbered balls into a bag, and picked one randomly. The choice made for that question (i.e. A or B) then determined how much money was delivered, and when.

Upon completion of this third game participants were informed about their revenues in the social orientation game, the payments for the risk preference game were determined according to the procedure explained above, and they received the associated earnings, plus their show-up fee.

The pay-off of the time preference game was placed in a sealed envelope and handed over to a representative of the regional office of the local non-governmental organization. All participants received a receipt stating the amount of money they were entitled to. At the relevant date (either the next day, or 15 days later), the representative went back to the community to deliver envelopes to the respective participants, in return for the their receipts. To ascertain that the money envelopes were indeed delivered by our local organization, we checked whether all receipts were collected – which was indeed the case. The participants were informed about this procedure in advance.

Appendix B: Data Description

Experimental variables

- *Social preferences*: degree of altruism resulting from 6 choices between participant (S) and randomly chosen community member (O), for each question holds $S^2+O^2 = (250)^2$, the resulting degree of altruism is $\alpha = \tan(O/S)$, with $37.5 < \alpha < 52.5$. Items were rescaled to $0 < \alpha < 100$. In community level regressions, variable for *j* -th community is created by averaging over all community households.
- *Risk preferences*: switch point [0 (risk averse), … , 3 (risk loving)] between risky gain (or loss) with a probability p = 0.3, and certain (y) gain (or loss). In community level regression, variable for *j* -th community is created by averaging over all community households.
- *Time preferences*: discount rate is switch point between receiving 1000 FBU in one day and receiving (1+ d)1000 FBU in two weeks and one day, *d* = [0, … , 100]. In community level regression, variable for *j* -th community is created by averaging over all community households.

Conflict variables

- *Rel. number of dead in attacks 1993-2003:* Number of dead in community attacks relative to population size. Reports total number of dead on the *colline* as a consequence of confrontations between rebels and army as well as one sided violence between 1993-2003 divided by population size, as stated by local administrators (BCS 2007).
- *Household level victimization index:* is an additive index of physical (death of household member, forced labor, torture, ambush) and non-physical (theft of assets, crops and money) exposure of any household member. Since the absolute scale of this variable is arbitrary it is normalized to have a mean of zero and standard variation of one (BPHS 2007).
- *Household level victimization index – physical attack:* is dummy variable taking unity if any household member was exposes to a physical attack (death of household member, forced labor, torture, ambush) (BPHS 2007).
- *Household level victimization index – no physical attack:* is dummy variable taking unity if any household member was exposes to a non-physical attack (theft of assets, crops and money) (BPHS 2007).

- *Attack during 1993-2003*: is a dummy variable taking unity if community was attached between 1993-1998 (BCS 2007).

Household and community variables

- *Age of head of household*: measured in years (BPHS 1998 and 2007 and ESD-SR) .
- *Gender of head of household*: in household level regressions variable is dummy variable taking unity if head of household is male, zero else. In community level regressions, variable household dummy's are averaged over number of households per community (BPHS 1998 and 2007 and ESD-SR).
- *Literacy of head of household*: in household level regressions variable is dummy variable taking unity if head of household is literate, zero elsewhere. In community level regressions, variable household dummy's are averaged over number of households per community (BPHS 1998 and 2007 and ESD-SR).
- *Total land holdings per capita* (m^2): total land size of household i in square meters, divided by number of adult equivalents present in household i (BPHS 2007).
- *Per capita total expenditure*: variable is the aggregated income of per capita expenditure for all goods purchased over a 15 day period valued at local market prices and divided by the adult equivalents of household i (BPHS 2007).
- *Perceived trust level in 1998*: measurement of trust in community members, rated on a 10 point scale 1 (very low)-10 (very high) (BPHS 1998).
- *Livestock farmer in 1993*: in household level regressions variable is dummy taking unity if household owned livestock in 1993, zero elsewhere. In community level regression, variable for j-th community is created by averaging over all community households (BEES 2009 and ESD-SR). Both variables are based on recall questions in the BEES 2009 and ESD-SR surveys respectively.
- *Share of cash crops in total production in 1998 and 2007*: share of cash crops (coffee, tea, tobacco and cotton) produced relative to total production (BPHS 2007).
- *Expenditures farm improvement in 1998 and 2007*: Expenditures on farm improvements (construction, repairs and purchase of equipment) in 2006-2007 (BPHS 1998 and 2007)
- *Land Gini coefficient*: variable based on household land holdings. Community level Gini coefficient is created by $G = 1 - 2\int_0^1 L(X)dX$ (BPHS 2007).

- *Social capital*: index comprising a weighted (and normalized) scale of respondents' participation in community organizations and the degree of membership (BPHS 2007).
- *Distance to market*: Average distance to main agricultural market where food and non-food items are traded, measured in time intervals of 15 minutes, where $t = 1,...,5$ (BCS 2007).
- *Density* (1990 and 2008): number of people in community per square kilometer (MPDRN 2006 and RNPH 2008).
- *Percentage of votes for Ndadaye*: percentage of votes in favor of Ndadaye at the commune level during the presidential elections in 1993
- *Distance to Bujumbura*: distance of *j*-th community to capital in kilometers.
- *Altitude (log):* average altitude of *j*-th community (MPDRN 2006).
- *Conflict over land:* percentage households which report to have conflicts over land in community (BPHS 2007).
- *Ethnic homogeneity*: percentage of Hutu population in community (BEES 2009).
- *Social homogeneity*: measure of perceived within village socio-economic inequality rated on a five-point scale 1 (no inequality)-5 (high degree of inequality). (BCS 1998 and 2007).
- *Severe draught*: dummy taking unity if household was exposed to severe draught in past three years (2007-2009) (BEES 2009).
- *Excess rain*: dummy taking unity if household was exposed to excessive rain in past three years (2007-2009) (BEES 2009).
- *Manioc crop disease*: dummy taking unity if community was exposed to cassava crop disease in 2006 (FAO 2006).
- *Upcoming ceremony*: dummy taking unity if household was expecting a ceremony in the near future (BEES 2009).
- *Present in village in 1993 and 1998:* dummy taking unity if household was present in village in 1993 and 1998 (ESD-SR).
- *Present in village in 1998 and 2009:* dummy taking unity if household was present in village in 1998 and 2009 (BPHS 2007, BEES 2009).

Data sources

- BPHS. Burundi Priority Household Survey 1998 and 2007
- BCS. Burundi Community Survey 1998 and 2007

- BEES. Burundi Experiments Exit Survey 2009
- FAO. Valuation Des Recoltes, Des Approvisionnements Alimentaires et de la Situation Nutritionelle, 2006
- MPDRN. Monographies Communales Burundi. Ministre de la Planification du Développement et de la Reconstruction Nationale, Bujumbura, 2006
- ESD-SR. Enquête Socio-Démographique, Santé et Reproduction 2002
- ACLED. Raleigh, C., A. Linke, H. Hegre and J. Karlsen. 2010. Introducing ACLED: An Armed Conflict Location and Event Dataset. Journal of Peace Research, 47(5): 1-10.
- Sinunguruza, Térence, 2001. Les Elections au Burundi. Tout Savoir et Tirer des Leçons de Juin 1993. London: Editions Luc Pire.
- RNPH. Recensement National de la Population et de l'Habitat, Bureau Central de Recensement, Gitega 2008.

Chapter 6

Peasant Grievance and Insurgency in Sierra Leone:

Judicial Serfdom As a Driver of Conflict

Abstract: Was the civil war in Sierra Leone (1991–2002) fought for diamonds, or was it a peasant insurgency motivated by agrarian grievances? The evidence on both sides is less than conclusive. This article scrutinizes the peasant insurgency argument via a more rigorous methodology. Hypotheses concerning intra-peasant tensions over marriage and farm labor are derived from an examination of the anthropological literature. These are tested using econometric tools, applied to data from a randomized survey of 2,239 households in 178 villages surrounding the Gola Forest in eastern and southern Sierra Leone, the cradle of the war. It is shown that a decade after the war ended peasant disputes over marriage continue to mark out an incipient class divide in isolated rural communities, as evidenced by cases presented in local courts and family moots. Disputes mainly involve a village elder suing a young man with weak social protection. Fines are exceptionally high, and mostly paid off in the form of coerced farm labor. It is argued that grievance over this long-standing form of labor exploitation fed insurgency, and contributed to the otherwise puzzlingly high levels of peasant-upon- peasant violence associated with the war in Sierra Leone.

Publication Information: Mokuwa, E, M. Voors, E. Bulte and P. Richards, 2011. Peasant grievance and insurgency in Sierra Leone: Judicial serfdom as a driver of conflict. *African Affairs*, 110(440): 339-366

6.1 Introduction

The civil war in Sierra Leone (1991–2002) has occasioned much academic dispute. To some, the insurgency of the Revolutionary United Front (RUF) is a typical instance of 'new war' fought with economic rather than political objectives (Smith et al 2000). The RUF was purely and simply a bandit organization motivated by pursuit of diamond wealth. To others, the conflict was an instance of peasant insurgency, as commonly encountered in backward agrarian societies (Richards 2005).[81] Ranajit Guha has provided a richly detailed analysis of such insurgencies (Guha 1999). He accounts for the political nature of looting, for example, by explaining the 'negative' mentality expressed in such actions as desire to turn the world of oppressive landlords, chiefs, and money lenders on its head; peasant insurgency reflects not pursuit of material gain but a rudimentary class consciousness, and the failure of national politics to embrace and shape it.

Testing either of these hypotheses in regard to the war in Sierra Leone is not far advanced. Some cross-country studies have implicated diamonds as a determinant of conflict, but the applicability of 'average effects' for specific countries (such as Sierra Leone) is an open question (Olsson 2007). A comprehensive conflict-mapping exercise for Sierra Leone uncovers remarkably little evidence to link rebel activities with mining in the first part of the war, a necessary connection if the country's alluvial diamond deposits are to be counted as drivers rather than sustainers of conflict (Smith 2004).[82] Some interview material has been offered as evidence to support the argument concerning agrarian backwardness, (Richards 2005) but testimony from ex-combatants is not always fully convincing.

In this article we aspire to advance the peasant insurgency side of the debate, but through use of a more rigorous methodology than hitherto attempted by supporters of this explanatory approach. Our research strategy is to derive hypotheses from anthropological accounts of agrarian institutions indicated in the literature as important sources of incipient peasant inter-generational or class conflict, and then to test these hypotheses quantitatively, using econometric methods. Our main data source is a large randomized survey undertaken in 2010 in which we recorded answers to questions about

[81] One study –Humphreys and Weinstein (2008) – finds quantitative support for both greed and grievance hypotheses in Sierra Leone.

[82] In this source, all but one of the early reports of diamond mining by militia factions are credited either to government forces or to international (Nigerian and Liberian) auxiliaries (see, for example, chapter for Pujehun District, pp. 484 and 497). Crucially, rebel RUF units entered the main Kono mining district from 5 June 1991 (and so had the chance to become involved in mining at an early stage) but are reported to have been concerned only with ideological and organizational activity (see chapter for Kono District, pp. 332–9). There is no dispute, however, that all factions became involved in diamond mining during the later stages of the war – in part, at least, to support their armed struggles.

household organization, marriage patterns, farming activities, and everyday disputes, from 2,239 households in 178 villages around the Gola Forest in eastern and southern Sierra Leone. This region, bordering Liberia, was the initial focus of the war, and the only part of the country where the rebel RUF could claim any degree of voluntary support. We do not rely upon any data gathered in direct response to questions about causes of war, to avoid the problem of self- serving testimony.

The RUF first entered the country from Liberia at two points north and south of the Gola Forest, and subsequently entrenched itself in the Gola region. For a number of years the south-western arm of the Gola chain of reserved forests sheltered its headquarters camp (the Zogoda, in Koya Chiefdom, on hills overlooking the Moa River). There were foci of support for the RUF in Pujehun District (notably Soro Gbeima Chiefdom) and Kailahun District, lying to the south and north of the main forest belt respectively.

The RUF remained in control of large parts of Kailahun District to the end of the war in 2002. Its stated war aims were to overthrow a one-party regime, restore democracy, and introduce a populist (Libyan-inspired) style of government (RUF 1995). If the peasant insurgency argument is to be sustained it is necessary to show that there were divisive agrarian issues – equivalent to the rack-renting and loan sharking described in Guha's classic account of peasant insurgency – affecting the region in which the RUF took root, and with which it could align.

Here we examine the case of rural marriage – and in particular disputes over 'woman damage' (adultery) – as a key divisive agrarian issue. The Mende phrase is *nyaha yia* (*nyaha* = woman, *njia/yia* = dispute or trouble). 'Woman trouble' would avoid the misunderstanding that the term refers to violence against women. But since the translation 'woman damage' is widely used in Sierra Leone we retain its use here. We show that the nub of the problem over 'woman damage' is agrarian labor mobilization. An underclass of young men becomes tied and indebted to larger farmers through inability to find legal marriage partners. Farm labor rates, not land rents, become the fulcrum of exploitation linking village elites and an underclass, and thus a factor plausibly predisposing exploited young male farm laborers towards insurgency.

6.1.1 An overview of the peasant marriage problem in rural Sierra Leone

A randomly sampled national survey of ex-combatants from the war in Sierra Leone found that the two peak years for recruitment into the RUF were 1991–2, and that more

than 60 percent of fighters were from Eastern Province (Humphreys and Weinstein 2004). This implies that a substantial number came from the Liberian border zone, where the movement first established itself. When asked about benefits of joining the movement only a handful mentioned the lure of diamonds. A surprisingly large number – nearly a quarter – stated that by joining the movement they had been able to find a husband or wife (ibid: 27) Could difficulties over forming marriage partnerships have been a factor driving some young people into the arms of the RUF?

Marriage among peasant communities in rural eastern and southern Sierra Leone depends on bridewealth and bride-service payments. For a young man to find a spouse – with whom to farm independently and found a family – the assistance of an elder is generally required.[83] This might be the mother's brother, from whom the young man can claim a daughter under the institution of the avunculate (literally 'uncle marriage' but often the marriage of cross-cousins). Little or no bridewealth will be demanded for such a marriage. The disadvantage is that the young man remains indefinitely obligated to his uncle. In former times an uncle could sell his nephew into in repayment of debts.

The father or other elders in a wealthier peasant family might help a young man seeking a marriage partner to make the higher payments required for a spouse from a family unconnected by the avunculate. A stranger (someone resident in the village but born outside the chiefdom) might turn to his patron (an established citizen) for help in finding a wife. In former times, a wealthy farmer might redistribute young women under his care as partners to his male slaves.[84]

In addition to bridewealth payments, lengthy periods of bride service (work without pay on the farm of the father-in-law) may also be required. The existence of quite high rates of polygyny among wealthier village males leads to a shortage of younger unmarried women in the village, and marriage contracts are sometimes made for new-born girls. This in effect means that the man in question will not marry until he approaches middle age.

Young men and women commonly form liaisons outside these marriage rules. For a young man from a poor family (one with a former slave background, for example) unable to raise the necessary bridewealth, or for a recently arrived migrant stranger,

[83] In the ranked lineage societies of eastern Sierra Leone an 'elder' implies seniority in a ruling lineage, and not necessarily age. The Mende language has the term *numu wai* (big person). 'Big persons' can be female as well as male, and some Mende paramount chiefs are female. A female paramount chief will be a woman of especial ability. But she must also come from a ruling family (that is, a high-ranked lineage). In what follows the word 'elder' generally implies a male elder.

[84] On the institution of slavery among the Mende, see Grace (1977)

taking a 'girl friend' is often the only option. Such alliances are sought by young women married to elderly and neglectful polygynists. Sometimes, as we shall see, the neglect is deliberate, since it enables the elder to bring the young man into his pool of loyal clients. In particular, it allows the elder to demand farm labor from his wife's young paramour. If such service is refused, he takes the young man to court and sues for 'woman damage'. The fines are generally steep. Often, the young man cannot pay, and will work off his fine by laboring for the offended party, as a kind of retrospective bride service. This puts into context the finding that significant numbers of RUF cadres found the movement a source of marriage partners. It allowed them to break free from a restrictive customary institution.

The remainder of this article further characterizes the marriage institution just described, asks about the frequency and continuing importance of 'woman damage' cases, both across the country and in the region from which the war sprang, and assesses statistically significant associations between 'woman damage' disputes and farm labor demands, to test the hypothesis that it is, in fact, an institution enmeshed in labor coercion.[85] The relevance of the study is that, in addition to suggesting the plausibility of an argument linking conflicts between poorer and wealthier male peasants over 'woman damage' to recruitment to the RUF, it also shows that woman damage disputes remain of current significance in the area from which the war originated. This throws light on debates about both causes of the war and post-war reconstruction policy in Sierra Leone.

Since approximately 2000, neo-traditional policy has favored the rein- statement of the pre-war system of rural governance based on paramount chieftaincy and local courts, as devised under British colonialism for purposes of indirect rule. This neo-traditional policy was closely linked to a controversial counter-insurgency strategy of strengthening 'indigenous' civil defense.[86] Since the war, attempts have been made to reform chieftaincy institutions from within. Some local courts now boast posters advising villagers to keep

[85] The connection between 'woman damage' and enslavement goes back several centuries. Fr. Manuel Alvares, writing c. 1615, states that among the Sapes (the inhabitants of Sierra Leone and its vicinity) 'adulterers are punished with death or enslavement' (unpublished manuscript, 'Ethiopia Minor and a geographical account of the Province of Sierra Leone', in the provisional translation of Paul Hair, 1990), Chapter 3. Thomas Winterbottom, writing about courts at Sierra Leone in the 1790s, states that 'woman palaver or adultery' is one of the kinds of 'cases in which life or liberty of the accused are endangered', adding that 'slavery is the usual punishment'. See An Account of the Native Africans in the Neighbourhood of Sierra Leone, Volume 1 (C. Whittingham, London, 1803).

[86] The British High Commissioner to Sierra Leone, Peter Penfold, saw himself as an architect of the paramount chief restoration policy (according to his statement to a meeting on post-war reconstruction in Sierra Leone, held at the UK Foreign Office in 2001, attended by Paul Richards). Mr Penfold was also privy to meetings to arrange private security assistance to civil defense forces in Sierra Leone in 1997 (see Legg and Ibbs 1998).

their marriage disputes within the family, and court clerks state that they try and discourage suits for 'woman damage'. Our data show that it is indeed the case that most 'woman damage' cases are settled at the informal level, by family moots, but that fines levied at this level are still substantial. Taken with our other main finding, which establishes a link between 'woman damage' and farm labor exploitation, this suggests that the agrarian marriage institution continues to bear down heavily on rural young men without strong social support. The local market for agrarian labor is far from free, and while this situation prevails a risk that serfdom will feed peasant revolt remains.

6.1.2 'Woman damage' and local justice.

According to legal ideas among the Mende people, the tort of 'woman damage' arises when it is established that the accused has formed a sexual liaison without meeting the requirements for having such a relationship (such as by paying bridewealth, performing bride service, or entering into some kind of clientship agreement with the plaintiff). Accessories can also be included in the charge. A pioneer analysis of 'woman damage' was provided by Kenneth Crosby, a missionary working among the Mende from 1929 (Tuchscherer 1998). Crosby roots 'woman damage' in polygamy (polygyny), which he states 'is a social system, and is intimately bound up with the subject of property and labor, and the difference in status among men and women' *Crosby 1937).

Characterizing different forms of rural Mende marriage, as he encoun- tered them in the 1930s, Crosby draws attention, first, to the incidence of cross-cousin (avunculate) marriage, which he associates with matrilocal residence and monogamy, as a secure form of relationship for partners from non-elite backgrounds, involving little or no bridewealth but a sustained commitment to provide work on the mother's brother's farm.

Second, he turns to the marriages of the Mende rural elite – those involving men from high-ranked lineages. In a survey of 20 small towns and villages in eastern Sierra Leone, Crosby found that 51 percent of all married men had two or more wives. In all, 842 men were married to 1,973 women. Only 82 women of marriageable age were unmarried, but there were 673 unmarried men of marriageable age in the sample. The upshot is that many commoner men of marriageable age had no repro- ductive partner, and found female 'friends' where they could, especially among the large number of unsupervised wives of high-ranking chiefs.[87]

[87] Crosby adds that some chiefs had up to 300 wives (but these chiefs were not part of his sample).

Crosby points to the strong link between marriage and upland rice farming (the main mode of subsistence among the Mende). Rice farming is based on a gender partnership (Richards 1986, Ferme 2001). Men fell and clear trees and plant; women plant, tend and weed the farm, and process the crop. Both genders are involved in the harvest. The upshot is that one man with several or many wives has enough labour to tend a farm, but perhaps not enough to prepare it. 'The chief difficulty (Crosby writes) is in getting the necessary young men ... . It is, however, only a theoretical difficulty. The usual way is to neglect one's wives and employ their 'friends'.' He then describes a 'woman damage' case: *Aruna had a wife Jeneba, with whom a young man had been co-habiting for years. This young man arranged to go away ... but on the eve of his departure he was taken to court on a charge of adultery with Jeneba. As he had nothing with which to pay, he had to stay where he was. In other words he had become a serf.* (Crosby 1951:142)

Crosby perhaps used the term 'serf' (unusual in a country which never knew feudalism) because of local sensitivity surrounding the (then) recent emancipation of slaves (in 1928). We adopt Crosby's term in this article, since it clearly distinguishes our topic – coerced and unwaged farm labor – from slavery as a system in which persons are bought and sold.

Crosby's account is consistent with later ethnographic literature for the forest region of eastern Sierra Leone and north-western Liberia. For the Mende, Kenneth Little states that '*Polygyny represents a form of capital investment ... . By deliberately allowing their wives to attract young men, some husbands are able to turn the misdemeanour ['woman damage'] to a profitable account.*' (Little 1951: 142). Writing about the Kpelle, a group culturally and linguistically related to the Mende living to the east of the Gola Forest in Liberia, Bledsoe states that '*the most important way in which elders lure and hold on to young men is by the careful accumulation and deployment of young women*' (1980: 55). Like Crosby and Little, she is explicit about a 'honey trap' element: [elders] 'use rights in young women not only to reproduce and to gain labor for supporting their immediate families, but also to lure young men into ties of debt and obligation' (ibid: 48).

Below, in Table 2, we present specific data on the frequency of cases, showing that 'woman damage' remains one of the most important classes of action before local courts in the Gola region, but first we want to comment on the current relevance of Crosby's claim that cases were particularly associated with meeting labor demands on upland rice farms.

Among the Mende, the upland rice farm remains the focus for household food

security. Swamp rice farming has been resisted by Mende farmers for many years (Richards 1986). Most households in the Gola region engage in some swamp production, but it is generally seen as a supplement not a replacement for the household upland rice farm. This is because dryland rice is liked better, and upland farms also produce a large range of inter-crops. But it is also because upland farming is a household project, involving a division of labor between men and women and adults and children that reinforces notions of familial social cohesion.[88] A village household without an upland farm is seen as somehow not fully constituted (Richards 1986). In this sense, the upland farm not only produces food but also has social significance in reproducing the (conservative) agrarian values of a ranked lineage society.

It is this conservative nexus that is particularly significant to our analysis, since this is where we feel there may be a clue to the causes of the war. The RUF proclaimed the slogan 'No more master, no more slave' (Richards 2005). This specifically targeted unpaid labor, not poor rural wages. Below, we look particularly closely at evidence that 'woman damage' is connected with the unpaid labor requirements of subsistence rice farming, and upland rice farming in particular, rather than other areas of agrarian production, such as plantation crops, where labor is rewarded in cash.[89]

6.1.3 Local courts and informal dispute resolution

Now we turn to the courts in which cases of 'woman damage' are judged. Local courts, created by the colonial regime as institutions of indirect rule, were retained as the basis for rural justice after the end of the colonial period. Local courts administer customary law. A main documentary source of customary law remains a national survey undertaken by a District Commissioner, James Fenton, published in 1927 (revised 1948) (Fenton 1948). Cases divide into civil and criminal. Civil cases mainly concern disputes over land, debt, and 'woman damage'. Criminal cases concern theft, affray, defiance of the authority of a chief, and breaking local bye-laws. Bye-laws are proposed by chiefdom councils, scrutinized by the local courts officer for each province, and presented to parliament for endorsement. Table 1 lists the current bye-laws for one Gola Forest chief- dom

[88] On the household division of labor as a basis for social cohesion see Durkheim (1964).
[89] It is relevant to ask why young men do not escape this traditional net by planting tree crops or making swamp rice farms. The answer is that some do, but generally only those with strong rights of land access (that is, those belonging to the main land-owning lineages).

(Malema).[90]

Local court premises are found in every chiefdom headquarters town, and in section towns in larger chiefdoms. Courts typically meet for several days each month, depending on case load. Court chairmen preside over sittings, assisted by assessors. Chairmen are a type of lay magistrate, knowledgeable in local custom and history. They are appointed by central government.[91] Sentences are enforced by court officers, known as chiefdom police. The first line of appeal from a local court is to the local courts officer (a trained lawyer) of the provincial administration. Local court decisions in conflict with national law can be struck down by this officer. Appeals from customary cases are considered by a special panel of customary law experts in the court of appeal in Freetown.

Local courts are not the only sources of local justice in rural Sierra Leone. To gain an idea about disputes settled informally outside the ambit of local courts we asked panels of village informants to estimate the number and type of disputes affecting people in their village over the past decade, in relation to those reaching court. From these estimates it became apparent that local courts handle only a small proportion of local disputes, and that we needed to modify our methodology to embrace informal dispute resolution as well.

There is often a preference by disputants to have their case heard informally by the paramount chief or some lesser chief (most commonly, a village chief). These informal hearings have no official status but belong to the sphere of alternative dispute resolution (ADR). The family moot is one such means (Gibbs 1963). Arbitration by moot is popular because it often works out cheaper: formal court fees are avoided, though fines are still paid.

Formal court proceedings are sometimes favored by plaintiffs, despite costs, when there is significant doubt about the willingness of the parties to settle. A defendant found guilty in an informal setting is often more likely to succeed in 'begging' a reduction in the punishment. In a local court the defendant unable to pay the fine or reach an out-of-court agreement faces a period in the lock-up attached to the court (in the 'dark room', as it was graphically described to us by one court clerk) until a patron or relative steps in to

[90] It is worth noting that four out of 14 of these bye-laws concern sexual misconduct. More serious cases (murder, for example) are handed over to the state police and thereafter to the district magistrate's court, to be tried under national criminal law.

[91] Historically, local courts belonged to paramount chiefs. Subsequent reforms handed the chairmanship to persons appointed by government. This supposedly was to check abuses by high-handed chiefs, but opened local courts to political manipulation from the center.

cover the fine.

Table 1: Typical Bye-laws (as enacted by Malema Chiefdom)

1	Refusing to answer the Chief's call
2	No fighting
3	No use of abusive language
4	No theft
5	Failing to do communal work
6	No sex with other man's wife
7	Children are not allowed to appear in court
8	Impregnating female child under 18 years
9	Breach of contract - Defaulting [to] refund money and food for work
10	No killing of bush cow, crocodile
11	Refusal to do general cleaning - last Saturday of each month
12	No sexual intercourse in the bush
13	No raping
14	All power saw owners must have license

Source: From local court records, Courts Survey, 2010

A second, more drastic option, to deal with steep fines imposed both by local courts and ADR is to abscond. However, becoming a fugitive from justice is a far from cost-free solution, since all 'strangers' (Mende: hota, a person living outside their chiefdom of birth) need to find a patron and protector (Mende: *hota kɛɛ*, literally 'stranger father') in any location to which they move. This patron is responsible to the village and chiefdom authorities for the good behavior of the guest, and where it is suspected that the new arrival is 'on the run' the patron will attach tougher conditions, such as more help in farming, as the price of protection. A number of ex-combatants have testified that they first became vulnerable to militia recruitment after having absconded from local court cases (Richards 2005).

6.1.4 Measuring 'woman damage': data and statistical strategy

Our study draws on court-level data and randomized household data collected in 178 Mende-speaking villages around the Gola Forest. We use these data to find out about patterns and classes of cases appearing before local courts and in ADR. Local courts ceased to function in the war and many records were lost in the chaos. Court personnel (and sometimes court buildings) were targeted by the rebel RUF, because the movement had its own system of justice, based on the populist precepts of the Green Book of Colonel Gaddafi (Smith 2004). Local courts administering customary law were revived with assistance from the British aid programme after the war, and began to function from September 2001. After regime change resulting from the 2007 presidential and

parliamentary elections most, if not all, court chairmen were replaced. Court clerks – the chiefdom civil servants who run the administration of justice, and who are often highly knowledgeable about custom – have also since been rotated. As a result, many court records may have been returned to the provincial secretariat, or still remain in the possession of departing chairmen loyal to the previous regime, and are currently inaccessible.[92]

This posed a problem for our study, as one of our working methods was to go through case books with the court clerk explaining each case and its context. Therefore we sought additional sources of information. This included asking village panels about sources of conflicts, and pat- terns of resolution, including recourse to ADR, and surveying household heads about their experiences with the judicial system – formal and informal.

Our analysis is thus based on village and household survey data, as well as on two complementary if rather limited sets of court records. We tried to collect characteristics of all cases brought to court in nine chiefdoms bordering and adjacent to the Gola Forest for the periods covered by extant records. These data cover all cases we could retrieve for the period 2000–10, but there were only a few cases available before 2004. These data form a monthly panel by which we can analyse variation in 'woman damage' cases through time. In addition, we used court data from a survey commissioned by the World Bank in 2006–7.[93] This survey extended the range of our analysis to 26 chiefdoms across Sierra Leone (albeit for a smaller timeframe), allowing us to probe spatial variation in 'woman damage' for a sample of 363 cases. These spatial data confirmed that 'woman damage' cases are especially numerous in the Gola Forest region.

Turning to our village and household data, we implemented our survey (2,239 households, 178 villages) in seven chiefdoms around the forest during the first half of 2010. We asked questions about local disputes and modes of dispute settlement over the 2000–10 period, and collected data on 3,202 cases for that period. We also organized focus groups and asked village stakeholders (typically including the town chief and deputy chief [speaker]) to list the most important conflicts in the village during the same

[92] In 2006 one of us (Paul Richards) commissioned a preliminary survey of post-war records from three local courts (Kakuwa, Valunia, and Dama chiefdoms) for the period 2001–6. See Chauveau and Richards (2008). Seemingly, much of the source material for this survey is now no longer available in the courts themselves.

[93] This study, the Local Courts Record Analysis Survey in Sierra Leone 2007, was carried out by Braima Koroma of Njala University, under the Justice Sector Development Program (World Bank). For further information see <http://www.britishcouncil.org/jsdp. htm> (6 April 2011).

period. In addition we use land use data from a chiefdom survey carried out in 2009.[94]

The data provide a typology of the civil and criminal cases recorded at the court, village, and household level.[95] The most frequent types of civil cases handled in local courts are those over business transactions (typically disputes over trade in agricultural produce – 41 percent of all cases). 'Woman damage' cases (29 percent) are second in importance, and bad debts (24 percent) third. These data constitute a national picture, as rep- resented by 26 chiefdoms in the amalgamated data set. For the household and village survey data sets covering the Gola Forest region we document that 'woman damage' cases are the most important form of local conflict (28 percent of all cases), followed by land and debt cases.[96]

The predominance of the village (informal) level in the settling of certain disputes should be stressed. These data also show the level of arbitration of all cases in our household survey. The type of dispute matters. While the great majority of land cases are settled informally, a large proportion of business cases are brought to court (hence the discrepancies in percentages across columns). Overall, however, the vast majority of these cases are settled in the village, mediated by family members, elders, and village chiefs, and only 4 percent of all cases were settled in the chiefdom local court. From our data it appears that the village chief is a key actor in local arbitration (settling 40 percent of all cases, and 45 percent of all 'woman damage' cases). It is important to stress that 'woman damage' rises in importance as we drop from the formal level of local courts to the ADR level. This is not a phenomenon limited to a few high profile cases at the chiefdom level, but is a basic element in the fabric of local conflict found throughout the villages of the Gola Forest region.

Part of the settlement includes levying fines. Our data suggest there is substantial variation in fines. In local courts, a part of the fine is paid to the court itself, on average Le20,000 (c. US$5), though this varies from zero to Le355,000 (c. US$89). Fines to plaintiffs reflect the seriousness of the offence, and can reach up to Le721,000 (c. US$180). The (unweighted) average level of fine for all offences is Le86,000 (c. US$22). People found guilty of 'woman damage' pay an average fine of Le120,000 (c. US$30), 40 percent higher than the average for all offences, and a steep sum given that average non-

[94] Unpublished survey carried out for the Ministry of Agriculture and Food Security of the government of Sierra Leone, in cooperation with the Food and Agriculture Organization of the United Nations, 2009.
[95] The raw data collected are summarized in four tables placed in an appendix at the end of the article. Key aspects of these data are discussed here and also presented graphically in Figure 1, while the relationships are tested and described in the sections which follow.
[96] Abusive language to an elder (cf. 'sauce palaver', Winterbottom, An Account, p. 128) and breaking bye-laws are the most important criminal cases.

skilled earnings in rural Sierra Leone amount to about US$1.25 per day. At the village level, 'woman damage' fines also seem to vary with respect to the social relationship between the accuser and the accused. Strangers within the village (Mende: hota) pay more than citizens (Mende: *tali*), and the highest rates are paid by accused persons from outside the village.[97]

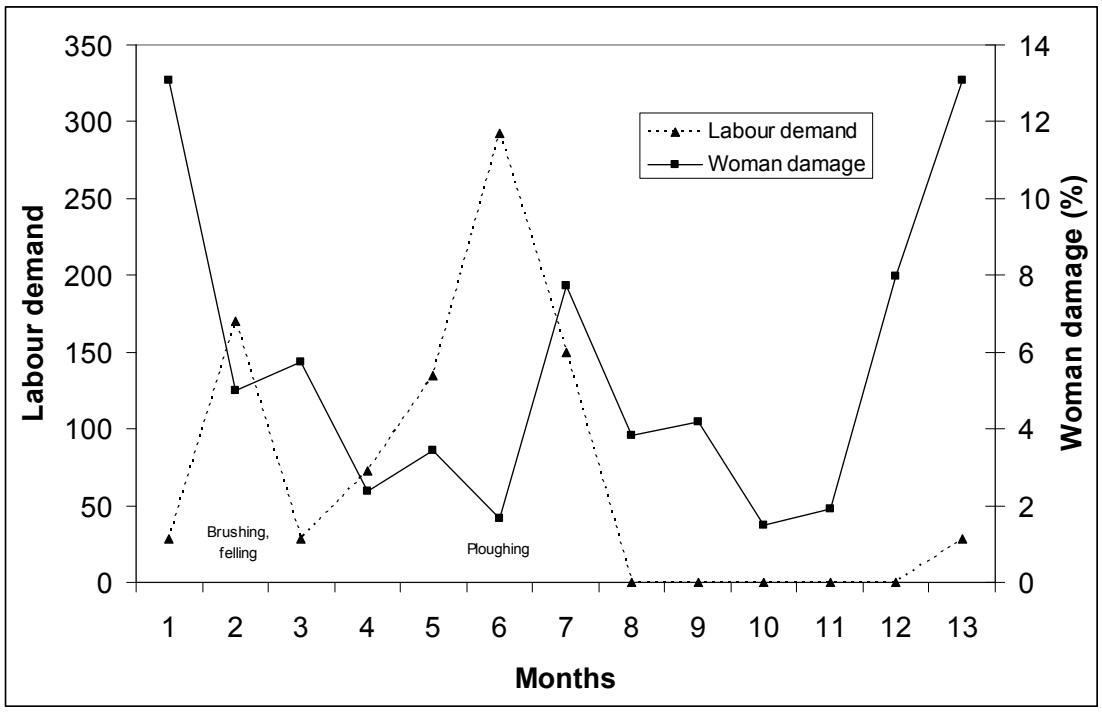


Figure 1: The number of woman damage cases and upland farm labour demand for brushing and "ploughing"

There is both regional and seasonal variation in 'woman damage' cases. 'Woman damage' cases take up the largest share of the case load in Kailahun and Pujehun districts. These are the districts bordering Liberia, north and south of the Gola Forest, from where the war was launched, and where up to half the rural population in some chiefdoms had slave status at emancipation in 1928 (Grace 1977). Figure 1 thus summarizes the case load of courts by month, revealing a clear seasonal pattern – 'woman damage' accusations are predominant in the December/January period and then again in July. These spikes in 'women damage' cases coincide with the months in which demand for (male) labor on upland rice farms peaks. During January–March, male labor

[97] Some accusations also involve members of the same family group. This requires further work, since it is broadly our hypothesis that 'woman damage' cases will involve plain- tiffs with higher social status and defendants with lower. But it is worth noting that variation in social status occurs at an intra- as well as inter-lineage level. It is conventional not to mention slave origins or client status, so underclass elements may simply appear 'members of the family' to the outsider.

is in peak demand for brushing and felling of new upland farms. Male labour demand for rice 'ploughing' (that is, planting) peaks in June–July.[98]

To test our main hypothesis about the association between 'woman damage' allegations and demand for labor, we use our chiefdom, village, and individual level data, as summarized in Table 2, and adopt a two- pronged identification strategy, testing for the spatial and time relationship between demand for agricultural labour and woman damage cases. That is, we analyse: (1) the pattern of 'woman damage' convictions over space, and see if more people are convicted in regions where demand for cheap farm labour is greatest, and (2) the pattern of 'woman damage' cases over time, and see whether this pattern matches demand for male labour in agriculture, as dictated by the agricultural calendar.

We then examine the relationships between these data. The core of our empirical strategy consists of two types of model.[99] First, we estimate the impact of labour demand on 'woman damage' cases, using cross-section data collected in 26 chiefdom courts and in 178 villages. The latter sample includes both court data and information on informal dispute settlement. Second, we use panel data on court cases in the nine chiefdoms. Monthly data are available for the period 2004–10, and we aim to explore whether there is a correlation between 'woman damage' cases and demand for upland agricultural labor (as dictated by the agricultural calendar). We pool data for the period 2004–10 and sort them by month (our regression results are unaffected if we include annual dummies to pick up year effects).

Finally, and as an extra analysis for robustness, we also probe the characteristics of 'woman damage' accusers and accused, to explore whether these characteristics match anthropological evidence about local grievances discussed above.

[98] The data on peaks in labour demand rest on detailed work undertaken on 98 upland rice farms in the Mende-speaking village of Mogbuama in central Sierra Leone in 1983 (Richards, 1986). It is an obvious weakness of the present study that we have no figures for current farm labor inputs for villages in the Gola region, and hope to remedy this in future work. Mogbuama has a lot of low-lying moisture retentive soil, and the 'ploughing' peak is 2–3 weeks earlier than on the free-draining soils of the relatively hilly Gola Forest region. This perhaps explains why the peak for 'woman damage' (Figure 1) appears to lag the ploughing labour peak by a week or two.

[99] See Appendix for full details

Table 2: Descriptive Characteristics

Variable	Obs	Mean	Std. Dev.	Min	Max
Panel A Chiefdom level					
Relative number of woman damage to total court cases	26	0.35	0.32	0	1
Upland area farmed >30% of total chiefdom	27	0.48	0.51	0	1
Cash crop area farmed >30% of total chiefdom	27	0.41	0.50	0	1
Panel B Village level					
Number of woman damage cases	161	0.52	0.33	0	1
Upland area farmed to total farm land	146	0.39	0.20	0	0.83
Plantation area farmed to total farm land	146	0.39	0.28	0	1
Swamp area farmed to total farm land	146	0.23	0.18	0	1
Village population 2010	174	386.84	488.36	10	3000
Males to land ratio	163	0.55	1.01	0	8.57
High prices in 2009	165	0.63	0.48	0	1
Fraction polygamous households	145	0.22	0.16	0	1
Panel C Individual level					
Accuser of woman damage	346				
Accused of woman damage	555				
Respondent is male	901	0.48	0.50	0	1
Respondent age	901	22.69	18.91	0	118
Respondent age^2	901	872.43	1300.93	0	13924
Respondent is literate	901	0.33	0.47	0	1
Upland farmed (acres)	901	2.01	2.56	0	25
Plantation farmed (acres)	901	7.38	8.61	0	282
Swamp farmed (acres)	901	0.99	1.34	0	10
Respondent is married	901	0.14	0.34	0	1
Number of wives	901	0.18	0.50	0	6
Non agricultural income (log)	901	12.12	1.42	0.69	21.42
Number of conflicts in household	901	1.29	1.36	0	8

Source: Courts Survey 2010,World Bank Courts Survey 2007, MAFFS/FAO Chiefdom Survey 2009, Village Survey 2010, Household Survey 2010

6.2 Empirical results

Our data show a considerable amount of variation in agricultural production and polygyny, labor demand, and 'woman damage'. In this section we investigate the pattern of 'woman damage' cases across space and time.

6.2.1 'Woman damage' in local courts

Table 3 summarizes our results[100] for our two sets of court data. Across all columns we control for district fixed effects, which capture all relevant variables that are invariant across communities in the same district. Column 1 is a simple ordinary least squares (OLS) model based on the cross-chiefdom data (for 23 chiefdoms) provided by the World Bank.

Columns 2 and 3 present outcomes of monthly panel models, based on the data collected in nine courts. The difference between columns 2 and 3 is that in the latter model we include both a dummy for upland farming, as well as a measure of the next month's labor demand.

[100] By estimating equations 1 and 2 detailed in the technical notes in the Appendix

Table 3: Woman Damage Across Chiefdoms and Time (Court data)

Dependent variable:	(1) percentage woman damage cases	(1) percentage woman damage cases	(2) percentage woman damage cases
	ols	tobit	tobit
Upland area farmed >30% of total chiefdom farm land	0.419** (0.188)	.	0.827* (0.429)
Cash crop area farmed >30% of total chiefdom farm land	0.113 (0.252)		-0.117 (0.630)
Next month's labour demand		0.209* (0.124)	0.241* (0.139)
District FE	yes	yes	yes
Constant	-0.0426 (0.227)	-0.241** (0.105)	-0.580 (0.643)
N	26	200	200

Standard errors in parentheses, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

The results support the hypothesis that 'woman damage' is associated with demand for male labor. The frequency of 'woman damage' cases systematically varies with the amount of upland farm land cultivated in a chiefdom (columns 1 and 3, for evidence on 26 and 9 chiefdoms, respectively) as well as with the seasonal demand for labor (columns 2 and 3) – chiefdoms with more than 30 percent of their farm area used for upland farming have significantly more 'woman damage' cases and, similarly, the number of 'woman damage' cases in months preceding peak labor demand is higher than in other months. Interestingly, our data suggest a sort of 'threshold effect'. That is, a minimum area of upland farm land is necessary for a positive correlation between 'woman damage' and upland farming to eventuate – simply inserting a continuous variable indicating the percentage of upland farming yields a positive coefficient, but one that is not significantly different from zero. We find that our result is rather robust to varying the level of this upland farm land threshold (to 20 percent or 40 percent of farm land per chiefdom, for example).

In general it requires a leap of faith to jump from correlations to causal inference. Our assumption that demand for labour translates into more 'woman damage' cases might be countered with the view that a supply of cheap labour (via the judicial system, from commutation of fines) is used, opportunistically, to enlarge the area of upland farming[101] In other words, causality might actually run in the opposite direction to the one we here suppose. However, this seems unlikely, since the double-peaked pattern of seasonal labour demand on upland rice farms is exogenous to the workings of the judicial

[101] Patrons or relatives sometimes help the accused by what is known as 'buying the case' (covering the fine). Some then recoup their losses by accepting help on their farms in lieu of repayment of loans.

system. Conceivably, a riotous Christmas might lead to a large spike in adultery cases, paid for by work opportunities in the brushing season that immediately follows, but it would be hard to explain why there was a similar spike of 'woman damage' accusations in the planting season, at a time of year when food, energy, and leisure opportunities are in notoriously short supply for both men and women. The fact that the spikes in 'woman damage' cases track a double peak in demand for the labour of young men on upland rice farms seems to help confirm the hypothesis that labour demand is the driver of the fluctuation in court activity.

6.2.2 'Woman damage' at village level

We now turn to the household and survey data, and examine the corre- lation between 'woman damage' and various village characteristics. The dependent variable across all models is the percentage of households in the village accused of at least one case of 'woman damage'.[102] Across all specifications we control for chiefdom fixed effects (Gaura, Koya, et cetera), capturing invariant variables across communities within the same chiefdom.

Our estimation results corroborate our earlier findings. As above, 'woman damage' cases are more prevalent in villages with a greater percentage of upland rice farmland – specifically, a 10 percent increase in the share of upland farmland results in a 3.4 percent increase in 'woman damage' cases. The same is not true for swamp land, where a crop tends to be owned individually, where labour peaks fall differently, and where farming activities are less implicated in forging household cohesion and reproduction of family groups (see above). In addition, we find that 'woman damage' cases are more frequent in small villages; these are typically remote places, at the forest frontier, where male labour demand for farm brushing and (especially) felling is greatest. These are also villages where total 'free' labour supply is very limited, sometimes to the point where there is no labour market at all in peak seasons (Compare Richards 1986). All labour is tied up by own land work or contracted via membership in peer-managed rotational labour teams, and elders resort to the judicial system to break into this 'locked-down' labour source (idem). Pursuing action for 'woman damage' is in effect a means for an elder to force a young man to abandon work on his own farm or give up his turn in a pre-contracted

[102] Because of multicollinearity we cannot include all village variables simultaneously (for example, there is a mechanical, negative association between the percentage of farmland, plantation land, and swamp land).

labour group.[103]

Table 4: Explaining the incidence of woman damage (village data)

Dependent variable: percentage of households with at least one woman damage case 2007-2010					
	(1)	(2)	(3)	(4)	(5)
Percentage upland farmland	0.342**			0.430**	0.334**
	(0.152)			(0.167)	(0.152)
Percentage plantation land		-0.276**			
		(0.118)			
Percentage swamp farmland			0.146		
			(0.175)		
Village population				-0.000234**	
				(0.0000900)	
Fraction polygamous Households				0.204*	
				(0.124)	
Males to land ratio					-0.0567*
					(0.0311)
Very high yields in 2009					0.108*
					(0.0631)
Gaura	0.217**	0.260**	0.257**	0.290	0.285**
	(0.104)	(0.104)	(0.108)	(0.409)	(0.109)
Koya	0.131	0.186	0.159	0.156	0.162
	(0.113)	(0.115)	(0.120)	(0.114)	(0.117)
Makpele	0.0711	0.0896	0.126	0.0324	0.121
	(0.101)	(0.0992)	(0.100)	(0.105)	(0.104)
Malema	0.0976	0.155	0.124	0.0963	0.194*
	(0.0950)	(0.0984)	(0.103)	(0.0946)	(0.103)
Nomo	-0.0168	0.0214	0.0357	-0.0425	0.0748
	(0.114)	(0.112)	(0.116)	(0.114)	(0.121)
Tunkia	0.0222	0.0683	0.0525	-0.0104	0.0462
	(0.118)	(0.119)	(0.123)	(0.118)	(0.131)
Constant	0.323***	0.524***	0.391***	0.186	0.110
	(0.0891)	(0.0789)	(0.0955)	(0.153)	(0.148)
N	130	130	130	111	115
adj. R^2	0.037	0.040	0.002	0.077	0.115

Standard errors in parentheses, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Interestingly, and also consistent with our hypothesis, we find that large yields on upland rice farms are associated with more frequent 'woman damage' cases, perhaps because the extra labour thereby secured ensures more timely brushing and planting in relation to irregularly advancing rains. Lack of timely burning or planting is a major factor in poor yields (Richards 1986). 'Woman damage' cases are also more common in areas where male labour is more scarce and in areas where higher levels of polygyny occur (as proxied by the percentage of households with more than one wife).

Finally, an important negative result is presented in column (2) of Table 4. There

[103] In future work we plan to follow payment of fines in kind from 'woman damage' cases to see if they lead directly back to farms.

are fewer cases for 'woman damage' where there is a greater percentage of plantation land. Plantations require regular maintenance, including brushing in the early dry season. There is much coffee and cocoa plantation land around the Gola Forest – abandoned and over- grown in the war – that is now being renovated with great difficulty, due to labour shortage. Lack of significant positive association between 'woman damage' and plantations would be enough to refute the idea that 'woman damage' is a correlate of agrarian labour shortage in general. All the specific associations are, in fact, between 'woman damage' and house- hold upland rice plantations. The woman's dereliction is a threat to the household group – and specifically to the large polygynous household group. Conversely, trapping her paramour to work as a temporary son-in-law strengthens the large polygynous household. The issue, as Crosby rightly surmised, is the inequality implied between those men who can and cannot command large polygynous households. However, a negative correlation between 'woman damage' and area devoted to cash-crop plantations, as in Table 4, might say something new – that traditional sanctions become less important when the cash nexus intervenes.

6.2.3 'Woman damage' at individual level

Our final set of evidence derives from the household survey data, which allow us to identify the characteristics of both 'woman damage' accusers and the individuals accused. The anthropological data (from Crosby, Little, and Bledsoe) imply that the accusers will be older, wealthier, poly- gynously married men, and that the accused will be younger, poorer, and perhaps unmarried men. Our sample contains information about 349 accusers and 555 accused individuals. We estimate a series of (logit) models to see which individual variables differentiate between the two groups. Our main results hold up when we include village fixed effects, controlling for (unobserved) village-level variables.

Interestingly, while both accusers and accused appear rather similar across a few dimensions (income, marital status, and number of conflicts in the households), there are crucial differences between the two groups consistent with our overall hypothesis. First, while the upland farm area enters positively for accusers (implying large demand for male labour), this is not the case in the models identifying the accused. Indeed, in the models for the accused (columns 3 and 4), upland farm area enters nega- tively. The smaller their upland farm the more likely they are to be accused.[104] This finding seemingly underscores

[104] Chauveau and Richards (2008), note that only 4 percent of men accused in their sample of woman damage cases from three courts in central and eastern Sierra Leone were acquitted. Marginality is apparent in the fact that (on these figures) to be accused is to be guilty.

the fragile and marginal position of these individuals in local society. This is consistent with the observation that the literacy variable enters with opposite signs for the accusers and accused, suggesting that those unable to read or write are more likely to be charged with 'woman damage' accusations (even if this difference is not significant at conventional levels).

Table 5: Profile of woman damage accuser and accused (individual data)

Dependent variable: accuser or accused of woman damage case 2007-2010				
	(1)	(2)	(3)	(4)
	accuser	accuser	accused	accused
	logit	logit	logit	logit
Age at accusation	0.0260***	0.0375***	-0.0192***	-0.0277***
	(0.00701)	(0.00938)	(0.00678)	(0.00907)
Respondent is literate	0.0965	0.231	-0.204	-0.296
	(0.175)	(0.236)	(0.171)	(0.230)
Upland farmed (acres)	0.0739*	0.113**	-0.0769**	-0.138**
	(0.0393)	(0.0526)	(0.0391)	(0.0536)
Plantation farmed (acres)	-0.0129	-0.00865	0.0150	0.0131
	(0.0113)	(0.0115)	(0.0116)	(0.0132)
Swamp farmed (acres)	0.141*	0.142	-0.123	-0.104
	(0.0825)	(0.113)	(0.0811)	(0.113)
Number of wives	0.277*	0.534**	-0.306*	-0.651***
	(0.167)	(0.217)	(0.168)	(0.219)
Non agriculture income (log)	0.104	0.110	-0.0777	-0.0677
	(0.0641)	(0.0851)	(0.0623)	(0.0840)
Number of conflicts household is involved in	0.0693	0.0160	-0.0373	0.0113
	(0.0623)	(0.0832)	(0.0613)	(0.0825)
Village FE	no	yes	no	yes
Constant	-3.324***	-3.766**	2.475***	1.614
	(0.854)	(1.651)	(0.822)	(1.643)
N	641	556	643	551
adj. R^2	0.06	0.17	0.05	0.16

Standard errors in parentheses, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Second, we find that accusers tend to be older than the accused. This is consistent with the gerontocracy assumption behind our hypothesis. Third, we find that men with multiple wives are more likely to be accusers. This result is consistent with the 'honey trap' perspective offered by Crosby, Little, and Bledsoe, in which polygamously married men use their wives as bait to gain access to cheap labor from fellow villagers. However, and somewhat unexpectedly, we observe that marital status enters significantly and with a positive sign for both accusers and those accused. This would indicate that the 'honey trap' at times captures some married men as well as marginal unmarried youth.[105]

[105] Although an elder fishing for labour expects to land mainly unmarried men, we presume he does not mind if the catch is bigger. It is the fine, or labour in lieu, that interests him.

6.3 Discussion and Conclusion: Peasant Grievance Feeds Conflict?

Our data show that 'woman damage' remains a major component of social conflict at all levels of the rural social system in the Gola region. Furthermore, we have shown that the hypothesis – originally propounded by Crosby – linking 'woman damage' to high rates of polygyny and labour demand on household upland rice farms receives substantial statistical support from analysis of both recent court data and information derived from a large random sample of rural households. Findings concerning social asymmetry between accuser and accused in 'woman damage' cases are consistent with the claim that elders control the labour and reproductive opportunities of cadets. Our study thus backs up claims of agrarian grievances reported by some ex-combatants, and also supports anthropological models of class-based social reproduction in ranked lineage society (Richards 2005, Dupre and Rey 1973).

We should point out, however, that the mechanism of agrarian inequality is specific to polygyny and upland rice farming (that is, to an agrarian subsistence-oriented mode of production). We find no evidence that 'woman damage' is a more general mechanism for agricultural labour exploitation. Indeed, our findings of no significant relationship between 'woman damage' and swamp rice cultivation, and of a negative relationship between 'woman damage' cases and plantation size (as a proxy for labour demands in cash crop production), underline the specificity of this type of exploitation. This might in turn be taken to support a conclusion that grievances formed through 'woman damage' accusations will eventually be mitigated via broader participation in crop markets.

If this is the best eventual treatment, it is nevertheless worth noting a difference between post-war strategy in Liberia and Sierra Leone. In Liberia, the government has moved to reform customary law, and it is no longer legal to pursue 'woman damage' cases.[106] In Sierra Leone, customary institutions have been rebuilt with donor assistance, and appetite to sue for 'woman damage' remains high, especially in the Gola Forest region, where the subsistence-oriented farm economy continues to make young farm laborers into serfs.

This returns us to the two basic models of explanation mentioned at the outset of this article – greed-not-grievance and peasant insurgency. All insurgencies require a material resource base, so proponents of the greed-not-grievance hypothesis need to show that they are not just measuring the tendency for insurgency and resources to be

[106] See Republic of Liberia, Act to Govern the Devolution of Estates and Establish the Rights of Inheritance for Spouses of Both Statutory and Customary Marriages, House of Representatives, 7 October 2003 (Ministry of Foreign Affairs, Interim Government of National Unity, Republic of Liberia, Monrovia).

correlated (that, in effect, war necessitates greed). In treating violence as an expressive means, the peasant insurgency model follows a different line of causal reasoning. Here, it is hypothesized that the recurrent marginalization and humiliation of exploited fractions of backward agrarian society repeatedly erupts into a violence that seeks – either physically or symbolically – to challenge and invert an oppressive social order.

In Ranajit Guha's work the model is tested (qualitatively) by the accumulation of what he terms (echoing Durkheim) the elementary [or generalizable] aspects of more than one hundred documented peasant insurgencies in South Asia (Guha 1999). Struggle over land and rent is a major focus. There is much less historical material on African agrarian uprisings, but enough is known to attest that these focus on reversing the privileges of those who monopolize labour rather than land. In other words, in a land-abundant forest region, such as Sierra Leone, the tendency has been for peasant insurrection to assume the form of the slave revolt.[107] What our analysis in this article does is to establish that agrarian grievances over labour exploitation in the Liberian borderlands of Sierra Leone were, and remain, real. It requires a further step in the argument to link these grievances to the actual circumstances of the recent Sierra Leone insurgency. This gives us the topic for a brief concluding discussion.

What is needed is a better, more explicit model of how a mechanism of peasant-upon-peasant conflict might have been engaged in the social- political circumstances of the Sierra Leone insurgency in the 1990s. Is there evidence that the intake into the RUF was significantly agrarian in background, and is it plausible to suppose that the leadership of the RUF responded to issues of peasant injustice?

Humphreys and Weinstein offer important information on the make-up of the RUF. Farmers comprised the second largest occupational group (about one in five of all recruits). The largest occupational group (42 percent) comprised school pupils (Humphreys and Weinstein 2004) At first sight, this might suggest privilege, rather than exploitation, until attention is paid to timing and context.

Peak years for recruitment into the RUF were at the outset (1991–2). It follows that many of the schools raided by rebels for recruits were to be found in the isolated, forested borderland region. These were, in fact, among the most run-down and neglected in the country (Richards 1996). Larger land holders in the area were often short of household labour on farms because their own children had been sent to better schools in town. In the hope of accessing educational basics poorer families in the outlying (former

[107] For a late eighteenth-century instance in Sierra Leone, see Mouser (2007).

slave) villages frequently fostered their offspring with these larger land holders, for whom the children did farm and household work while also attending school.[108] The arduousness of Mende child fosterage is well documented (Bledsoe 1990). If we add together exploited farmers and ill-treated school children, this contributes to the RUF a sizeable intake of aggrieved rural young people potentially spoiling to overturn the existing village order. It would not be hard to see why the movement might have then shifted from marching on the capital towards peasant rampage.

On the responsiveness of the RUF leadership to the concerns of its peasant intake we have until recently lacked any direct information. That the movement undertook a major agrarian 'turn' after the failure of its early campaign (1991–2) is for the first time apparent from recent work with former cadres by Krijn Peters (2011). Peters makes it clear that this development in part reflected necessity (the movement needed to feed itself), but was also to a significant degree a result of internal ideological realignment around issues of peasant oppression. The intake of a number of 'organic intellectuals' associated with the rural teacher training college at Bunumbu seems to have been an important factor in this shift of focus (Richards 2001).

It also seems reasonable to infer that as the original Libyan-inspired student element in the RUF dropped away, and the intake of disgruntled village youths increased, Foday Sankoh – now the movement's undisputed leader – adapted his style to the needs and concerns of new peasant followers. From Guha's analysis it is clear that leaders of Indian peasant insurgencies were often charismatic figures who communicated with their movements through quasi-religious means. Sankoh was of this type – saintly to his followers and given to occult and prophetic utterance.

Thus, if the RUF war began as a revolt of young intellectuals, it soon took on the features of a classic peasant revolt, aimed not at conquering the commanding heights of the state but at smashing and overturning an agrarian world in which landholders had over-taxed young laborers, thus explaining the horrific intensity of the violence visited upon rural communities the RUF had originally set out to redeem. This, in broad outline, is the kind of explanatory vehicle into which we envisage fitting the mechanism of judicial oppression demonstrated in this article.

[108] In addition, Humphreys and Weinstein that a third of all ex-combatants were orphans, and that '*controlling for age, lower ranked combatants were especially likely to be drawn from cohorts that had lost their mothers before the conflict started*' (2004: 24). Loss of a parent increases the chances of being fostered.

Appendix: Data and Details about Identification Strategy

Table A1: Types of Cases

	Court data		Village data		Household data	
	Freq.	Percent	Freq.	Percent	Freq.	Percent
Panel A Civil cases						
Land	8	2.20%	68	16.79%	676	21.11%
Woman damage	105	28.93%	117	28.89%	895	27.95%
Debt Money / Credit	88	24.24%	36	8.89%	566	17.68%
Business transactions (i.e. debt in kind)	148	40.77%	49	12.10%	144	4.50%
Religion					49	1.53%
Witchcraft					79	2.47%
Labour			49	12.10%	363	11.34%
Alcohol					44	1.37%
Bush fire					99	3.09%
Gambling					41	1.28%
Theft	14	3.86%	76	18.77%	215	6.71%
Other			10	2.47%	31	0.97%
TOTAL	363	100%	395	100%	3202	100%
Panel B Criminal cases						
Abusive language / slander	99	32.46%	Na		1028	44.52%
Breaking bylaw (not doing community work, disobeying chief, failure to pay tax, ..)	94	30.82%	Na		1086	47.03%
Damage to property	11	3.61%	Na			
Physical abuse	45	14.75%	Na		136	5.89%
Contempt of court	11	3.61%	Na			
Other, specify	45	14.75%	Na		59	2.56%
TOTAL	305	100%	Na		2309	100%

Criminal cases not covered in village survey
Source: Courts Survey 2010,World Bank Courts Survey 2007,, Village Survey 2010, Household Survey 2010

Table A2: Level of Arbitration Civil Cases

	All cases		Woman damage cases	
	Freq.	Percent	Freq.	Percent
1. Amongst ourselves	483	15.37%	90	10.19%
2. Family elders	526	16.74%	124	14.04%
3. Village elders	311	9.90%	106	12.00%
4. Religious leader	42	1.34%	7	0.79%
5. Village / town chief	1254	39.91%	397	44.96%
6. Section chief	248	7.89%	78	8.83%
7. Paramount chief	110	3.50%	30	3.40%
8. Chiefdom court	118	3.76%	44	4.98%
9. Magistrate court	22	0.70%	4	0.45%
10.Other, specify	28	0.89%	3	0.34%
TOTAL	3142	100%	883	100%

Source: Village Survey 2010, Household Survey 2010

Table A3: Fines (in Leones)

Variable	Obs	Mean	Std. Dev.	Min	Max
Panel A Court data					
All civil cases					
- paid to court	382	19582.46	27924.4	0	355000
- paid to plaintiff	338	86248.52	110489.7	0	721000
Woman damage cases					
- paid to court	78	19198.72	15108.98	2.000	100000
- paid to plaintiff	60	120066.7	101097.1	5.000	560000
Panel B Household data					
All civil cases	2603	52545.56	109210.1	0	2000000
All civil cases (fine > 0 Le)	1822	75069.2	123896.5	15	2000000
All woman damage cases	797	62272.42	106085.5	0	1000000
Woman damage case with					
- Household member	58	39034.48	96710.27	0	600.000
- Extended family member	102	68049.41	149127.6	0	1000000
- Same village (not stranger)	379	59612.14	93432.8	0	1000000
- Stranger in same village	123	62536.59	91176.11	0	400000
- Other village, same section	91	81626.37	117455.3	0	600000
- Other section, same chiefdom	18	36666.67	29555.53	0	100000
- Other chiefdom	14	69862.5	91954.52	75	300000

Source: Courts Survey 2010, Household Survey 2010 (exchange rate at the time of survey c. Le 3500/$US 1.00)

Table A4: Cases by district

	All cases		Woman damage cases		
District	Freq.	Percent	Freq.	Percent	as a percentage of all cases in district
Kailahun	85	14.2%	16	16%	19%
Kenema	190	31.8%	26	26%	14%
Koinadugu	29	4.9%	2	2%	7%
Pujehun	264	44.2%	52	52%	20%
Tonkolili	29	4.9%	4	4%	14%
TOTAL	597	100%	100	100%	100%

Source: Courts Survey 2010,World Bank Courts Survey 2007,

Technical specification of our identification strategy:

As indicated in the main text we have estimated a series of models.

First, we use a cross-section sample of data from 26 chiefdoms for the period 2006-7 and regress the number of woman damage cases on our proxy for the demand for agricultural labour:

$$WD_i = \alpha_i + \beta_1 RFA_i + \beta_2 C_i + \varepsilon_i , \quad (1)$$

where WD_i is the relative number of woman damage cases to total cases for court i (i = 1,…,26), RFA_i is the percentage upland farmland to total land in the court area,[109] C_i ais a vector of Chiefdom controls, and ε_i is a random error term.

Second, we estimate a simple panel model for the nine chiefdoms for which we have monthly data (period 2004-2010), and zoom in on the agricultural calendar:

$$WD_{it} = \alpha_i + \beta_0 L_{t+1} + \beta_1 RFA_i + \beta_2 C_i + \varepsilon_{it} , \quad (2)$$

where WD_{it} now refers to the relative number of woman damage cases to total civil cases in Chiefdom i (i = 1,…,9) in month t (t = 1,…,12). We pool data for the period 2004-2010 and sort them by month (the results are unaffected if we include annual dummies to pick up year effects). In (2), L_{t+1} is (next month's) agricultural labour demand in month t+1, to capture the idea that large landowners anticipate next month' s labour demand and seek to secure access to labour. For details on our measure of labour demand, refer to explanatory footnote 18.

Third, we re-estimate equation (1) for our sample of 178 villages, and analyse how the total number of 'woman damage' cases (i.e. handled in court or otherwise) varies with key village, household and individual characteristics.

[109] Population pressure in the area around the Gola Forest is relatively low, so there may still be some unused land (i.e. land not part of the rotational cycle) in some communities. Forest reservation has accounted for any large areas of unused land, so areas still accessible to villagers are likely only to be small. We implicitly assume that the amount of unused land is not correlated with the number of woman damage cases. Note that this potential concern about endogeneity of the land use variable is attenuated in the panel regressions where we consider communities over time.

Chapter 7

Experiments and the Incentives for Conservation:

Using Artefactual Field Experiments to Learn about the Incentives for Sustainable Forest Use in Developing Economies

Publication Information: Voors, M., T. Turley, A. Kontoleon, J. List, and E. Bulte, 2011. Public Good Games and Behavior in the Forest: Evidence from Sierra Leone. American Economic Review (papers and proceedings), 101(3): 329–33

7.1 Introduction

One vexing problem facing mankind is how to promote sustainable use of common property resources. Within developed and developing countries alike, misuse of fresh water, pastures and forests is commonplace. While developed countries have in many cases designed principles and laws to promote sustainable resource use, developing countries often lack the institutional know-how and means to enforce property rights. This has lead researchers to look for alternative means to address common property resource problems.

We study behavior and attitudes towards forest conservation in Sierra Leone, and aim for two contributions related to common property resources. First, we attempt to establish if artefactual field experiments (AFEs) can explain behavior in the field. If AFEs can explain variability in treatment of the commons, then we can have greater confidence in testbedding mechanisms in the laboratory and transferring the lessons learned to naturally occurring settings. This approach relies on the framework of Levitt and. List (2007, hereafter LL), who propose that behavior in laboratory experiments is influenced by not only monetary calculations but also by at least five other factors: (a) the presence of moral and ethical considerations, (b) the extent to which one's actions are scrutinized by others and the nature of that scrutiny, (c) the context in which the decision is embedded, (d) the subject pool of respondents, and (e) the stakes of the game.

The most straightforward version of the LL model predicts that behavior in AFEs will correlate perfectly with behavior in the field so that those who exhibit stronger social preferences in the lab will also do so in the field. The factors in the LL model serve as shift parameters, and if these shifters are isomorphic across people, then relative rankings are preserved across lab and field settings. The older psychology literature and more recent experimental work in economics provide evidence that suggests one should reject this most straightforward version of their model (see, e.g., Bouma *et al.* 2008). Allowing heterogeneity in the shift parameters yields potentially very different relative rankings across the lab and field. Given that we both i) observe people in the lab (in AFEs) and ii) gather information on their behavior in the field (in a social intervention and in related surveys of household behavior), we can test whether relative rankings are preserved between the lab and field (see also Anderson *et al.*, 2010, Carpenter and Seki, 2010, and Rustagi *et al.* 2010). If relative rankings are not preserved we can examine which observables explain heterogeneity in the shift parameters.

Our second contribution is to examine if social phenomena that are rarely explored, such as having a relative killed in civil war or living in a community with strong beliefs in witchcraft, directly influences social preferences and treatment of the common property resource. People in the Gola Forest region of southeastern Sierra Leone experienced high levels of violence during the civil war of 1992 to 2002. Witchcraft is also a significant problem in this area, with accusations and hiring of witch doctors a common occurrence.

7.2 Experiments and Data

We collected experimental and survey data in the region of the Gola Forest Reserve, in southeastern Sierra Leone. The Gola Forest is one of the largest remnants of the Upper Guinea forest in West Africa, a major stock of standing carbon, and a global biodiversity hotspot. However, it is located in one of the poorest regions in the world, torn by a recent civil war. Local populations depend to a large extent on forest-related goods and services. The Gola Forest Reserve was established in the 1920s, and restricts the exploitation of resources within the designated area of the reserve. National laws dictate forest use and extraction behavior outside the reserve as well. While strictly speaking the reserve is located on private lands, it is essentially a regulated common pool resource providing locally and globally valued ecosystem services. In what follows we focus on extraction behavior and conservation efforts with respect to the forest reserve.

We ran two different field experiments related to public good games in the summer of 2010 in 35 villages across the seven chiefdoms comprising the Gola Forest region. These villages lie within one mile of the Gola forest. In total, 632 households participated in these studies. The first experiment was a social intervention meant to provide livelihood aid and mimic a public goods game ("PG Aid"). We endowed a representative from each household with $20 (or 80,000 Le), more than a month's worth of unskilled wages. We then asked participants to divide this endowment between private goods for themselves and a community project fund for the entire village. On average, participants allocated 75% of their endowment to private livelihood support goods and 25% to a community project fund.

The second experiment was a conventional public goods game ("PGG") with three players. Players were endowed with 5 tokens. Every token kept was worth $0.25 (or 1,000 Le) and each token invested was worth half that to all players in the group. The payoff under full co-operation amounted to 7,500 Le, and the maximum possible

payoff for a free rider (assuming his peers invested their complete endowment in the public good) was 10,000 Le. On average households invested 2 tokens in each round with an average payoff of 6,000 Le.

Interestingly, and somewhat to our surprise, behavior in the two games is uncorrelated (ρ=0.04, p=0.33). The framework by LL suggests several explanations for the differences in measured behavior. The stakes are much higher in PG Aid than in PGG. Moreover, PG Aid is framed as a livelihood support initiative so the context within which the decision is embedded also varies. This suggests applying the outcomes of one particular PG experiment and interpreting them as "the proxy" of social preferences may be problematic. We therefore include both types of behavior as explanatory variables in the regression analysis that follows.

We also collected data with household and village level surveys, ascertaining demographic, socio-economic, institutional and conflict related information. We collected survey data in 25 of the 35 villages included in the study, and sampled 170 households from the 632 participating in the AFEs. The household surveys provided data on behavioral and attitudinal variables related to forest management and use. The three variables we focus on in this paper are: (i) "*commercial interests*," a variable indicating the degree to which an individual is involved in business with commercial miners, loggers or hunters—an activity that violates national law as well as local by-laws (mean: 4.5, s.d.: 1.5)[110]; (ii) "*illegal extraction*," the total per capita value (consumption and sales) of illegally hunted animals, typically endangered species (mean: 5,485 Le, s.d.: 20,430); and (iii) "*pro conservation,"* the answer to a 5-point scale question, *Do you support conserving the Gola Forest*? (mean: 3.2, s.d.: 1.5).

We include a series of conventional household controls (age, income, education) and use community fixed effects to control for inter-community heterogeneity. In addition, we include two unconventional household variables that we believe are relevant in the Sierra Leone context. First, we expect victims of civil war violence to display more pro-social behaviour (John Bellows and Edward Miguel 2006), so we control for household exposure to violence during the war in the 1990s. Second, we control for witchcraft beliefs, which we hypothesize are a mechanism to enforce social norms in Africa (Platteau 2000). While economists have studied witchcraft before (e.g. Miguel 2005), such work has focused on the causes of witchcraft accusations, not the

[110] Additive scale from 0 to 8 based on answers to, *Do you think that miners, traders and loggers are welcome in your village?* and, *Do you do business with the commercial miners, and/or hunters and/or loggers*?

consequences. Witchcraft may have evolved to enforce social order, and facilitate punishment in the event of failure to provide public goods.

The estimation strategy at this stage is exploratory and aims at unearthing robust correlations. It does not aim to fully address endogeneity issues; this is left for future work.

7.3 Regression Results

To analyze how social preferences and household characteristics are associated with forest conservation, we estimate the six models in Table I with village-level fixed effects and a range of relevant individual controls. These models include each of the three conservation outcomes identified above. To explore evidence of heterogeneity in shift parameters, as discussed by LL, we also introduce a full set of interaction terms.

Table 1: Experimental Play and Forest Conservation FE model

	(1) Commercial Interests	(2) Commercia l Interests	(3) Illegal Extraction	(4) Illegal Extraction	(5) Pro Conservation	(6) Pro Conservation
PG Aid	-0.00000307	-0.00009*	-0.395*	-2.259**	0.00000415	0.0000748*
	(0.00000940)	(0.000053)	(0.236)	(1.111)	(0.00000754)	(0.0000442)
PGG	-0.145	-0.275*	-2281.2	-2663.8	-0.213*	-0.179
	(0.108)	(0.142)	(2157.8)	(3091.1)	(0.123)	(0.158)
Age	-0.00869	-0.00805	-74.95	5.248	-0.00745	0.0250
	(0.00591)	(0.0201)	(173.0)	(443.9)	(0.00621)	(0.0197)
Education Level	0.0346	-0.669	-2001.5*	-9012.9*	-0.204***	-0.580*
	(0.0780)	(0.420)	(1136.8)	(5273.3)	(0.0710)	(0.323)
Total Household Income	0.00418	-0.0492	-349.7	-1602.4	-0.0720**	0.0536
	(0.0299)	(0.0920)	(798.6)	(2689.2)	(0.0287)	(0.106)
Witchcraft is a Problem	0.161*	0.178*	2541.0	2215.5	0.144	0.176
	(0.0985)	(0.106)	(2608.0)	(2593.0)	(0.109)	(0.127)
Family Member Died due to War	0.880*	-3.804*	-5692.4	822.4	0.545	-5.238**
	(0.473)	(2.093)	(8122.6)	(35269.1)	(0.399)	(2.112)
Age x PG Aid		0.0000004		-0.00110		-0.0000005
		(0.0000005)		(0.00933)		(0.0000005)
Education x PG Aid		0.0000200		0.269		-0.0000013
		(0.0000136)		(0.171)		(0.0000084)
Income x PG Aid		0.000002		0.100		-0.0000024
		(0.000004)		(0.0659)		(0.0000023)
Witchcraft x PG Aid		0.00002		0.649		-0.00006***
		(0.0000185)		(0.484)		(0.00002)
War dead x PG Aid		0.0000586		1.197		0.0000410
		(0.0000410)		(0.944)		(0.0000448)
Age x PGG		-0.00443		-32.06		-0.00925
		(0.00581)		(167.7)		(0.00777)
Education x PGG		0.125		1027.0		0.142*
		(0.0823)		(1254.5)		(0.0838)
Income x PGG		0.0177		-91.02		-0.0329
		(0.0263)		(648.7)		(0.0328)
Witchcraft x PGG		0.638		6080.6		0.692
		(0.497)		(5550.4)		(0.460)
War dead x PGG		2.065**		-17791.5		2.667***
		(0.951)		(14182.1)		(0.941)
Constant			13881.5	44319.4		
			(11275.4)	(28664.6)		
Community FE	Yes	Yes	Yes	Yes	Yes	Yes
N	99	99	104	104	103	103
R^2	0.06	0.10	0.19	0.25	0.09	0.14

Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

When simply considering the signs of the coefficients for the two experimental variables (PGG and PG Aid), we observe they tend to match intuition. Specifically, more "pro-social play" in PPG and PG Aid tends to be correlated with (i) a reduced willingness to interact with commercial parties with commercial interests damaging the forest and its biodiversity; (ii) a lower level of involvement in the illegal extraction of wildlife, and (iii) more positive attitudes towards forest conservation. However, the latter finding is only true for PG Aid; the reverse is true for PGG, which enters with the "wrong sign" in column (5).

This counter-intuitive result, however, should perhaps not be unexpected in light of the earlier remark about the lack of correlation between behavioral play in the two PG experiments. PGG and PG Aid enter "differently" across many of our models. While both PGG and PG Aid enter significantly in the interaction model explaining "commercial interests," we find that only PG Aid is correlated with illegal extraction. Moreover, PG Aid and PGG enter with opposite signs in the "pro-conservation" models, and only enter significantly in different specifications.

Our three dependent variables are intended to capture related dimensions of the household's willingness to contribute to the provision of a public good (sustainable forest management). However, it appears that these dimensions are somehow "distinct" in the sense that they correlate with different behavioral proxies and controls. Taken together, we interpret this as evidence that social preferences for conservation may be unstable or highly context dependent.

Turning to the issue of heterogeneity, a few insights stand out. Importantly, including interaction terms matters. For the PG explanatory variables, coefficient size tends to increase, as does the significance level. When we include interaction terms, PGG and PG Aid enter significantly in the "commercial interest" model. Including them also alters the significance of the PG proxies in the "pro-conservation" model. We interpret this as strong evidence of heterogeneity in the mapping between lab and naturally occurring behavior.

Although some of the interaction terms are significant, they are not consistently so across our dependent variables. No interaction term is significant in the illegal extraction model. Interestingly, the unconventional control variables interacted with AFE play especially seem to have explanatory power. The witchcraft and war violence interaction terms are sometimes significant, while interaction terms based on the conventional controls (age, education and income) tend to be not significant. These

controls are substantively social and we would expect them to influence the mapping of social preferences between lab and real world behavior. Education interacted with PGG also enters with a positive correlation in the pro conservation model, and it could be argued that education is also a social phenomenon. Hence, heterogeneity matters, but one should think about social factors in particular when considering the dimensions along which to explore heterogeneity.

Finally, turning to our controls, we note that age and income are not significantly correlated with common pool management and attitudes towards conservation when controlling for behavior in the experiments. Education is negatively correlated with illegal extraction, but also with pro-conservation preferences, a mixed outcome. Respondents who state that witchcraft is a problem in the village tend to be the same ones consorting with private parties with commercial interests. Perhaps this reflects that such individuals stand to lose more from informal enforcement of by-laws associated with forest conservation. Exposure to war also appears to be relevant. For example, in the "commercial interests" model it enters directly, but also via the interaction term. Interestingly, and reflecting some of the discussion above, the interaction term with PGG is significant, but the interaction term with PG Aid is not. The direct effect of war on conservation is negative, but this is mediated via increased sharing in the PGG.

7.4 Concluding Remarks

We have explored the relation between behavior in two distinct field experiments and various forest conservation behaviors and attitudes. One main result is that there does not appear to be one simple metric of pro-social preferences. There is no correlation between behavior as measured in the two experiments, and no robust pattern of correlations between play in the games and conservation behavior and attitudes in the real world.

This result is consistent with an oft-found result in psychology on the weak correlations between behaviors in different settings (even across lab settings; see, e.g., Mischel, 1968; Ross and Nisbett, 1991, for reviews). For instance, Hartshorne and May (1928) find evidence that the same people are not consistently the cheaters in different situations. For general experimental purposes, that does not mean that there is something necessarily wrong with one of the experimental situations in this study. Rather, it means that either "(a) there is not a general cross-situational trait called "[treating the commons with care]," and/or (b) the subjects view one situation as

relevant to [managing the commons] and one as irrelevant" (Levitt and List, 2007, p. 160-1).

Our second main result is that heterogeneity matters. It may not be feasible to assume a parallelism between laboratory evidence and the real world. Individuals behaving in a pro-social manner in the lab may not necessarily be the same individuals who provide most of the public goods in the field. Other mitigating factors should be identified and incorporated into models of social preferences. NGOs and government agencies should not expect to be able to testbed interventions and select the most responsive partners for project interventions based on simple behavioral games.

Chapter 8

Behavior in Context-Free Experiments is not Predictive of Behavior in the Field: Evidence from Public Good Experiments in Rural Sierra Leone

Abstract: We use a sample of subsistence farmers in Sierra Leone as respondents to compare behavior in a context-free experiment (a standard public goods game) and behavior in the field (a real development intervention). There is no meaningful correlation in behavior across contexts. This casts doubt on the prospect of using lab experiments as "predictors" of behavior in real life.

Publication Information: Voors, M.J., T. Turley, E. Bulte and A. Kontoleon, 2012. Behavior in Context-Free Experiments is not Predictive of Behavior in the Field: Evidence from Public Good Experiments in rural Sierra Leone, Economics Letters, In Press

8.1 Introduction

Important questions have been raised about the external validity of laboratory-style experiments. In the context of altruism and public good giving, for example, Laury and Taylor (2008) ask whether decisions made in sterile lab settings using tokens, anonymous partners, and benefits accruing to a small number of players (those concurrently in the experiment) are predictive of altruistic behavior in naturally occurring settings. They conclude "*one should be cautious when using the results from laboratory public good experiments to make inferences about altruism outside of the laboratory"* and argue that *"further investigation into the external validity of decisions made in context-free situations is warranted*" (p.28). We agree. If behaviors across context are correlated, behavior in the lab would be predictive of behavior in real life. We would then be able to credibly "testbed" real policies and interventions in the lab, at modest cost, and could identify individuals for targeting by such interventions (or weed out free riders).

The evidence with respect to the external validity of lab-style experiments is mixed. Some studies document positive correlations between behavior in the game and real life (Karlan 2005, Benz and Meier 2008, Bouma *et al.* 2008, Carpenter and Seki 2010) while others reject consistency in behavior across contexts (e.g. List 2006, Voors *et al.* 2011). In what follows we focus on one specific form of behavior—contributions to a (local) public good. Such contributions are usually believed to reflect pro-social preferences, most notably altruism. Laury and Taylor (2008) find that pro-social behavior in the lab predicts contributions to a local public good, but not in a uniform way. Specifically, while some measures of altruism based on standard public good (PG) games in the lab are correlated with the likelihood of acting altruistically in real life, the same is not true for other measures.

Why may respondents' behavior differ across contexts? Levitt and List (2007) propose such divergence may be explained by several factors, including the presence or absence of moral and ethical considerations; the subject pool; the context in which the choice is embedded; the extent to which one's actions are scrutinized by others and the nature of that scrutiny; and the stakes of the game. These factors may serve as shift parameters. Obviously, a subject's contribution relative to others in her group (her "ranking") across lab and field settings are preserved if these shifters are isomorphic across people (even if the "levels" of pro-social preferences displayed will vary). In contrast, allowing heterogeneity in shift parameters implies we may observe different relative rankings across lab and field.

We revisit the question whether behavior in the lab spills over to the field. Our analysis differs from earlier studies because of at least three factors. First, we use an under-researched yet highly relevant pool of respondents—poor villagers from small and remote forest edge communities in Sierra Leone, increasingly subjected to community-based interventions by NGOs, often explicitly engaged in attempts at social engineering. Second, we compare choice behavior in related dilemma situations by inviting subjects to participate in a lab-style public goods game and a development intervention with similar structure (in our case: involving the allocation of an endowment between self and others). Important differences between the lab game and field experiment are that in the latter (i) the stakes are much higher (a month of wages versus a day's wage), and (ii) the public good accrues to a larger number of people (affecting the entire village instead of two other co-players). Third, we also compare experimental play with survey-based evidence on actual public good contributions.

8.2 Experiments and Data

In the summer of 2010 we visited 500 randomly sampled households in 25 villages in rural Sierra Leone. These respondents participated in a livelihoods intervention implemented by an international NGO, and a standard public goods game. In the livelihood intervention, respondents were endowed with $20 (or 80,000 Leones, Le), and asked to divide this endowment between private goods for themselves and a community project fund for the entire village. On average, participants allocated 75% of their endowment to private goods and 25% to the community project fund.

After completing the intervention, we played a conventional five round PG game where respondents were randomly and anonymously matched with two peers.[111] Players were endowed with five tokens. Tokens kept were worth 1,000 Le, and tokens "invested" were worth 500 Le to all group members. The payoff under full co-operation thus amounts to 7,500 Le, and the maximum possible payoff for a free rider (assuming full contributions by his peers) was 10,000 Le. On average, households invested two tokens (40%) in each round, and the average payoff was 6,000 Le.

Next, we collected additional data using household and village level surveys in the same 25 villages (but now involving only a subsample of 170 respondents). We collected demographic, socio-economic, institutional and civil war information as control

[111] In each village the aid experiment preceded the public goods game. Subsequently, ordering effects may bias the level of contributions though it is not evident that ordering affects the ranking of individuals. In a similar experiment Laury and Taylor (2008) document that experimental order does not affect their results.

variables, and also used the household survey to construct additional dependent variables. As survey-based proxies for pro-social preferences, we use the degree to which the respondent contributes labor to community projects or to the village farm (both measured on a 4-point scale).[112]

Our main identification strategy is simple: we use an interval regression model and regress our measures of "real life" pro-social preferences Z_{ij}—contributions in the livelihood intervention by individual i (i = 1, …, 500) in village j (j = 1, …, 25) as well as measures of stated behavior—on lab data (PGG_{ij}), household controls (X_{ij}) and village fixed effects (V_j):[113]

$$Z_{ij} = \alpha + \gamma_1 PGG_{ij} + \gamma_2 X_{ij} + \gamma_3 V_j + \mu_{ij}^{FE} \quad (1)$$

where μ is an error term. To explore whether there is evidence of systematic heterogeneity in shift parameters, we also estimated models with a full set of interaction terms:

$$Z_{ij} = \alpha + \gamma_1 PPG_{ij} + \gamma_2 X_{ij} + \gamma_1 X_{ij} PPG_{ij} + \gamma_3 V_j + \mu_{ij}^{FE}. \quad (2)$$

Finally, we have undertaken a series of non-parametric tests. Based on their choices in the PG game we divide our sample of respondents in 3 subsamples, distinguishing between (weak) free riders (respondents contributing 1 token or less in all five rounds)[114], mild cooperators (consistently contributing 2 or 3 tokens) and altruists (always contributing 4 or 5 tokens). We then explore whether the behavior across these groups in the livelihoods intervention was different, or not.

8.3 Results

Table 1 summarizes the regression results. The main result is that behavior in the lab game is not correlated with our measures of pro-social preferences in the field. In some models, the correlation is even negative, but standard errors are so large that lab play never enters significantly. This is true when explaining choices in the aid intervention

[112] Local public goods provision is heavily dependent on the private provision of labor by community members. Community Farm is a variable measured on a 1 to 4 scale, where a higher score means subjects contribute more labor to the common farm, the revenues of which accrue to the community as a whole for festivities or visiting dignitaries. Community Labor was also measured on a 1 to 4 scale with a higher score implying that subjects contribute more labor to community projects, such as road clearing, water flow maintenance, village clean up, and other community projects.

[113] We are interested in the (conditional) correlation between our pro-sociality measures, and the regression structure is *not* intended to suggest that we are looking for a causal effect of lab behavior on behavior in the field. To emphasize this point: we have also estimated a linear seemingly unrelated regression (SURE) model to test whether the observed responses in the experiments are correlated (see below).

[114] Note that we classify our subjects based on behavior throughout the five PG game rounds. Dropping the last round (when subjects have a reduced incentive to make pro-social choices) does not change the results.

experiment (columns 1-3) as well as the voluntary contributions to community projects (columns 4-7). It does not matter whether we control for household or respondent characteristics (age, education, income, etc.), or not. Also, including a full set of interaction terms—household controls times behavior in the lab game—does not affect this result. Interestingly, the interaction terms tend to enter insignificantly, suggesting we cannot capture heterogeneity across respondents in such a simple fashion (results not shown, but available on request).

Table 1: Explaining real life altruism by altruism displayed in the lab.

	(1) PG Aid	(2) PG Aid	(3) PG Aid	(4) Community Labor	(5) Community Labor	(6) Community Farm	(7) Community Farm
PGG	-161.8	439.8	-1701.9	-0.230	0.396	-0.0710	-2.075
	(943.3)	(2013.1)	(9520.7)	(0.383)	(1.466)	(0.419)	(1.431)
Household controls	No	Yes	Yes	Yes	Yes	Yes	Yes
Interaction terms	No	No	Yes	No	Yes	No	Yes
Village fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	453	111	111	115	115	113	113
adj. R^2/ Pseudo R^2	0.13	0.17	0.19	0.19	0.19	0.18	0.21

Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Household controls are age, education, household income, conflict exposure in the civil war, and witchcraft beliefs. The full set of interaction terms are the product of these household controls and the PGG variable. Columns (1)-(3) are estimated using an interval model and Columns (4)-(7) are estimated using ordered logit.

The lack of correlation between behaviors is robust. A linear SURE model, used to test whether the observed responses in the experiments are correlated, yields similar results: the error terms of the models explaining behavior in the lab game and in the framed field experiment are not correlated (details available on request). This lack of correlation is also confirmed using a non-parametric analysis. Figure 1 reports behavior in the livelihoods intervention for our three sub-groups of respondents (weak free riders, mild cooperators, and altruistic respondents). The distribution of contributions in the intervention does not vary across groups—this is confirmed by formal tests.[115] Taking together, we conclude that for our pool of respondents, play in lab experiments has no predictive power for behavior in naturally occurring settings. Indeed, the SURE model suggests the lab game may measure a distinct latent variable altogether.

[115] Two-sided between group comparisons are all non-significant. There is a residual category of respondent who make inconsistent choices through the five rounds of the experiment, their contributions in the aid experiment are also not significantly different from the contributions of subjects in the other groups.

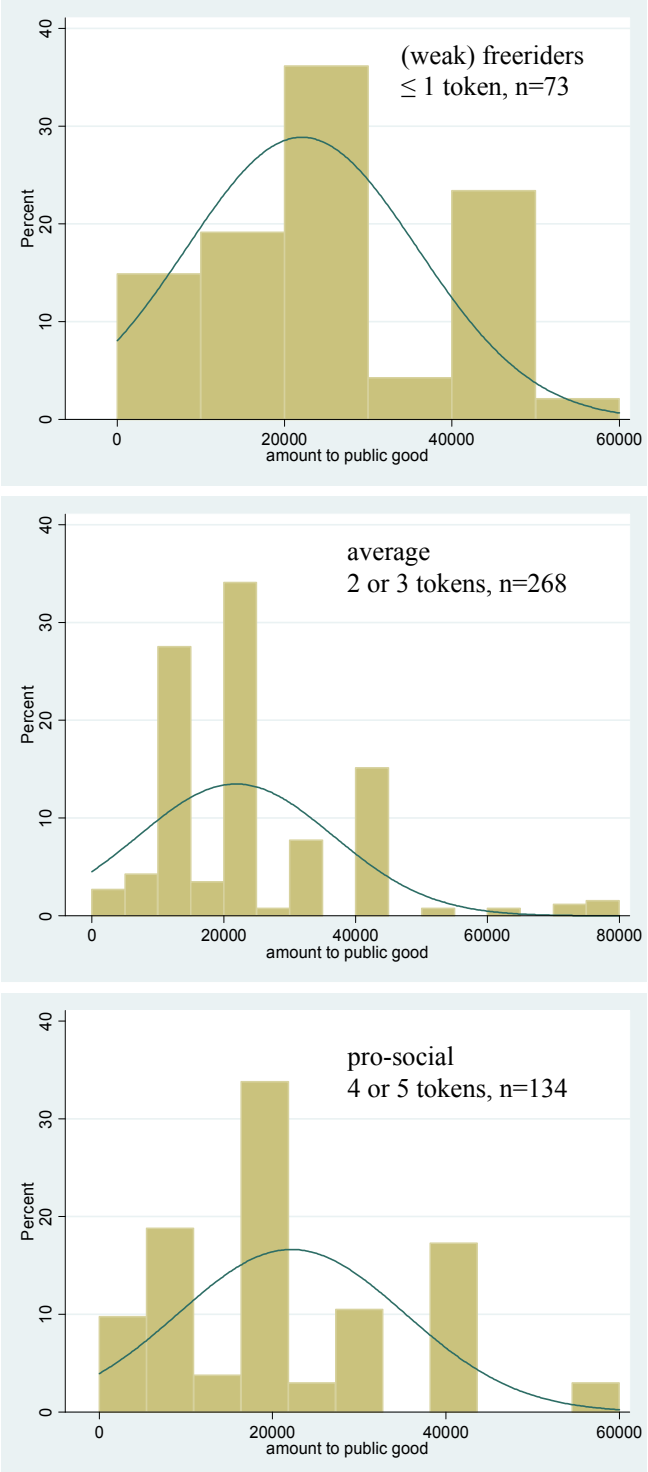


Figure 1: Non-parametric analysis of altruism in the lab and in the field

8.4 Discussion

Why are pro-social preferences measured in the lab not correlated with pro-social preferences measured in real life, where salient choices are made in a familiar context? Our analysis does not allow us to identify the non-isomorphic shifter that is responsible for this finding—doing so would require a more elaborate experimental design in which we randomly vary different candidate shifters (such as the nature of scrutiny, or the stakes of the choice experiment) while keeping other shifters constant. This is left for future work.

Nevertheless, we believe the results summarized on these pages are of interest. They are based on a particular yet relevant subject pool (adding to the generality of the main insight, also obtained in "more conventional" settings). The lack of correlation suggests aid agencies considering lab games as a cheap way to help them design their policies should think twice. The robustness of our main result is strengthened by the fact that the game and real behavior settings are similar, and the fact that we obtain similar results using survey-based evidence on actual public good contributions.

Our results are consistent with the finding in psychology that behavior in different settings is only weakly correlated (even across lab settings; see, e.g., Ross and Nisbett 1991 for a review). This does not mean that there is necessarily something wrong with one of the experimental mechanisms in this study. Rather, it may mean they are measuring different latent variables or preferences—perhaps a general cross-situational preference for pro-social behavior does not exist?

Chapter 9

Discussion

9.1 Introduction

The research presented in this thesis is motivated by three observations. One, in order to understand development in Africa it is necessary to focus on the performance of institutions. Two, too little is known about the factors that shape institutions. Three, much of the available evidence on institutions has focused on the macro level and there is too little understanding of its micro foundations.

Consequently, this thesis takes institutions as a starting point and aims to broaden the literature by presenting several empirical contributions, at both the macro (cross country) and micro (within country) level. At the macro level, I focus on developments in Africa in general. For the micro level contributions, I focus on two countries: Burundi and Sierra Leone. I "unbundle" institutions and distinguish between several dimensions of the institutional framework, both vertical (interactions between people and the government, such as economic and political institutions) and horizontal (social interactions between people, such as cooperation and trust).

Several core insights emerge from this thesis. First, I show at the macro level that institutions respond to windfall gains resulting from changes in productivity (chapter 3). Second, and switching to the micro level, I show that institutions change due to shocks of civil war in Burundi (chapter 4). A third main finding is that violent conflict not only affects the quality of institutions, but may also lead to changes in preferences (chapter 5). Fourth, I show that grievances over (informal) institutions are related to the onset of civil war (chapter 6). Fifth, I make a methodological contribution by measuring social institutions experimentally and assessing whether these measures are predictive of socially oriented behavior in the field (chapter 7 and 8). Below I discuss these findings in more detail and provide some implications for policy and future research.

9.2 Institutional Responses to Windfall Gains and Civil War

Today, few economists would dispute the primacy of institutions in the development process. The modernization theory holds that as incomes increase, people demand and create better political and economic institutions (Lipset, 1960). Empirically, these

assertions have been supported by positive cross-country correlations between incomes and various types of institutions on the long run – such as democracy (Acemoglu *et al.* 2008) and property rights protection (Acemoglu and Johnson 2005).[116] Unfortunately, less is known about how institutions develop in the short run. One interpretation (see Acemoglu and Robinson 2008) is that institutions are an equilibrium outcome resulting from the competition between social groups. The nature of interaction, combined with the strength and interests of the groups, then determines how the institutional framework evolves. Such model is useful when trying to understand how shocks may impact on institutions. Windfall gains resulting from shocks in productivity (or other income shocks) may alter the incentives of social groups in an economy and shift the balance of power between them, inviting a process of change. Other types of shocks such as civil war may affect the formation of institutions via a similar mechanism.

Empirical contributions have shown that institutions respond to windfall gains (Dalgaard and Olsson 2008) resulting from changes in productivity (Glaeser et al 2004), foreign aid (Djankov et al. 2008, Easterly 2009) and resource rents (see van der Ploeg 2011 for a review). Yet, in turn institutions are a key driver of economic outcomes (Lipset 1960). As both incomes and institutions move together as societies develop, the analysis of the underlying causal relationships is challenging. I focus on this question in chapter 3, where I test whether a virtuous cycle between income and improved institutions (or more specifically: less corruption) exists in Africa. To address issues of causality, I use (plausibly exogenous) variation in rainfall to explain changes in income as a first step to explaining governance quality. I find no statistical evidence of a virtuous cycle of income and reduced corruption, at least in the short run. Instead it appears that relationship is non-linear: income booms tend to increase corruption in countries with high levels of corruption. For countries with manage to bring corruption under control, income booms may actually help to decrease corruption further.

A limitation of the analysis in chapter 3 is that it focuses on institutional adaptation to windfalls in the (very) short run. By focusing on yearly variation in incomes and institutions the analysis may likely gloss over development on longer time scales, as part of the institutional formation is likely governed by slower dynamics. It is important to know at what time scales institutional change occurs. Extending the analysis to longer

[116] In the past decade a suite of paper has emerged highlighting the impact of historical events on current institutional quality; see Acemoglu *et al.* (2001) on the influences of colonization, Nunn (2008a) on the effects of slave trade and Dell (2011) on the effects of slave labor.

time scales is an important area for future work.[117] Another essential area of research is assessing via what channels institutions change. In chapter 3, I show that levels of corruption respond to changes in windfall gains. The analysis however does not identify which mechanisms are responsible for these changes. Do windfall gains invite additional rent seeking, patronage, and nepotism? Or does it affect institutions via politically motivated but unproductive investments? Uncovering the channels via which institutions change is important future work.

In addition to windfall gains, institutions also respond to civil war. Conflicts and institutions are naturally linked if conflict shakes up social relations and creates opportunities for innovation and redistribution. Tilly (1975) for example shows how wars promoted European state formation and contributed to nation building by strengthening institutions. In similar vein, and at the micro level, Bellows and Miguel (2009) show that social institutions are affected by warfare. In chapter 4, I investigate the institutional responses to civil war in Burundi. Comparing affected and non-affected areas, the analysis shows that types of institutions respond to civil war in a heterogeneous way (see below): while warfare increases the quality of economic and social institutions, it tends to decrease the rule of law.

Taken together, the results of chapter 3 and 4 imply that instructions respond to various types of shocks and that the responses are heterogeneous. What can we learn from these results for policy? When coupled with the evidence that institutions matter for long-term growth and development, the findings of chapter 3 imply that the impact of positive income shocks could be short-lived and possibly negative, unless institutional reform is a conscious choice of policy makers. The results therefore seem to support the need for on-going investment in governance and institutional improvements as a prerequisite to sustaining growth. At the same time, the findings of chapter 4 imply that adverse shocks from civil war can alter institutions permanently, even if the shocks themselves are temporary. This implies that policy should take the institutional responses to conflict into account if it is to effectively implement development programs in countries with a history of violence.

One important avenue to pursue is to study the impact of different types of shocks simultaneously. For example, research could focus more on modeling the macro level impacts of civil war and windfall gains on institutions jointly. In addition, research

[117] Arguably, a different set of (time variant) instrumental variables (such as lagged income levels using a GMM estimation) is required for such analysis as averaging rainfall levels over multiple years reduces the correlation with income levels.

into how types of windfall gains, stemming from fluxes in aid transfers, resource rents or changes in productivity impact on institutional quality (see Dalgaard and Olsson for one such attempt at the macro level). Another fruitful area of research lies at the micro level. Many development programs are targeted at local communities, where NGOs aim to alleviate poverty or improve social cohesion (see for example Fearon *et al.* 2009). From the viewpoint of local communities often these programs comprise a windfall gain. The insights from the macro literature can be useful here. If there are mechanisms by which windfall gains undermine economic, institutional and social development at the macro level, it would be interesting to study whether these pernicious effects materialize at the micro level as well. This question is important for NGOs and governments aiming to help the poor through transfer programs. If, in fact, there are social costs to these transfers, it changes the cost-benefit analysis of any intervention involving windfalls. A focus on post-war countries could provide insight into the impact of windfall gains given a background of civil war – an important area for future work. [118]

9.3 'Civil War is Not a Stupid Thing'[119]

Civil war has affected nearly every African country since independence and the destructive effects of war on mortality, health, education, incomes, and potentially institutions have been demonstrated across many settings. Yet research into the micro-level impacts of civil war has been minimal. In an overview piece on civil war Blattman and Miguel note: *"…[b]oth theory and evidence are weakest in assessing the impact of civil war on the fundamental drivers of long-run economic performance—institutions, technology, and culture—even though these may govern whether a society recovers, stagnates, or plunges back into war."* (2010: 8).

Many questions related to the state of postwar behavior, institutions and welfare remain under researched. This thesis aims to contribute to filling this void. For the work presented in chapters 4 to 8, we collected detailed data in Burundi and Sierra Leone, and used specific survey modules and behavioral experiments to assess the quality of local institutions. In chapter 4, I show that institutions respond to civil war in a heterogeneous way. While the intensity of violence is related to improvements in property rights protection and measures of social capital, it seems to weaken the rule of law. In light of the positive correlation between violence and political participation

[118] As part of the work in Sierra Leone (presented here in chapters 6-8), I am collaborating with researchers from several universities and an international NGO to implementing a randomized control trial (RCT) to assess the impact of windfall gains (see Banerjee and Duflo 2009 for an overview of RCTs, and Deaton 2009 for a commentary).
[119] Cramer (2006).

presented by Bellows and Miguel (2009) and Blattman (2009) this creates an interesting puzzle. Does greater cooperation develop in response to poor local governance, implying that social capital and political institutions are substitutes in development?

A second attempt to investigate the micro level responses to civil war is presented in chapter 5, where I find that individuals who have either experienced violence themselves, or who live in communities that have been violently attacked, display more altruistic behavior, are more risk seeking, and act less patiently. Interestingly, these results materialize in both experimental and survey data.

The most captivating result may be the positive impact of civil war on social preferences. Interestingly, a small literature is emerging that appears consistent with these findings (see Blattman 2009, Bellows and Miguel 2009, Gneezy and Fessler 2011, Gilligan *et al.* 2011, and Bauer *et al.* 2011).

Combined with the findings presented in chapter 4 and 5, the merging evidence suggest that temporary shocks can induce a shift in preferences and the demand for institutions (or a shift in the supply via increased efforts to reform) pushing communities to a new institutional and social equilibrium. It appears that in the aftermath of conflict, augmented institutions materialize - such as the protection of property and greater cooperation - to improve social and economic interactions (see also Tilly 1975). As such, civil wars can have long-run consequences for poverty and growth. The implication for policymakers is that post-conflict recovery programs should take the institutional and behavioral responses to violent conflict into account. In light of the importance of preferences (as the driver of investment and savings decisions) and institutions (providing a growth enhancing environment) for development, this implies a need for a greater marriage between research and policymaking. Post-conflict development agencies could better tailor (and prioritize) their interventions if the heterogeneous responses of communities are taken into account.

In this light, important follow-up research could focus on the link between conflict induced behavioral and institutional outcomes and local measures of economic growth or incomes (see exemplary work on cross country comparisons by Acemoglu and Johnson 2005, and micro level work by Banerjee and Iyer 2005 and Grimm and Klasen 2008). For example, it would be interesting to investigate whether improved property rights security translates into improved incomes, as both macro and micro literature suggests (e.g. Acemoglu and Johnson 2005 and Besley 2005). Also, various types of institutions may interact and serve as either complements or substitutes in the

development process (see Ahlerup *et al.* 2009). In an ideal scenario, data collection efforts focus on the creation of panel data sets of both survey and behavioral experimental. A primary advantage of panel data is that it allows researchers to control for many unobserved time invariant variables thereby enabling a clean study of the co-evolution of various types of institutions in response to shocks and trends.

9.4 Endogenous Preferences

Chapter 5 showed that behavior over social, risk and time dilemmas, as measured in behavioral experiments responds to shocks. Unfortunately, the data do not allow me to exactly identify the mechanism linking conflict shocks to behavioral change. There are several candidate explanations: selection effects (people with a certain set of preferences have a higher propensity to flee or fight), changes in beliefs (people change their expectation of the behavior of others), social structure (the composition of the social network in which an individual is embedded is changed due to the conflict), preference discovery and a shift preferences. In the chapter, it is argued that a shift in preferences may be driving the results. It is interesting to contemplate the possibility of a shift in preferences. In psychology it is widely accepted that large (temporary) shocks can have persistent effects on someone's outlook on life. For example in their book on 'post traumatic growth', Tedeschi *et al.* note that: *"It is clear that individuals who have faced a wide array of negative life challenges report that their struggle with those difficulties have, paradoxically, had positive effects on their lives."* (2008:215). There is considerable more resistance within economics to this view, as generally economists regard preferences as exogenous and fixed, at least in the short run. "*...one does not argue about tastes for the same reason that one does not argue about the Rocky Mountains – both are there, and will be there next year, too, and are the same to all men."* Stigler and Becker (1997: 76). The notion of endogenous, or context-dependent, preferences as suggested here, challenges the foundations of welfare theory. Despite the opposition, the discussion on endogenous preferences is however not new to economics (see Bowles 1998). Empirically for example, research has shown that preferences respond to market integration (Henrich *et al.* 2004).

Future work could focus more explicitly on determining the relation between shocks and preferences. For example, the findings in chapter 5 are limited to showing that war increases pro-social behavior towards fellow villagers. This seems to be in line with theories on parochial altruism (as in Choi and Bowles 2008) but provides no formal test of this hypothesis. It would be interesting to see along what dimension altruism

changes. Do people become more hostile toward out groups? And is there a gradient along which this effect increases or decreases?

In addition, one intriguing avenue for future research (highlighted in chapter 5) is to investigate the potential neurobiological basis of preference formation. Several studies show that trauma may influence the expression of the genes regulating the hormone balance in humans (Caspi *et al.* 2003). Hormones have been linked to human behavior (see Crockett *et al.* 2008, Schweighofer *et al.* 2008 and Kuhnen and Chiao 2009). A direct test of the link between hormones and behavior in and after trauma offers an interesting alternative channel through which shocks may affect our behavior.

9.5 Persistence of Institutions and the Onset of Conflict

Chapter 6 presents data from another post-conflict country, Sierra Leone. The chapter addresses an important question: whether local grievances, such as inter-generational tensions and agrarian factors that contributed to the start of the civil war, are still present in Sierra Leone's post-war society. The macro level literature on the causes of civil war proposes two types of explanations. Rebels are motivated by either greed or grievance (see Collier and Sambanis 2005, Fearon and Laitin 2003). The consensus is that rebels predominantly fight for material gain.[120] Yet, the majority of the literature has been at the macro level. Much less is known about the micro level factors that contribute to the onset of violent struggle. In chapter 6, I revisit this debate and use data from a large survey among a random sample of households at the margins of the Gola Forest, the cradle of the war. In this chapter, I make a side step to a more qualitative approach and present an analysis using a mix of anthropological and econometric methodologies. The chapter demonstrates that social disputes, remain widespread and potentially form a destabilizing factor in post-war Sierra Leone. As such, chapter 6 provides evidence that stress the importance of grievance-based explanations of the outbreak of civil war. Combined with the insights from chapter 4 and 5, these findings pose important questions the theories on the evolution of institutions – why are some elements of the institutional framework persistent and invariant to shocks and do other elements change? More theoretical and empirical work on the institutional change remains to be done.

Also, using qualitative methods could enhance our understanding of civil war, but may also be useful for other topics relevant to development economics. For one, using

[120] But see Collier et al (2009) for an analysis on how the feasibility of war (in terms of namely finance, military deterrence, and the availability of suitable recruits) interacts with greed and grievance motivations for the onset of conflict.

qualitative approaches could enhance the quality of the data by improving the choice of relevant variables in a survey. Secondly, it may help identify mechanisms through which to changes occur. Chapter 6 is an example of such work. There is great potential for such analysis on many topics.

9.6 Measuring Institutions and Preferences

While research has been successful in showing the pivotal role they play in explaining development (or lack thereof), there is little consensus on what institutions are. In this thesis, I have taken a broad view of institutions defining them loosely as a system of rules, beliefs, norms, and organizations that together conduce a regularity of (social) behavior (see Greif 2006). As such it includes both short-term changes, such as policy priorities, and structural factors that change at much slower time scales. For the individual chapters, I have divided institutions into various types - political, economic and social institutions – and have studied them at various levels (macro and micro).

Several methods are applied to measure institutions. At the macro level, institutional indicators – such as the level of corruption – commonly rely on perception-based surveys of country experts (as used in chapter 3). It is not obvious that perception based measures reflect objective levels of institutional quality (see Olken 2009). Thankfully, advances are being made enabling a more fine-grained analysis of the evolution of institutions. Although most of the new measures are still predominantly subjective, in the past decade several initiatives have sprung up to better measure institutional at the macro level (see International Country Risk Guide and the World Bank Governance Indicators amongst others), creating larger panel data sets covering a wider range of countries and variables. These will enable researchers to address institutional questions with much more rigor in the future.

At the micro level only scant efforts have been made to assess the quality of institutions. Here the focus has been limited to economic institutions, such as those governing property rights (Besley 2005) and various measures of social capital (Putnam 1993, Bellows and Miguel 2009). For an adequate understanding of the evolution of institutions at the micro level, more research is clearly needed. The analysis presented in chapter 4-8 based on data collected in Burundi and Sierra Leone contributes to this need by attempting to measure institutions in a survey and through behavioral experiments. An advantage of these types of experiments is that they present participants with choices over dilemmas with monetary payoffs. As such the choices are incentive compatible and

potentially provide a better measure of individual or group preferences. An important methodological question lies in the external validity of these experiments. If the experimental outcomes are not correlated with behavior in the field, they may still reflect some behavioral trait and can thus be used for comparative studies, but they may be uninformative with respect to their actual contribution to development. The evidence is mixed. In chapter 5, I obtain similar results for the impact of civil war on preferences in both survey and experimental data, suggesting that the external validity of our experimental findings is high. In chapters 7 and 8, I investigate the external validity of behavioral experiments more formally by focusing on a social institution: public good provision. Combining data from both standard public good games, a large field experiment framed as a public goods dilemma and survey data, I find that the outcomes are not robustly correlated (see also List 2006, Laury and Taylor 2008). Chapter 7 shows that choices in a public goods game are not robustly correlated with efforts to provide a local public good: conserving the surrounding rain forest. In chapter 8, I show public good game behavior is neither correlated to a similar public goods dilemma where participants made choices on the allocation of development aid to a village project. One candidate explanation is a general cross-situational preference for altruism does not exist, but rather that the various contexts (the public goods game, the field experiment and the survey questions) appeal to different social preferences. Yet, the chapters fall short in identifying which factors explain the lack of parallelism between the laboratory setting and the field. In fact multiple factors can be identified and future work could focus on developing a formal test of the external validity of behavioral experiments.

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Summary

This thesis presents several contributions to the scientific literature on institutions, windfall gains, violent conflict and economic development. It consists of an empirical investigation of the interrelationship of these themes for sub-Saharan Africa in general and for two countries specifically, Burundi and Sierra.

In chapter 1, I set the stage and introduce the research topics central to this thesis – institutions, civil war, windfall gains and economic development - and present the research questions guiding the individual chapters. In chapter 2, I discuss the main contributions in the literature around these concepts and show where the chapters contribute to the literature on these topics. Specifically, research questions break down to: How do windfall gains alter the quality of institutions (chapter 3)? How does violent conflict shape institutions and behavior (chapter 4 and 5)? To what extent have grievances over social institutions, that are thought to have contributed to the onset of civil war in Sierra Leone, persisted in the post-war period (chapter 6)? To what extent can behavioral experiments be used to study social institutions (chapter 7 and 8)?

In chapter 3, I focus on the impact of windfall gains resulting from income shocks, on the quality of institutions at the macro (cross country) level. The chapter revisits the debate on income (GDP) and institutions and asks whether a virtuous cycle exists. Using a measure of corruption as a proxy for the quality of governance, and rainfall as an instrument for income to address endogeneity issues, I find evidence to the contrary. For a panel of African countries, positive income shocks on average tend to invite extra corruption. Closer inspection, however, reveals that this result can be attributed to the most corrupt countries. Conversely, countries with a sufficiently low level of corruption can escape the detrimental effect of income booms on corruption and may actually experience a virtuous cycle of development.

In chapter 4, I switch to the micro level and look at the impact of the civil war in Burundi on a set of institutional indicators using household and village level data. Specifically, I distinguish between tenure rights security, the quality of the rule of law and the strength of social capital. I find that conflict increases tenure security and social capital and appears to decrease the quality of local political institutions. The effect on institutions several years after the end of the war demonstrates that (temporary) conflict shocks can have long lasting effects on income and growth. In chapter 5, I extend this analysis and look at the impact of conflict on behavior. Using the a series of behavioral

experiments to measure social, risk and time preferences, I find that conflict affects behavior: individuals exposed to violence display more altruistic behavior towards their neighbors, are more risk seeking, and have higher discount rates. Confirming the analysis presented in the previous chapter this study shows that large adverse shocks can thus alter savings and investments decisions, and potentially have long-run consequences – even if the shocks themselves are temporary.

In chapter 6, I look at the core grievances, such as inter-generational tensions over marriage and labor, that are said to have contributed to the start of the civil war in Sierra Leone, and investigate whether they have persisted in the post-war landscape. Using a mix of anthropological methods and econometrics, I find that these grievances continue to mark out divisions in isolated rural communities. It is an open question, whether these grievances will again result in violence.

Chapter 7 and 8 are methodological contributions. In these chapters I combine a series of experiments and a survey data to assess the external validly of behavioral experiments in the field. In chapter 7, I look at the relationship between public spiritedness, as measured in a public goods game, and conservation behavior and discuss the role of conflict therein for post war Sierra Leone. In similar fashion I look at whether behavior in a public good game is predictive of behavior in a village public good aid project in chapter 8. In both cases, I find that the external validity appears low: there is no evidence that behavior in an experimental setting maps out to other domains.

Finally; chapter 9 provides a synthesis and offers some recommendations for future research.

Samenvatting

Dit proefschrift draagt bij aan de literatuur over instituties, *windfall gains*, burgeroorlog en economische ontwikkeling. Het behelst een empirisch onderzoek naar de interrelaties tussen deze thema's richt zich hierbij op Afrika ten zuiden van de Sahara. en twee landen in het bijzonder: Burundi en Sierra Leone.

In hoofdstuk 1, worden de hoofd thema's van deze scriptie – instituties, burgeroorlog, *windfall gains* en economische ontwikkeling – en de onderzoeksvragen geïntroduceerd. Vervolgens wordt in hoofdstuk 2, de belangrijkste literatuur rond deze thema's uiteengezet en laat ik zien hoe de navolgende hoofdstukken aan deze literatuur bijdragen. De onderzoeks vragen waar dit proefschrift op ingaat zijn: Hoe beïnvloeden *windfall gains* de kwaliteit van instituties? (hoofdstuk 3)? Hoe beïnvloed burgeroorlog lokale instituties en individueel gedrag (hoofdstuk 4 en 5)? Tot op welke hoogt komen lokale grieven, die hebben bijgedragen aan de start van de oorlog in Sierra Leone nog voor in de post-oorlog tijd (hoofdstuk 6)? Hoe kunnen gedrags-experimenten gebruikt worden om sociale instituties te meten (hoofdstuk 7 en 8)?

In hoofdstuk 3, wordt op macro niveau de invloed van inkomens schokken op de kwaliteit van instituties onderzocht. Het hoofdstuk richt zich op de vraag of er een zichzelf versterkend proces bestaat tussen economische groei en goed bestuur. Met corruptie als indicator voor de kwaliteit van bestuur en regenval als instrument voor inkomen, vind ik bewijs voor het tegenovergestelde. Voor een reeks Afrikaanse landen blijkt dat positieve inkomens schokken leiden tot meer corruptie. Nadere analyse laat zien dat dit resultaat met name terug te voeren is op de meest corrupte landen. Landen met een voldoende laag corruptie niveau kunnen aan de negatieve effecten van een inkomens schok ontsnappen.

In hoofdstuk 4, verleg ik mijn focus naar het micro niveau en kijk naar de invloed van de burgeroorlog in Burundi op een reeks aan institutionele factoren. Hierbij maak ik gebruik van survey data op zowel huishoud en dorpsniveau en maak onderscheid tussen drie typen instituties: (land)eigendomszekerheid, sociaal kapitaal en de kwaliteit van de rechtsstaat. Uit de analyse blijkt dat oorlog een positieve invloed heeft op eigendomszekerheid van land en sociaal kapitaal, maar leidt tot een vermindering van de kwaliteit van de rechtsstaat. Hier op voortbouwend, kijk ik in hoofdstuk 5, naar de invloed van de Burundi burgeroorlog op gedrag. Hiervoor maak ik gebruik van gedragsexperimenten om sociaal, risico en tijdsvoorkeuren te meten. Uit de analyse blijkt

dat individuen die meer oorlogsgeweld hebben meegemaakt meer sociaal gedrag vertonen richting hun dorpsgenoten, meer risico durven nemen en minder geduldig zijn. Samen genomen tonen deze hoofdstukken aan dat een tijdelijke schokken als burgeroorlogen, via veranderingen in spaar en investeer gedrag, consequenties kunnen hebben op de lange termijn.

In hoofdstuk 6, onderzoek ik of de lokale grieven die hebben bijgedragen aan de start van de oorlog in Sierra Leone zoals de intergenerationele spanningen en agrarische conflicten, , nog voorkomen in de post-oorlog tijd. Gebruikmakend van een mix van antropologische en econometrische methoden concludeert het hoofdstuk ik deze grieven nog steeds dominant aanwezig zijn in rurale en geïsoleerde gemeenschappen in Sierra Leone. Het is een open vraag of deze grieven in de toekomst wederom aanleiding zullen zijn voor geweld.

Hoofdstuk 7 en 8 zijn methodologisch van aard. In beide hoofdstukken wordt de externe validiteit van gedragsexperimenten bekeken. In hoofdstuk 7, kijk ik naar de relatie tussen vrijwillige bijdragen aan publieke goederen, gemeten in een *public goods game*, en natuurbeheer van een gemeenschappelijk bos in Sierra Leone. Op gelijke wijze analyseer ik in hoofdstuk 8, of gedrag in een *public goods game* voorspellende waarde heeft voor vrijwillige bijdragen aan dorpsproject. In beide gevallen vind ik dat de externe validiteit van de gedragsexperimenten gering is en bespreek ik de mogelijke oorzaken hier van.

Tot slot geef ik in hoofdstuk 9 een synthese van de hoofdstukken en maak ik een aantal aanbevelingen voor toekomstig onderzoek.

Acknowledgements

While researching and writing this thesis I have benefitted from the guidance, trust, enthusiasm and reflections of many people, from many backgrounds. Their contributions have helped shape this thesis (and potentially the course of my life). I am very grateful to them all and will mention a few in particular.

First of all, many of the pages presented in this thesis are based on papers that are co-authored by others. In alphabetical order, I am very grateful to have collaborated with Erwin Bulte, Richard Damania, Andreas Kontoleon, Robert Lensink, John List, Esther Mokuwa, Eleonora Nillesen, Paul Richards, Daan van Soest, Ty Turley and Philip Verwimp. Thank you all, and I look forward to our continued collaboration.

A special word of thanks goes to Erwin Bulte, my supervisor and promotor. He made starting a PhD sound like a Disney ride and I must admit, it had all elements of a Disney ride: the waiting, the fear of heights, the thrill and excitements (of a finished paper), the disappointment (of rejection letters). Erwin taught me how to spot an angle, how to *sort the wheat from the chaff*, to not take it all too seriously and that there is always something to laugh about.

Equally important is my gratitude to the many respondents in Burundi and Sierra Leone, who so patiently shared their time with me and the research team. In addition, I want to thank my local research assistants in both countries and the staff of the NGOs I collaborated with – League Iteka in Burundi, the Gola Forest Programme in Sierra Leone and UK's Royal Society for the Protection of Birds – for their kind collaboration.

Many thanks go to my colleagues at the Economics Cluster and in particular to the Development Economics Group of Wageningen University. I want to reserve a special place to thank my dear colleague, roommate and good friend Eleonora Nillesen, and the departments' great admin staff: Betty, Dineke, Karin and of course Marian. Thanks also to my friends and co-conspirators who were such great company and of such superb assistance in Burundi, Sierra Leone and back in Wageningen: Francesco, Froukje, Gonne, Jan, Lizzy, Wytse and above all Koen. You make a great team.

Last, but certainly foremost, I want to thank my friends, in particular Martijn, my parents and brother and my wonderful family Anna and Mara for their love, support and endurance.

Annex to statement

Name Maarten Jan Voors
PhD candidate, Wageningen School of Social Sciences (WASS)

Completed Training and Supervision Plan

Name of the course	Department/ Institute	Year	ECTS
I. General part			
Scientific Writing	Language Center	2008	1.5
II. Mansholt-specific part			
Mansholt Introduction course	MG3S	2008	1,5
Mansholt Multidisciplinary Seminar	PhD day WUR	2008	1
Presentation 1 EAAE Conference	Gent	2008	1
Presentation 2 Economics Cluster	WUR	2008, 2009, 2010	1
Presentation 3 CSAE Conference	University of Oxford	2010	1
Presentation 4 Nordic Conference Development Economics	UNU-WIDER	2010	1
Presentation 5 Inaugural Conference Poverty, Equity and Growth in Developing Countries Center	University of Göttingen	2010	1
Presentation 6 African Studies Association Biennial Conference	University of Oxford	2010	1
III. Discipline-specific part			
Quantitative Research Methods	WUR	2008	4
Development Economics	NAKE	2008	6
Advanced Econometrics	WUR	2008	6
Advanced Micro Economics	WUR	2008	6
Advanced Macro Economics	WUR	2008	6
Summerschool Conflict and Development	MICROCON	2008	1
Workshop Experiments (J. Shogren, M. Humphreys)	WUR	2009	1,5
Summerschool Econometrics (A. van Soest)	NAKE	2009	1,5
Summerschool Impact Evaluation (G. Imbens)	Foundation of the Swiss National Bank	2010	1
IV. Teaching and supervising activities			
Introduction Development Economics (TA)		2008, 2009, 2010, 2011	
Methods and Techniques of Field Research (TA and Lecturer)		2009, 2010, 2011	
Economics and Governance (Lecturer)		2011	
MSc Student A		2008-2009	
MSc Student B		2010-2011	
MSc Student C		2010-2011	
MSc Student D		2010-2011	
TOTAL			43

Funding

The research described in this thesis was financially supported by the Dutch Organization for Scientific Research (NWO) grant nr. 452-04-333, MICROCON, a research project funded by the European Commission under FP 6, UK's Cambridge Conservation Initiative (CCI 05/101005), the Wageningen School for Social Science and the Social Science Group of Wageningen University.

Cover

School children in front of abandoned ECOMOG armored vehicle in Segbwema, Eastern Province, Sierra Leone. Credits: Koen Leuveld. Layout: Marieke van Diepen.