Political Economy of Violence and Post-Conflict Recovery in Sub-Saharan Africa

Jacobus Cilliers

Balliol College

University of Oxford

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Statement of Originality

Chapters one and four are original single-authored work. Chapters two and three are original joint papers with Oeindrila Dube and Bilal Siddiqi. I outline below my contribution in each of these chapters.

Chapter 2. "Reconciliation, Conflict, and Development: Evidence from Sierra Leone

I designed and piloted the village and household surveys, managed data collection, did most of the data analysis, and wrote up the paper. Furthermore, I took the conceptual lead and performed the majority of work in writing the pre-analysis plan.

Chapter 4. "'White man's burden'? A field experiment on generosity and foreigner presence"

I designed and piloted the household and village surveys, and made the largest contribution to the design of the scripts for the behavioural games. All co-authors jointly conducted the empirical analysis and put together the paper text.

Abstract: Political Economy of Violence and Post-Conflict Recovery in sub-Saharan Africa

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Balliol College, University of Oxford

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This thesis presents theoretical work on armed group activity and empirical work on post-conflict recovery.

In chapter two, I develop a general equilibrium model of violence to explain observed variation in coercive practices in conflict zones. Armed groups own land in the resource sector and allocate military resources between conflict and coercion, which assign *de facto* ownership over land and labour respectively. I find that coercion is higher if labour is scare relative to land, production is labour-intensive, or if one group is dominant relative to others. Furthermore, contrary to other studies, I find that coercion could decreases with price if military power is sufficiently decentralised, since conflict draws resources away from coercion. In chapter three, I evaluate a reconciliation program in post-conflict Sierra Leone. The program provides a forum for villagers to air war-time grievances, and also forges institutions designed to improve conflict resolution and build social capital. I find that respondents who received the intervention are more forgiving and are more charitable in their views of ex-combatants. Furthermore, conflict resolution improved and involvement in village groups and activities increased. However, psychological health—depression, post-traumatic stress disorder and anxiety—deteriorated. This study has direct implications for the design of transitional justice programs, as well as programs that aim to promote institutional change. In chapter four, I experimentally vary foreigner presence across behavioural games conducted in 60 communities in Sierra Leone, and assess its effect on standard measures of generosity. I find that foreigner presence substantially increases player contributions in dictator games, by an average of 19 per cent. Furthermore, the treatment effect is smaller for players who hold positions of authority; and subjects from villages with greater exposure to development aid give substantially less and are more inclined to believe that the behavioural games were conducted to test them for future aid. In chapter five, I use a model of repeated bargaining with one-sided asymmetric information to investigate the difficulties of reaching and sustaining power sharing agreements. I show that asymmetric information can explain the persistence of conflict, since learning slows down when there are future opportunities for bargaining.

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Chapter 1

Introduction

1.1 Motivation

Civil conflict has massive welfare implications for a country, both during the conflict and long after it has ended. Not only does conflict lead to a destruction of human and physical capital and a diversion of economic activity away from productivity, but also to an erosion of institutions, which can negatively impact the long run growth prospects of a country (Collier, 1999, 2003). Furthermore, civil conflict is common and disproportionately affects poor countries (Blattman and Miguel, 2010). Out of the fifteen countries listed at the bottom of the 2013 Human Development Index, six are subject to ongoing conflict[1] and five have experienced a civil war at some point in the past two decades.[2] Regardless of causality, it is clear that any scholar who cares about development, and who is concerned about the lives of the poorest, should take great interest in understanding conflict.

Moreover, and perhaps more importantly, there is immense human suffering during conflict. Civilians are indirect victims—they are displaced and their basic livelihoods and access to services disrupted—but they are also often direct targets of violence: subject to reprisal attacks, mass killings, and exploited for their labour and resources. For example, in the ongoing conflict in the eastern provinces of the Democratic Republic of Congo, the International Rescue Committee (Coghlan, 2007) estimates that over a million people have died either as a direct or indirect consequence of the conflict. In Darfur up to two million

[1] Niger, Democratic Republic of Congo, Chad, Mali, Central African Republic, Burkina Faso, Afghanistan

[2] Mozambique (1977 - 1992), Burundi (1993), Sierra Leone (1992 - 2002), Liberia (1989 - 1996; 1999 - 2003), Ethiopia (1974 - 1991)

have been displaced and malnutrition is estimated at 40 per cent, far higher than the rest of the continent (Watkins et al., 2009). The effects of conflict, however, do not cease once the violence ends. Those who survive can suffer severe psychological stress and trauma (Myers et al., 2009; Somasundaram and Sivayokan, 1994).

It is not surprising then that the international community spends considerable resources on countries suffering from conflict. The United Nations currently has 16 peacekeeping forces, tasked with the protection of civilians, which cost $7.33 billion in 2013 alone.[3] In addition, the developed world places restrictions on the purchase of conflict commodities, such as 'blood diamonds' from Sierra Leone and 'blood phones' from the Democratic Republic of Congo, in order to limit the economic incentives for violence. Yet it is unclear how these policies impact civilian conditions on the ground.[4] We need a theoretical understanding of the economic and institutional forces that drive armed groups' use of violence, if we hope to design better policies that contain it.

Similarly, large development aid flows to societies emerging out of conflict. For example, in 2009 the World Bank allocated $3.2 billion in aid to projects in fragile and post-conflict states (Bezerra, R, 2010). These programs want to promote economic development and lasting peace, but there is limited evidence of their effectiveness. We need impact evaluations to identify the most appropriate interventions for post-conflict recovery.

In response, there is a growing theoretical literature which aims to better understand the incentives and behaviour of armed groups in conflict,[5] as well as empirical work to evaluate post-conflict programs. However, as this introduction will show, there is a lot we still do not know. In particular, although there is a lot of theoretical literature on armed groups' incentives to win wars or reach peace agreements, little is known about armed groups' use of violence against civilians *during* conflict. Second, although there is a growing number of studies that evaluate post-conflict interventions, none have looked at effectiveness of transitional justice programs at fostering reconciliation.

My D.Phil thesis contributes to these important gaps in the literature. Chapters two and five are theoretical chapters on armed group behaviour.

[3] `https://www.un.org/en/peacekeeping/operations`, retrieved 28 March 2013

[4] The peacekeeping force in the Democratic Republic of Congo is 15 years old and 20,000 men strong, yet human rights groups document ongoing violence against civilians

[5] Blattman and Miguel (2010) provide an excellent literature review on the field. I will not provide a comprehensive review, but will rather identify where my research fits into the field

Chapters three and four are experimental papers on post-conflict policy interventions. In these chapters I ask the following questions: What explains armed groups' coercion of civilians during conflict (chapter 2)? What is the impact of reconciliation programs on social cohesion and individual wellbeing (chapter 3)? Does foreigner presence bias impact evaluations and aid targeting in developing countries (chapter 4)? What explains armed groups' decision to continue with war, rather than reach a peace agreement (chapter 5)?

In the subsequent sections of the introduction, I discuss how my chapters relate to the literature.

1.2 Related Literature

1.2.1 Theoretical Work on Modern Conflicts

All conflict models depart from standard economic theory in that they do not assume a strong state with a monopoly over violence which can protect property rights and enforce contracts (Skaperdas, 1992). The implication is that resources are also allocated towards acquiring and protecting resources, and agreements are not binding. Two standard workhorse theories of conflict characterise these features: (i) contest functions formally model the trade-off between allocating resources to predation or production; and (ii) bargaining models of war look at the decision to fight, rather than reach a peace agreement.

The first theoretical work on conflict have made very important contributions to our understanding of conflict. Contest functions show that conflict is higher if there are more resources to capture, but an armed group will fight less if it is very productive, since the opportunity cost of fighting is larger (Garfinkel and Skaperdas, 2007; Grossman, 1991). Furthermore, the decision to fight also depends on the 'technology of conflict' (Hirshleifer, 1989, 1995), the relative efficiencies of fighting. For example, Grossman and Kim (1995) shows that the relative efficiency of converting resources into defending existing resources rather than acquiring new resources has an impact on equilibrium levels of conflict. Grossman (1991) extends the model setting where peasant farms decide between farming, participating in an insurrection, or fighting for the ruler. Conclusions remain mostly the same: peasants are more willing to fight if (a) their productivity is low, (b) fighting is effective, or the (c) size of the economy large.

Bargaining models provide the insight that all 'rationalist' explanations for war can be attributed to a breakdown in bargaining (Fearon, 1995; Powell, 2002; Wagner, 2000). If fighting is costly, then a mutually beneficial agreement to share the gains from winning should always exist *prior* to fighting, since there are more resources to bargain over. Understanding conflict thus requires understanding why agreements break down. This is either due to commitment problems (Fearon, 1994; Powell, 2006) or asymmetric information (Filson and Werner, 2002; Powell, 2004). With no binding agreements, a strong group would prefer to fight now, if it anticipates that the weaker group would renege when it grows strong in the future. If one group does not know the other's strength, it might risk fighting by making a small offer that only a weak group would accept.

1.2.1.1 Modern models for modern conflicts

A commonality between all the papers cited above is that economic incentives for violence relate to the *outcomes* of war: The returns to fighting are only realised when one armed group emerges victorious, or if the competing groups reach a peace agreement. These models are applicable to inter-state wars, or the Civil Wars fought during the Cold War period,[6] characterised by two distinct groups fighting each other for complete military victory.

However, the nature of war has changed. Civil Wars in sub-Saharan Africa are in the decline and the remaining conflicts operate almost exclusively in 'fragile states', where there are many armed groups, but no group has a realistic chance of winning.[7] The implication is that armed groups do not try to win the war, but rather profit from the ongoing state of anarchy: they prey on civilians and get involved in economic activity. Furthermore, UN peacekeeping forces operate within this state of anarchy, as they have no mandate to end conflict through direct combat.

Two examples of modern conflicts are the ongoing conflict in the Kivu provinces in Democratic Republic of Congo, and the decade long Civil War in Sierra Leone. There are currently up to 22 armed groups operating in the Kivu provinces. These groups are intimately involved in economic activity

[6] For example Mozambique, Angola, Ethiopia, Chad and Sudan

[7] Currently in the Democratic Republic of Congo, Somalia, Chad, Mali, Central African Republic, and Afghanistan.

and benefit financially from exploitation of natural resources.[8] Similarly in Sierra Leone, although the stated objectives of the rebels were to overthrow the state, their actions revealed otherwise. Both the rebel leader and the state had limited control over their military units; and these units often preferred to direct violence towards civilians rather than at each other.[9]

Given the changing nature of conflict, we need to ask new questions and make different assumptions. Rather than ask 'why do groups fight?', the most pressing concern is the widespread use of violence against civilians. Furthermore, since conflict is continuous, violence is aimed at owning the factors of production, rather than total output.[10] In other words, new models of violence need to look at armed groups' incentives in the *process* of conflict.

There is a growing theoretical literature which better describes modern conflicts. New studies aim to explain armed groups' use of violence against civilians. Kalyvas et al. (2006) shows that rebel groups often use retaliatory violence against civilians who are suspected of providing information to the state. This argument is expanded and tested empirically by Eynde (2011) and Kalyvas and Kocher (2009). Esteban et al. (2010) argue that mass killings, such as those committed in Rwanda and Darfur, are done strategically to limit the future claims on resources by the competing ethnic group. Azam and Hoeffler (2002) contrasts two competing forms of violence against civilians: looting for economic gain, or terrorizing to gain territorial control.

Dal Bó and Dal Bó (2011), combine violence with a general equilibrium model with endogenous change in factor prices. They find that conflict increases (decreases) with the price of the less (more) labour intensive resources, because the returns to predation increase by more than the opportunity cost of fighting.[11]

[8] According to the 2010 UN Panel of Expert Report, armed groups "are reaping profits possibly worth millions of dollars a year from the trade of minerals..." Global Witness (2009), further notes that "although the exploitation of natural resources was not the main raison d'être of the FDLR when it was first formed, it's economic activities ... have become an end in itself."

[9] Keen (2005) documents many cases where the Revolutionary United Front (RUF) and the Sierra Leonean army colluded in economic activity. In fact, the distinction between rebels and soldiers was often so blurred, that some were called 'sobels': soldiers by day and rebels by night.

[10] It is true that Skaperdas (1992) motivated the use of contest functions as protection of property rights. Yet in his model conflict was not aimed at owning the factors of production.

[11] This theory was confirmed empirically by Dube and Vargas (2013).

1.2.2 Impact Evaluations on Post-Conflict Recovery

Whereas chapters two and five are theoretical work on conflict, chapters three and four relate to empirical work on post-conflict recovery. Over the past few years, there has been a growing use of randomised controlled trials to evaluate programs that aim to promote development and limit the resumption of conflict in post-conflict societies. Two common interventions are transitional justice and Community-Driven Development/Reconstruction (CDD/R) programs.

All countries emerging out of conflict adopt a range of transitional justice programs to address war-time human rights abuses. Examples include criminal courts that trial the worst offenders,[12] truth commissions to uncover facts over past atrocities, and reconciliation programs that encourage forgiveness and repentance. All these programs have in common a 'truth-telling' component, which requires survivor and perpetrators to talk about their war-time experiences. The stated outcomes are highly ambitious: talking about the past leads to personal healing, social harmony, and lasting peace. Yet, to date there has been no systematic evaluation of their impact. In a review of the evidence Mendeloff (2004) concludes that "claims about the peace-promoting effects of formal truth-telling mechanisms rest far more on faith than on sound logic or empirical evidence".

Furthermore, the international community spends vast sums of money on CDD/R, that aim to induce long-term institutional change.[13] These programs create democratically elected and inclusive committee structures that channel development aid, and typically also hold civic education workshops that promote more liberal and democratic values. The purpose is to forge both 'hard' and 'soft' institutions: create more inclusive, transparent and accountable governing structures, but also engender new norms and beliefs, and strengthen social cohesion.[14]

However, recent studies that evaluate CDD/R programs show limited or no evidence of success.[15] In Liberia, public goods provision increased (Fearon

[12]For example, the Special Court in Sierra Leone and the *Gacaca* courts in Rwanda

[13]The World Bank lends on average $1.3b per year to CDD programs. Source: `http://tinyurl.com/q33w4cx`

[14]For example the CDR program in Democratic Republic of Congo sought to: "improve the understanding and practice of democratic governance, improve citizens relationships with local government, and improve social cohesion and thereby communities ability to resolve conflict peacefully"

[15]See King et al. (2010) for a recent synthetic review.

et al., 2009). However, in Sierra Leone and the Democratic Republic of Congo (Casey et al., 2012; Humphreys et al., 2012) decision-making was no more inclusive, accountable, nor transparent; and there was no evidence of improved social capital, such as strengthened networks, in Sudan (Avdeenko and Gilligan, 2012). We need more research to determine whether programs *could* induce positive institutional change.

1.3 Contribution to Literature

1.3.1 Chapter Two

Chapter two forms part of a growing theoretical literature which better describes modern conflicts in sub-Saharan Africa. Coercion, the restriction of civilian movements to extract forced labour, is common in modern day civil conflicts, resulting in large civilian casualties. Yet armed groups do not always chose to coerce. To explain observed variation in coercive practices, I embed a two-sector trade model, as in Mussa (1974) and Neary (1978), in a model of violence. Armed groups decide between conflict and coercion, which assigns *de facto* ownership over land and labour respectively. If find that coercion is higher if labour is scare relative to land, if production is labour-intensive, or if one group is dominant relative to others. Furthermore, the impact of the price of the commodity depends on the distribution of military strength. In contrast to Acemoglu and Wolitzky (2011), I find that coercion could actually decrease with price if military power is highly decentralised, as conflict draws military resources away from coercion.

The results of this model have implications for both trade and military policy. Peace is not necessarily good for civilians. Cease-fire agreements can interpreted as collusion between armed groups to redirect their resources towards coercion. The presence of peace-keeping forces could make it easier for armed groups to collude by increasing the cost of conflict. A peacekeeping force therefore needs to be combined with a process of disarmament if it wants to contain violence against civilians. Furthermore, a reduction in commodity prices could actually lead to an increase in coercive practices if military power is very decentralised or production is not very labour intensive.

1.3.2 Chapter Three

Chapter three is an empirical study on post-conflict recovery. With my collaborators, Oeindrila Dube and Bilal Siddiqi, I evaluate a community reconciliation program in Sierra Leone implemented by local NGO, Fambul Tok ("family talk in Krio, a local language), to address the widespread human rights abuses that were committed during Sierra Leone's decade long civil war. The cornerstone of the program is a traditional bonfire ceremony to facilitate truth-telling. At this ceremony, survivors share their stories and perpetrators seek—and on many occasions receive—forgiveness for crimes committed during the war. In addition, Fambul Tok introduces a set of institutional structures aimed at strengthening social capital: a Reconciliation Committee, which includes traditional leaders and other community members, shepherds the reconciliation process; and a Peace Tree serves as a focal point for promoting dispute resolution.

We find that talking about the past is a mixed blessing. Respondents who received the intervention are more forgiving and are more charitable in their views of ex-combatants. Furthermore, conflict resolution improved and involvement in village groups and activities increased. However, there was no impact on trust, social networks, conflict incidence, or economic activity. Most strikingly, psychological health—depression, post-traumatic stress disorder and anxiety—deteriorated.

These results have important implications to the design of both transitional justice and CDD/R programs. First, the creation of new institutions, in particular the new forms of conflict resolution, *can* have a positive impact on social relations. One possible reason for the success is that it builds on existing power relations within the community. Second, 'truth-telling' interventions need to take in account the risk of *re-traumatization* (Rose et al., 2002) for the survivors who testify.

1.3.3 Chapter Four

Chapter four makes a methodological contribution to the appropriate design of impact evaluations and aid targeting in developing countries. There has been an explosive rise in use of field experiments and behavioral games in developing country contexts to evaluate aid programs. Furthermore, aid is increasingly disbursed through participatory approaches, with greater interaction between

disbursers and recipients. Both trends entail foreigners disbursing resources to locals, who differ starkly—and often visibly—in terms of their ethnic identity. Yet little is known about the extent to which these differences bias individual behaviour

With my collaborators, Oeindrila Dube and Bilal Siddiqi, I conduct a field experiment that randomizes the presence of a white foreigner across behavioural games in 60 villages in rural Sierra Leone, and assess its impact on generosity as measured by player contributions in dictator games. We find that the mere presence of a white foreigner increases player contributions in dictator games by 19 per cent. Furthermore, using household and village surveys, we show that the treatment effect is smaller among players from powerful customary authority households, who have higher social status within their communities. Differential giving is also smaller among villages with greater exposure to development aid, where villagers disproportionately believed that the games were conducted to test them for aid suitability.

These results have implications for both the appropriate interpretation and design of behavioural games in field settings and the use of 'needs assessments' for aid targeting. Past studies have found variation in measured giving across communities (Cappelen et al., 2011; Henrich et al., 2006) and attribute this to cultural differences. Our results suggest that differential exposure to foreign aid or foreigner presence could contribute to these observed differences. Looking forward, field experiments should be designed so the composition of the research team is balanced across treatment and control groups. Furthermore, efforts by aid practitioners to assess needs may be biased if aid-exposed communities act strategically to receive more aid.

1.3.4 Chapter Five

Chapter five contributes to the theoretical literature on bargaining and war. I look at armed groups' decision to end conflict through a power sharing agreement. Power sharing, the allocation of government positions between the most powerful factions in a country, is seen as a solution to protracted civil wars (Lijphart, 1991). However, such agreements often fail in practice: "Power sharing agreements are difficult to arrive at and even when implemented, such agreements rarely stand the test of time" (Spears, 2000).

I explain the difficulty of reaching and sustaining an agreement with a game of repeated bargaining under asymmetric information. With asymmetric

information, fighting becomes a necessary component of the bargaining process. Learning takes place through fighting; and fighting takes place in anticipation of future bargaining opportunities. War thus allows armed groups to strengthen their bargaining position, through signalling their strength and resolve . It is for this reason that I analyse bargaining in a repeated setting.

I find that fighting can persist, because learning slows down when there are future opportunities for bargaining. The rebel group anticipates a higher future offer if he fights now, and as a result is more willing to fight. Since fighting has become a noisy signal, asymmetric information is resolved more slowly, leading to a persistence of conflict. This is in contrast to previous authors (Fearon, 2004; Powell, 2006; Walter, 2009) who argue that models of asymmetric information cannot explain the long duration of Civil Wars, since learning is expected to take place through fighting. Furthermore, I show that asymmetric information can lead to a breakdown of agreement after it has been reached, provided each group maintains its own separate armies during peace. Any sustainable power sharing deal therefore needs to account for the integration of armies, to ensure binding agreements. Finally, when the probability of stalemate is high, social welfare can actually be improved by prolonging the period of fighting between bargaining, as the rebel group is less likely to hold out for a higher future offer if it is distant and unlikely.

Chapter 2

Conflict, Commodities, and Coercion

Abstract

In this paper, I develop a general equilibrium model of violence to explain observed variation in coercive practices in conflict zones. Armed groups own land in the resource sector and allocate military resources between conflict and coercion, which assigns *de facto* ownership over land and labour respectively. If find that coercion is higher if labour is scare relative to land, if production is labour-intensive, or if one group is dominant relative to others. Furthermore, the impact of the price of the commodity depends on the distribution of military strength: coercion increases with price if one group is dominant, but this effect is potentially reversed if military power is highly decentralised. These results are consistent with different coercive regimes in the rubber plantations in Amazonia compared the Congo Free State 19^{th} century, and also variation in coercion during the Sierra Leonean Civil War. Results of the model have implications for both trade and military policy. Trade policy aimed at reducing domestic commodity prices could actually lead to an increase in coercion. Similarly, a cease-fire agreement between armed groups can be interpreted as a form of collusion, as military resources are redirected from conflict to coercion.

"with minor exceptions, the objective of military activity is either to secure access to mining sites or ensure a supply of captive labour"[1]

2.1 Introduction

Coercion is common in current day conflict areas. Armed groups restrict civilian movements in order to force them to work on their farms and mines. During the protracted civil war in Sierra Leone, for example, whole villages were transformed into forced labour camps: civilians worked in the rice plantations and were killed if they tried to escape (Keen, 2005). A recent survey reveals that a striking 28% of villages were subjected to such conditions at some point during the war.[2] This practice was so pervasive that some claim "more died from starvation whilst working on the rice farms than were killed by combatants" (Keen, 2005:43). Yet, armed groups do not always coerce. In the ongoing conflict in the Kivu provinces in eastern Democratic Republic of Congo, for example, there are many accounts of armed groups taxing and trading with civilian artisanal miners, without restricting their movements (United Nations Security Council, 2009, 2010).

Why do armed groups sometimes coerce labour and sometimes not? If we better understand the economic and institutional forces that potentially explain variation in coercive practices, we can hope to design better policies to influence it. Two policies available to the international community are trade sanctions and military interventions. Human rights groups have successfully lobbied against purchase of commodities from conflict zones, such as the 'blood diamonds' in Sierra Leone and 'blood phones' from the Kivu provinces today.

[1]The United Nations Panel of Experts Report on Rwandan Army activities in the Kivu provinces, Democratic Republic of Congo in 2001

[2]Of 237 villages surveyed, 77 report that the rebels formed a base at their village at some point during the year. Of these villages, 67 claim that they were forced to work for the rebels and were not allowed to leave.

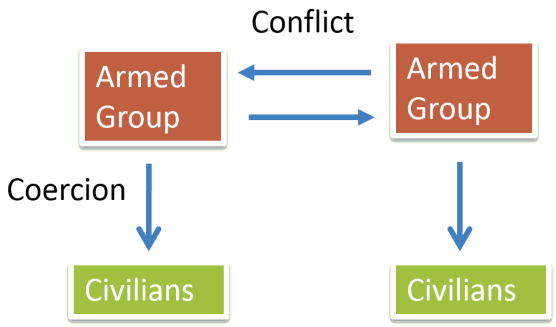


Figure 2.1: Armed groups can either fight each other for control over land or coerce civilians in resource extraction

There are currently 16 peacekeeping operations in the world.[3] However, it is unclear how these interventions will impact civilians conditions on the ground. A theoretical model can help identify the mechanisms through which armed groups respond to changed economic and military conditions.

To answer this question I combine a two-sector general equilibrium trade model, as in Mussa (1974) and Neary (1978), with a model of violence, as depicted in Figure 2.1. Two armed groups own land in the resource sector and simultaneously decide on the level of employment and the allocation of military resources between coercion and conflict, which captures more labour and land respectively. An asymmetric contest success function, as used by Grossman and Kim (1995) and discussed by Clark and Riis (1998), maps the conflict levels of each armed group to land ownership in the resource sector. Coerced civilians work for free in the resource sector. Free civilians own land in the yeoman sector and decide which sector to work in. Land is specific to each sector.

Three main results emerge from the model. First, coercion depends on the factor and resource endowments in the economy. Coercion is higher when

[3] `https://www.un.org/en/peacekeeping/operations/current.shtml`, retrieved 28 March 2013

labour is scarce, or production is more labour intensive. In both cases, free civilians can demand a higher wage, which makes coercion more attractive. This result provides a formal proof of the argument by many economic historians that slavery developed in regions and time periods where labour was scarce relative to land (Austin, 2005, 2009; Domar, 1970; Fenske, 2011, 2013; Nieboer, 1910)

Second, coercion depends on the distribution of military strength. Coercion is higher when one group is dominant relative to others. Armed groups face a social dilemma in their conflict decisions, which is mitigated by a strong group. They would prefer not to fight each other and allocate all their resources to coercion; yet, each group fights in anticipation that the other will do the same. A stronger group is more willing to reduce its own level of conflict, because it is less vulnerable to attack from the weaker group.

Third, coercion depends on the interaction between the economy and the military. Previous work (Acemoglu and Wolitzky, 2011) suggests that coercion is higher if the commodity is more valuable. I find this result holds, but only if one group is sufficiently dominant or production is sufficiently labour intensive. At a higher price both coercion and conflict become more attractive, because the values to both land and labour increase. If production is very land intensive then the rental rate is more responsive to price than the wage rate, so conflict becomes more attractive relative to coercion. If military power is very decentralised then conflict is very responsive to price, thus drawing military resources away from coercion.

Although the main contribution of this paper is theoretical, two case studies further support this result. First, the exogenous rubber boom in the late 19^{th} century (brought upon by a surge in demand for rubber tires in Europe and North America) lead to a very coercive regime in the Congo Free State

(Hochschild, 2011), but to a less extent in Amazonia (Barham and Coomes, 1994a). In the Congo Free State the Belgian *Forces Publique* was the strongest armed group in the area and did not need to allocate large military resource towards acquiring or defending their land. In contrast, in Amazonia there was a long-standing conflict between Portuguese and Spanish colonies, which intensified during the rubber boom (Barham and Coomes, 1994b). Landlords passed legislation to restrict movements of labour but this was rarely enforced, possibly because all military resources were allocated to conflict. Furthermore, the legislation was particularly difficult to enforce where "violent feuds divided the local elite" (Weinstein, 1983:30).

Second, this result also provides an explanation for the surprising observation that during the civil war in Sierra Leone there were many accounts of coercion in the rice plantations, but no such evidence in the diamond mines. Humphreys and Weinstein (2004) find that civilian abuse was actually lower in the diamond-rich districts, even though conflict was higher in chiefdoms with more mines (Bellows and Miguel, 2006). Since land was the valuable factor of production, resources were allocated towards capturing land not labour.

A key contribution of this chapter is to draw a distinction between two substitutes in violence, conflict and coercion. Previous theoretical research has looked at either conflict or coercion (Acemoglu and Wolitzky, 2011; Lagerlöf, 2009), but none have looked at the interaction between the two. This distinction is important, since it picks up a critical trade-off faced by armed groups in modern day conflict zones. With no strong state to enforce a monopoly over violence, armed groups can prey on unarmed civilians, but they are also always under threat of attack from other groups. Coercion, the restriction of workers' movements, requires regular monitoring combined with the threat of violence—military resources that could have been used fighting. As an example of this

trade-off, in contested Karen region in Burma, coercion by the national army increased after a cease-fire agreement was signed between conflicting parties (The Karen Human Rights Group, 2006).

This study is different to previous theoretical work on predation (Garfinkel and Skaperdas, 2007; Grossman, 1991; Skaperdas, 1992), in that violence is aimed at owning the factors of production rather than total output. Two recent papers also incorporate endogenous change in factor prices in a model of violence. Dal Bó and Dal Bó (2011) show that the impact of product price on conflict depends on the relative labour intensities of production, which was empirically confirmed by Dube and Vargas (2013). McLaren (2008) shows how conflict and poverty can reinforce each other: "a shortage of jobs promotes war, but war also creates a shortage of jobs". In contrast, I model violence aimed at two different factors of production, labour and land.

These results have implications for both trade and military policy. Peace is not necessarily good for civilians. Cease-fire agreements can interpreted as collusion between armed groups to redirect their resources towards coercion. The presence of peace-keeping forces could make it easier for armed groups to collude by increasing the cost of conflict. A peacekeeping force therefore needs to be combined with a process of disarmament if it wants to contain violence against civilians. Furthermore, a reduction in commodity prices could actually lead to an increase in coercive practices if military power is very decentralised or production is not very labour intensive.

This chapter proceeds as follows. Section two introduces the model. Section three presents results. Section four relates the results back to the case studies. Section five concludes.

2.2 Model

2.2.1 Set-up of model

Consider a two commodity three factor economy. Civilians, in the 'yeoman sector', produce a subsistence good, Y. Armed groups in the 'resource sector' produce a natural resource, R. Both sectors have a Cobb-Douglas production function:

$$Y \equiv F\left(T_Y, L_Y, \sigma\right) = T_Y^{1-\sigma} L_Y^{\sigma} \text{ and } R \equiv G\left(T_R, L_R, \beta\right) = T_R^{1-\beta} L_R^{\beta}$$

Armed groups and civilians are endowed with T_R and T_Y units of land respectively. Land is specific to each commodity. Total quantity of labour used in both sectors is equal to the fixed aggregate supply of labour: $L = L_Y + L_R$.[4] The price of R is p_R and the price of Y is normalised to 1. The labour market is perfectly competitive so both armed groups and civilians take the market wage, w, as given.

I lay out the model in two stages. I first discuss a limiting case where there is only one armed group. Next I introduce the full model, where there are two armed groups who also fight each other for ownership over land. The baseline model serves two purposes. First, it helps us separate mechanisms by looking at the incentives for coercion in the absence of conflict. Second, the simpler model is interesting in its own right, as there are many historic cases of coercion that the simpler model can explain.

[4]Unsurprisingly, conflict zones are characterised by low levels of capital. The economy thus consists mostly of artisanal mining or agricultural production

2.2.1.1 Baseline Model: One Armed Group

In the baseline model there is only one armed group that owns all land, T_R. The armed group simultaneously chooses the level of employment, L_R, and the level of coercion, c, which provides $L_C(c) = \phi c$ workers, where ϕ is a measure of the 'exportability' of labour. The cost of coercion is $\psi(c) = \psi c^2/2$. Coerced civilians, L_C, work for armed group at zero wages and the remainder, $L_R - L_C$ are paid market wage, w. Free civilians, $L - L_C$, decide between farming for themselves or renting out their labour to armed groups. Equilibrium is thus characterized by the following:

$$\max_{c,L_R} p_R G(T_R, L_R, \beta) - w(L_R - L_C(c)) - \psi c^2/2$$

$$\max_{L_Y} F(T_Y, L_Y, \sigma) + w(L - L_C(c) - L_Y)$$

Subject to $L_C < L_R$[5]

2.2.1.2 Full Model: Competing armed groups

In the full model there are 2 armed groups who fight each other for ownership over land in the resource sector. Total endowment of land in the resource sector is T_R. Group i decides on the number civilian workers, L_R^i, the level of coercion, c_i, which captures $L_C^i(c_i) = \phi c_i$ workers, and the level of fighting, f_i, which assigns a proportion $\pi(f_i, f_{-i})$ of land to group i, based on the following contest success function:

$$\pi(f_1, f_2) = \frac{\alpha f_1}{\alpha f_1 + (1-\alpha) f_2} \text{ and } \pi(f_2, f_1) = \frac{(1-\alpha) f_2}{\alpha f_1 + (1-\alpha) f_2}$$

α captures the degree of asymmetry in military strength. If $\alpha = 1$ or

[5]This condition means that armed groups cannot sell coerced labour.

$\alpha = 0$ then one group has all the strength and we are back in the limiting case of one armed group. The joint cost of conflict and coercion is $\Psi(c_i, f_i) = \psi_i (c_i + f_i)^2 /2$. $(c_i + f_i)$ can be interpreted as the total number of soldiers that are allocated to conflict and coercion, where ψ_i indicates the cost of recruiting more soldiers,[6] which could vary due to factors such as external sources of financing. Free civilians, $L - \sum L_C^i(c_i)$, decide which sector to work in. Further define: $T_R^i(f_i, f_{-i}) = \pi(f_i, f_{-i}) \cdot T_R$. Equilibrium is thus now characterized by the following:

$$\max_{L_i, c_i, f_i} p_R G(T_R^i(f_i, f_{-i}), L_R^i, \beta) - w(L_R^i - L_C(c_i)) - \Psi(c_i, f_i)$$

$$\max_{L_Y} F(T_Y, L_Y, \sigma) + \left(L - \sum L_C^i(c_i) - L_Y\right) w$$

subject to the constraints, $L_C^i(c_i) \leq L_R^i$. I further define total labour in the resource sector as $L_R = \sum L_R^i$ and the marginal rate of transformation between conflict and coercion:

$$\kappa_i(c_i^*, c_{-i}) = \frac{\partial T_R^i(f_i, f_{-i})}{\partial L_C^i(c_i)} \cdot \frac{\partial c_i}{\partial f_i}$$

2.2.1.3 Motivation of assumption

In this sub-section I further motivate some of the assumptions of the model.

First, I assume there is a trade-off between the two dimensions of violence, conflict and coercion. Coercion requires monitoring of civilians to make sure they don't escape, soldiers and guns that could have been used against opponents. The more guns that are pointed at civilians the less can be pointed at other armed groups. For example, Keen (2005:43) notes that the forced labour camps in Sierra Leone were "kept under close surveillance". Further evidence

[6] Convex costs can be attributed to the difficulty in maintaining control and disciplining soldiers as the size of the army increases.

to this trade-off is in the contested Karen region in Burma, where coercion increased after a cease-fire agreement was signed: "[s]ince an informal cease-fire in January 2004, there has been comparatively little fighting ... [armed groups] have focused their energies more on oppressing the civilian population" (Karen Human Rights Group, 2006)

Second, I assume that armed groups employ labour, although in practice armed groups often tax output in areas under their control (United Nations Security Council, 2009, 2010). In this model there is no distinction between setting a tax rate or choosing a level of employment: taking a share of output is the same as taking all output and then paying a portion of this back to labour.[7] The insight remains that armed groups can only increase tax or decrease wages up to the point where civilians want to leave.

Third, there is no labour market for soldiers. This is a more realistic assumption, because the number of soldiers that a rebel leader can recruit depends on a list of non-market factors such as ethnicity, local grievances, external support, or the charisma of a rebel leader. For example, in a survey of ex-combatants in Sierra Leone, Humphreys and Weinstein (2004) conclude that "voluntary recruits rarely joined for . . . financial reasons and received little or no share of the loot".

Fourth, land is specific to each sector. Not all land is endowed with the same natural resources. Furthermore, even if land were fungible between resources, the proliferation of violence restricts the development of a rental market for land.

Fifth, armed groups only operate in one sector. Economic motives will keep armed groups in the more lucrative sector. However, the model also directly applies to settings where both civilians and armed groups produce the same

[7]For example, if $G(T_R, L_R) = L_R$, then a wage rate, w, is the same as a tax rate, $\tau = \frac{p_R - w}{w}$

commodity (as for example rice farming in the diamond-poor regions of Sierra Leone). This is still a two sector model, because the production function and prices could vary due to differences in market access and capacity to make use of economies of scale.

2.3 Analysis

2.3.1 Baseline Model: Monopoly armed group

I discuss first the results of the baseline model—the limiting case where there is no conflict between armed groups. For exposition purposes I make the further simplifying assumption that both sectors face the same production function, $\sigma = \beta$.

2.3.1.1 Solution

The equilibrium is best described graphically as in Figure 2.2. The x axis is total labour in resource sector. At the origin, all labour is in the yeoman sector and no labour in the resource sector. Moving to the right along the x axis labour moves from the yeoman to the resource sector up to point L where all labour is in the resource sector. The downward sloping curve is the marginal value product of labour in the resource sector, $V_R \equiv p_R G_2 (T_R, L_R, \beta)$; and the upward sloping curve is the marginal value product of labour in the yeoman sector, $V_Y \equiv F_2 (T_Y, L - L_R, \sigma)$. V_Y is also the labour supply curve in the resource sector. This is because armed groups need to pay free civilians at least their reservation wage, which is determined by the returns to subsistence farming.

It is useful to first look at equilibrium in absence of coercion. The far left diagram in Figure 2.2 shows the standard Ricardo-Viner solution. Labour,

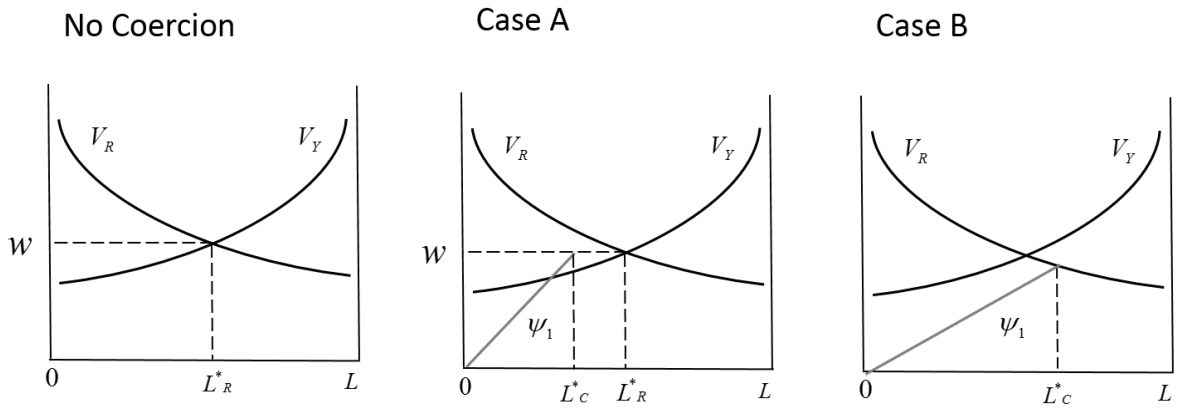


Figure 2.2: In **Case A**, armed groups coerce up to the point where it becomes cheaper to employ civilians. With **Case B**, it never becomes cheaper to employ civilians.

L_R, enters the resource sector until L_R^*, where the two marginal value product labour schedules are equalized:

$$p_R G_2(T_R, L_R^*, \beta) = F_2(T_Y, L - L_R^*, \sigma) \tag{2.1}$$

The equilibrium wage rate, w, is therefore V_Y evaluated at the equilibrium level of employment, L_R^*.

Next, I introduce coercion. The equilibrium depends on whether the constraint, $L_C \leq L_R$, binds or not. I refer to the situation when the constraint does not bind as **Case A** (centre diagram in Figure 2.2). In this case there is both forced and free labour in the resource sector. The market wage is determined in equilibrium by Equation (2.1), just as in the standard model with no coercion. The marginal benefit of coercion is the market wage rate, w: the cost that would have been paid if a civilian were freely employed. Armed groups therefore coerce up to L_C^*, where the marginal cost of coercing an additional unit of labour is equal to the market wage rate:

$$c^* = \frac{\phi}{\psi}\beta\left(T_Y/L + p_R^{\frac{1}{1-\beta}} T_R/L\right)^{1-\beta} \tag{2.2}$$

After this point it becomes cheaper to employ workers at the market wage.

It is in this sense that we can say that coercion results in the ownership of labour: all rents that would have accrued to labour, w, now accrue to the armed group.

Consider next **Case B** (far right in Figure 2.2): the corner solution where the constraint $L_C \leq L_R$ binds. In this case it is never cheaper for armed groups to employ civilians and all labour in the resource sector is coerced. Armed groups coerce up to point C, where the marginal cost of coercion is equal to the marginal revenue product of labour: $c^* = p_R G_2 \left(T_R, L_C \left(c^*\right)\right)$, which simplifies to:

$$c^* = \left(\frac{\phi}{\psi} p_R T_R^{1-\beta}\right)^{\frac{1}{2-\beta}}.$$

Under **Case B**, the marginal benefit of coercion is no longer the cost of labour, w, since there is no longer a market for free labour. The marginal benefit of coercion is now the productivity of labour in the resource sector, V_R. Note that production is inefficient under **Case B**, since total labour in the resource sector is above the optimal level of employment in a perfectly competitive equilibrium without coercion. Total output would therefore be higher in the absence of coercion. Yet, armed groups still find coercion more profitable. The efficient outcome is not realised, since prices are distorted: armed groups pay a price, ψ/ϕ, for labour, which is lower than the market wage. As a result, there is over-production in the resource sector.

From Figure 2.2, it is easy to see that **Case B** occurs when the marginal cost of coercion remains low, or when the yeoman sector is very productive, relative to resource sector.

2.3.1.2 Results

Comparative statics depend on whether the constraint $L_C \leq L_R$, binds or not.

Proposition 1 *Coercion depends on the* ***nature of the economy****. In Case A, coercion is higher if:*

1. *Labour is scarce, relative to land*
2. *Production in either sector is more labour intensive*
3. *The price of either commodity, p_R or p_Y, increases.*

In Case B, coercion is higher if p_R increase or if production in the resource section is more labour intensive.

Proof. All proofs are in the appendix, unless otherwise stated. ■

With **Case A**, the marginal benefit of coercion is wage rate, w, since the armed group trades off paying for labour or coercing labour. Coercion is more attractive when labour is more expensive. Civilians can earn a higher wage if labour in the yeoman sector is more valuable, either due to more productive labour, F_2, or more valuable produce, p_Y. Labour is more productive when it is scarce relative to land, or if production is more labour intensive. I define production as more "labour intensive", if labour contributes to a larger share of production given factor utilisation. In the case of a Cobb-Douglas production function this would mean a higher β.

Furthermore, the equilibrium wage is also higher when the price or endowment of land in the resource sector increases. This is because a more lucrative resource sector will attract labour away from the yeoman sector. As labour enters the resource sector the remaining labour in the yeoman sector becomes more productive due to a lower labour/land ratio.

In **Case B**, coercion no longer increases with w. Coercion now depends on the productivity of labour, V_R, not the cost of labour. In this case, a change in the value to labour in the yeoman sector will have no impact on the level of coercion. The total supply of labour, L, and endowment of land in the yeoman sector, T_Y, therefore has no impact on the level of coercion.

2.3.1.3 Historical Evidence - Factor Endowments and Slavery

Proposition one is consistent with the observation of many historical economists the variation in slavery across regions and time can be partly explained by the relative factor endowments in an economy.

According to Domar (1970) and Nieboer (1910) serfdom reemerged in Russia during 16^{th} and 17^{th} centuries, due to a scarcity of labour relative to land brought on by foreign conquests. Farm workers' marginal product of labour was higher and as a result they could demand higher wages. It was thus in the interest of landlords to prohibit workers from moving to a different land, so they cannot demand the market wage: "The scarce factor of production was not land but labo[u]r. Hence it was the ownership of peasants and not of land that could yield an income to the servitors or to any non-working landowning class" (Domar, 1970:19).

Similarly, Austin (2005) and Fenske (2012, 2013) argue that low population densities explain the prevalence of coercive labour markets in pre-colonial Ghana and the Egba in Nigeria. With abundant land, labour was the most valuable factor of production. As a result, property rights in land were not well defined, wage labour was absent, and slaves constituted the majority of labour.

This empirical support for the baseline model lends confidence to the validity of the full model, which I discuss in the following section.

2.3.2 Full Model Competing Armed groups

I now move to the full model where each armed group decides on both the level coercion, c_i, and conflict, f_i, at a joint cost $\Psi(c_i, f_i)$. I restrict discussion to the interior solution, where the constraints, $L_C^i(c_i) \leq L_R^i$, hold. I no longer assume that $\sigma = \beta$.

2.3.2.1 Solution

The only difference to the baseline model is that armed groups now also fight each other for control over land. Since there is a trade-off between conflict and coercion, the cost of coercion is now the opportunity cost of not fighting.

I first determine equilibrium factor prices. Both armed groups face the same wage rate, w, which is the value marginal product of labour in every sector, evaluated at the equilibrium level of total employment in the resource sector, L_R^* (as before defined by Equation (2.1)). Furthermore, both armed groups face the same implicit rental rate, $r = p_R G(T_R, L_R^*)$, the equilibrium marginal revenue product of land in the resource sector. r is the value to owning land, since the total revenue that accrues to each armed group, in absence of any coercion, is $T_R^i r$.[8] Note that w and r are independent of each armed group's decision to fight or coerce, because employment by each group adjusts to equalize the marginal revenue products of labour. This significantly simplifies analysis, since we can look at the choice of violence in absence of its impact on factor prices.

As before, the marginal benefit of coercion is the wage rate, w. Furthermore, fighting secures ownership over land so the marginal cost of coercion is: $\kappa(c_i^*, f_i, f_{-i}) \cdot r$, where κ is the total land that an armed group can capture if it foregoes one unit of coerced labour. The equilibrium level of coercion is

[8] $T_R^i r = p_R G(T_R^i, L_R^i) - w L_R^i$

therefore succinctly defined by the following equations:

$$w - \kappa\left(c_i^*, c_{-i}\right) \cdot r = 0 \tag{2.3}$$

which yields the following lemma.

Lemma 1 *Equilibrium level of coercion for each armed group, c_i^*, is:*

$$c_i^* = \left(\frac{1}{\psi_i}\right) \phi w - \alpha\left(1-\alpha\right)\left(\frac{1}{\phi}\right)\left(\frac{r}{w}\right) T_R \tag{2.4}$$

2.3.2.2 Results

It is evident from Equation (2.3) that the decision to coerce depends on the nature of the economy, the technologies of violence, and the distribution of military strength. I discuss first the impact of the distribution of military strength on the level of coercion, then the impact of the interaction between the economy and the military.

Proposition 2 *Coercion depends on the* ***distribution of military strength****: coercion is higher if one group is dominant relative to the other*

Proof. Inspection of Equation (2.4) ■

Equation 2.4 shows that coercion is smaller if the groups are of equal strength. Due to strategic fighting, armed groups resemble oligopolist producers of violence. They would prefer not to fight each other and allocate all their military resources to coercion. However, this is not in equilibrium. Each group wants to fight if the other does not, but needs to fight by more in anticipation that the other will do the same.

If there is one strong armed group it has more 'market power' in the production of violence. The strong group need not increase its fighting by much,

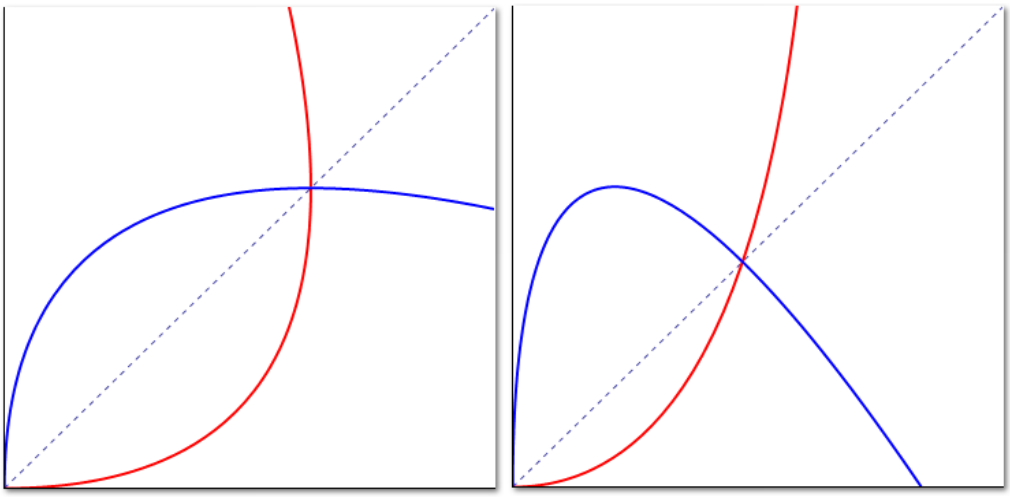


Figure 2.3: Reaction functions in conflict. In the left hand side, the groups are of equal strength. In the right hand side, one group—the red line at the bottom—is stronger relative to the other

as it less vulnerable to the other group's decision to fight. Total coercion in the economy is therefore higher if military power is centralised in one group.

Graphically, the left-hand-side and right-hand-side graphs in Figure 2.3 show the reaction functions if two groups have symmetric and asymmetric strength respectively. Two forces at play. Armed groups' conflict decisions are strategic compliments at low levels of conflict, but strategic substitutes at high levels. Both these effects are dampened for the stronger group (bottom line). He is less responsive to the weak group's level of conflict, because it has less influence on his own ability to capture land. For the same reason the weaker group (top line) is more responsive. Furthermore, the switch from compliments to substitutes also happens at a lower level of fighting for the weaker group, because he is more responsive. The net effect is that total conflict in the economy is lower.

A more intuitive way is to interpret the strategic element of conflict as defensive fighting. Imagine that each group has to at least match the other group's level of conflict if it wants to defend its land. If military power is

concentrated in one group, the strong group needs fewer resources to defend its land and can allocate the remainder to coercion. Furthermore, since the strong group allocates fewer resources to conflict, the weaker group can do the same. However, if the groups are of equal strength then each group has to allocate more resources to conflict to defend itself.

Proposition 3 *Coercion depends on the* ***interaction between the economy and military****. Coercion increases with price if:*

- *Production is very labour intensive*
- *Military power is centralised in one group*
- *Labour is easily exploitable*

Next, I discuss the impact of the economy on coercion. Comparative statics with regards to factor endowments and the labour intensity of production remain the same as in the baseline model. A higher endowment of land relative to labour or more labour intensive production leads to an increase in w, at the expense of r, shifting military resource to coercion.

The key difference to baseline model is that the impact of the resource price, p_R, on coercion is now ambiguous. This is because, as price increases, both the value to land and labour, w and r, increase, so coercion becomes both more attractive and expensive. The net effect depends on how responsive the factor prices are to a change in price and the relative effectiveness of conflict relative to coercion.

First, coercion decreases (increases) with price if production is sufficiently land (labour) intensive. This is because the rental (wage) rate is more responsive to a change in price, if the marginal product of land (labour) is higher.

Second, coercion decreases with price if military power is sufficiently decentralised, because conflict is more responsive to the rental rate. The intuition

follows from the discussion of Proposition 2. If the price increases both groups want to fight more both because fighting is more profitable, and in anticipation that the other group will do the same. Strategic fighting thus amplifies the impact of price on conflict. However, a more powerful group is less responsive to the other group's increase in conflict, because it is less vulnerable to attack from a weaker group. It is thus able to keep the total level of conflict down. This is result is perhaps best understood in the limiting case, where $\alpha \to 1$ or $\alpha \to 0$. We are again in a situation with only one armed group and coercion then depends only on wage rate, w.

Third, the comparative statics also depends on the technologies of violence. Coercion is higher and conflict lower if labour is more exploitable (high ϕ). Furthermore, conflict is also less responsive to price if labour is more exploitable, because conflict is less effective relative to coercion. As a result, an increase in price can only lead to an increase in total coercion if labour is sufficiently exploitable.

2.4 Case Studies

Although the main contribution of this paper is theoretical, I discuss two case studies that support the main result of the paper: the impact of the price of a commodity on coercion depends on the distribution of military strength.

2.4.1 Rubber Boom in the late 19^{th} century

The sharp increase in the global price of rubber in the late 19^{th} century provides a useful natural experiment for how an exogenous increase in price can have a different impact on coercion depending on underlying institutional factors.

In the 1890's there was a surge in demand for rubber, after Charles Goodyear perfected a process to produce rubber tires. Supply responded slowly, since rubber plantations would take decades to cultivate. As a result, the areas in the world that had a supply of wild rubber—in particular, Amazoniain South America and the Congo Free State[9] in central Africa— profited immensely from the tapping of rubber. In both the Congo Free State and Amazonia, the authorities faced a "labour problem" (Melby, 1942): tapping labour is arduous work and rubber needs to be collected deep in the forest, making monitoring of activity and restriction of movements difficult. Nonetheless, the labour relations that developed in the two regions differed dramatically.

In the Congo Free State a brutal coercive system of "unexampled horror and brutality" emerged, as documented in detail by Doyle (1909) and Edmund Morel. According to a testimony by a young officer in the area, "formerly the natives were well treated, but now expeditions have been sent in every direction, forcing natives to make rubber and to bring it to the stations" (Hochschild, 2011). The Congo Free State was run by a private company headed by King Leopold of Belgium, with its own private army, the *Forces Publique*. King Leopold developed a very effective method of restricting labour movements: holding family members hostage. Belgian officials posted throughout the colony would dispatch a 'capita'—a local Congolese who has received some military training—to all the neighbouring villages in the area. The capita would then request a quota of rubber from each able villager. If the local inhabitant did not bring in the required amount of rubber he was killed; if he fled, his family was killed. This coercive regime was systematically implemented all across the Congo Free State.

In contrast, the labour relationships that developed in Amazonia were not

[9]present day Democratic Republic of Congo

nearly as coercive. Weinstein (1983:15) argues that: "Rather than eroding existing relations of extraction and exchange, the Amazon rubber trade built upon ... traditional modes of extraction and exchange". Extraction was based on a series of trading relationships. A *seringueiro*[10] tapped the rubber and sold it to the *patraõ*, who owned rights to tap rubber on that land. The *patraõ*'s, in turn, sold it to traders who navigated the Amazon river and resold the rubber at economic centres.[11]

This is not to say that the *seringueiro's* lived under benign conditions. The proportion of revenue they could retain depended on the supply of labour, which varied substantially across areas. Furthermore, there were some cases of coercion, most famously the case of extreme cruelty in the Putumayo district, in current Peru (Hardenburg, 1912). However, these cases were not nearly as systematic as in the Congo Free State (Barham and Coomes, 1994a; Weinstein, 1983). In fact, Barham and Coomes (1994a) argue that tapping of rubber actually "provided substantial surplus to local participants", due to the low supply and high mobility of labour.

It is exactly under these conditions—a low supply of labour extracting a valuable commodity—that one would expect a coercive regime to emerge. Indeed the landlords attempted to restrict labour movements. They passed the "Rules of the Rubber Fields" which prohibited *seringueiro*'s from leaving their original *patraõ*. However, these laws were never adequately enforced (Weinstein, 1983:30).

A possible reason for the highly coercive regime in the Congo Free State, in contrast to Amazonia, is the difference in the distribution of military powers in the two regions. In the Congo Free State, the Belgian army was the only viable

[10] usually an indigenous inhabitant, but sometimes also European fortune-seekers

[11] There was a lot of variation in property rights over land. In some cases the *seringueiro* also owned concessionary rights. Sometimes individuals owned the land, sometimes the state owned the land

military force. In Amazonia, there was a long-standing conflict between the Spanish and Portuguese colonies, which intensified during the rubber boom. This conflict required "extraordinary expenditures of state funds" (Barham and Coomes, 1994b:96), so less military resources remained for coercion. As a result, the landlords were themselves responsible to enforce the coercive laws, but lacked the military resources to do so. Notably, in the one region with documented cases of coercion (the Putumayo region), a private company was granted exclusive policing rights and thus exercised "complete and unmediated power" (Weinstein, 1983:26).

Furthermore, Weinstein (1983) notes that the "Rules of the Rubber Fields" were most poorly enforced in areas of violent feuds between local elites. So, the increase in price not only lead to more conflict between colonial powers, but also conflict between landlords within the colony, which further limited their ability to coerce.

2.4.2 Coercion in the Rice Plantations in Sierra Leone

Proposition three also provides a possible explanation to the surprising observation that during the Sierra Leonean Civil War coercion was far more pervasive in the rice plantations than in the diamond mines.

According to human rights reports of that time, armed groups would set up camp in a village and then force villagers to work on their own farms (Keen, 2005:43). Villagers did not flee for fear of the lives of their family members left behind.[12] In contrast, a detailed reading of human rights reports reveals no such accounts of coercion in the diamond mines. In fact, a study that documented civilian abuse during the civil war through interviewing ex-combatants actually found that abuse was far *lower* in the diamond-rich districts of Kono and

[12]their strategy to restrict was therefore very similar to King Leopold's *Forces Publique* in the Congo Free State

Kainadugu (Humphreys and Weinstein, 2004).

This geographic variation in coercion cannot be explained by the characteristics of the armed groups or by different institutional contexts. Armed groups were involved in both mining and farming, and this all occurred within the same country. Furthermore, it is unlikely to be driven by ecological factors: Sierra Leone is a very small country with limited ecological variation and the rice plantations can be grown throughout the country. The key difference is that in some areas there are diamonds and others not. It is thus plausible that any difference in coercion can be attributed to the characteristics of the two resources.

A possible interpretation for fewer accounts of coercion in the diamond mines is that land was the valuable factor of production, not labour. Military resource were thus allocated to conflict and not coercion. As further evidence, even though abuse was lower, conflict was demonstrably higher in the diamond-rich areas: Bellows and Miguel (2006) find that chiefdoms with more diamond mines experienced more attacks and battles during the Civil War.

Although suggestive, neither of these case studies can be considered conclusive proof. There were a lot of other differences between Amazonia and Congo Free State, and not only the distribution of military strength. In Sierra Leone the commodities also differ in the technology of production. Coercion might be higher in rice plantation because production is more labour intensive or the resource more diffuse, for example.

Nonetheless, the combination of these examples tell a convincing story that armed groups make strategic decisions between conflict and coercion, and this decision is driven, in part, by a change in factor prices and the anticipation of other armed groups' use of violence.

2.5 Conclusion

In this chapter I combined a simple two-sector trade model with a model of violence to explain variation in coercive practices employed by armed groups. Armed groups operate in the resource sector and decide how to allocate military resources between conflict and coercion, to capture land and labour respectively. I showed how coercion depends on both the nature of the economy and the distribution of military strength. First, coercion is higher if production is labour intensive or labour is scarce relative to land. Second, coercion is higher if one group is dominant relative to the others. Third, the impact of the price of the resource on coercion is ambiguous. Contrary to other studies, I show that coercion could actually decrease with price if military strength is very decentralised, or production is very land intensive. These results are supported by anecdotal evidence of variation in coercive practices.

The results of this paper provide two insights to policy makers. First, the impact of trade policy on violence against civilians depends on the type of commodity and military conditions on the ground. Trade sanctions aimed at reducing domestic commodity prices could lead to an increase in coercion and reduction in conflict if one group is dominant relative to others, or if production is very labour intensive. The same policy could have opposite effect if military power is decentralised or production not that labour intensive.

Second, peace is not necessarily good for civilians. For example, in the contested Karen province in Burma coercion increased after a cease-fire agreement was signed (Karen Human Rights Group, 2006). Cease-fire agreements could thus be seen as a form of collusion between armed groups and peace-keeping forces might make it easier to collude by increasing the cost of conflict. For example, Hultman (2010) finds that violence by rebels against civilians is higher when there are peace operations in an area. An effective peacekeeping force

therefore needs to be combined with a process of disarmament. Similarly, centralization of power, although necessary for peace, could lead to an increase in the exploitation of civilians. Things could get worse before they get better.

Very little is known about armed groups' use of violence in civil conflicts. Yet, this is where the development challenges are most acute and civilian suffering most severe. Characterising armed groups as "firms with a gun"—economic agents who use violence to control the factors of production—can provide some insights into their behaviour. Theoretical contributions to the question of armed groups' use of violence against civilians have so far been limited. This paper is thus an important contribution to a question which has received little attention from economic theorists up to now.

2.A Appendix

2.A.1 Extension - Many armed groups

I extend the model to many armed groups, but make the simplifying assumption that each group has equal strength and equal cost of recruiting soldiers.

2.A.1.1 Model

There are n armed groups, who fight each other for ownership over land in the resource sector, T_R. Group i decides on the number civilian workers L_R^i, the level of coercion, c_i, which captures $L_C^i(c_i) = \phi c_i$ workers, and the level of fighting, f_i, which assigns a proportion $\pi(f_i, f_{-i})$ of land T_R to group i, where:

$$\pi\left(f_i, f_{-i}\right) = \left(\frac{f_i}{\sum f_j}\right)$$

Equilibrium is thus characterized by the following:

$$\max_{L_i, c_i, f_i} p_R G(\pi\left(f_i, f_{-i}\right) \cdot T_R, L_R^i) - w(L_R^i - L_C\left(c_i\right)) - \psi \frac{1}{2}\left(c_i + f_i\right)^2$$

$$\max_{L_Y} F(T_Y, L_Y) + \left(L - \sum L_C^i\left(c_i\right) - L_Y\right) w$$

subject to the following constraints

$$L_C^i\left(c_i\right) \leq L_R^i$$

Total labour in the resource sector is defined as $L_R = \sum L_R^i$.

2.A.1.2 Results

Proposition 4 *1. Each group's individual level of coercion, c^*, decreases with the number of armed groups, n.*

2. The total level of coercion, $\sum c^*$, decreases with the number of armed groups, provided that:

$$n^2 < \frac{\psi r T_R}{(\phi w)^2}$$

3. Total coercion decreases with price, p_R, provided that the number of armed groups is sufficiently large.

The existence of many armed groups is analogous to the case where groups have symmetric strength. In both cases, military power is decentralised and no armed group has 'market power' in the production of violence. As competition for land become more intense due to a proliferation of armed groups, then groups are less able to allocate of resources towards coercion. Furthermore, conflict is more responsive to the rental rate, because groups are more responsive to each others' levels of fighting.

2.A.2 Proofs

Proof. Of proposition 1

When the constraint $L_C \leq L_R$, does not bind, then equilibrium level of coercion, c, is:

$$c^* \left(T_Y, T_R, L, \beta, \sigma, p_R, \psi\right) = \frac{\phi}{\psi} F_2 \left(T_Y, L - L_R^*, \sigma\right)$$

where L_R^* is defined by:

$$V_R \left(T_R, L_R^*, \beta, p_R\right) - V_Y (T_Y, L - L_R^*, \sigma) = 0 \tag{2.5}$$

Comparative statics with respect to any exogenous variable, X, is:

$$\frac{\partial c^*}{\partial X} = \frac{1}{\psi}\left[\frac{\partial V_Y}{\partial X} + \frac{1}{\Delta}\left(\frac{\partial V_Y}{\partial L}\right)\left(\frac{\partial V_R}{\partial X} + \frac{\partial V_Y}{\partial X}\right)\right]$$

where

$$\Delta = p_R G_{22} + F_{22} < 0$$

Coercion therefore increases if the marginal value product in either sector increases. It is clear that coercion increases with p_R, T_R, β, and σ, and decreases with L.

When the constraint, $L_C \leq L_R$, binds then coercion, c^*, is defined by:

$$c^* = \frac{\phi}{\psi} p_R G_2(T_R, L_C(c^*), \beta) \tag{2.6}$$

Total differentiating Equation (2.6):

$$\begin{aligned}
\frac{\partial c^{**}}{\partial p_R} &= -\frac{G_2}{p_R G_{22}} > 0 \\
\frac{\partial c^{**}}{\partial T_R} &= -\frac{p_R G_{12}}{p_R G_{22}} > 0 \\
\frac{\partial c^{**}}{\partial \beta} &= -\frac{p_R G_{23}}{p_R G_{22}} > 0
\end{aligned}$$

■

Proof. Of lemma 1

Restrict solution to the case where $\sum L_C^i(c_i) \leq L_R$.

Equilibrium is characterized by the following:

$$\max_{L_i, c_i, f_i} p_R G(T_R(f_i, f_{-i}), L_i, \beta) - w(L_R^i - L_C^i(c_i)) - \frac{\psi_i}{2}(c_i + f_i)^2$$

$$\max_{L_Y} F(T_Y, L_Y, \sigma) + w(L - \sum L_C^i(c) - L_Y)$$

The first order conditions are:

$$f_i : p_R G_1(T_R \cdot \pi(f_i, f_{-i}), L_R^i, \beta) \cdot T_R \cdot \pi_1(f_i, f_{-i}) = \psi_i \left(c_i + f_i\right) \tag{2.7}$$

$$c_i : \phi w = \psi_i \left(c_i + f_i\right) \tag{2.8}$$

$$L_R^i : p_R G_2(T_R \cdot \pi(f_i, f_{-i}), L_R^{i*}, \beta) - w = 0 \tag{2.9}$$

$$L_Y : F_2(T_Y, L_Y, \sigma) - w = 0 \tag{2.10}$$

Given constant returns to scale, we know from Equation (2.9) that the factor ratio's for armed groups in the resource sector are the same:

$$\frac{T_R^i}{L_R^{i*}} = \frac{T_R}{L_R^*}$$

As a result, the marginal products are also the same:

$$G_2\left(T_R^i, L_R^i, \beta\right) = G_2\left(T_R^{-i}, L_R^{-i}, \beta\right) = G_2(T_R, L_R, \beta)$$

$$G_1(T_R^i, L_R^i, \beta) = G_1\left(T_R^{-i}, L_R^{-i}, \beta\right) = G_1(T_R, L_R, \beta)$$

Now we can compute total level of employment in the resource sector, L_R, which equalises the marginal value products between sectors. Substituting Equation (2.9) into Equation (2.10) we get:

$$p_R G_2(T_R, L_R^*, \beta) = F_2(T_Y, L - L_R^*, \sigma)$$

The factor prices, w and r, are thus uniquely determined in equilibrium by the level of employment in the resource sector, L_R^*:

$$r = p_R G_1(T_R, L_R^*, \beta) \text{ and } w = p_R G_1(T_R, L_R^*, \beta)$$

Since each armed group faces the same factor prices, and factor prices are independent of f_i, the first order conditions from Equation (2.7) are significantly simplified. Combining Equations (2.7) and (2.8) yield:

$$f_1 : T_R r \frac{\alpha\left(1-\alpha\right) f_2}{\left(\alpha f_1 + \left(1-\alpha\right) f_2\right)^2} = \phi w \tag{2.11}$$

$$f_2 : T_R r \frac{\alpha\left(1-\alpha\right) f_1}{\left(\alpha f_1 + \left(1-\alpha\right) f_2\right)^2} = \phi w \tag{2.12}$$

Combining equations 2.11 and 2.12 yields $f_1 = f_2$. Total fighting for each group is therefore:

$$f_i = \alpha\left(1-\alpha\right)\left(\frac{1}{\phi}\right)\left(\frac{r}{w}\right) T_R \tag{2.13}$$

Substituting Equation (2.13) into Equation (2.8):

$$c_i = \phi\left(\frac{1}{\psi_1}\right) w - \alpha\left(1-\alpha\right)\left(\frac{1}{\phi}\right)\left(\frac{r}{w}\right) T_R \tag{2.14}$$

■

Proof. of proposition 3

Equation (2.14) can also be written as:

$$C^* = \left(\frac{\psi_1 + \psi_2}{\psi_1 \psi_2}\right)\phi w - 2\alpha\left(1-\alpha\right)\left(\frac{1}{\phi}\right)\left(\frac{1-\beta}{\beta}\right) L_R^* \tag{2.15}$$

The first term always increases with price, whereas the second term always decreases with price, since $\partial L_R^* / \partial p_R > 0$.

Total differentiating Equation (2.15) yields:

$$\frac{\partial C^*}{\partial p_R} = \frac{\partial L_R}{\partial p_R}\left(\left(\frac{\psi_1 + \psi_2}{\psi_1 \psi_2}\right)\phi \frac{\partial V_Y}{\partial L_R} - \alpha\left(1-\alpha\right)\left(\frac{1}{\phi}\right)\left(\frac{1-\beta}{\beta}\right)\right)$$

Which is positive provided that

$$\frac{\psi_1 + \psi_2}{\psi_1 \psi_2} \phi \frac{\partial V_Y}{\partial L_R} > \alpha (1 - \alpha) \left(\frac{1}{\phi} \right) \left(\frac{1 - \beta}{\beta} \right)$$

It is clear that this is true if β is large, ϕ is large, or α is close to $\frac{1}{2}$. ■

Proof. Of proposition 4

The first order conditions in the use of violence are:

$$\frac{\partial \pi (f_i, f_{-i})}{\partial f_i} T_R r = \psi (c_i + f_i) \tag{2.16}$$

$$\frac{\partial L_C}{\partial c_i} w = \psi (c_i + f_i) \tag{2.17}$$

where, as before $r = p_R G_1(T_R, L_R^*, \beta)$ and $w = p_R G_1(T_R, L_R^*, \beta)$ and L_R^* is defined by Equation 2.1

Due to symmetry, $f_i = f_{-i} = f$. Solving Equation (5.2) yields:

$$f = T_R \frac{r}{\phi w} \left(\frac{n - 1}{n^2} \right)$$

Substituting into Equation (5.1), each group's level of coercion, c^*, is:

$$c^* = \frac{\phi}{\psi} w - T_R \left(\frac{r}{\phi w} \right) \left(\frac{n - 1}{n^2} \right) \tag{2.18}$$

Each group's level of coercion decreases with the number of armed groups, provided that $n > 2$, since:

$$\frac{\partial c}{\partial n} = -\frac{(n - 2)}{n^3} \left(\frac{r}{\phi w} \right) T_R < 0$$

I further define total level of coercion, $C^* = \sum c^*$, as:

$$C^* = \frac{\phi}{\psi} nw - T_R \left(\frac{r}{\phi w}\right)\left(\frac{n-1}{n}\right)$$

so

$$\frac{\partial C^*}{\partial n} = \frac{\phi}{\psi} w - T_R \left(\frac{r}{\phi w}\right)\left(\frac{1}{n^2}\right)$$

Total coercion, C^*, therefore decreases with number of armed groups, n, if:

$$n^2 < \frac{\psi r T_R}{(\phi w)^2}$$

Equation (5.3) can also be written as:

$$C^* = n\frac{\phi}{\psi} w - L_R^* \left(\frac{1}{\phi}\right)\left(\frac{n-1}{n}\right)$$

$$\frac{\partial c^*}{\partial p_R} = n\frac{\partial L_R}{\partial p_R}\left(\frac{\phi}{\psi}\frac{\partial V_Y}{\partial L} - \left(\frac{1}{\phi}\right)\left(\frac{n-1}{n^2}\right)\left(\frac{1-\beta}{\beta}\right)\right)$$

Which is positive for small n. ■

Chapter 3

Reconciliation, Conflict, and Development: Evidence from Sierra Leone

Abstract

Can reconciliation generate social cohesion and economic development within societies emerging out of civil war? We conduct a randomized controlled trial of a community-based reconciliation intervention in post-conflict Sierra Leone. The program provides a forum for villagers to air war-time grievances, and also forges institutions designed to improve conflict resolution and build social capital. We find that respondents who received the intervention are more forgiving and are more charitable in their views of ex-combatants. Furthermore, respondents are more active in village organisations and conflicts are more satisfactory involved. However, there is no impact on trust, social networks, conflict incidence, or economic activity. Most strikingly, psychological health—depression, post-traumatic stress disorder and anxiety—deteriorated. This study has direct implications for the design of transitional justice programs, as well as programs that aim to promote institutional change.

3.1 Introduction

Reconciliation programs—that aim to heal fractured relationships through airing of war-time grievances—are considered a critical component of post-conflict recovery. Almost every country emerging out of conflict over the past

Co-authored with Oeindrila Dube (New York University) and Bilal Siddiqi (Stanford University)

two decades instated such a program.[1] The stated objectives are ambitious: talking about the past leads to personal healing, improved inter-personal relations, economic development, and lasting peace. Yet, advocates are able to offer little rigorous evidence for this. In a review of the evidence, Mendeloff (2004) concludes that these claims "rest far more on faith than on sound logic or empirical evidence".

We evaluate a community reconciliation program in Sierra Leone implemented by local NGO, Fambul Tok ("family talk in Krio, a local language), to address the widespread human rights abuses that were committed during Sierra Leone's decade long civil war. The cornerstone of the program is a traditional bonfire ceremony to facilitate truth-telling. At this ceremony, survivors share their stories and perpetrators seek—and on many occasions receive—forgiveness for crimes committed during the war. In addition, Fambul Tok introduces a set of institutional structures aimed at strengthening social capital: a Reconciliation Committee, which includes traditional leaders and other community members, shepherds the reconciliation process; and a Peace Tree serves as a focal point for promoting dispute resolution.

Fambul Tok is distinct from common state-driven traditional justice programs. Most countries opt for Truth and Reconciliation Commissions (TRC)[2], and international criminal tribunals that trial the worst offenders during the war. Both of these programs are extremely costly and can only reach a small sub-set of the population. Fambul Tok is an inclusive, cost-effective alternative to these programs. However, Fambul Tok also shares a 'truth-telling' component with all other transitional justice programs, as they all require survivors of past atrocities to talk about their war experiences. Results of this study can

[1]Some examples include: Chad, Columbia, Congo, El Salvador, Fiji, Ghana, Guatemala, Kenya, Liberia, Morocco, Nigeria, Peru, Rwanda, Sierra Leone, Solomon Islands, South Africa, South Korea, Sri Lanka, Sudan, East Timor, Uganda

[2]27 countries to date have set up TRC's

thus inform design of other transitional justice programs.

Fambul Tok identified 40 communities in five of Sierra Leone's 13 districts that were willing to participate in the program.[3] We surveyed these communities and randomly assigned half of them to the program. We revisited each community nine months after they have undergone the reconciliation ceremony, and resurveyed our original respondents.

We chose to evaluate the intervention on a broad set of dimensions related to reconciliation, as well as the possible downstream benefits on collective action and economic welfare. Thus, we ask questions related to forgiveness and psychological wellbeing, attitudes towards ex-combatants and women, trust, the strength of social networks, conflict incidence and resolution, economic activity, public goods provision, and household socioeconomic welfare. Given the large set of hypotheses and outcomes of interest, we pre-registered our evaluation design to tie our hands and resist the follies of 'fishing' (Casey et al., 2012; Humphreys et al., 2013).[4]

The results show that those that received the reconciliation treatment were more likely to join and partake in community organizations. In addition, conflict resolution improved: more conflicts were satisfactory resolved and more were resolved by the chief or village elders. However, there was no effect on levels of trust, conflict incidence or economic activity.

Furthermore, we find striking evidence that talking about the past is a mixed blessing: while treated respondents are more forgiving and charitable towards ex-combatants, their psychological wellbeing deteriorated significantly: respondents who received the treatment were also more likely to express symp-

[3]The study was structured to proceed in three waves, enabling Fambul Tok to work within its capacity. Fambul Tok identified an additional 120 communities in two subsequent waves. Here we only present results for the first wave.

[4]Our pre-analysis plan is registered with the Experiments in Governance and Politics (EGAP) design repository and can be accessed at `http://bit.ly/FambulTok`.

toms of post-traumatic stress disorder (PTSD) and depression. The price of forgiveness is high. This finding resonates with studies in the psychological literature that advise against debriefing patients that suffer from PTSD, as they are vulnerable to re-traumatization (Rose et al., 2002).[5]

This is the first randomised impact evaluation of a reconciliation program. Staub et al. (2005) examine the psychological impact of a healing intervention in Rwanda, conducted via a seminar in which facilitators were trained to lead discussions on coping with painful experiences. However, the intervention did not bring together victims and perpetrators or examine social capital outcomes.

Our work makes two important contributions to the design of post-conflict programs. First, we find that the creation of new institutions *can* have a positive impact on social relations. This is in contrast to previous studies of Community Driven Development/Reconstruction (CDD/R) programs that found little or not impact on attempts to induce institutional change in post-conflict societies (Avdeenko and Gilligan, 2012; Casey et al., 2012; Humphreys et al., 2012).[6] Although it is difficult to draw strong comparisons between Fambul Tok and other CDD/R programs, one possible reason for Fambul Tok's success is that it worked with, and built on, existing power structures, in particular the village chiefs. Previous attempts at institutional reform in Sierra Leone tried to circumvent the power of the chiefs by creating new institutions such as local councils and magistrate courts. However, community decision-making was no more inclusive or accountable (Casey et al., 2012) and *de facto* power still remained with the chiefs (Jackson, 2006; Sawyer, 2008). This suggests that programs might be more effective at changing institutions, if they reform

[5]Previous qualitative studies have also suggested that victims could suffer from truth commissions. Brouneus (2008), for example, records re-traumatization, ill-health, and social exclusion experienced by 16 women who testified in the *Gacaca* courts in Rwanda. However, this is the first study that can infer a causal relationship.

[6]See King et al. (2010) for a recent synthetic review.

existing power structures, rather than create new ones.

Second, our finding on psychological health has direct implication to all 'truth-telling' transitional justice programs. Programs that require testimonies from survivors should be designed to minimise psychological harm.

The remainder of the paper is organised as follows. Section two provides some background on the conflict and institutional context in Sierra Leone. Section three discusses the intervention and evaluation design. Section four develops the research design with particular reference to the pre-analysis plan. Section five discusses the data, while section six presents the results. Lastly, section seven discusses the results and section eight concludes.

3.2 Background

3.2.1 Conflict and Reconciliation in Sierra Leone

Sierra Leone experienced a brutal Civil War, lasting between 1991 and 2002. The war had no overt ethnic dimensions, but the violence was both pervasive and personal, committed between people from the same community who know each other (Keen, 2005). The purpose of Fambuk Tok is therefore to address personal wartime grievance between members of the same community, rather than promote reconciliation between different ethnic groups.

Following the conflict, the Sierra Leonean government and international community set a Special Court to trial the largest perpetrators. The Special Court is very expensive—it cost $125 million in the first four years (Cassese, 2006)—yet indicted only 13 people after a decade of operation.[7] The Sierra Leonean government also set up a TRC which heard over 7,000 statements,[8]

[7] `http://www.sc-sl.org`, retrieved 16 May 2013
[8] `http://www.sierraleonetrc.org`, retrieved 16 May 2013

This is, however, only a fraction of all the atrocities committed. Fambul Tok was created to fill this gap and reaches the many communities and individuals whose war-time grievances were not addressed by the court and TRC.

3.2.2 Chiefs and Institutional Reform in Sierra Leone

A key institutional legacy of British indirect rule in Sierra Leone is the strong chieftaincy system. With the creation of the protectorate in 1898, the Paramount Chiefs (the highest level of traditional chiefs) were given substantial powers over their subjects (Acemoglu et al., 2013; Fanthorpe, 2001; Jackson, 2006). In fact, chiefs are so powerful, that some have attributed the civil war to local grievances generated from chiefs' abuse of power (Mokuwa et al., 2011; Richards, 2005a).

As a result, there were attempts after the war to reform the chieftaincy system and create more accountable, formal decentralized sources of power. This included democratically elected district councils that set tax rates, and local magistrates that administer justice. However, *de facto* power still remains with the chiefs (Jackson, 2006; Sawyer, 2008). Although there is concern of abuse of power, chiefs play an important positive role in enforcement of local public goods provision, collection and distribution of resources, and dispute resolution. In sort, they therefore perform many the functions normally reserved for the state.

3.3 Intervention and Evaluation Design

3.3.1 Fambul Tok

Fambul Tok is a Sierra Leonean NGO that aims to promote post-conflict reconciliation and development. The NGO started in 2007 and operates in five

of Sierra Leone's 13 districts. The cornerstone of the Fambul Tok program is a reconciliation bonfire ceremony, which is organized to facilitate the airing of wartime grievances. The intervention is targeted at the level of a section, which are contiguous clusters of up to ten villages. Thus, several villages participate in a joint ceremony. During the bonfire ceremony, victims share their stories and perpetrators seek—and on many occasions receive—forgiveness for crimes committed during the nation's thirteen-year civil war. In our own monitoring of the ceremonies, about 40% of confessions were peacefully reconciled.

The ceremony is preceded by a period of consultation and community sensitisation. As a first point of entry, Fambul Tok has a consultation meeting with all the village chiefs of the section to get consent and support for the project. Next, Fambul Tok establishes two groups: a Reconciliation Committee, consisting of village chiefs, religious and youth leaders, as well as survivors and/or perpetrators from the war; and an Outreach Committee, consisting mostly of youth. These groups receive training and responsibility for publicising and planning the ceremony.

In addition to the bonfire ceremony, Fambul Tok also establishes new institutions, designed to encourage collective action and improve conflict resolution. Fambul Tok helps set up communal farms, where labour and output are shared on land set aside by the community as a pledge towards reconciliation. Furthermore, Fambul Tok instates a 'Peace Tree', that provides a focal point for the community to gather and resolve disputes; and also creates a 'Peace Mothers' group, that seeks to promote women's economic activities and facilitate discussion of atrocities perpetrated during the war.

Fambul Tok emphasises use of local traditions and institutions. The chiefs, traditional harbourers of authority in the villages, play a large component in organisation leading up to the bonfires. Fambul Tok's first point of entry

into the section is a discussion with the section and village chiefs. Conflict mediation through the new 'peace tree' relies on elders and chiefs, who are traditionally responsible for exercising justice. The bonfire ceremony draws from old hunting traditions and combined with traditional and religious rituals, including prayers, dancing, and a 'cleansing' ceremony.

3.3.2 Evaluation Design

We evaluate the program via a randomized field experiment. Fambul Tok identified 40 sections in which they wish to operate: 8 sections in five of Sierra Leone's 13 districts (Bombali, Kailahun, Koinadugu, Kono, and Moyamba). These sections were identified before Fambul Tok started the consultation process. We matched sections into pairs, stratified by district, based on the 'optimal greedy' algorithm (Greevy et al., 2004; Imai et al., 2009), using baseline data collected prior to the intervention. We then randomly assigned one section in each pair to treatment via public lotteries. Matching was based on six aggregate statistics: total frequency of lending and borrowing and sharing of farm labour; group membership; conflict and crime incidence; and an index of asset ownership, exposure to violence and psychological health.

We conducted household and village surveys in two villages within each section. One village was the section headquarter, where the ceremony is typically held, and the second a randomly chosen village. We revisited each community 9 months after the reconciliation ceremony to conduct the endline survey.

Respondents were randomly sampled in-field. Since the household survey includes both household-level and individual-level questions, the in-field randomization occurred in two phases: first, households were randomly chosen, and then an individual respondent was randomly selected from each household. Typically 12 respondents were sampled in each village, although in some

villages the number was 10 or 11, so the total number of baseline observations are 952. A village-level survey was also conducted in both the baseline and endline rounds.

The evaluation was divided into three waves, to allow Fambul Tok to work within its capacity. Fambul Tok identified an additional set of 120 sections in two subsequent waves. This chapter only presents results from the first wave, as we have not yet collected endline data for the subsequent waves.

There was contamination in one control section, where 11 out of the 24 respondents report to have attended a bonfire ceremony. This could bias the results towards zero. However, since we performed pair-wise matching for the randomisation, we can drop the section pair where contamination took place (King et al., 2007). The restricted sample is unbiased but less representative. We therefore report results for both the restricted and full sample.

3.3.3 Potential Mechanisms

Reconciliation, the restoration of fractured relationships, can lead to changed attitudes and behaviour at a personal, inter-personal and group level.

First, it can affect individual levels of forgiveness and psychological well-being, as survivors talk about their past experiences and perpetrators are given the opportunity to express remorse and request forgiveness. The act of forgiveness and repentance, or even the act of writing about trauma, has been shown to aid psychological healing (Esterling et al., 1999; Exline et al., 2000; Reed et al., 2006; Smyth et al., 1998; West, 2001).

Second, it is possible that community members' beliefs and attitudes towards ex-combatants change, due to a reinterpretation and reframing of the past. As an ex-combatant expresses remorse, the community distinguishes be-

tween a bad person and a bad act.[9] As a result, villagers could become more trusting towards perpetrators. Changed beliefs would also help to integrate perpetrators back into the community, as more community members are willing to interact with the perpetrator.

Third, changed beliefs and network structure can in turn lead to improved inter-personal relations, which manifests itself in reciprocal economic activity, increased willingness to contribute to public goods, and less conflicting relations. Ongoing trusting relationships act as an informal enforcement mechanism, thus overcoming commitment problems in economic exchange (Fehr et al., 1997). The role of informal relationships to enforce contracts is even more important in the Sierra Leonean context, where the state's reach is limited.

However, the intervention could also have the opposite effect. Talking about the past could worsen psychological health, by triggering old memories of traumatic events. Similarly, perpetrators could become less trusted and integrated as community members are reminded (or maybe informed) of the atrocities committed by the individual. Whether the net effect is positive or negative is an empirical question, which the field experiment aims to answer.

3.4 Empirical Strategy and Pre-Analysis Plan

3.4.1 Pre-Analysis Plan

Prior to the analysis we constructed and pre-registered[10] a pre-analysis plan (PAP) (in appendix), as done by many recent experiments (Beath et al., 2012;

[9] Rigby (2006) for example, quotes a human rights worker who "is able to justify forgiveness by reference to the fact that they were not fully responsible - 'they were drugged'

[10] submitted at the Experiments in Governance and Politics (EGAP) design repository on 12 December, 2012

Casey et al., 2012; Humphreys et al., 2012). This pre-commits us to a detailed list of all hypotheses we aim to test, the outcomes that constitute each hypothesis, and the empirical strategy to test this. The analysis plan was written while the endline data was collected but prior to the time we analyzed the data.[11]

The 16 hypotheses outlined in the PAP are: successful project implementation (H01); forgiveness (H02); psychological wellbeing (H03); attitudes towards ex-combatants (H04); war-related and gender attitudes (H05 and H06); incidence of crime and conflict (H07); conflict resolution (H08); traditional justice (H09);trust (H10); inclusion and tensions (H11); social networks (H12); group membership (H13); public goods provision (H14); economic activity (H15); and economic welfare (H16). The grouping of specific survey questions for each of the hypotheses are also outlined in greater detail in the PAP document.

Furthermore, since we use multiple outcomes to test the impact of Fambul Tok, a secondary concern is an over-rejection of the null hypothesis, due to multiple inferencing. We mitigate this concern by aggregating outcomes into a mean index for each hypothesis, as in Anderson (2008) and Kling et al. (2007). In both cases, the mean index is a sum of all the standardised outcomes in each hypothesis:

$$z_{ij} = \frac{y_{ij} - \bar{y}_{jk}}{\sigma_{jk}}$$

where σ_{jk} and $\bar{y}_{jk}$ are the control standard deviation and mean for outcome j.

The Anderson index further weights the standardised outcomes by the inverse of the variance-covariance matrix. This is a more efficient estimator, since it places less weight on outcomes that add no extra information, due to high variance or high correlation with other indicators. Kling (2007) does not

[11]The data was encrypted and password protected by our research assistant to ensure that we could not access it prior to the completion of the PAP.

weight the outcomes and unlike Anderson it imputes the mean of the control (treatment) group to missing values in the control (treatment) group.

3.4.1.1 Deviation from pre-analysis plan

We made three deviations from the pre-analysis plan, due to an analytical constraint not identified before the study.

As in Casey et al. (2012) and Humphreys et al. (2012), some of our hypothesises combined 'conditional' outcomes, that relate to a sub-sample of respondents, with 'unconditional' outcomes, that relate to the full sample. However, this could create a bias in the Anderson due to sample selection, and also a false rejection of the null in the Kling index. For example, in the trust hypothesis (H10) all respondents answered how much they trust people in general, but only respondents who knew ex-combatants answered how much they trusted ex-combatants. If the treatment increases the proportion of people who know ex-combatants, and this new group of people have higher levels of 'generalised trust' compared to the whole population, then the treatment effect using the Anderson index would be upwardly biased. In contrast, the imputation of missing values in the 'unconditional hypothesis' artificially reduces the standard errors for the Kling index, because the sample size increases, whilst holding the treatment and control means unchanged and not increasing variation.

We therefore made changes in three hypotheses that contained both 'conditional' and 'unconditional' outcomes. In the trust hypothesis (H10), we excluded the trust questions that relate to ex-combatants/migrants from the mean index and reported them separately. Results remain unchanged. Similarly, in the 'traditional justice' hypothesis (H09), the outcome 'conflict resolved by chief' only relates to villages where individuals experienced conflicts in the past, whereas 'fines' relates to all individuals. However, since the hypothesis

contains only 2 outcomes, separating them would render the mean index meaningless. For this reason, we decided to subsume the 'conflict resolved by chief' in the 'conflict resolution' hypothesis (H08). We feel this is the appropriate category, since all outcomes in that hypothesis relate to the same sub-group of villages that have experienced conflicts. The same issues apply to the forgiveness hypothesis (H02), because it aggregates forgiveness index across 3 nested sub-groups: those that experienced hurt in the past, those who know the perpetrator, and those whose perpetrator still resides in the village. The sample sizes of the final two subgroups are too small for any useful analysis. We therefore only look at the first group: forgiveness of all people who experienced hurt in the past.

We report the results of the original mean effects in the appendix. As a general point, we feel that good analysis should not be victim to a pre-analysis plan if mistakes are identified *ex post*. Furthermore, by being transparent over the deviations from the pre-analysis plan and the change in results, the reader can judge for herself the validity of results.

3.4.2 Empirical Strategy

For our main results we utilize an analysis of covariance (ANCOVA) estimator for all outcomes for which we have baseline data.

$$y_{1ivslp} = \beta_0 + \beta_1 T_s + \beta_2 y_{0ivslp} + \rho_p + \mu_l + \mathbf{Z}'_{sp}\Gamma + \mathbf{X}'_{vsp}\Phi + \varepsilon_{ivslp} \tag{3.1}$$

where y_{vsple} is the outcome for individual i in village v, section s and section-pair p, with dominant language group, l; T_s is the treatment dummy; ρ_p is the section-pair fixed effect; μ_l is a language group fixed effect; ε_{vslp} is the random error term, clustered at the section level (the unit of treatment allocation). The

section-pair fixed effects are included to account for section-level matching in the allocation of treatment (Bruhn and McKenzie, 2009). In addition, some specifications include a set of individual and village level controls, $\mathbf{Z}'_{ivsp}$ and $\mathbf{X}_{vspe}$.

The ANCOVA has more power than a difference-in-difference (DD) estimator, as it accounts the covariance between pre- and post-treatment outcomes (Frison and Pocock, 1992; McKenzie, 2012). The improvement in power is greatest when the correlation between pre and post measures is very low,[12] which is the case for our study.[13] Intuitively, the ANCOVA is the same as a DD, except that it places less weight on pre-treatment outcome measures if they are less informative of post-treatment values. As robustness checks we also present DD specifications in the appendix tables.

For outcomes where we do not have any baseline values, we do a simple comparison of means test:

$$y_{1ivslp} = \beta_0 + \beta_1 T_s + \rho_p + \mu_l + \mathbf{Z}'_{sp}\Gamma + \mathbf{X}'_{vsp}\Phi + \varepsilon_{ivslp} \tag{3.2}$$

Furthermore, to compare the treatment effect across different subgroups, we estimate:

$$y_{1ivslp} = \beta_0+\beta_1 T_s+\beta_2 y_{0ivslp}+\beta_2 y_{0ivslp}+\beta_3 (z\times T_s)+\beta_4 z+\rho_p+\mu_l+\mathbf{Z}'_{sp}\Gamma+\mathbf{X}'_{vsp}\Phi+\varepsilon_{ivslp} \tag{3.3}$$

where $z \in (0, 1)$ indicates the relevant subgroup.

Finally, we can get some indication of the intensity of treatment, based on individual attendance to the ceremony. The Fambul Tok treatment, including

[12] McKenzie (2012) estimates that at $\rho = 0.25$, the sample size needs to increase by 60% for the difference-difference estimator to get the same power as ANCOVA

[13] Autocorrelation, ρ, ranges between 0.04 for psycho-social indicators (H03), 0.22 for conflict resolution, and 0.27 for group membership (H13), whereas ρ typically ranges between 0.5 and 0.7 for clinical trials and between 0.2 and 0.5 in education interventions

the bonfire ceremony, occurs at the section level. However, not all respondents attended the ceremony, and attendance is unlikely to be random. Thus, use the section-level treatment as an instrument for individual-level attendance. The first stage equation in the instrumental variables estimation is:

$$A_{ivselp} = \alpha_0 + \alpha_1 T_s + \alpha_2 y_{0ivslpe} + \rho_p + \mu_e + \kappa_l + \mathbf{Z}_{ivsp}\gamma + \mathbf{X}_{vsp}\phi + \varepsilon_{ivsp} \quad (3.4)$$

where $A_{ivsp} = 1$ if the individual attended the ceremony. The second stage is:

$$y_{ivsp} = \beta_0 + \beta_1 \widehat{A_{ivsp}} + \beta_2 y_{0ivslpe} + \rho_p + \mu_e + \kappa_l + \mathbf{Z}_{ivsp}\gamma + \mathbf{X}_{vsp}\phi + \varepsilon_{ivsp} \quad (3.5)$$

3.5 Description of Data

This section outlines all the data from the village and household surveys, as they relate to the pre-analysis plan.

A forgiveness scale (H02) was constructed from the Enright Forgiveness Inventory (EFI) (Subkoviak et al., 1995), which consists of 12 questions on a 4 point Lickert scale.[14] The EFI inventory has strong internal consistency and retest reliability (Subkoviak et al., 1995) and is a standard measures in many psychological studies.[15] Those questions were only asked to respondents who report that they were physically or emotionally hurt during the war.

For psychological wellbeing (H03) we aggregated three indices for PTSD,

[14]This scale captures three dimensions of forgiveness (Enright and The Human Development Study Group, 1991): affect (feelings against the perpetrator, such as resentment or anger, or compassion or love), judgement (opinions and beliefs about perpetrator) and behaviour (actions, or expressed desired actions, against perpetrator such as revenge, or acts of kindness). Each dimension can be categorised as cessation or negative or increased positive affect/judgement/behaviour.

[15]The EFI has the further advantage that it is a definition of forgiveness which has shown to have positive psychological impacts (ibid, 1995). In particular, it does not legitimise the act ("it wasn't that bad"), which would undermine the feeling of hurt and potentially open up the victim for future abuse.

anxiety, and depression. The PTSD index is based on a checklist from the 4^{th} Diagnostic and Statistical Manual of Mental Disorders (DSM-IV) (American Psychiatric Association, 2000), as developed by (Foa et al., 1997). Anxiety and depression were measured using the Zung anxiety and depression indices respectively (Zung, 1971b). All these measures are known for their high internal consistency and test-retest validity (Knight et al., 1983) and are still commonly used by psychologists today.[16] We inverted these indicators, so a reduction indicates worse psychological health (higher levels of depression etc.).

Furthermore, we adapted the wording for the forgiveness and psycho-social indices to the Sierra Leonean context and changed wording to better reflect the informal Krio language.[17] This was done during the piloting of the survey, in close consultation with the enumerators.

The survey also includes war-related and gender attitudes. We measured if beliefs over the character and culpability of ex-combatants (H04) changed by asking how much the respondent agrees with the following statements: "those that did bad things in the past would do it again if they had the chance" and "people who joined the RUF are not responsible for what they did since they were drugged". For war attitudes (H05), we asked if the respondent would fight again, or believes that others would fight again. We measured gender attitudes (H06) by asking how the respondent agrees with seven hypothetical situations that support gender violence and one statement regarding gender equality.

We also asked for the number and type of conflicts that the respondent, and the village as a whole, had experienced the past 6 months (H07), as well as

[16]These questions also correspond closely to other studies in post-conflict societies. For example, the Survey of War-Affected Youth (SWAY) conducted in Uganda asks the same PTSD questions and 6 out of the 8 anxiety questions.

[17]For example, one PTSD question is: "Have you had recurrent or intrusive distressing thoughts or recollections about the assault". We changed this question to: "In the last month, did you sit and think of bad bad things that happened to you even though you don't want to think of it?"

the method of conflict resolution and degree of satisfaction with the resolution (H08).

We used a battery of social capital questions as developed by the World Bank[18] relating to trust (H10), level of tensions (H11), group membership (H13) and collective action (H14). One innovation of the survey was to ask about trust towards ex-combatants[19] and migrants,[20] since trust to these sub-groups are mostly likely to change due to the intervention. Furthermore, we only asked trust questions if the respondent actually knew an ex-combatant/migrant in the community. This is because we wanted to measure the impact of beliefs towards specific individuals. The definition of trust was discussed in detail during the training to assure that the enumerators have a shared understanding of the meaning of the word. We also distinguished between trust towards villagers and trust towards outsiders, since it is plausible that an intervention that promotes stronger ties within the community, could do so at an expense of trust towards those outside the community.

For the social network (H12) questions we asked the respondents to list people from the 11 other respondents, whom they: (*i*) consider a very good friend; (*ii*) would ask to go collect money for them; (*iii*) would ask for advice and help; and (*iv*) would be willing to share a farm boundary with.[21] We used this information to construct how many times a respondent was named

[18] `http://go.worldbank.org/BOA3AR43W0`

[19] We distinguish between different types of ex-combatants—the rebels from the Revolutionary United Front (RUF), Community Defense Forces (CDF) known as 'Kamajores', and members of the army. It is possible that the intervention can affect trust towards different types of ex-combatants differently, since CDF's are considered to have committed less atrocities than the rebels.

[20] There is anecdotal evidence that migrants who arrived after the end of the war are treated with mistrust, since ex-combatants who committed atrocities in their own communities often fled to other communities

[21] The enumerator first named the other 11 villagers who are being surveyed, then asked whom the respondent would choose. The enumerators were also trained to emphasise that the respondent should not name everyone. For example, they should only mention a *good* friend.

by someone else. At another part of the survey, the respondent was also asked to list *all* the people in the village they would (*i*) ask to go collect money for them; or (*ii*) they would go to for advice. We only used the endline data, since the social networks were asked differently in the baseline survey.[22]

We also asked questions about economic reciprocity (H15): the frequency and size lending and borrowing, and the frequency and time spent working on each other's farms over the past 3 months. In a poor agricultural society, exchange often operates through sharing of farm labour and is thus a major form of economic exchange. We also ask about the number of traders and frequency of use of traders in the community.

As for economic well-being (H16), we constructed an aggregate wealth index using principal component analysis, based on both asset ownership and the quality of the house; and also asked respondents to report their ability to meet basic household needs.

Since responsiveness to the intervention is likely to depend on the level of prior exposure to violence, we also measure individual and village exposure to violence. We ask if the respondent was abducted, raped, beaten, maimed, saw violence, or had a family member killed. At the village level, we asked the number of times that the village was attacked by armed groups, the number of houses burnt, and if a rebel group formed a base in the village.

Respondents are also asked if they joined an armed group during the war. This is likely to be highly under-reported. As a result, we also asked the respondents who were abducted whether they were forced to carry or shoot a gun. This helps mitigate under-reporting: respondents are more likely to answer yes, since it implies less culpability.

Out of the above-mentioned outcomes we construct three different measures

[22]The baseline questions were too strongly prompted, so respondents reported a link with each of the other 11 respondents.

of exposure to violence. First, for individual exposure to violence we construct a binary measure indicating if a respondent had been abducted, maimed, raped, beaten, or seen violence. We also construct an ordinal measure of the intensity of exposure to violence by adding up the five indicators. The second metric needs to make a strong ordinality assumption that each indicator has an equal impact on the intensity of exposure to violence (it seems unlikely, for example, that seeing violence has the same impact as being raped or maimed) and is therefore only used as a robustness check. We constructed a binary indicator of village-level exposure to violence: whether a village has been attacked multiple times by armed groups, or the armed groups formed a base there for more than a year.

Finally, we construct an indicator variable for each language spoken by the majority of the respondents in each village, and also the identity of the enumerator. Ethnicity is geographically concentrated and identified by language. The language spoken by the majority of villagers therefore provides a measure of the majority ethnic group in the village. Ethnicity is a strong predictor for baseline outcomes and it is therefore important to control for it in our final analysis. It also serves the additional function of accounting for heterogeneity in the quality of data collection. There were seven ethnic groups among our sampled villages (Fullah, Kono, Koranko, Loko, Mende, Temne and Yalunka).

3.5.1 Missing Data

We used a variety of different measures to deal with missing data, which arose either due to non-response, or enumerator error.

First, respondents were given the option not to respond to the violence exposure questions. About 3% of respondents chose not to answer any questions and others only answered some of the questions. The challenge is how to treat

missing values in the aggregate index of exposure to violence, especially if data is missing for only a subset of indicators. Either dropping these observations or assigning a value of 1 or 0 creates a bias, because non-response is likely to be non-random. Furthermore, treating an observation as missing if at least one indicator is missing drastically reduces sample size. We chose a middle way and treat an observation as missing if at least 2 of its 5 constituent indicators are missing and all of the non-missing responses are zero.

Second, due to technical survey programming issues, there is missing baseline village data for five out of the forty villages, four of which are in the control group. As robustness check, we dropped the treatment sections pairs where the village data is missing.

3.5.2 Descriptive Stats

Table 3.1 presents descriptive statistics of selected individual and village level characteristics in the baseline data. What is striking from this table is the extent of exposure to violence. Over a quarter of respondents were abducted, over a third were beaten, 4% raped, and 2% maimed. Taken together, 55% of respondents have experienced at least one of the above. This is possibly an under-estimate since those who opted not to answer any questions are plausibly most affected by the war. Furthermore, over half of our sample claim to know an ex-combatant who lives in the village and 19% of respondents were betrayed by a fellow villager at some point during the war.

Table 3.2 shows balance on all the mean indices for which we have baseline data. In the first three and final three columns the hypotheses are aggregated using the Anderson and Kling index respectively. Columns (1) and (4) indicate the difference between treatment and control means, columns (2) and (5) the standard errors, and columns (3) and (6) the number of observations. All out-

comes are balanced, except for forgiveness, war attitudes (at 10% level), and the Kling index for public goods provision. We address imbalance by controlling for the baseline outcomes in all our specifications. Furthermore, with the exception of forgiveness, none of the hypotheses that provide significant results are imbalanced in the baseline.

Our attrition rate is low, at only around 7% of the sample and is also random and balanced across treatment and control. In the appendix (Table A-1) we show the results for regressing attrition status (dummy indicator which takes one 1 if data is missing in the endline) on all baseline mean indices. Attrition status has no impact on any baseline mean index, except group membership.

3.6 Results

3.6.1 Mean Effects

Table 3.3 shows the treatment effects of the aggregate indices of all the hypothesis, as estimated by Equation 3.1. For the hypotheses that have no baseline data (H01, H11, H12), we estimate the treatment effect using Equation 3.2. All specifications include the treatment pair dummies and village language fixed effects, but no other controls. In the first (final) two columns, the hypotheses are aggregated using the Anderson (Kling) index. Columns (1) and (3) show results for the full sample. Columns (2) and (4) show the results of a restricted sample, excluding one treatment pair where a bonfire ceremony took place in the control section.

Fambul Tok's impact is mixed. Respondents who received the treatment are significantly more forgiving (H02)[23] and charitable in their approach towards

[23]However, since respondents in the treatment communities were significantly less forgiving prior to the intervention, we should place less confidence in this result, because we cannot rule out mean reversion.

ex-combatants (H04), and also display better attitudes towards women (H06). In addition, conflict resolution (H08) improved and respondents in the treated communities are more likely to join and participate in village groups (H13). There is weak evidence that public goods provision increased (H14), but this result is not significant using Anderson index. The largest improvement is in conflict resolution, with a 0.468 standard deviation increase.

However, the intervention had no impact on interpersonal relations such as trust, conflict incidence and social networks, nor did it lead to any changed economic activity. Furthermore, respondents' psychological wellbeing actually *deteriorated* (H03) because of the intervention, by 0.14 standard deviations.

All results are the same if we look at the effect of attending the bonfire ceremony, instrumented by treatment (Table A-2, in the appendix).

We further perform Kolmogorov-Smirnov (Kolmogorov, 1933; Smirnov, 1948) distribution tests on all the mean indices. Results are the same as above (not shown). The hypotheses where the post-treatment distributions in the treatment and control groups are significantly different to each other for both the Anderson and Kling index, evaluated at the 10% confidence level are: psychological healing (H03), attitudes towards ex-combatants (H04), and conflict resolution (H08).

As a robustness check, we repeat the analysis, but with an additional set of controls (Table A-3 in appendix). All specifications now also include enumerator fixed effects and individual and village level controls: respondent gender and educational attainment, and village size and an indicator if village has access to a market. Results on psychological wellbeing (H03), conflict resolution (H08), and group membership (H13) remain unchanged.

Finally, we repeat analysis using both the difference-in-difference specification and a simple comparison of means test (Table A-4, in appendix), once

again controlling for village language and section pair fixed effects. Unsurprisingly, some of coefficients are insignificant in the difference-in-difference estimator which is consistent with greater power in the ANCOVA specification. Note, however, that the coefficients are of similar in magnitude in these specifications, which gives us further confidence in our results.

3.6.2 Disaggregated Outcomes

It is informative to also look at the individual outcomes within each hypothesis.

Tables 3.4 and 3.5 present results for hypotheses that were significantly effected by the treatment, using the same specification as in Table 3.3. The forgiveness inventory and psycho-social indicators are normalised to mean zero and standard deviation one. Respondents in the treated communities reported a 0.13 standard deviation increase in depression. Post-traumatic stress disorder increased by a similar magnitude, but is less precisely estimated. There is no evidence for an increase in anxiety. The proportion of conflicts that are satisfactory resolved and the proportion that are resolved the chief, elders, or court increased dramatically—by 25.5 and 20 percentage points respectively.

Respondents from the treatment communities are 8 percentage points more likely to join a religious group, 10 percentage points more likely to attend meetings of a religious group, and 6 percentage points more likely to attend meetings of a parents-teachers association (PTA). Results for group membership are even stronger if we drop the contaminated treatment section pair (Table A-5, in appendix). With the restricted sample respondents are also significantly more likely to become member of a PTA and attend general community meetings.

As for public goods provision, none of the individual outcomes are significantly different from zero. Furthermore, the coefficient on the Kling index is

more than 6 times larger than the coefficient of the Anderson index. This raises some concern that missing values created a bias. As discussed above, data on contribution to community projects is missing for five villages, four of which are in the treatment group. The Anderson index amplifies any selection bias, because the sample is also restricted for all the other outcomes in the index. In addition, there is a further concern that a positive coefficient on 'community projects' merely picks up project implementation, as one component of the intervention is the creation of community farms.

We address these concerns in Table A-6 (in the appendix). First, we drop all the treatment section pairs where village data was missing. Second, we do not include any community projects which involve farming. Results are stronger than before. Coefficient sizes for both Anderson and Kling index increase. The coefficient on Anderson is now closer to the Kling index is significantly different from zero. Communities that received the treatment have started 0.2 more community projects on average, from a base of 0.7.[24]

Finally, Table A-7 (in the appendix) reports results for the three hypotheses—conflict resolution (H08), traditional justice (H09) and trust (H10)—as they appear in the original pre-analysis plan. Conflict resolution (H08) is now insignificant. Nonetheless, the coefficient on satisfaction is still large and significant. Our two main conclusions—more conflicts are satisfactory resolved and more conflicts are resolved by chiefs—remain, regardless of the grouping.

3.6.3 Sub-group analysis

Next we look at the impact of the intervention on particular sub-groups, as specified in the pre-analysis plan. Fambul Tok's impact may vary based on household and village-level characteristics. The reconciliation ceremony

[24]Results also hold if we impute zero or one to missing values in the road-brushing and financial contribution variables.

specifically targets individuals who were survivors and perpetrators of violence, so war-affected individuals and ex-combatants may be differentially affected by the intervention. Similarly, it is possible that entire villages that experienced higher levels of violence will respond differently to the intervention. Finally, the program may exert varying effects by gender, both because female war experiences differed owing to factors such as gender-based violence and because aspects of the program, such as the 'Peace Mothers' group, are designed to focus on women's issues.

Table 3.6 shows the interaction effects based on individual exposure to violence. All specifications now also control for individual level characteristics, namely the gender, age and level of education of the respondent. Column (1) is the treatment effect for individuals not exposed to violence (coefficient β_1 in Equation 3.3), column (2) is the treatment effect for war-affected individuals ($\beta_1 + \beta_3$ in Equation 3.3), and column (3) is the difference (β_3 in Equation 3.3).

Surprisingly, individuals who are more exposed to violence seem to benefit less from the intervention. Those not exposed are more trusting due to the intervention and significantly more so than individuals who were exposed to violence.

As robustness check, we also compute an ordinal value of the intensity of the exposure to violence (Table A-8 in appendix), where we add up the 5 composite indicators. With this specification, the interaction term (coefficient β_3) now indicates a change in the effect of the treatment due to a one unit increase in the exposure. Results are the same as before.

There is no evidence that any of the other sub-groups—war-exposed villages, females, or ex-combatants—are differentially affected due to the treatment (results not shown).

3.7 Discussion

The results of the study have implications, both for the design of programs that aim to promote institutional change, and 'truth-telling' programs that require testimonies from those affected by the war.

3.7.1 Institutional Change

First, results from this study suggest that short-term efforts to induce institutional change *can* have positive impacts. Fambul Tok created a new dispute resolution forum, formed new committee's, and also set up a communal farm to promote collective action. As a result, more people made use of village institutions to resolve their disputes and a higher proportion of conflicts were more satisfactory resolved. Furthermore, villagers are more involved in village organisations and there is weak evidence that public goods provision improved.

This is in contrast to previous studies that document the failure of Community Driven Development/Reconstruction (CDD/R) programs to forge new institutions in post-conflict societies. Born of the belief that weak or extractive institutions contributed to the conflict, CDD/R programs aim to create more transparent and accountable governing structures, as well engender more democratic values and strengthen collective action. To affect change, they channel large sums of development aid through newly formed, democratically elected committee structures. This is typically also combined with civic education and training programs that promote more liberal, democratic values. However, in both Sierra Leone and the Democratic Republic of Congo (Casey et al., 2012; Humphreys et al., 2012) decision-making was no more inclusive, accountable, nor transparent due to the intervention; and in Sudan there was no evidence of improved social capital, such as strengthened social networks (Avdeenko and

Gilligan, 2012). In Philippines, participation in village assemblies increased, but there was also a decline in reported collective action (Labonne and Chase, 2008).[25]

We do not have enough evidence to say why this program was successful relative to other programs, because Fambul Tok is a package of different interventions, all of which are different to CDD/R programs. Nonetheless, one possible reason is that Fambul Tok emphasised local customs and beliefs, and worked through traditional sources of authority. As evidence, we see increased involvement in traditional institutions: villagers were more likely to take disputes to chiefs and were more likely to join and participate in religious groups. This is in contrast to previous programs in Sierra Leone (Casey et al., 2012) that tried to circumvent the power of the chiefs by creating formal source of authority, such as magistrate courts and democratically elected district councils, but with limited success (Jackson, 2006).

Existing local power structures are resistant to institutional change that could undermine their power base. Programs will be more effective if they reform existing institutions—for example, putting in codes of conduct and accountability measures that limit scope for abuse of power—rather than create new ones.

3.7.2 Truth-telling

Second, contrary to the popular belief of the therapeutic benefits of reconciliation programs, respondents' psychological health actually deteriorated. This result has large implications for the design of transitional justice programs. All these programs—including international courts that trial perpetrators, truth commissions that uncover facts surrounding past atrocities, and

[25]This study was not experimental, but used panel data and propensity matching to infer causal relationship.

reconciliation programs that encourage repenting and forgiveness—require that survivors talk about their war experience. If survivors are worst off because of their testimonies, then interventions that promote transitional justice need to be reformed.

There is a lot we can learn from the psychological literature on dealing with post-traumatic stress disorder (Foa, 2009). Exposure to the past traumatic event is important for personal healing, but can lead to re-traumatisation if not done correctly. In fact, psychologists advise against 'debriefing'—a once-off talk about the traumatic event (Rose et al., 2002). The survivor needs to relive the experience, but in an environment where s/he feels safe, comfortable and in control of the memory until the fear, sense of threat and resultant anxiety associated with the event subsides (Brouneus, 2008).

These conditions are not necessarily present in 'truth-telling' programs. With Fambul Tok, the survivors gave testimonials in front of a large group of people (on average 300), many of whom are strangers from different villages. Most importantly, the testimonials were often in front of the past aggressor. This is not a setting where the survivor feels safe and comfortable. Furthermore, a joint community event creates social pressure to testify and express forgiveness. This takes away the sense of control for the survivor.

So, what can be done? Group therapy is plausibly the most appropriate treatment to address trauma in post-conflict societies. Resource and capacity constraints combined with wide-spread exposure to violence limits the scope for individual therapy by professionals counselors. Furthermore, there is evidence that properly designed group therapy can effectively treat PTSD: it creates a space to discuss the trauma in safe and trusting environment, provides important social support, and also allows for social learning (e.g. sharing of coping strategies) (Foa, 2009). In a field experiment which trained facilitators to lead

group discussions focusing on psychological healing and dealing with painful experiences, psychological health of the participants improved (Staub et al., 2005).

None of this means that 'truth-telling' interventions should be stopped. They provide important long-run benefits for society that are difficult to quantify. Recorded testimonies guard against selective retelling of history for political purposes at later stages. A perception that justice was done can limit festering resentment. However, it is important to rethink how we design these interventions, so the survivors themselves can benefit from it.

3.8 Conclusion

This is the first randomised, controlled evaluation of a reconciliation program. We evaluate Fambul Tok, an NGO which facilitates truth-telling to air past war-time grievances in post-conflict Sierra Leone, and also forges institutions designed to increase conflict resolution and build social capital. We find mixed evidence. Respondents in the treatment communities are more forgiving and have improved attitudes towards ex-combatants. In addition, they are more involved in village groups and report higher satisfaction in conflict resolution. However, there was no change in trust, social networks, or economic activity. Finally, psychological health actually worsened because of the intervention.

This study has large implications for CDD/R programs that aim to induce institutional change, and transitional justice programs that address war-time human rights abuses. We find that institutional chance can work and speculate it is because Fambul Tok built on existing institutions and power structures. Furthermore, 'truth-telling' programs as they are designed today can lead to

a retraumatisation of the survivors who testify. Policy makers should draw lessons from the psychological literature in the appropriate dealing of trauma.

Talking about the past *does* matter and institutional reform *can* work. The real question is how it is done.

3.9 Tables

Table 3.1: Descriptive statistics of selected variables

	Observations	Mean	Std. Dev.	Min.	Max.
Key Characteristics					
Female	882	0.55	0.50	0.00	1.00
No formal education	885	0.66	0.47	0.00	1.00
Market in village	754	0.06	0.24	0.00	1.00
Age	882	42.08	16.00	15.00	99.00
Exposure to Violence					
Respondent is Ex-Combatant	789	0.04	0.20	0.00	1.00
Abducted	794	0.29	0.46	0.00	1.00
Raped	783	0.04	0.20	0.00	1.00
Maimed	780	0.02	0.15	0.00	1.00
Beaten	783	0.35	0.48	0.00	1.00
Family member killed	843	0.56	0.50	0.00	1.00
Betrayed by fellow villager	771	0.20	0.40	0.00	1.00
Knows ex-combatant	871	0.55	0.50	0.00	1.00
Exposed to violence	811	0.55	0.50	0.00	1.00
Level of exposure to violence	744	0.79	0.93	0.00	5.00
Buildings burnt during war	821	1.87	2.55	0.00	15.00
Attacked during the war	821	0.97	0.16	0.00	1.00
Many attacks during war	799	0.67	0.47	0.00	1.00
Rebel base for more than 1 year	766	0.49	0.50	0.00	1.00
Selected Outcomes					
Number of Inter-village conflicts	821	0.19	0.50	0.00	3.00
Number of inter-household conflicts	885	0.21	0.51	0.00	4.00
Member of secret society	885	0.49	0.50	0.00	1.00
Member of PTA	885	0.17	0.37	0.00	1.00
Brushed road	838	0.39	0.49	0.00	1.00
Contrib. to VDC	885	0.12	0.32	0.00	1.00
Borrow Lend	885	0.56	0.87	0.00	7.00
Traders in community	810	6.91	9.97	0.00	50.00
Use trader	862	0.69	0.46	0.00	1.00

Table 3.2: Baseline balance on mean outcomes

	Anderson Index			Kling Index		
	$T-C$	Std. Err.	Obs.	$T-C$	Std. Err.	Obs.
H2. Forgiveness	−0.216***	(0.064)	663	-0.216***	(0.064)	663
H3. Psychological wellbeing	−0.041	(0.045)	755	-0.065	(0.045)	874
H4. Ex-combatant attitudes	0.011	(0.046)	869	0.004	(0.046)	879
H5. War attitudes	−0.102*	(0.056)	837	-0.113*	(0.059)	880
H6. Gender attitudes	−0.070	(0.076)	850	-0.072	(0.073)	881
H7. Conflict incidence	0.021	(0.067)	821	0.037	(0.049)	885
H8. Conflict resolution	−0.003	(0.078)	753	0.085	(0.105)	753
H10. Trust	−0.026	(0.062)	850	-0.031	(0.062)	882
H11. Tensions	−0.010	(0.051)	761	0.016	(0.046)	882
H13. Group membership	−0.010	(0.031)	875	-0.009	(0.030)	885
H14. Public goods	−0.125**	(0.052)	775	-0.087**	(0.036)	885
H15. Economic activity	−0.099	(0.064)	731	-0.064	(0.048)	885
H16. Economic welfare	0.057	(0.064)	799	0.042	(0.056)	881

Note: Each row represents a separate regression of treatment status on mean baseline outcomes, with the inclusion section pair and village language fixed effects. The left-most column lists each hypothesis. The first (final) three columns show regression output for the Anderson (Kling) index. Columns (1) and (2) ((4) and (5)), report the coefficient and standard errors of T, where $T \in [0, 1]$ indicates treatment. Columns (3) and (5) report sample size for each mean index. Standard errors are clustered at the section level. *** is significant at the 1% level, ** is significant at the 5% level and * is significant at the 10% level.

Table 3.3: Mean treatment effects

	Anderson Index		Kling Index	
H1. Implementation	0.834***	0.912***	1.598***	1.727***
	(0.123)	(0.119)	(0.285)	(0.291)
H2. Forgiveness	0.121**	0.116**	0.121**	0.116**
	(0.047)	(0.050)	(0.047)	(0.050)
H3. Psychological wellbeing	-0.140*	-0.141*	-0.146*	-0.149*
	(0.073)	(0.079)	(0.080)	(0.087)
H4. Ex-combatant attitudes	0.125**	0.086	0.126**	0.088
	(0.059)	(0.057)	(0.058)	(0.055)
H5. War attitudes	-0.023	-0.019	-0.029	-0.026
	(0.056)	(0.061)	(0.054)	(0.059)
H6. Gender attitudes	0.098**	0.081*	0.086**	0.072
	(0.045)	(0.047)	(0.042)	(0.045)
H7. Conflict incidence	-0.022	-0.030	-0.066	-0.077
	(0.045)	(0.048)	(0.055)	(0.060)
H8. Conflict resolution	0.464***	0.464***	0.468*	0.468*
	(0.158)	(0.156)	(0.233)	(0.230)
H10. Trust	0.048	0.044	0.048	0.045
	(0.069)	(0.074)	(0.066)	(0.071)
H11. Tensions	-0.040	-0.044*	-0.023	-0.029
	(0.024)	(0.026)	(0.027)	(0.028)
H12. Network strength	-0.013	0.001	-0.001	0.004
	(0.033)	(0.035)	(0.027)	(0.029)
H13. Group membership	0.033	0.053**	0.056**	0.078***
	(0.023)	(0.021)	(0.027)	(0.024)
H14. Public goods	0.012	0.039	0.077**	0.098***
	(0.040)	(0.036)	(0.033)	(0.032)
H15. Economic activity	-0.078	-0.072	-0.015	-0.023
	(0.055)	(0.059)	(0.051)	(0.055)
H16. Economic welfare	0.014	0.029	0.022	0.034
	(0.047)	(0.048)	(0.040)	(0.042)
Dropped Contaminated Pair	No	Yes	No	Yes

Note: Each cell reports the coefficient on T, where $T \in [0, 1]$ indicates treatment, estimated using Equation 3.1, but excluding controls. The leftmost column lists each hypothesis. The first two and final two columns indicate regressions using the Anderson and Kling index respectively. Columns (1) and (3) show results for the full sample, in columns (2) and (4) the contaminated treatment pairs are dropped. Hypotheses H01, H11 and H12 are estimated using Equation 3.2. Standard errors are clustered at the section level. *** is significant at the 1% level, ** is significant at the 5% level and * is significant at the 10% level.

Table 3.4: Forgiveness, psychological health, ex-combatant attitudes, and conflict resolution

	Coefficient	Std. Err.	Obs.	Clusters	Adj. R^2
H1. Implementation					
Heard of FT	0.325***	(0.053)	883	40	0.301
FT held bonfire	0.580***	(0.116)	885	40	0.643
FT communal farm	0.338***	(0.092)	885	40	0.405
FT peace tree	0.370***	(0.078)	885	40	0.433
FT peace mothers	0.445***	(0.098)	885	40	0.481
Kling07 index	1.598***	(0.285)	885	40	0.510
Anderson08 index	0.834***	(0.123)	883	40	0.373
H2. Forgiveness					
Forgive perp.	0.121**	(0.047)	550	40	0.115
H3. Psychological wellbeing					
Less PTSD	−0.110	(0.072)	801	40	0.144
Less anxiety	−0.007	(0.056)	829	40	0.109
Less depression	−0.129***	(0.041)	843	40	0.084
Kling07 index	−0.146*	(0.080)	872	40	0.124
Anderson08 index	−0.140*	(0.073)	753	40	0.112
H4. Ex-combatants					
Ex-coms won't fight	0.099*	(0.052)	868	40	0.032
Ex-coms not responsible	0.102*	(0.053)	872	40	0.051
Kling07 index	0.126**	(0.058)	875	40	0.031
Anderson08 index	0.125**	(0.059)	865	40	0.033
H8. Conflict resolution					
Satisfaction	0.255*	(0.142)	47	32	-0.108
More conflicts resolved	0.120	(0.141)	47	32	-0.055
More resolved without intermediary	0.090	(0.100)	47	32	0.216
More resolved by chief/elders/court	0.204	(0.163)	47	32	0.347
Kling07 index	0.468*	(0.233)	47	32	0.100
Anderson08 index	0.464***	(0.158)	47	32	0.416

Note: Each row reports a separate regression based on Equation 3.1, without controls. The leftmost column lists dependent variables grouped by hypothesis. Columns (1) and (2) reports the coefficients and standard error on T, where $T \in [0, 1]$ indicates treatment. Standard errors are clustered at the section level. *** is significant at the 1% level, ** is significant at the 5% level and * is significant at the 10% level.

Table 3.5: Group membership and public goods provision

	Coefficient	Std. Err.	Obs.	Clusters	Adj. R^2
H13. Group membership					
Member PTA	0.042	(0.036)	885	40	0.182
Member VDC	−0.004	(0.025)	885	40	0.144
Member youth group	−0.001	(0.028)	885	40	0.162
Member women's group	−0.012	(0.025)	885	40	0.137
Member secret society	−0.037	(0.040)	885	40	0.247
Member religious group	0.079∗	(0.045)	885	40	0.105
Meetings PTA	0.062∗	(0.036)	885	40	0.109
Meetings VDC	0.011	(0.021)	884	40	0.110
Meetings youth group	0.003	(0.027)	884	40	0.093
Meetings women's group	0.015	(0.022)	884	40	0.070
Meetings secret society	0.007	(0.015)	885	40	0.025
Meetings religious group	0.100∗∗∗	(0.031)	883	40	0.043
Meetings in general	0.039	(0.023)	876	40	0.081
Kling07 index	0.056∗∗	(0.027)	885	40	0.155
Anderson08 index	0.033	(0.023)	871	40	0.135
H14. Public goods					
Brushed road	0.022	(0.022)	834	40	0.125
Contrib. money or labor	0.041	(0.038)	837	40	0.155
Community projects	0.012	(0.097)	832	39	0.552
Contrib. to PTA	0.017	(0.035)	885	40	0.062
Contrib. to VDC	−0.001	(0.015)	885	40	0.108
Contrib. to youth group	0.006	(0.019)	885	40	0.059
Contrib. to women's group	−0.004	(0.019)	885	40	0.061
Kling07 index	0.077∗∗	(0.033)	885	40	0.211
Anderson08 index	0.012	(0.040)	771	39	0.247

Note: Each row reports a separate regression based on Equation 3.1, but without controls. The leftmost column lists dependent variables grouped by hypothesis. Columns (1) and (2) reports the coefficients and standard error on T, where $T \in [0, 1]$ indicates treatment. Standard errors are clustered at the section level. *** is significant at the 1% level, ** is significant at the 5% level and * is significant at the 10% level.

Table 3.6: Individual level exposure to violence

	Anderson Index			Kling Index		
	Unexposed	Exposed	Diff.	Unexposed	Exposed	Diff.
H1. Implementation	0.992∗∗∗	0.707***	-0.285***	1.671***	1.515***	-0.156
	(0.122)	(0.135)	(0.084)	(0.279)	(0.317)	(0.112)
H2. Forgiveness	0.805	0.578	-0.228	0.805	0.578	-0.228
	(0.922)	(0.579)	(1.367)	(0.922)	(0.579)	(1.367)
H3. Psychological wellbeing	−0.248∗∗	-0.077	0.170	-0.227**	-0.121	0.106
	(0.106)	(0.102)	(0.136)	(0.112)	(0.113)	(0.147)
H4. Ex-combatant attitudes	0.147	0.130*	-0.017	0.135	0.146**	0.010
	(0.088)	(0.069)	(0.101)	(0.089)	(0.067)	(0.099)
H5. War attitudes	−0.001	-0.053	-0.052	0.004	-0.061	-0.065
	(0.091)	(0.069)	(0.102)	(0.088)	(0.068)	(0.098)
H6. Gender attitudes	0.038	0.089	0.051	0.036	0.081	0.045
	(0.086)	(0.057)	(0.101)	(0.086)	(0.058)	(0.108)
H7. Conflict incidence	−0.022	0.013	0.035	-0.048	-0.057	-0.008
	(0.058)	(0.074)	(0.090)	(0.056)	(0.076)	(0.082)
H8. Conflict resolution	0.450∗∗∗	0.473***	0.023	0.424**	0.532***	0.108
	(0.122)	(0.101)	(0.050)	(0.165)	(0.158)	(0.075)
H10. Trust	0.135	-0.028	-0.162*	0.138*	-0.043	-0.181**
	(0.083)	(0.073)	(0.090)	(0.078)	(0.068)	(0.085)
H11. Tensions	−0.012	-0.081**	-0.069	-0.008	-0.077**	-0.069
	(0.039)	(0.032)	(0.055)	(0.039)	(0.034)	(0.056)
H12. Network strength	0.003	0.041	0.037	-0.004	0.042	0.046
	(0.055)	(0.042)	(0.083)	(0.058)	(0.046)	(0.092)
H13. Group membership	0.061	0.015	-0.046	0.089*	0.035	-0.055
	(0.041)	(0.037)	(0.060)	(0.048)	(0.038)	(0.063)
H14. Public goods	0.003	-0.033	-0.037	0.055	0.060	0.005
	(0.052)	(0.044)	(0.060)	(0.045)	(0.046)	(0.054)
H15. Economic activity	−0.136∗∗	-0.044	0.092	-0.075	0.015	0.091
	(0.057)	(0.072)	(0.062)	(0.056)	(0.064)	(0.058)
H16. Economic welfare	−0.000	-0.001	-0.001	-0.009	-0.006	0.004
	(0.074)	(0.051)	(0.085)	(0.065)	(0.045)	(0.080)

Note: Each row reports a separate regression based on Equation 3.3, excluding controls. The leftmost column lists each hypothesis. The first (final) three columns report regressions using the Anderon (Kling) index. Columns (1) and (4) show β_1, the treatment effect of individuals not exposed to war. Columns (2) and (5) show $\beta_1 + \beta_3$, the treatment effect of war-exposed individuals. Columns (3) and (6) show β_3, the difference in treatment effects. Standard errors are clustered at the section level. *** is significant at the 1% level, ** is significant at the 5% level and * is significant at the 10% level.

3.10 Appendix

Table A-1: Attrition

	Anderson Index			Kling Index		
	Attrition	Std. Err.	Obs.	Attrition	Std. Err.	Obs.
H2. Forgiveness	−0.070	(0.165)	705	-0.070	(0.165)	705
H3. Psychological wellbeing	−0.061	(0.128)	811	-0.034	(0.109)	940
H4. Ex-combatant attitudes	−0.028	(0.110)	935	-0.006	(0.111)	946
H5. War attitudes	−0.127	(0.085)	897	-0.096	(0.083)	946
H6. Gender attitudes	0.072	(0.081)	913	0.082	(0.075)	948
H7. Conflict incidence	0.057	(0.060)	882	0.053	(0.054)	952
H8. Conflict resolution	−0.021	(0.053)	810	-0.015	(0.058)	810
H10. Trust	−0.045	(0.076)	915	-0.053	(0.080)	949
H13. Group membership	−0.090∗	(0.045)	940	-0.100**	(0.047)	952
H14. Public goods	−0.037	(0.066)	831	-0.040	(0.057)	952
H15. Economic activity	−0.058	(0.065)	784	-0.083	(0.051)	952
H16. Economic welfare	−0.014	(0.091)	860	-0.026	(0.082)	948

Note: Each row represents a separate regression of attrition status on mean baseline outcomes, controlling for section pair and village language fixed effects. The leftmost column lists each hypothesis. The next (final) three columns show regression output for the Anderson (Kling) index. Columns (1) and (2) ((4) and (5)), report the coefficient and standard errors of Attrition status, where $Attrition \in [0, 1]$ indicates whether data is missing in endline. Columns (3) and (5) report sample size for each mean index. Standard errors are clustered at the section level. *** is significant at the 1% level, ** is significant at the 5% level and * is significant at the 10% level.

Table A-2: Individual attendance, instrumented by treatment

	Anderson Index		Kling Index	
H1. Implementation	2.313***	2.328***	4.430***	4.411***
	(0.220)	(0.219)	(0.405)	(0.403)
H2. Forgiveness	1.989**	1.805**	1.989**	1.805**
	(0.787)	(0.779)	(0.787)	(0.779)
H3. Psychological wellbeing	-0.340**	-0.305*	-0.338*	-0.316*
	(0.172)	(0.165)	(0.185)	(0.183)
H4. Ex-combatant attitudes	0.354**	0.225	0.349**	0.224
	(0.178)	(0.147)	(0.171)	(0.141)
H5. War attitudes	-0.066	-0.051	-0.082	-0.068
	(0.160)	(0.158)	(0.152)	(0.151)
H6. Gender attitudes	0.274*	0.209*	0.239*	0.184
	(0.141)	(0.127)	(0.130)	(0.120)
H7. Conflict incidence	-0.037	-0.049	-0.172	-0.183
	(0.102)	(0.106)	(0.137)	(0.137)
H8. Conflict resolution	1.068***	1.068***	1.153***	1.153***
	(0.199)	(0.199)	(0.358)	(0.358)
H10. Trust	0.131	0.111	0.133	0.115
	(0.189)	(0.185)	(0.183)	(0.180)
H11. Tensions	-0.145**	-0.125*	-0.132*	-0.117*
	(0.070)	(0.067)	(0.071)	(0.068)
H12. Network strength	0.054	0.080	0.050	0.058
	(0.070)	(0.066)	(0.066)	(0.065)
H13. Group membership	0.093	0.135***	0.154**	0.198***
	(0.059)	(0.049)	(0.067)	(0.059)
H14. Public goods	0.028	0.088	0.212**	0.247***
	(0.089)	(0.081)	(0.096)	(0.094)
H15. Economic activity	-0.163	-0.147	-0.041	-0.059
	(0.115)	(0.119)	(0.135)	(0.133)
H16. Economic welfare	0.000	0.005	-0.006	-0.005
	(0.117)	(0.111)	(0.102)	(0.097)
Dropped Pair	No	Yes	No	Yes

Note: Each cell reports the coefficient value of $\widehat{A}$, the predicted attendance rate, based on a 2SLS regression using Equations 3.4 and 3.5. The leftmost column lists each hypothesis. The first two and final two columns indicate regressions using the Anderson and Kling index respectively. Columns (1) and (3) show results for the full sample, in columns (2) and (4) the contaminated treatment pairs are dropped. Standard errors are clustered at the section level. *** is significant at the 1% level, ** is significant at the 5% level and * is significant at the 10% level.

Table A-3: Robustness checks: additional controls

	Anderson Index		Kling Index	
H1. Implementation	1.092***	1.103***	2.062***	2.121***
	(0.072)	(0.076)	(0.198)	(0.205)
H2. Forgiveness	0.083	0.096	0.083	0.096
	(0.064)	(0.066)	(0.064)	(0.066)
H3. Psychological wellbeing	-0.195**	-0.194**	-0.196**	-0.196**
	(0.088)	(0.095)	(0.083)	(0.090)
H4. Ex-combatant attitudes	0.059	0.012	0.065	0.019
	(0.067)	(0.061)	(0.066)	(0.061)
H5. War attitudes	-0.021	-0.029	-0.032	-0.041
	(0.074)	(0.080)	(0.073)	(0.079)
H6. Gender attitudes	-0.019	-0.045	-0.019	-0.042
	(0.038)	(0.034)	(0.038)	(0.035)
H7. Conflict incidence	-0.044	-0.054	-0.141**	-0.157**
	(0.052)	(0.056)	(0.069)	(0.075)
H8. Conflict resolution	0.479***	0.478***	0.516***	0.516***
	(0.116)	(0.116)	(0.177)	(0.177)
H10. Trust	-0.086	-0.089	-0.087*	-0.088*
	(0.052)	(0.054)	(0.047)	(0.050)
H11. Tensions	-0.067*	-0.071*	-0.066*	-0.075*
	(0.036)	(0.038)	(0.038)	(0.040)
H12. Network strength	-0.034	-0.008	-0.027	-0.013
	(0.037)	(0.036)	(0.036)	(0.038)
H13. Group membership	0.028	0.057*	0.039	0.072**
	(0.035)	(0.030)	(0.038)	(0.032)
H14. Public goods	-0.048	-0.011	-0.040	0.002
	(0.041)	(0.032)	(0.044)	(0.031)
H15. Economic activity	-0.073	-0.058	-0.056	-0.041
	(0.049)	(0.051)	(0.052)	(0.054)
H16. Economic welfare	-0.006	0.034	-0.000	0.035
	(0.058)	(0.054)	(0.053)	(0.049)
Dropped Contaminated Pair	No	Yes	No	Yes

Note: Each cell reports the coefficient on T, where $T \in [0, 1]$ indicates treatment, estimated using Equation 3.1, with enumerator and village language fixed effects, and additional individual and village level controls. The first two and final two columns indicate regressions using the Anderson and Kling index respectively. Columns (1) and (3) show results for the full sample, in columns (2) and (4) the contaminated treatment pairs are dropped. Hypotheses (H11) and (H12) are estimated using Equation 3.2. Standard errors are clustered at the section level. *** is significant at the 1% level, ** is significant at the 5% level and * is significant at the 10% level.

Table A-4: Robustness checks: alternate specifications

	Anderson Index		Kling Index	
	Diff-Diff	Cross-Section	Diff-Diff	Cross-Section
H1. Implementation	0.834***	0.834***	1.598***	1.598***
	(0.123)	(0.123)	(0.285)	(0.285)
H2. Forgiveness	0.230*	-0.014	0.230*	-0.014
	(0.139)	(0.051)	(0.139)	(0.051)
H3. Psychological wellbeing	−0.191	-0.144*	-0.175	-0.148*
	(0.209)	(0.076)	(0.210)	(0.080)
H4. Ex-combatant attitudes	0.113	0.131**	0.115	0.131**
	(0.122)	(0.059)	(0.120)	(0.059)
H5. War attitudes	0.030	-0.033	0.048	-0.028
	(0.113)	(0.054)	(0.110)	(0.053)
H6. Gender attitudes	0.155*	0.079*	0.155*	0.078*
	(0.089)	(0.044)	(0.087)	(0.043)
H7. Conflict incidence	−0.074	0.001	-0.227*	-0.065
	(0.087)	(0.041)	(0.131)	(0.054)
H8. Conflict resolution	0.218	0.442***	0.073	0.535***
	(0.208)	(0.131)	(0.222)	(0.172)
H10. Trust	0.112	0.049	0.115	0.044
	(0.092)	(0.075)	(0.092)	(0.072)
H11. Tensions	−0.041*	-0.041*	-0.039	-0.039
	(0.024)	(0.024)	(0.028)	(0.028)
H12. Network strength	0.026	0.026	0.038	0.038
	(0.026)	(0.026)	(0.025)	(0.025)
H13. Group membership	0.069	0.036	0.091	0.053*
	(0.058)	(0.024)	(0.057)	(0.029)
H14. Public goods	0.111	0.061	0.114	0.056
	(0.088)	(0.037)	(0.080)	(0.035)
H15. Economic activity	0.061	-0.042	0.072	-0.035
	(0.075)	(0.067)	(0.066)	(0.058)
H16. Economic welfare	−0.030	0.023	-0.015	0.034
	(0.073)	(0.056)	(0.065)	(0.057)

Note: The leftmost column lists dependent variables grouped by hypothesis. First (final) two columns report outcomes using the Anderson (Kling) index. Columns (1) and (3) report treatment effect for a difference-in-difference specification and columns (2) and (4) report a cross-sectional comparison of means, controlling for section pair and village language fixed effects. Standard errors are clustered at the section level. *** is significant at the 1% level, ** is significant at the 5% level and * is significant at the 10% level.

Table A-5: Group membership with restricted sample

	Coefficient	Std. Err.	Obs.	Clusters	Adj. R^2
H13. Group membership					
Member PTA	0.066∗	(0.034)	840	38	0.187
Member VDC	0.006	(0.025)	840	38	0.148
Member youth group	−0.005	(0.030)	840	38	0.161
Member women's group	−0.007	(0.026)	840	38	0.132
Member secret society	−0.026	(0.043)	840	38	0.238
Member religious group	0.096∗∗	(0.047)	840	38	0.103
Meetings PTA	0.096∗∗∗	(0.030)	840	38	0.123
Meetings VDC	0.018	(0.022)	839	38	0.114
Meetings youth group	−0.006	(0.029)	839	38	0.094
Meetings women's group	0.015	(0.024)	839	38	0.069
Meetings secret society	0.008	(0.016)	840	38	0.022
Meetings religious group	0.109∗∗∗	(0.033)	838	38	0.045
Meetings in general	0.052∗∗	(0.023)	831	38	0.076
Kling07 index	0.078∗∗∗	(0.024)	840	38	0.162
Anderson08 index	0.053∗∗	(0.021)	826	38	0.138

Note: Each row reports a separate regression based on Equation 3.1, but without individual and village level controls. The contaminated treatment section pair is dropped. The leftmost column lists dependent variables grouped by hypothesis. Columns (1) and (2) report the coefficients and standard error on T, where $T \in [0, 1]$ indicates treatment. Standard errors are clustered at the section level. *** is significant at the 1% level, ** is significant at the 5% level and * is significant at the 10% level.

Table A-6: Public good provision, alternate specification

	Coefficient	Std. Err.	Obs.	Clusters	Adj. R^2
H14. Public goods					
Brushed road	0.027	(0.028)	662	32	0.141
Contrib. money or labor	0.069	(0.045)	664	32	0.186
Community projects	0.207**	(0.089)	707	32	0.590
Contrib. to PTA	−0.003	(0.033)	707	32	0.065
Contrib. to VDC	−0.002	(0.015)	707	32	0.122
Contrib. to youth group	−0.002	(0.024)	707	32	0.075
Contrib. to women's group	0.017	(0.019)	707	32	0.084
Kling07 index	0.093***	(0.026)	707	32	0.278
Anderson08 index	0.102***	(0.035)	649	32	0.283

Note: Each row reports a separate regression based on Equation 3.1, excluding controls. The sample is restricted to treatment pairs where there is no missing baseline village date. Communal farming is no longer listed as a community project. The leftmost column lists dependent variables grouped by hypothesis. Column (1) reports the coefficients on T, where $T \in [0, 1]$ indicates treatment. Standard errors are clustered at the section level. *** is significant at the 1% level, ** is significant at the 5% level and * is significant at the 10% level.

Table A-7: Original hypotheses, as stated in the pre-analysis plan

	Coefficient	Std. Err.	Obs.	Clusters	Adj. R^2
H8. Conflict resolution					
Satisfaction	0.255∗	(0.142)	47	32	-0.108
More conflicts resolved	0.120	(0.141)	47	32	-0.055
More resolved without intermediary	0.090	(0.100)	47	32	0.216
Kling07 index	0.468	(0.433)	47	32	-0.001
Anderson08 index	0.492	(0.427)	47	32	-0.002
H9. Traditional justice					
More fines	−0.017	(0.016)	815	40	0.006
More resolved by chief	0.193∗	(0.112)	518	32	0.687
Kling07 index	0.109	(0.081)	874	40	0.241
Anderson08 index	0.202	(0.165)	492	32	0.311
H10. Trust					
Trust people in general	0.049	(0.077)	877	40	0.106
Trust fellow villagers	0.020	(0.047)	872	40	0.101
Wallet return	0.034	(0.040)	860	40	0.100
People would not betray	0.012	(0.069)	870	40	0.047
Trust RUF	0.162	(0.097)	241	37	0.251
Trust CDF	0.082	(0.090)	342	37	0.205
Trust SLA	−0.301∗∗∗	(0.101)	243	34	0.233
Trust migrants	0.105	(0.063)	653	40	0.090
Kling07 index	0.038	(0.043)	878	40	0.160
Anderson08 index	0.080	(0.074)	184	33	0.121

Note: Each row reports a separate regression based on Equation 3.1, excluding controls. The leftmost column lists dependent variables grouped by hypothesis. Columns (1) and (2) report the coefficients and standard errors of T, where $T \in [0, 1]$ indicates treatment. Standard errors are clustered at the section level. *** is significant at the 1% level, ** is significant at the 5% level and * is significant at the 10% level.

Table A-8: Individual war exposure—ordinal measure

	Anderson Index		Kling Index	
	Not Exposed	Interaction	Not Exposed	Interaction
H1. Implementation	1.307***	-0.187***	2.365***	-0.083
	(0.079)	(0.045)	(0.167)	(0.049)
H2. Forgiveness	1.483	-0.731	1.483	-0.731
	(0.895)	(0.725)	(0.895)	(0.725)
H3. Psychological wellbeing	−0.319**	0.124	-0.232	-0.018
	(0.135)	(0.122)	(0.145)	(0.134)
H4. Ex-combatant attitudes	0.119	-0.074	0.118	-0.065
	(0.077)	(0.061)	(0.078)	(0.062)
H5. War attitudes	−0.031	-0.004	-0.025	-0.012
	(0.107)	(0.065)	(0.103)	(0.062)
H6. Gender attitudes	0.049	-0.035	0.048	-0.031
	(0.078)	(0.051)	(0.075)	(0.053)
H7. Conflict incidence	−0.051	0.016	-0.116*	0.002
	(0.056)	(0.049)	(0.062)	(0.052)
H8. Conflict resolution	0.488***	0.000	0.492**	0.043
	(0.137)	(0.031)	(0.191)	(0.048)
H10. Trust	0.063	-0.102*	0.069	-0.111**
	(0.086)	(0.054)	(0.082)	(0.053)
H11. Tensions	−0.017	-0.041	-0.029	-0.030
	(0.035)	(0.035)	(0.035)	(0.038)
H12. Network strength	0.002	0.029	-0.028	0.051
	(0.053)	(0.051)	(0.054)	(0.059)
H13. Group membership	0.086**	-0.037	0.095**	-0.030
	(0.037)	(0.038)	(0.043)	(0.040)
H14. Public goods	0.056	-0.007	0.075*	-0.019
	(0.047)	(0.038)	(0.041)	(0.036)
H15. Economic activity	−0.117*	0.049	-0.080	0.044
	(0.059)	(0.039)	(0.061)	(0.034)
H16. Economic welfare	−0.003	-0.039	-0.004	-0.051
	(0.069)	(0.050)	(0.058)	(0.043)

Note: Each row reports a separate regression based on Equation 3.3, excluding controls. The leftmost column lists each hypothesis. The first (final) two columns report regressions using the Anderson (Kling) index. Columns (1) and (3) show β_1, the treatment effect of individuals not exposed to war. Columns (2) and (5) show β_3, the marginal change in the treatment effect due to an increase in z, an ordinal measure of exposure to violence, ranging between 0 and 5. Standard errors are clustered at the section level. *** is significant at the 1% level, ** is significant at the 5% level and * is significant at the 10% level.

3.10.1 Pre-Anaysis Plan - Hypotheses

Below are the hypotheses as outlined in the PAP. The bold letter and numbers in parenthesis indicate the question number from the household survey. Questions from the village survey are denoted by 'VS' within parentheses.

Hypothesis 1. *The Fambul Tok program was implemented according to stated objectives.*

Relevant indicators include:

1. The respondent has heard of Fambul Tok [**S01**].
2. The section held a bonfire ceremony [**S03**].
3. Fambul Tok established at least one [**S18**]
   (a) Community Farm
   (b) Peace Tree
   (c) Peace Mothers Group.

A. Forgiveness and psychological wellbeing

Hypothesis 2. *Fambul Tok affects levels of forgiveness among respondents who were hurt during the war.*

The impact on forgiveness will be assessed for three groups:

1. For all respondents who have experienced hurt in the past [**F01**].
2. For respondents who experienced hurt in the past and know the perpetrator [**F02**].

3. For respondents who experienced hurt in the past, know the perpetrator, and the perpetrator still resides in the village **[F03].**

We construct a forgiveness index: an aggregation of 12 questions [**F04B–F04K, F04M F04O**], based on a 4 point Likert scale, as is standard in the forgiveness literature. The questions are taken from the Rye et al. (2001) forgiveness scale and adapted to the SL context and the specifics of the intervention.

Hypothesis 3. *Fambul Tok affects individual psychological wellbeing.*

Psychological wellbeing will be measured using indicators of:

1. Post-Traumatic Stress Disorder (PTSD), as measured by the most recent edition of the Diagnostic Statistical Manual of Mental Disorders (DSM IV) check-list, adapted for the SL context **[F23 – F33].**

2. Anxiety, as measured by the Zung anxiety index (Zung, 1971a), adapted for SL context. **[F06 – F15].**

3. Levels of depression, as measured by Zung depression index, adapted for SL context **[F16 – F22].**

Each of the three indicators will be constructed by aggregation of the questions indicated parenthetically, which are based on 4-point Likert scales, as is standard in the psychology literature.

B. Attitudes and beliefs

Hypothesis 4. *Fambul Tok affects individual attitudes towards ex-combatants.*

Relevant indicators include:

1. Change in the likelihood of believing that "those that did bad things in the past would do it again if they had the chance" [**T20**].

2. Change in the likelihood of believing that "people who joined the RUF are not responsible for what they did since they were drugged" [**T20**].

Hypothesis 5. *Fambul Tok affects individual attitudes towards war.*

Relevant indicators include:

1. Change in the likelihood of believing that people would fight in another war [**T22**].

2. Change in the likelihood of believing that people would be part of another rebellion [**T23**].

3. Change in the likelihood of becoming a fighter if there were another war [**T24**].

Hypothesis 6. *Fambul Tok affects individual attitudes towards gender.*

Relevant indicators include:

1. Change in the likelihood of believing that under certain circumstances, it is acceptable for a man to beat his wife [**P24 – P29**].

2. Change in the likelihood of believing that wife has right to express her own opinions (aggregate index, ranging from 1 to 18) [**P30**].

C. Conflict and conflict resolution

Hypothesis 7. *Fambul Tok affects incidence of conflict and crime within the community and between communities.*

Relevant indicators include:

1. Change in the total frequency of conflicts [**C01–C05**].

2. Change in the likelihood of being a victim of crime [**C52**] or violent crime [**C22**].

3. Change in the number of reported conflicts between villages [**VS: D01 D02**].

Hypothesis 8. *Fambul Tok affects conflict resolution, including whether conflicts are resolved satisfactorily and how they are resolved.*

Relevant indicators include:

1. Change in the proportion of reported conflicts that are satisfactory resolved [**C01F**].

2. Change in the proportion of reported conflicts that are resolved, satisfactorily or not [**C01E**].

3. Change in the proportion of reported conflicts that are resolved by "compromise between conflicting parties", rather than by a third party [**C01D**].

Hypothesis 9. *Fambul Tok affects the entrenchment of traditional sources of power.*

Relevant indicators include:

1. Change in the proportion of conflicts that are resolved by the village chief or elders [**C01 – C05**].

2. Change in the frequency of fines imposed by traditional authorities [**P11**].

D. Social capital

Hypothesis 10. *Fambul Tok affects the level of trust among community members.*

Relevant indicators include:

1. Change in self-reported trust in:
   (a) people in general [**T01**]
   (b) members of the community [**T02**]
   (c) former members of the Revolutionary United Forces (RUF) [**T18a**]
   (d) former members of the Civil Defence Forces (CDF) [**T18b**]
   (e) former members of the Sierra Leone Army (SLA) [**T18c**]
   (f) migrants [**T18d**].
2. Change in likelihood of believing that a purse, wallet, or money accidentally left out in public will not be stolen [**T07**].
3. Change in likelihood of believing that people in the community would betray each other [**T04**].

Hypothesis 11. *Fambul Tok affects social divisions and the inclusion of groups that have traditionally been marginalized.*

Relevant indicators include:

1. Change in the extent to which respondents feel that the community is divided between men and women; young and old; rich and poor; migrants and non-migrants [**T14**]. Likert scale based on self-reported degree of division, added across all the categories of divisions.

2. Change in the likelihood of the above mentioned differences having escalated into conflict in the past 6 months [**T15**]. Likert scale based on self-reported degree of division, added across all the categories of divisions.

3. Change in the likelihood of respondents feeling that a dominant group (elder, male, ruling family) will benefit more from a donation to the community [**P17 P19 P20**]. Sum number of groups that respondent feels that group will benefit more.

4. Change in the likelihood of respondents feeling that an excluded group (youth, poor, migrants, ex-combatants) will benefit less from a donation to the community [**P18; P21 - P23**]. Sum number of groups that respondent feels that group will benefit less.

5. Change in the number of respondents who are listed X or less times by other participants in social network questions [**S2 S3 S4 S5**]. The number X will be calculated from the control data to find the maximum number of connections that the bottom 10% of respondents have for each social networks question.

6. Change in subjective feelings of inclusion, measured by the likelihood of respondents feeling: (a) influential in decision-making [**P12**] (b) respected in the community [**P13**] (c) that people listen and concerns are heard [**P15**].

Hypothesis 12. *Fambul Tok affects the strength of networks within the community.*

Relevant indicators include:

1. Change in the number of people that respondent would hypothetically ask for help [**SN2**] or ask to go collect money for them [**SN1**]. Based on unprompted responses to social network questions.

2. Number of people that listed the respondent as someone they would ask to go collect money for them, share a farm boundary with, go to for advice, or consider a close friend [**S2 S3 S4 S5**]. Based on prompted responses to social network questions.

Hypothesis 13. *Fambul Tok affects the strength of group membership.*

Relevant indicators include:

1. Change in proportion of respondents who are members of:

   (a) Parent-Teacher Association (PTA) [**G04**]

   (b) Village Development Committee (VDC) [**G39**]

   (c) Youth group [**G57**]

   (d) Women's group [**G66**]

   (e) Secret society [**G48**]

   (f) Religious group [**G22**].

2. Change in likelihood of attending meetings of the above groups.

3. Chagne in likelihood of attending community meetings in general.

Hypothesis 14. *Fambul Tok affects the extent to which individuals contribute to public goods.*

Relevant indicators include:

1. Change in likelihood of having contributed money or labor to public facilities in the past six months [**P01 P02**].

2. Change in likelihood of having participated in road brushing in the past month [**P09**].

3. Change in the number of community projects that the community has worked on in the past six months [**VS: CA01 CA03**].

4. Change in the likelihood of giving money or food to someone in need in past three months [**P03**].

5. Change in the proportion of respondents who have contributed labor or money to:

   (a) Parent-Teacher Association (PTA) [**G08 G10**]

   (b) Village Development Committee (VDC) [**G43 G45**]

   (c) Youth group [**G16 G63**]

   (d) Women's group [**G70 G72**].

E. Economic activity and welfare

Hypothesis 15. *Fambul Tok affects market activity and economic cooperation within the community.*

Relevant indicators include:

1. Change in the frequency of borrowing [**E28 E32 E37 E42 E47 E52 E54**] and lending [**E01 E05 E10 E15 E20 E25 E27**] in the past three months.

2. Change in the total monetary value of borrowing [**E30 E35 E40 E45 E50**] and lending [**E03 E08 E13 E18 E23**] in the past three months.

3. Change in likelihood of being a member of an *osusu* (savings group) [**G02**].

4. Change in the number of traders in the community [**VS: E01**].

5. Change in the number of people who buy from traders in the community [**E55**].

6. Change in the number of communal farms [**VS: F03**].

7. Change in the likelihood of being a member of a labor gang [**G74**].

8. Change in the frequency of working on each others' farms [**F01 F03 F05 F07**].

Hypothesis 16. *Fambul Tok affects household economic welfare.*

Relevant indicators include:

1. Change in household asset ownership, as measured by an asset index using the principal component of asset ownership [**H06**].

2. Change in the likelihood of reporting problems in satisfying food/school fee/health care needs [**H19 – H22**]. Aggregate index, ranging from 1 to 20.

3. Change in reported satisfaction with the overall economic situation of the household compared to one year ago [**H23**].

Chapter 4

'White man's burden'? A field experiment on generosity and foreigner presence

Abstract

Can the presence of white foreigners influence measured behavior in developing countries? We experimentally vary foreigner presence across 60 behavioral games conducted in Sierra Leone, and find that the mere presence of a white foreigner increases player contributions in dictator games by 19 percent. Exploiting household and village survey data, we find that the treatment effect is smaller among players from powerful customary authority households—suggesting that perceived power differentials between player and experimenter, based on identity, mediate the effect. Players from villages with greater exposure to development aid also gave less, and disproportionately believed that the games were conducted to test them for aid suitability. This is consistent with players giving less in order to signal need, and suggests that expectations regarding development assistance also shape behavioral responses to foreigner presence. Our findings hold direct implications for how to measure altruism, target aid, and evaluate aid effectiveness in the developing world.

4.1 Introduction

A vast number of interactions in the developing world involve foreigners disbursing resources to locals, who differ starkly—and often visibly—in terms

Co-authored with Oeindrila Dube (New York University) and Bilal Siddiqi (Stanford University)

of their ethnic identity. The extent of such interaction has also risen dramatically in recent years, with the emergence of at least two trends. First, aid is increasingly disbursed through participatory approaches, which entail greater interaction between disbursers and recipients in the field. For example, over the last decade, the World Bank has allocated $85 billion in participatory development assistance (Mansuri and Rao, 2013). Simultaneously, there has been an explosive rise in the use of field experiments and behavioral games in developing country contexts (Figure 4.1). These two trends have overlapped through greater program evaluation of localized development assistance projects, which often employ behavioral experiments (Avdeenko and Gilligan, 2012; Fearon et al., 2009; Humphreys et al., 2012; **?**).

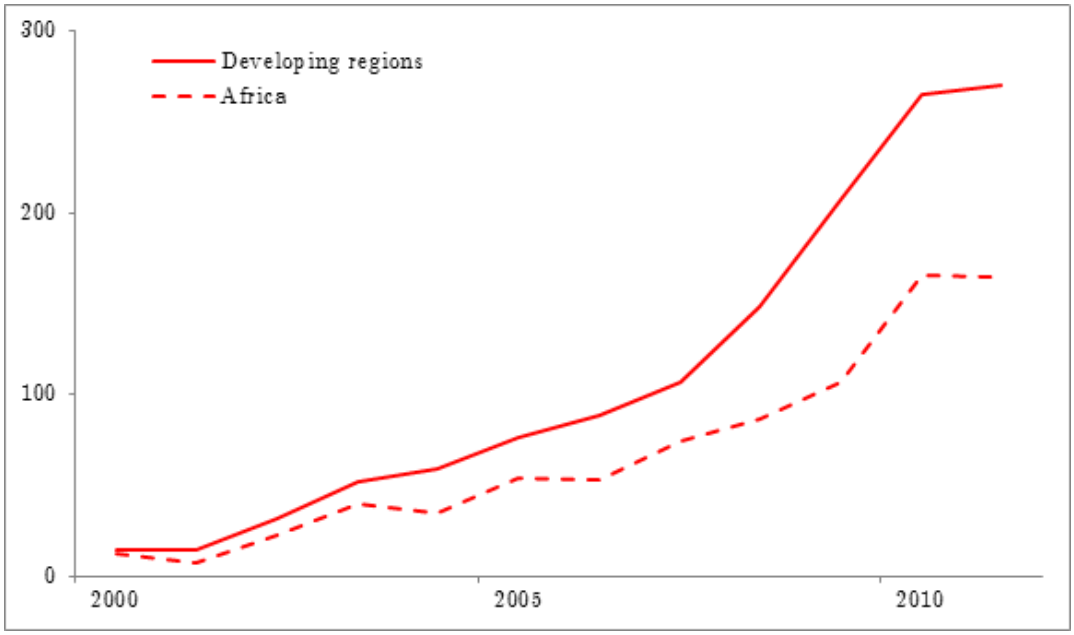
The results from such experiments are interpreted as unbiased metrics of human behavior. Yet, in both the experimental and aid contexts, differences in race and nationality strongly signal differences in wealth, power and authority. Understanding the extent to which these differences bias individual behavior is imperative given the growing use of behavioral techniques, and considering the implications of such bias for aid targeting and evaluation. However, to date, no study has directly examined this issue.

Our paper addresses this gap in the literature. We examine the effect of researcher race and nationality on individual behavior in a developing country. Specifically, we conduct a field experiment that randomizes the presence of a white foreigner across behavioral games in 60 villages in rural Sierra Leone, and assess its impact on generosity as measured by player contributions in dictator games.[1] We find that the mere inclusion of a white foreigner on the research team increases the amount given by a substantial 19 percent.

This 'white-man' treatment effect is consistent with the documented occur-

[1]Player contribution in a dictator game is the common measure of altruism in behavioral experiments (Cardenas and Carpenter, 2008)

Figure 4.1: The increasing use of dictator games in developing countries



Note. This graph shows the number of mentions of dictator games in developing countries (solid line) and in Africa (dashed line), based on Google Scholar counts.

rence of 'Hawthorne effects'—the broad idea that participants alter their behavior in experimental settings because they are being observed (Adair et al., 1989; Diaper, 1990; Haley and Fessler, 2005; Hoffman et al., 1994; Levitt et al., 2011; McCarney et al., 2007; Zwane et al., 2011). We interpret our findings as suggesting that Sierra Leonean respondents give more in the presence of the white foreigner believing that this is what he desires. Thus, the findings accord with a particular type of Hawthorne effect called 'experimenter demand effects', which arise when research subjects change behavior to conform to the perceived desire of the researcher (Levitt et al., 2011; Masling, 1966; Nichols and Maner, 2008). However, in contrast to past work on experimenter demand effects, we show that it is not the mere presence of the researcher that matters, but *who* the researcher is. In addition, by focusing on the experimenter's ethnicity, our paper differs from previous studies which have examined the import of players' racial, ethnic and national identities in determining behavioral game allocations (e.g. Adida et al., 2011; Burns, 2006, 2012; Cappelen et al., 2011; Habyarimana et al., 2007; Whitt and Wilson, 2007).[2,3]

Our study is also novel both in linking these behavioral effects to the foreign aid setting, and in providing evidence on the channels through which they operate. Conceptually, the aid and experimental settings are similar because the power differential embodied in aid disbursement is also present in behavioral games, where subjects play for real resources on the table. As such, the interaction between researcher and subject often resembles the interaction between aid-workers and recipients. Given these links between the experimental and aid

[2]The social psychology literature has also explored the impact of a number of surveyor and experimenter characteristics, including race, on relevant outcomes—see Rosenthal (1963) for a summary.

[3]Our paper also relates to studies showing that researcher characteristics such as race, education, income, gender, and religion influence subject response in survey data (Anderson et al., 1988; Bailar et al., 1977; Blaydes and Gillum, 2012; Cotter et al., 1982; Finkel et al., 1991; Hyman, 1954; Mensch and Kandel, 1988; Miyazaki and Taylor, 2007; Reese et al., 1986; Webster, 1996).

setting, we utilize rich household and village-level survey data collected from participating communities to explore heterogeneity in the white man treatment effect along two dimensions: (*i*) power and status differentials and (*ii*) the village's past exposure to development aid.

We document substantial heterogeneity in the treatment effect along both dimensions. First, we find that players of a relatively high social status—those from the household of a chief, religious leader, or powerful organizations called secret societies—respond to a lesser degree. In fact, in contrast to other participants, they give *less* in the presence of the white foreigner. This is consistent with the idea that experimenter demand effects reflect perceived differences in power (Schultz, 1969)—higher-status individuals respond less because they view the experimenter as closer to them along the status dimension.

Second, we find that the treatment effect is substantially smaller among villages with greater prior exposure to development aid, as measured by longer engagement with NGOs. Correspondingly, players from villages with greater aid exposure are also more inclined to believe that the behavioral games were conducted to test their community's suitability for aid disbursement. This pattern suggests that those who have more experience with foreign aid behave strategically: they display smaller increases in generosity under the white-man treatment to signal that they have smaller leeway in giving away resources, as a means of demonstrating their need for future aid. In short, the treatment effect is driven by lower status individuals, and villages with relatively little experience with foreign aid.

These results have several implications for the interpretation and design of behavioral games in field settings. Past studies have found variation in measured giving across countries and cultural groups (Cappelen et al., 2011;

Henrich et al., 2006).[4] Our results suggest that differential exposure to foreign aid or foreigner presence among these countries could contribute to these observed differences. Looking forward, since measured behavior may be biased in the presence of white foreigners or other outsiders, behavioral experiments seeking to evaluate programs should be designed so the composition of the research team is balanced across treatment and control groups.

The finding that past aid exposure affects how individuals respond to outside researchers holds implications for the design of aid targeting mechanisms. Decentralized aid delivery emerged partly as a response to charges that top-down aid-delivery mechanisms had been generally ineffective (Easterly, 2006), with the implication that greater interaction between aid-givers and communities would improve targeting and delivery of services. Our study suggests that efforts to assess needs by outsiders may be biased if aid-exposed communities strategically signal higher need, and in that vein, needs assessments conducted within and by the community itself may be more effective.

Finally, if communities exposed to aid act strategically in behavioral games (e.g., through need-signalling), assessments of aid effectiveness using behavioral games may underestimate the true impact of aid. This is important given that several recent studies have found small or no effects associated with community driven development or reconstruction (CDD/R) programs (Avdeenko and Gilligan, 2012; Casey et al., 2012; Humphreys et al., 2012).

Below, section two provides institutional context; three and four discuss the empirical strategy and data; five presents the results, and six concludes.

[4]See Cardenas and Carpenter (2008) for additional examples.

4.2 Institutional context

Two features of Sierra Leone are relevant for this study—its extensive exposure to foreign aid, and its entrenched traditional authority system, including the strong local power base of chiefs, religious leaders, and secret societies.

Sierra Leone suffered a long and brutal civil war, spanning from 1991 to 2002. The war contributed to the escalation of poverty, leaving the nation amongst the ranks of the world's poorest and least developed—in 2011, the country ranked 180th out of 187 countries on the United Nation's Human Development Index, up from 187th out of 187 in 2003. With the end of the conflict, the country experienced a surge in foreign aid (Fanthorpe, 2003). Official Development Assistance as a proportion of GNI peaked at 43 percent in 2001, and remained over 24 percent in 2010, making it the 11th most aid-dependent nation in the world (World Bank, 2012). Given this dependence, exposure to development aid is highly likely to shape Sierra Leoneans' perception of white foreigners.

Sierra Leone has a strong chieftaincy system, a key institutional legacy of British indirect rule. With the creation of the protectorate in 1898, the Paramount Chiefs—the highest-level traditional chiefs—were given substantial powers over their subjects (Acemoglu et al., 2013; Fanthorpe, 2001; Jackson, 2006). Despite postwar attempts to reform the chieftaincy system and create more accountable, formal sources of administrative authority, *de facto* power still remains with the chiefs: "chiefs are regarded as legitimate traditional rulers, with broader political power bases than the council chairperson" (Jackson, 2006). They are still called upon to settle disputes (Sawyer, 2008), and they retain the power to tax, request labor, administer justice, and allocate land. Nonetheless, NGOs and donors have increasingly preferred to operate through the new formal channels and bypassed chiefs due to concerns of cor-

ruption and a desire to strengthen formal governance structures (Fanthorpe, 2003; Jackson, 2006). As a result, there is sometimes an adversarial relationship between chiefs and foreign aid workers, and chiefs may perceive foreigners as a potential threat to their power.

Informal power in Sierra Leone also resides in secret societies, cultural institutions or "cult associations" (Richards, 2005b) that regulate social behavior through secret rituals and practices (Fanthorpe, 2007). Most Sierra Leoneans are members of secret societies and their influence permeates all aspects of social and political life in Sierra Leone (Murphy, 1980).

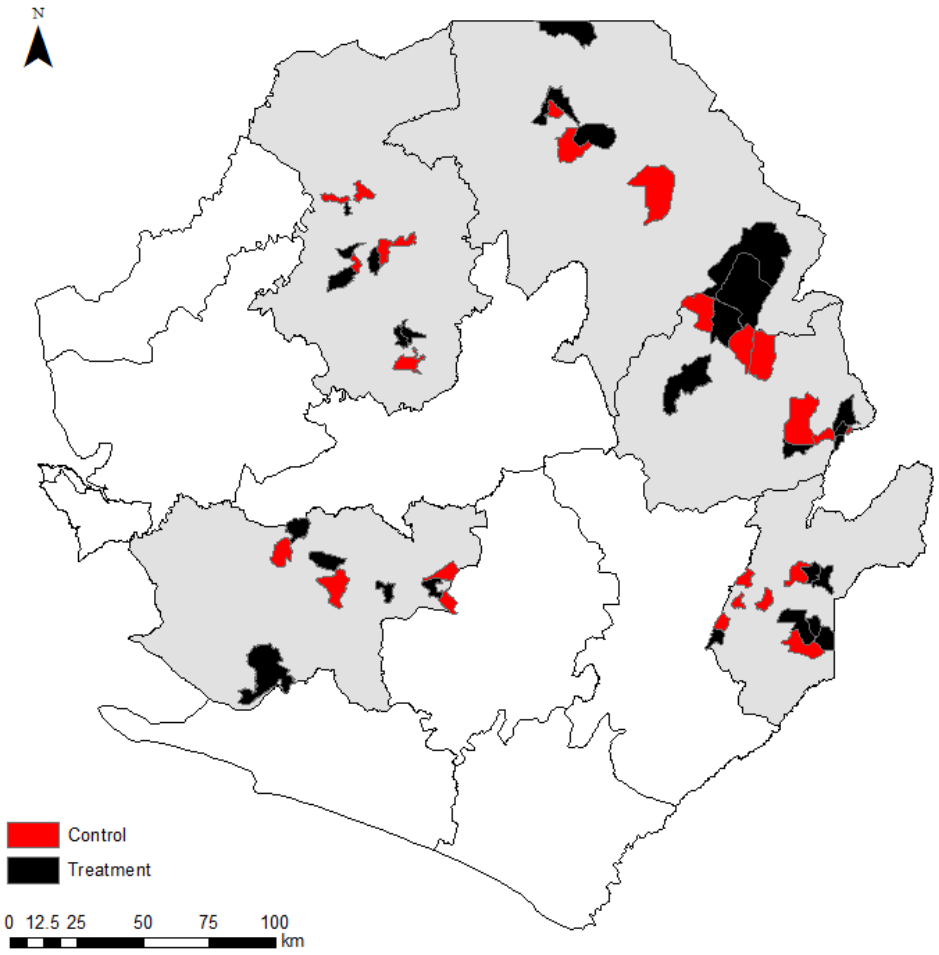
Traditional and missionary religions overlap considerably with both these institutions. While most Sierra Leoneans identify as either Muslim (60%) or Christian (30%), the practice of these religions is intertwined with traditional religious beliefs (Conteh, 2009). Religious leaders are often part of secret societies and administer various rituals. Local religions imbue higher powers with chief-like authority—the word for god in Limba means, literally, "The Great Chief". The chief's powers are also intricately linked to the workings of the secret societies Jackson (2006).

4.3 Experimental design

To causally identify the impact of white foreigner presence on measured generosity, we randomized the presence of a white 'supervisor' in the administration of a series of behavioral games. The experiment was carried out in 60 villages across five districts in Sierra Leone: Bombali, Kailahun, Koinadugu, Kono, and Moyamba. There are six treatment and six control villages in each district. Figure 4.2 shows these treatment and control communities.

All games were administered by a core team of Sierra Leoneans comprising

Figure 4.2: Treatment and control communities



Note. This figure shows the five districts of Sierra Leone included in the experiment (grey), the 30 treatment sections (black) and the 30 control sections (red). One pre-designated village from each of these sections comprises the treatment and control communities in the study.

one trained facilitator, two assistants, and a local translator recruited from the area where the games were played. In addition, the team included a 'supervisor' whose identity varied systematically across control and treatment villages. In the control villages, the supervisor was a Sierra Leonean from Freetown, the national capital. In the treatment villages, the supervisor was a white American. Both supervisors were chosen for similarity on key characteristics: male, college-educated, and friendly demeanors.

One possible concern is that some other unobserved characteristic of the two individuals drove participants' responses. However, we took several precautions against this. First, neither 'supervisor' was made aware of the experiment being conducted in order to minimize the chance of expectancy effects (Rosenthal and Rubin, 1978). At the same time, to prevent other potential differences (such as proactivity) from confounding the 'white-man' treatment, all games were conducted according to a strict protocol and a set script that specified a silent, background role for the supervisors. Their role was limited solely to taking notes and distributing money to players in the beginning of each game. They did not speak during the administration of the game, or otherwise interact in any way with game participants. They were also not present in the room when players made their allocation decisions. Most importantly, Sierra Leone's rural population is homogenously black African. Thus, a white foreigner's race serves as his or her most salient characteristic.

The 12 players who participated in the game session of each village were randomly sampled during a household survey conducted up to two months prior to the implementation of the games. If one of the originally sampled individuals could not be found, he or she was replaced by someone else from the same household, and of the same gender as the original target respondent. Replacement occurred for 7.5 percent of the sample. As such, our sample is

reasonably representative of the broader population being studied—specifically, communities in the five of the most war-affected districts in Sierra Leone.

Our measures of generosity were generated using three versions of the standard dictator game. The advantage of the dictator game is that it provides a clean measure of other-regarding preferences, since payoffs to play are independent of beliefs over other players' expected behavior.

In each of our games, the player was given 4,000 Leones (approximately $1), which is an amount slightly higher than the average daily income in Sierra Leone. The player could decide how much of this money to keep and how much to 'give' to recipients.

The three games played were:

1. Game 1: Anonymous Own-village. The recipients were 12 additional individuals randomly sampled from the players' village. The players were told who these 12 recipients were, but not which recipient they would be matched to. Players were also informed that they would be re-matched to a different recipient in each game. Player contributions were strictly anonymous, i.e. no player was informed how much any other player had chosen to contribute.

2. Game 2: Non-anonymous Own-village. Here, we relaxed the between-subject anonymity condition. Instead, players were told that their giving decision would be announced at the end of the session, after all games had been played. All other game elements of Game 1 and 2 remained the same. Recipients were the same individuals from Game 1, and as in that game, players were not told who they were matched to. To minimize feedback across games, the announcement was made in front of the players and recipients at the end of the session, after all games had been played.

Recipients were not present during the game but arrived at the end after all games were played to receive their allocations.

3. Game 3: Anonymous Other-village. Players were told that we had identified 12 recipients in "another village" who would receive the amount they chose to give. All other game elements were the same as in Game 1—no player was informed of any other player's contribution, who they were matched to, etc.

Games were conducted in two rooms, a public room where the group received instructions, and a private room where they individually entered to make their allocation decision. No talking was allowed during the explanation of the games, to prevent strategic interaction and framing. To ensure that players understood the game, the facilitator repeated the explanation of the game in the private room, allowing players to ask questions. After the second explanation, the facilitator would exit the room, allowing the player to make her or his allocation decision privately. The players were instructed to place their allocations in an envelope, and place the envelop into the slit of a locked box.

To maximize players' comprehension, the games were also translated into seven local languages in three of the five districts.[5] To assure consistency between translations, each of these translations were back-translated to English. To account for the possibility that the teams may have improved in their communication skills over time, particularly in the translated districts, we varied the ordering of treatment and control villages, so that games were conducted first in treatment villages in half the districts, and first in control villages in the other half.

[5]The facilitator spoke the local languages in the other two districts.

4.3.1 Empirical strategy

Based on this experimental design, we use the following specification to estimate the impact of the white foreigner effect:

$$y_{ivld} = \alpha_d + \delta_l + \beta(\textit{white-man}_{vld}) + \mathbf{Z}_{vld}\rho + \mathbf{X}_{ivld}\phi + \omega_{ivld} \tag{4.1}$$

where i is individual, v is village, l is majority language group, and d is district. y_{ivld} is the amount given in the dictator games, $\textit{white-man}_{vld}$ is an indicator of treatment to assignment, α_d are district fixed effects, and δ_l are majority language fixed effects. We include district fixed effects to account for substantial regional heterogeneity in economic development, urbanization, war exposure, and other factors that may influence generosity. We include majority language fixed effects to control for variation in underlying generosity levels across ethnic groups, since language groups correspond closely to ethnic/tribal affiliation in Sierra Leone. Majority language fixed effects also reduce noise created by the varying quality of communication across games: some games did not need to be translated because the facilitator spoke the majority language; moreover, translator quality varied across translated games. $\mathbf{Z}_{vld}$ and $\mathbf{X}_{ivld}$ are additional village and individual level covariates included in some specifications. β captures the white-man treatment effect.

When we pool the observations from the three different game types we estimate:

$$y_{givld} = \gamma_g + \alpha_d + \delta_l + \beta(\textit{white-man}_{vld}) + \mathbf{Z}_{vld}\rho + \mathbf{X}_{ivld}\phi + \omega_{givld} \tag{4.2}$$

where g denotes a game, and γ_g are game fixed effects. In all specifications,

we cluster the standard errors at the village level.

4.4 Data

The behavioral games data used in the analysis is supplemented by household and village level survey data collected prior to implementation of the games. We have village-level data for all 60 villages, and household-level data for 715 out of 720 participants, though the sample size varies for particular measures.

To account for ethnicity, we construct an indicator variable for each language spoken by the majority of the respondents in each village. Ethnicity is geographically concentrated and identified by language. The language spoken by the majority of villagers therefore provides a measure of the majority ethnic group in the village. There were seven ethnic groups among our sampled villages (Fullah, Kono, Koranko, Loko, Mende, Temne and Yalunka). However, there was only one Yalunka majority village, and thus, for robustness, we constructed two additional measures of ethnicity, generated on the basis of broader linguistic categories using parent groups from the third and fourth levels in the Atlantic language hierarchy.[6]

We control for violence-related effects by using a measure of the number of buildings burnt in each village during the war. We also measure village size based on the number of households residing in the village, and the level of market integration, with an indicator for whether there is a market in the village. We also use two proxies for village cooperation: an indicator of whether the village has communal farms and an indicator of whether it has a labor gang, which refers to a group of people who work on each others' farms.

To measure each village's exposure to foreign aid, we construct various

[6]The Atlantic language hierarchy is catalogued at `http://multitree.org`.

measures of NGO involvement in the provision of local public goods.[7] The broadest indicator is based on whether a NGO either owns a school or clinic, or currently provides resources to the local school or clinic, or has contributed to the construction of public facilities (including schools, clinics, wells, latrines, or the palava hut[8]). This variable is called "NGO aid", and 54 villages in our sample (all but six) have some form of aid engagement under this metric.

To test if perceptions of foreigners are likely to be mediated based on the extent of aid exposure, we construct two measures. The first variable, "Years of aid with NGO-owned facility," refers to the number of years that a NGO has owned either a school or clinic in the village. It is worth noting that NGOs own these public facilities in 31 (or just over half) the sample villages. As an additional measure, we construct "Years of aid with NGO activity," which is the number of years a NGO has either owned a school or a clinic, or the number of years since a NGO contributed to the construction of the school or clinic, if a NGO provides current support to the school or clinic. Although this variable has the advantage of being more inclusive (capturing NGO activity in 5 additional villages where they do not own facilities), we cannot verify whether these organizations engaged continuously over the period between their initial contribution and current support, and thus view it as an auxiliary measure.

To measure household socioeconomic status, we develop an asset index. As is standard in the literature (Vyas and Kumaranayake, 2006), we use principal component analysis to construct an aggregate measure based on ownership of a broad range of appliances and equipment,[9] the material of the roof and floor

[7]In the Sierra Leonean context, nearly all large NGOs involved in providing public goods in rural villages are foreign or have foreign funders.

[8]The palava hut is often the only public space in the village for community members to congregate.

[9]These include: iron, refrigerator, television, radio, mobile phone, generator, fan, bed, clock, sewing machine, modern stove, bicycle, motorcycle, car/truck, and pushcart.

of the house in which the respondent lives, as well as ownership of land.[10] Data on the respondent's educational attainment is used to generate an indicator of whether he or she has some formal education. We are also able to examine the player's past exposure to white foreigners, since the household survey asked how often the respondent has met a white person before. We also ask the respondent if someone in their household is a paramount, town, or village chief/headman; a religious leader including reverends, imams or pastors; or the leader of a secret society.

Finally, after the games were conducted, we asked the participants to choose what they believed to be the purpose of the research team's visit. Specifically, the participants were asked: "Why do you think the researchers were playing these games? Choose the statement you agree with most." The following three statements were then read out to them: "To give money to the community in a fun and educational way", "To test the community, to see which community is more deserving of aid", and "To find out more about how members in this community think about each other, interact with each other, and treat each other." The last option corresponds most closely to the stated purpose of the research team's visit prior to the start of the games, which was to find out about the community. In the text and tables, we refer to the first statement as 'Give money', the second as 'Aid test' and the third as 'Research'.

4.4.1 Descriptive statistics

Table 4.1 presents descriptive statistics of key variables. We utilize four dependent variables from the behavioral games: total giving, which is the sum of giving from the three dictator games, as well as giving in the anonymous

[10] We cannot use measures such as income or wages to control for wealth since the majority of respondents are subsistence farmers.

own village, anonymous other village, and non-anonymous own village games.[11] When we pool across all three games, the dependent variable is called 'Giving'. The average total giving was 3,216 leones out of 12,000 given to players in all three games, or an average 1,079 (=$0.25) per game, which is about a quarter of the daily wage in Sierra Leone.

Table 4.2 examines key individual and village level characteristics in the control and treatment groups. There are no statistically significant differences in these characteristics across the two groups with the exception of respondent age and whether the village has a communal farm. Though these differences are only significant at the 10 percent level, we show that the results are robust to controlling for these characteristics.

4.5 Results

We begin a with a simple graphical exploration of whether the white-man treatment induced differential giving. Figure 4.3 shows the CDF of total giving in treatment and control regions. The treatment distribution first-order stochastically dominates the control distribution: a higher proportion of the treatment group gave more, evaluated at each level of giving, indicating that the white-man treatment induced an upward shift in the distribution of giving in the dictator games.

Next, we present regression estimates of equation (4.1). The odd numbered columns in Table 4.3 show specifications without ethnicity fixed effects, and the even numbered columns present equivalent specifications including ethnicity fixed effects, which improve the precision of the estimates. The coefficients in columns (1) and (2) show that the white-man treatment significantly increased

[11]The anonymous other village game is only available for 708/720 observations owing to enumeration error, since the values for this game were not recorded in one of the villages.

Table 4.1: Descriptive Statistics of key variables

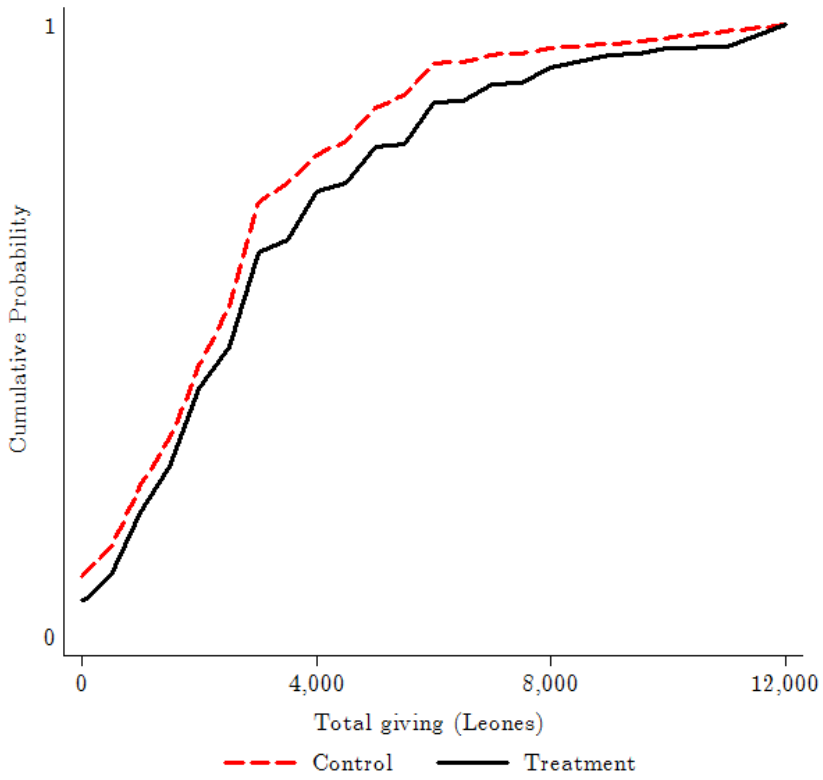
	Observations	Mean	Std. Dev.	Min.	Max.
Total giving	708	3216.95	2648.11	0.00	12000.00
Anonymous Own-village giving	720	1065.28	1032.12	0.00	4000.00
Non-anonymous Own-village giving	720	1211.25	995.59	0.00	4000.00
Anonymous Other-village giving	708	959.04	1005.16	0.00	4000.00
Giving	2148	1079.19	1015.91	0.00	4000.00
Aid test	719	0.14	0.35	0.00	1.00
Give money	719	0.64	0.48	0.00	1.00
Research	719	0.22	0.41	0.00	1.00
Customary authority	720	0.25	0.44	0.00	1.00
Met white person 1 to 10 times	715	0.46	0.50	0.00	1.00
Never met white person	715	0.10	0.30	0.00	1.00
Age	712	42.33	15.38	12.00	110.00
No formal education	711	0.61	0.49	0.00	1.00
Female	715	0.54	0.50	0.00	1.00
Ethnic majority	714	0.89	0.31	0.00	1.00
Household asset index	706	0.00	1.97	-1.72	20.34
NGO aid	720	0.90	0.30	0.00	1.00
Years NGO-owned facilities	720	12.18	22.50	0.00	106.00
Years NGO activity	720	13.42	22.41	0.00	106.00
Number of households	720	284.27	411.02	3.00	2000.00
Market community	720	0.13	0.34	0.00	1.00
Buildings burned during war	720	5.77	15.43	0.00	100.00
Labor gang	720	0.95	0.22	0.00	1.00
Communal farm	720	0.37	0.48	0.00	1.00

Table 4.2: Key individual and village-level characteristics

	Treatment	Control	Difference	Std. Err.
Individual				
Customary authority	0.23	0.28	-0.053	(0.032)
Met white person 1 to 10 times	0.44	0.48	-0.039	(0.048)
Never met white person	0.12	0.08	0.038	(0.032)
Age	41.23	43.43	-2.228*	(1.172)
No formal education	0.59	0.63	-0.038	(0.044)
Female	0.55	0.54	0.007	(0.039)
Ethnic majority	0.87	0.91	-0.039	(0.034)
Household asset index	0.00	-0.00	-0.003	(0.232)
Village				
NGO aid	0.93	0.87	0.067	(0.076)
Years NGO-owned facilities	10.13	14.23	-4.100	(5.633)
Years NGO activity	11.13	15.70	-4.567	(5.570)
White aid visitors	0.34	0.37	-0.012	(0.116)
White non-aid visitors	0.41	0.37	0.046	(0.127)
Number of households	334.23	234.30	99.933	(94.736)
Market community	0.17	0.10	0.067	(0.087)
Buildings burned during war	6.10	5.43	0.667	(3.559)
Labor gang	0.93	0.97	-0.033	(0.054)
Communal farm	0.27	0.47	-0.200*	(0.106)
Dominant ethnic group				
Fullah	0.03	0.03	-0.000	(0.044)
Kono	0.20	0.20	-0.000	(0.000)
Koranko	0.13	0.17	-0.033	(0.050)
Loko	0.07	0.07	-0.000	(0.055)
Mende	0.40	0.40	0.000	(0.000)
Temne	0.13	0.13	-0.000	(0.055)
Yalunka	0.03	0.00	0.033	(0.032)

Note: Columns 1-2 present treatment and control group means, respectively. Column 3 shows coefficients from a regression of each variable on the white-man treatment indicator. Column 4 displays robust standard errors clustered at the village level. * is significant at the 10% level.

Figure 4.3: CDF of total giving in control and treatment groups



Note. This figure shows the cumulative distribution function of total giving in the control and treatment groups.

the sum of total giving in the three dictator games. With ethnicity fixed effects, giving increases by 564 leones. This represents a 19 percent increase in average total giving, relative to the control mean (2944 leones). In Table 4.4, we show the robustness of this result to the inclusion of respondent age and communal farm indicator, as well as alternative ethnicity fixed effects, generated on the basis of broader linguistic categories.

Columns (3)-(8) in Table 4.3 separately examine each of the three game types. The effect is most precisely estimated for the Anonymous Own-village game, where the coefficient is significant at the 5 percent level. The coefficient is marginally insignificant (with a p-value of .101) for the Anonymous Other-village game. The magnitude of the coefficients are also largest in the these two games, where the implied effects are 23 and 21 percent, respectively. In contrast, the coefficient on the Non-anonymous Own-village game is both smaller in magnitude and less precisely estimated.

To further explore how giving varies across the games, in Table 4.5, we estimate equation (4.2) which pools giving in all three games. Here, all specifications include indicators for each game type, in which the anonymous own-village game serves as the omitted category. Column (1) presents the mean effect. The coefficient of 174.003 indicates that giving was 17 percent higher in treatment villages relative to the control group mean of 997.191, and this difference is statistically significant.

The coefficient on the Anonymous Other-village game shows that individuals give significantly less when the proceeds go to an outside village, relative to their own village. This indicates that members prefer to give to their own group relative to an out-group, which accords with past findings that players display more generosity toward those who are socially closer to them (Bohnet and Frey, 1999; Brañas Garza et al., 2012; Charness and Gneezy, 2008; Leider

Table 4.3: White man presence and measured generosity

	Total giving		Anonymous Own-village		Non-anonymous Own-village		Anonymous Other-village	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
White-man	537.320*	564.943*	219.444**	223.585**	103.056	118.994	173.740	183.908
	(312.028)	(296.734)	(107.248)	(102.443)	(105.064)	(103.127)	(117.578)	(110.449)
Control group mean	2943.966	2943.966	955.556	955.556	1159.722	1159.722	872.126	872.126
Ethnicity fixed effects	No	Yes	No	Yes	No	Yes	No	Yes
Observations	708	708	720	720	720	720	708	708

Note: Robust standard errors clustered at the village level in parentheses. All specifications include district fixed effects. ** is significant at the 5% level and * is significant at the 10% level.

Table 4.4: Robustness Checks

	(1) Total giving	(2) Total giving	(3) Total giving	(4) Total giving	(5) Total giving	(6) Total giving
White-man	604.539**	555.552**	564.943*	542.736*	564.681*	555.390*
	(282.152)	(277.340)	(296.734)	(291.258)	(297.182)	(293.607)
Age	8.200					
	(6.744)					
Communal farm		-45.387				
		(424.801)				
Replacement					30.307	-40.539
					(294.568)	(406.934)
Replacement x white-man						127.623
						(630.268)
Linguistic group for ethnicity f.e.s	Primary	Primary	Parent-4	Parent-3	Primary	Primary
Observations	700	708	708	708	708	708

Note: Robust standard errors clustered at the village level in parentheses. All specifications include district and ethnicity fixed effects. Columns 1, 2, 5 and 6 use ethnicity fixed effects based on the primary local language. Columns 3 and 4 use alternative measures of ethnicity constructed from the 3rd (Parent-3) and 4th (Parent-4) highest parent group of the Atlantic language family. Replacement equals one if the game participant was a replacement household member. *** is signiffcant at the 1% level, ** is significant at the 5% level and * is significant at the 10% level.

Table 4.5: White man presence, anonymity, and outgroup giving

	(1) Giving	(2) Giving
White-man	174.003*	229.105**
	(98.905)	(104.374)
Anonymous Other-village game	145.972***	204.167***
	(35.291)	(37.073)
Non-anonymous Own-village game	-107.481***	-83.127*
	(34.041)	(44.766)
White-man x Non-anonymous Own-village game		-116.389*
		(68.960)
White-man x Anonymous Other-village game		-48.818
		(67.544)
Control group means		
All games	997.191	
Anonymous Own-village		955.556
Non-anonymous Own-village		1159.722
Anonymous Other-village		872.126
Observations	2148	2148

Note: Robust standard errors clustered at the village level in parentheses. All specifications include district and ethnicity fixed effects. *** is significant at the 1% level, ** is significant at the 5% level and * is significant at the 10% level.

et al., 2009).

In contrast, the coefficient on the Non-anonymous Own-village indicator shows that giving is significantly higher when proceeds are given non-anonymously, relative to anonymously, in the players' own village. This is consistent with previous findings that reducing anonymity among players increases giving (Andreoni and Bernheim, 2009; Charness and Gneezy, 2008; List et al., 2004; Masclet et al., 2003; Rege and Telle, 2004), and underscores the idea that reputational concerns lead individuals to demonstrate greater generosity when allocations are publicly reported.

To see if the white-man treatment effect significantly differs across games, column (2) presents specifications where we interact each of these game types with the treatment dummy. Here, the coefficient on the white-man variable gives the treatment effect for the anonymous own village game. The interaction of white-man with the other village indicator (-48.818) is statistically insignificant, indicating that individuals do not respond differentially to the treatment in own-village versus other-village giving. In contrast, the coefficient on the interaction of white-man and the non-anonymous indicator shows that the treatment effect becomes significantly *smaller* when non-anonymity is introduced. This suggests that removing anonymity makes 'local' reputational concerns with regards to fellow villagers more salient, crowding out reputational concerns related to the white foreigner.

In summary, these results present robust evidence that the mere presence of a white foreigner on a team of five researchers increases measured total giving in the dictator games by 19 percent, with largest increases in the anonymous own-village case where giving increased by as much as 23 percent. In the subsections below, we additionally draw on our household and village survey data to explore mechanisms through which white foreigner presence affects measured

generosity.

4.5.1 Power and status differentials

First, we examine the role of power differentials between the foreign researcher and local players in driving higher giving. It is plausible that the desire to impress the white foreigner may manifest itself as an "experimenter demand effect" (Zizzo, 2010), where respondent behavior is driven by a desire to "please" the experimenter. According to Loewenstein (1999, p. F30), "[i]n social encounters, including laboratory experiments, most are engaged in a constant search for cues about how they are supposed to behave." Players infer that the experimenter wants them to display more generosity in the dictator games, and then act accordingly. This motivation could stem from deference to someone perceived to be in a position of authority. For example, Schultz (1969, p.221) describes the lab as a "superior-subordinate" relationship matched only by that of "parent and child, physician and patient, or drill sergeant and trainee."

In Sierra Leone, there are stark disparities in wealth and power embodied in interactions between locals and white visitors: foreign aid workers dispense aid and restructure governance, UN peacekeepers provide security, and businesses and miners buy and potentially exploit land. These differences provide an environment conducive to experimenter demand effects.

If the white-man treatment operates by reinforcing power differentials, the magnitude of the treatment effect should differ across players based on their position of power and relative authority within their communities. To test this hypothesis, Table 4.6 examines heterogeneous effects based on whether the individual comes from a customary authority household, which includes members belonging to the three groups described earlier that traditionally symbolize

power and authority in Sierra Leone: chiefs, religious leaders, and leaders of secret societies.

We use our household survey to generate an indicator of whether the respondent comes from a customary authority household. However, since these and other household characteristics are not observed players who were replacements, column (1) of Table 4.6 begins by showing that the average treatment effect continues to hold when the sample is restricted to non-replacements. Furthermore, Table A.2 shows that accounting for replacements does not affect the estimated effects.

In Column (2) of Table 4.6, we interact the white-man treatment with the "customary authority" variable. The negative coefficient on the interaction term shows that the treatment effect is smaller for this subgroup. While higher status persons from customary authority households give 948 Leones (or 29%) more than their lower status counterparts in control villages, they give 463 (or 13%) less in treatment villages. These heterogeneous effects suggest that the relative power differential between player and experimenter plays a role in mediating the treatment effect: lower status individuals respond positively and give more in response to the white-man treatment.

The lower allocations by high status individuals suggests they may be counter-reacting to a perceived demand from the experimenter to give more. This relates to a point made earlier—foreign aid has increasingly been channeled through new formal local government structures, circumscribing the power of traditional authorities, and creating a more adversarial relationship in the backdrop between traditional authorities and foreigners (Fanthorpe, 2001; Jackson, 2006).

Next we conduct a series of robustness checks to demonstrate that other demographic characteristics potentially correlated with customary authority

Table 4.6: Power and status differentials

	(1) Total giving	(2) Total giving	(3) Total giving	(4) Total giving	(5) Total giving
White-man	548.941*	918.656***	561.844*	659.956	896.787
	(292.644)	(314.997)	(283.814)	(1140.880)	(1564.535)
Customary authority x white-man		-1411.335***		-1318.475**	-1413.513***
		(472.147)		(500.431)	(500.372)
Customary authority		948.297***		794.595**	810.837**
		(289.489)		(311.818)	(306.791)
Met white person 1 to 10 times x white-man				411.691	293.081
				(484.232)	(483.378)
Never met white person x white-man				251.298	159.540
				(650.947)	(630.495)
Met white person 1 to 10 times				-62.034	-65.306
				(257.163)	(250.496)
Never met white person				-166.885	-342.244
				(405.213)	(402.063)
Individual controls	No	No	No	No	Yes
Observations	653	653	634	634	634

Note: See Table 4.5 for notes and text for control variables.

households do not drive these effects. Specifically, we control for age, gender, whether the individual is a part of the village's ethnic majority, as well as two proxies for wealth—a household assets index and an indicator of whether he or she has any formal education. It is possible that higher status individuals may be reacting differently due to greater past interaction with white persons relative to the rest of the community, which may reduce experimenter demand effects by mitigating how foreign or novel the white researcher seems. To account for this, we introduce controls for two variables (never met a white person before, and met a white person between one and 10 times, both defined relative to the omitted category of met a white person more than 10 times).

Since we lose sample size with the addition of individual level controls (due to missing observations), in column (3), we show that the average treatment effect remains significant in the reduced sample. Then, in column (4), we control for all of these variables, along with their interactions with the white-man treatment. The customary authority interactions remain significant with the inclusion of these control interactions, indicating that past exposure to white foreigners and other demographic characteristics cannot account fully for these effects.

4.5.2 Aid Exposure

Given Sierra Leone's history with foreign aid, it is possible that citizens generally perceive the presence of white foreigners to be associated with aid disbursement. To test this hypothesis, we examine if there are differential effects of the white-man treatment based on past aid exposure on two sets of dependent variables: measured generosity in the dictator games, as well as participant beliefs regarding the purpose of the games.

We begin by examining effects on generosity. Table 4.7 shows the results

from interacting the white-man treatment with our three measures of aid exposure: years of aid with NGO-owned facility, an indicator of NGO-owned facilities and years of NGO activity. In columns (1)-(3), the positive coefficient on the white-man base term indicates that communities receiving no aid give more in treatment communities, while the negative coefficient on the interaction term suggests that this effect falls with greater exposure to aid. For example, the coefficient 751 in the first column indicates that communities without any NGO-owned facilities (approximately half the sample villages) give 32% more in the presence of the white researcher above the control group mean (2,320). The interaction term coefficient implies that at mean aid exposure levels of 12 years, the effect falls to 510, a 10% drop relative to the non-aid communities.

It is possible that villages receiving aid for longer periods have other characteristics which determine this pattern of giving. For example, these communities might be larger, less integrated, better organized, richer, and have more exposure to white foreigners in general. To account for these potential confounds, we include interactions between the treatment and a number of individual and village-level characteristics, including: village size and the presence of a market, which are important indicators of economic development and the degree of market integration; whether the village has communal farms or a labor gang, which are measures of underlying societal cooperation; and the number of buildings burnt during the civil war, as post-conflict aid may have been targeted to areas with higher levels of historical violence. To account for wealth, we include the individual-level control interactions from column (4) of Table 4.6. To address the concern that it is general exposure to white persons, rather than the aid experience itself driving differential giving, we also continue to control for the white foreigner exposure variables. Column (4) of

Table 4.7: Aid exposure

	(1) Total giving	(2) Total giving	(3) Total giving	(4) Total giving	(5) Total giving	(6) Total giving	(7) Total giving
White-man	751.564**	2241.549***	794.805**	561.844*	1249.225	2040.937	1263.419
	(319.051)	(645.539)	(332.336)	(283.814)	(1473.148)	(1341.575)	(1462.317)
Years NGO-owned facilities x white-man	-20.157**				-21.174**		
	(9.540)				(9.313)		
Years NGO-owned facilities	6.150				5.148		
	(6.971)				(5.957)		
NGO aid x white-man		-1830.047**				-2279.308***	
		(695.979)				(846.640)	
NGO aid		324.747				646.819	
		(481.087)				(551.642)	
Years NGO activity x white-man			-22.357**				-22.152**
			(9.757)				(9.582)
Years NGO activity			6.057				5.591
			(6.673)				(6.054)
Individual controls	No	No	No	No	Yes	Yes	Yes
Village controls	No	No	No	No	Yes	Yes	Yes
Observations	653	653	653	634	634	634	634

Note: See Table 4.5 for notes and text for control variables.

Table 4.7 shows the robustness of the average treatment effect to the subset of observations for which all control variables are defined. Columns (5)-(7) show that the aid interaction terms remain negative and significant with the full set of individual and village-level controls.

This robust negative effect of the aid interactions is consistent with the idea that players from villages with greater aid exposure are engaged in "need-signalling"—they give relatively *less* in response to the treatment in order to signal that they are not in a position to give away resources and thus in need of development assistance. In essence, these individuals have come to associate white persons with the distribution of resources, and thus, anticipate that they will receive more money in the future if they demonstrate they are poor by giving less now. It is worth noting that aid-exposed communities, on average, still give more in treatment versus control areas. Thus, the overall pattern of results is consistent with the idea that 'experimenter demand effects' continue to influence participant giving, but the incentive to be strategic under need-signalling lowers the extent of differential giving in villages with more aid experience.

To what extent can we establish that aid-exposed players consider the potential implications of their game allocations on the likelihood of receiving future aid? If players are engaging in need-signalling, then they should be more inclined to believe that the purpose of the games is to test them for aid, when the white man is present. To examine this prediction, we next turn to data on perceived beliefs regarding the purpose of the games.

First, we examine overall respondent choice across treatment and control communities (not disaggregated by aid exposure). Column (1) of Table 6 uses a simple logit model where the dependent variable equals one if the respondent chooses 'Aid test' and zero otherwise. We find a significant treatment effect of

0.439, equivalent to 56.1% lower odds of choosing this option in the presence of the white researcher. However, since this logit specification groups together the money and research options, we next employ a multinomial logit model, using a categorical dependent variable of participant belief that includes all three choices. Columns (2)-(4) show the relative coefficients for each option pair: 'Aid test' over 'Research', 'Aid test' over 'Give money' and 'Give money' over 'Research'. The results are consistent with those in the first column—in the presence of the white foreigner, the relative risk ratio of choosing 'Aid test' is 54.5% lower than choosing 'Give money', and 60.8% lower than choosing 'Research'.[12] However, the treatment has no significant effect in the choice between 'Give money' and 'Research'. Thus, in the presence of the white foreigner, players were more likely to believe the stated purpose of the team's visit—which was to find out about the community—or to perceive what the team actually did—which was to give out money—as their actual intent. In contrast to perceiving an underlying agenda that the team was there to test them for aid worthiness.

However, we observe substantial heterogeneity on the belief outcomes based on past aid exposure. Table 7 introduces interaction terms between the white-man treatment and years of aid with NGO-owned facilities. The results indicate that, while communities with no exposure are less likely to choose the aid-test option, this effect is countered with additional years of aid experience. Column (1) presents the logit on 'Aid test'. The coefficient on the white-man term implies that people living in communities with no aid exposure have a 72.9% lower odds of believing that the purpose of the game is to test people for aid, in the presence of the white foreigner. However, each additional year of aid erodes this effect by 5.2%. Thus, communities that have received more than

[12]The relative risk ratio in the multinomial logit specification is analogous to the odds ratio in the logit specification.

Table 6. Participant beliefs

	Logit	Multinomial Logit		
	(1) Aid Test	(2) Aid Test over Give Money	(3) Aid Test over Research	(4) Give Money over Research
White-man	0.439**	0.455**	0.392**	0.862
	(0.156)	(0.161)	(0.155)	(0.159)
Observations	664	664	664	664

Note: Column 1 displays the odds ratio from a logit regression on the "Aid Test" indicator, while columns 2-4 display the relative risk ratios for all pairwise combinations of choices from a multinomial logit regression on the categorical variable of participant beliefs. See Table 3 for additional notes.

14 years of aid (representing the top 20% of the aid distribution), associate the white man positively with the aid test option.

Columns (2)-(4) indicate similar results with the equivalent multinomial logit specification: each year of aid exposure increases the relative risk of choosing 'Aid test' over 'Give money' by 5.2%, and over 'Research' by 5.5%, in treatment versus control areas. Again, the associated relative risk of choosing the research versus money options are not significantly different. Columns (5)-(8) shows robustness to the addition of our full set of individual and village level control interactions.

Table 8 shows the same pattern of results with two alternative measures of aid exposure (the NGO aid indicator and Years of aid with NGO activity). These results reiterate that communities receiving aid over a longer period were differentially more inclined to believe that the research team including a white foreigner was there to test the community for aid.

These results on participant beliefs provide further evidence for the 'need-signalling' channel: in communities with more aid exposure, players were more likely to believe that the purpose of the game was to test the community for aid in the presence of the white researcher. This is consistent with the idea that past experience with development assistance inclines players to adapt a more strategic approach of giving less in the game, with the aim of securing more resources in the future.

The evidence on beliefs also help us rule out an alternative interpretation of the results, that respondents give more in response to the white man treatment to signal they are altruistic and thus more deserving of aid. Under this "altruism-signalling" account, the less aid-exposed participants display differential levels of generosity in the hopes of securing future aid from the white foreigner. However, if this account held, then the less aid-exposed participants

Table 7. Aid exposure and participant beliefs

	Logit	Multinomial Logit			Logit	Multinomial Logit		
	(1) Aid Test	(2) Aid Test over Give Money	(3) Aid Test over Research	(4) Give Money over Research	(5) Aid Test	(6) Aid Test over Give Money	(7) Aid Test over Research	(8) Give Money over Research
White-man	0.271***	0.287***	0.224***	0.782	1.328	1.084	2.653	2.447
	(0.099)	(0.106)	(0.089)	(0.173)	(2.124)	(1.705)	(5.374)	(3.327)
Years NGO-owned facilities x white-man	1.052**	1.052**	1.055**	1.003	1.059***	1.058***	1.062***	1.004
	(0.022)	(0.022)	(0.024)	(0.005)	(0.021)	(0.020)	(0.023)	(0.006)
Years NGO-owned facilities	0.965***	0.967***	0.960***	0.993**	0.968***	0.970***	0.962***	0.992**
	(0.012)	(0.012)	(0.014)	(0.004)	(0.012)	(0.011)	(0.013)	(0.004)
Individual-level controls	N	N	N	N	Y	Y	Y	Y
Village-level controls	N	N	N	N	Y	Y	Y	Y
Observations	664	664	664	664	645	645	645	645

Note: See Table 6 for additional notes and text for controls.

Table 8. Alternative measures of aid exposure

	Logit	Multinomial Logit			Logit	Multinomial Logit		
	(1) Aid Test	(2) Aid Test over Give Money	(3) Aid Test over Research	(4) Give Money over Research	(5) Aid Test	(6) Aid Test over Give Money	(7) Aid Test over Research	(8) Give Money over Research
White-man	0.865	0.810	1.025	1.265	1.291	1.055	2.510	2.379
	(1.351)	(1.260)	(2.045)	(1.740)	(2.105)	(1.694)	(5.095)	(3.204)
NGO aid x white-man	11.410***	8.124***	39.525***	4.865***				
	(6.792)	(4.987)	(30.507)	(2.062)				
NGO aid	0.425**	0.469*	0.201***	0.428***				
	(0.185)	(0.203)	(0.113)	(0.114)				
Years NGO activity x white-man					1.064***	1.063***	1.066***	1.004
					(0.020)	(0.019)	(0.023)	(0.006)
Years NGO activity					0.969***	0.970***	0.963***	0.993
					(0.011)	(0.011)	(0.013)	(0.004)
Individual-level controls	Y	Y	Y	Y	Y	Y	Y	Y
Village-level controls	Y	Y	Y	Y	Y	Y	Y	Y
Observations	645	645	645	645	645	645	645	645

Note: See Table 6 for additional notes and text for controls.

should also be differentially inclined to believe that the purpose of the games is to test them for aid. Instead, we find precisely the opposite result.

4.6 Conclusion

This paper has examined whether researcher identity affects measured generosity, using a field experiment which varied the presence of a white foreigner across 60 behavioral games in rural Sierra Leone. We found this simple change in team composition altered measured generosity substantially, boosting total giving by 19 percent in the dictator games.

We interpret these findings as evidence that the race and nationality of the researcher plays an important role in inducing experimenter demand effects: players display greater generosity because they perceive this is what the experimenter desires, and cater differentially to this perceived desire in the presence of a white foreigner.

By linking our games to household and village level survey data, we document two important sources of heterogeneity in the treatment effect, which help shed light on the mechanisms through which these behavioral effects are transmitted. First, we find that the white-man treatment effect arises differentially among individuals who do not come from customary authority households. This suggests that relative power between player and experimenter plays a role in transmitting the treatment effect, since these individuals have a relatively lower social status and are plausibly more susceptible to perceived power differentials. Second, we find that the treatment effect is larger in villages that have had relatively little aid experience. In these communities, players are also more likely to take the stated purpose of the games at face value. Together, these results indicate that past exposure to development aid shapes behavioral

responses in the presence of white foreigners, and that the aid exposed are more likely to play strategically.

Our study holds direct and important implications for how we interpret current measures of generosity, target aid, and evaluate aid effectiveness in the developing world. First, it shows that measures of generosity may be upward biased relative to underlying other-regarding preferences, depending on who is carrying out the measurement. In addition, if there is heterogeneity in how players respond to foreign researchers, this makes it challenging to interpret variation in cross-national measures of generosity. For example, Henrich et al. (2006) find large differences in measured generosity across developing countries and attribute this to differences in cultural practices. However, if communities vary in their response to foreigners based on factors such as previous exposure to aid, then the results may be driven in part by different responses to foreigners rather than cultural differences *per se*.

Our results also hold implications for the conduct of aid targeting exercises. Communities in aid-dependent countries such as Sierra Leone are subject to frequent 'needs assessments' and other policy-specific data collection exercises that are meant to improve aid-targeting and respond to community needs. Two popular approaches toward aid targeting include proxy-means tests, which identify the poor based on outsiders' measurement of the asset and income position of households, and community-based targeting where needs assessments are left to the community (Alatas et al., 2012). If communities that have received aid in the past act strategically to signal need and secure more aid in the future, then targeting based on proxy-means tests may not work as well as intended and could favor those who have already received aid previously. As such, community-based targeting may be a more effective way of identifying the poor.

In addition, there are a number of implications for studies seeking to evaluate aid interventions. If communities receiving aid treatments respond strategically to signal need, then we may underestimate the impact of aid on economic outcomes. Aid evaluations increasingly use behavioral games and experiments to circumvent challenges associated with subjective responses and generate more objective measures of economic and social capital outcomes. However, these approaches may also underestimate the true aid of impact of aid if players are strategically signaling need during games conducted in intervention communities. This is important in light of the fact that a number of recent studies have found insignificant effects of CDD programs on social capital outcomes (Avdeenko and Gilligan, 2012; Casey et al., 2012; Humphreys et al., 2012).

Finally, if behavioral games are used for evaluation purposes, then imbalance in the racial and national composition of evaluators across treatment and control areas could generate a different type of bias. For example, if the foreign NGO implementing the program takes a greater interest in observing behavioral games in program areas, then their mere presence could lead to the spurious conclusion that the aid itself has engineered meaningful increases in generosity or other-regarding preferences. Overall, by demonstrating how the presence of a foreign researcher can affect measured altruism, our analysis points to the import of considering researcher identity broadly in measuring and interpreting behavioral outcomes.

Chapter 5

The Pitfalls of Power Sharing: A Game of Repeated Bargaining under Asymmetric Information

Abstract

I use a model of repeated bargaining with one-sided asymmetric information to investigate the difficulties of reaching and sustaining power sharing agreement. A breakdown of agreement is interpreted as a resumption to war. I find, contrary to previous studies, that asymmetric information can explain the persistence of conflict. The rebel group anticipates a higher future offer if he fights now, and as a result is more willing to fight. Since fighting has become a noisy signal, asymmetric information is resolved more slowly, leading to a persistence of conflict. Furthermore, asymmetric information can lead to a breakdown of a power sharing agreement after it has been reached, provided each group maintains its own separate army during peace. Any sustainable power sharing deal therefore needs to account for the integration of armies, to ensure binding agreements. Finally, when the probability of stalemate is high, social welfare can actually be improved by prolonging the period of fighting between bargaining, since the rebel group cares less about his reputation of being strong, if future bargaining opportunities are distant and unlikely.

5.1 Introduction

Why are lasting peace agreements to civil wars so hard to reach? Civil wars are common and persistent (Blattman and Miguel, 2010). A third of all nations have experienced civil war in the past; and two thirds of these wars lasted for more than 10 years. Given the large destructive consequences of civil wars (Collier, 1999), it is unsurprising that a large priority of the international community is to find negotiated peace agreements to ongoing conflicts.

Power sharing, the allocation of government positions between the most powerful factions in a country, is often seen as a solution to protracted civil wars (Lijphart, 1991). It provides an alternative to the high stakes 'winner-takes-all' elections. In countries with weak institutional constraints on power, the winner cannot credibly commit to share the rents of office with the loser (Besley and Persson, 2008). With such high stakes, political players often resort to violence to gain or maintain political power—the gamble of war is preferred when the alternative is nothing. For example, in the elections following the 1992 Bicesse Peace Accord in Angola, peace lasted only as long as the votes were being counted. As soon as the leader of the rebel group, Savimbi, realised his party would lose, he returned to violence as strategy for gaining power (Spears, 2000). Power sharing could in principle overcome this commitment problem, since both parties are assured a minimum level of access to government resources regardless the outcome of elections.[1]

However, power sharing often fails in practice: "Power sharing agreements are difficult to arrive at and even when implemented, such agreements rarely stand the test of time" (Spears, 2000). Licklider (1995) finds that only 15% of civil wars end in negotiated settlement; and half of negotiated settlements

[1]Some form of de facto power sharing is, in fact, seen in many African autocracies, where top ministerial positions are allocated to members of competing factions in the country (Francois et al., 2012).

have recourse to war. For example, compare the divergent peace processes in Mozambique and Angola, two countries with striking historical similarities. Both Mozambique and Angola are former Portuguese colonies that fought protracted civil wars since independence, sustained by US and Soviet interests.[2] For both, circumstances became fortuitous for a power sharing deal with the withdrawal of foreign financing of the war after the end of the Cold War. However, in Angola neither party was willing to share power and both preferred a 'winner-takes-all' election. At the same time a lasting peace deal was signed in Mozambique, based on "soft guarantees" of power sharing (Brown and Zahar, 2008)). Why was power sharing possible in Mozambique and not Angola, despite their strong historical similarities?

As another example, compare South Africa with Sudan, two countries with a history of ethnic and racial conflict. Sudan fought two long civil wars. The 1972 peace deal in Sudan, based on an agreement of constitutional federalism,[3] was regarded as "a rare example of negotiated end to a civil war" (Meredith, 2005), but did not last. Eleven years later, the same president who was instrumental in signing the peace agreement was responsible for terminating it. In contrast, South Africa was able to form a stable power sharing agreement in 1993 (Spears, 2002).

The purpose of this paper is to answer these two related questions: Why are power sharing agreements so hard to reach; and what contributes to the fragility of power sharing agreements, once they are reached?

I address both questions with a simple model of repeated bargaining with one-sided asymmetric information. A rebel group (R) and the incumbent gov-

[2]In both cases the incumbent was a Soviet backed Marxist government and the rebel group was funded by the USA

[3]The agreement provided for a sharing of oil revenues and regional autonomy and self rule. Power sharing therefore operated at a geographic level, rather than a sharing of cabinet ministers within government, as the case on Mozambique and South Africa.

ernment (G) fight for political power, in order to gain control over the resources of the economy. G does not know R's true cost to fighting, but holds (commonly known) prior beliefs over a distribution of possible costs. Before fighting there is an opportunity to bargain: G makes an offer to R to share power, which guarantees a share of the country's resources. R decides to accept or reject the offer and G updates his believes based on this. If R accepts the first offer, G has the possibility to revise her first offer in the next period. If an agreement is not reached there is a period of fighting: each party faces a commonly known probability of winning and there is a probability that neither party wins. If neither party wins, G has an opportunity to make another offer. If R rejects the second offer, fighting continues until one group emerges victorious.

Bargaining models are commonly applied in 'rationalist' explanations for war (Powell, 2002), since two rational actors should prefer reaching a peaceful agreement before fighting. Understanding war therefore requires an understanding of why mutually beneficial agreements are not reached. One possible reason for a breakdown in agreement is asymmetric information (Fearon, 1995): one group does not know the other's true value and under certain conditions he is willing to 'risk' conflict by making a low offer that a tough opponent would reject.[4] Furthermore, with private information, war is not only an outcome of failed negotiations, it becomes a necessary component of the bargaining process itself (Schelling, 1980:142). Learning takes place through fighting; and fighting takes place in anticipation of future bargaining opportunities. War thus allows armed groups to strengthen their bargaining position, through signalling their strength and resolve . It is for this reason that models of asymmetric information have been analyzed in a repeated setting (Wagner, 2000).

This paper contributes to the theoretical literature on conflict in four im-

[4]The second reason is commitment problems: a party won't agree if it knows the other party will renege on the agreement as soon as fortunes change (Fearon, 2004).

portant ways. First, it is the first paper to apply a model of asymmetric information to civil war, by showing that asymmetric information can explain the persistence of conflict. Civil wars typically last far longer than inter-state wars (Collier et al., 2004) and are less likely to end in negotiated settlement. Scholars such as Powell (2006) believe that information models of war "provide a poor account of prolonged conflict". Fearon (2004)) argues that: "(A)fter a few years of war, fighters on both sides of an insurgency typically develop accurate understandings of the other side's capabilities, tactics, and resolve". Similarly, Walter (2009): "[i]nformation asymmetries do not explain conflicts where combatants do not sign or implement settlements even long after wars have been fought and much information revealed".

I find that conflict can persist over time, because R's incentive to 'act tough' slows down learning. R is more willing to fight, since he anticipates that G will make a better offer the next period, since G knows that only tough types will fight. This increased willingness to fight can be interpreted as an investment in R's reputation of being tough. R's decision to fight is therefore less informative of his true cost of fighting, since even a 'soft type' will fight. As a result, asymmetric information and fighting persist. Previous arguments that asymmetric information cannot explain persistence of conflict fail to appreciate that fighting is not only an opportunity for groups to demonstrate strength, but also an opportunity to deceive.

Second, this is the first paper to look at bargaining and war when agreements are not binding. This is an important contribution (and a serious omission from previous papers) since a defining feature of civil wars is that no group has a monopoly over violence to act as a final enforcer of contracts. I find that, with non-binding agreements, there exists an 'all accept' equilibrium with no conflict in the first period, but a positive probability of a breakdown of agree-

ment in the final period. With no binding agreements, R's reputation cost of acceptance is far more costly, since he expects a downward revision in the next period if he accepts, since G revises her beliefs over R's costs downwards. This strengthens R's bargaining position: he will only accept the offer that even the toughest R would accept, since this is the only scenario where no information is revealed. As a result, G is forced to make the highest offer and agreement is reached in the first period. However, G will always make a lower in the final period, where there are no longer reputation concerns at play. Asymmetric information can thus explain a resumption of conflict after agreement has been reached.

This result provides an explanation for the empirical observation that negotiated settlements are more durable if they were preceded by a long period of fighting (Licklider, 1995). Prolonged conflict means that enough learning has taken place before agreement, so that G won't make a low offer afterwards.

This result further suggests that the existence of two separate militaries is a large cause for the instability of power sharing agreements after they have been reached. Each group is still able to renege on an agreement and resort to war; yet, no group is willing to let go of their military power, since it determines their bargaining position. This could explain the stability of the power sharing agreement in South Africa and Mozambique, compared to Sudan. In Mozambique the peace deal provided for disarmament and integration of the rebel group with the government and army, as well as power sharing at the top level of the army (Brown and Zahar, 2008). In South Africa, there never existed two separate militaries. In Sudan, the Northern and Southern regions always kept their own separate armies.

Third, I show under what conditions conflict is expected to persist. Agreements are less likely to be reached if fighting is less decisive. This is because R

is more willing to 'act tough', if he is more likely to receive a second offer, but he will only receive a second offer if there is a stalemate after the first period of fighting. This leads to a depressing conclusion that agreements are harder to reach when conflicts are expected to last longer. This result could explain why countries with a high resource endowment, such as Angola, struggle to find a negotiated peace settlement. Access to natural resources allows armed groups to sustain themselves (Collier *et al.*, 2004). Not only is fighting expected to last for longer, but armed groups are less willing to accept an agreement, since they can 'hold out' for a higher future offer.

The probability of agreement is also higher if a country is more industrialised. Both parties have more to lose from war, since the potential destruction of capital is higher. This could explain why agreement was more likely to occur in an industrialised country like South Africa, compared to other African countries.

Furthermore, the probability of reaching an agreement increases with the length of fighting between offers. R's reputation concern is weaker when future bargaining opportunities are distant and unlikely, so he is more willing to accept an offer now. This result implies that G faces a commitment problem in making future agreements. G would do better if he could commit in the first period to all future offers, since R would no longer be motivated to fight to get higher future offers. However, G always has an incentive to renegotiate *ex post*, given updated beliefs.

Fourth, I show that net welfare could actually *increase* with a longer period of fighting between offers, but only if the probability of stalemate is very high. Disagreement is more costly, since fighting lasts for longer. However, agreement is more likely, both because fighting is more costly and also because R's reputation concerns are weaker. When the probability of stalemate is high,

then delay between offers will lead to a sharper reduction in R's reputation concerns to fight, leading to a net improvement in welfare.

This leads to a counter-intuitive policy proposal: with lasting conflicts, the international community should intervene to make future agreements harder to reach, rather than easier. Furthermore, the international community should 'favour the incumbent' in creating credible ways to prolong fighting after a breakdown of agreement. The optimal design from a social planner's perspective thus involves a 'good-cop-bad-cop' strategy for the uninformed player, where a dove makes decisions during negotiations, but a hawk makes decisions during fighting. Declaring a 'state of emergency', which delegates all decision-making to the military after a breakdown of agreement, could be such a mechanism.

5.1.1 Related Theoretical Literature

This paper exhibits standard "Coasian Dynamics", shared with with all models of repeated bargaining under asymmetric information (Fudenberg et al., 1985; Grossman and Perry, 1986; Sobel and Takahashi, 1983). Particularly, in all these models the probability of agreement increases over time as the uninformed party makes increasingly generous offers due updated beliefs based on the privately informed agent's decision to reject the previous offer.

However, this model is different to these papers in two important ways. First, a breakdown of agreement leads to a period of fighting rather than a period of waiting. This is important, since the technology of conflict determines both the outside option and the signalling incentives of the privately informed agent: reputation concerns are stronger if the probability of stalemate is higher.

Second, with the exception of (Hart and Tirole, 1988). this is the only bargaining model that looks at non-binding agreements. Similar to Hart and

Tirole (1988), I identify an equilibrium where there is zero probability of a breakdown of agreement if the privately informed agent is sufficiently patient. My model differs from them in two ways. First, in my model the 'all accept' equilibrium holds even in the penultimate period, whereas with Hart and Tirole (1988) it only holds when there is prospect of many future bargaining opportunities. The reason is that bargaining is over political power, rather than one period of exchange. The agreement over political power will determine the size of an indefinite future flow of revenues. The reputation cost to R is therefore extremely high, even in the penultimate period. Second, the existence of the 'all accept' equilibrium depends, not only on the discount factor, but also on the probability of stalemate. In particular, if the probability of stalemate is very low, then the equilibrium only holds for very patient R, since his reputation concerns are weaker if the probability of future bargaining opportunities is lower.

One potential criticism to this paper is the "Coase Conjecture" (Coase, 1972), formalised by Gul and Schonneischein (1983), who show that with one-sided asymmetric information, "delay in agreement can only be explained by the time between offers". As time between offers shrinks, the privately informed agent becomes more patient and is thus more willing to wait for an improved future offer. In the limit, when $\delta \to 1$, he cares as much for the future offer as the present offer. The uninformed agent anticipates this and is forced to make the most favourable offer in the first period. There is no delay in agreement.

The Coase Conjecture is not relevant to this paper, for two reasons. First, since disagreement leads to a period of fighting, it is unlikely that the time between periods will shrink to zero. In fact, the uninformed agent benefits from creating delay. (Admati and Perry, 1987), for example, show that there is always delay in agreement if the uninformed party can endogenously set the

time between offers. Second, concern for future offers depends, not only on the discount factor, but also on the probability of stalemate. This suggests that Coase conjecture would not only hold, if the probability of reaching a stalemate also tends to one as time between offers tends to zero.

The paper proceeds in the following way. Section two introduces the model. Section three presents a simple one period bargaining game. Section four discusses a game of repeated bargaining where agreements are binding. Section five discusses the implications if agreements are not binding. Section six relates the model back to the case study and section seven concludes.

5.2 Model Setup

Two risk neutral agents, the incumbent (G) and a rebel group (R), are fighting to gain political power. Before fighting, G can make an offer to R to share power, which assures a share the future rents of holding office. The total rents of holding office provide a flow of 1 each period. A portion, κ, of resources are returns to capital. R's cost of fighting is private information.

The cost of fighting could be interpreted in two ways. First, it could be interpreted as the psychic costs of fighting, and is thus a measure of the hawkishness or resolve of the rebel leader. A rebel leader with low psychic costs of fighting, places a low weight on the destructive consequences of war and is therefore more willing to fight. Secondly, costs of fighting can also be interpreted as the leader's costs of forming and sustaining an army.

5.2.1 Timing of the game

1. Nature determines the cost of fighting, for R, c_R, which is private information. G's beliefs c_R are uniformly distributed: $c_R \sim U(c_L, c_H)$.

2. G makes an offer $x_1 < 1$ to R.

3. R chooses to accept the offer or not.

   If R accepts the offer, R and G receive a flow of x_1 and $(1 - x_1)$ each period respectively.

   If R does not accept then G and R fight each other. In a period of war R has control over a proportion q of the resources of the economy. War imposes a cost of c_R on R and c_G on G. A portion ϕ of capital is destroyed. G and R wins with probabilities P_G and P_R respectively. If one party wins the game ends and the victor receives a flow of payoffs of one for the rest of time. If neither party wins the game continues. The probability of neither winning is $\alpha = (1 - P_G - P_R)$.

4. G makes another offer, $x_2 < 1$ to R.

5. R chooses whether to accept the second offer or not

   If R accepts the offer, the game ends. R and G respectively receive a flow of x_1 and $(1 - x_1)$ each period thereafter.

   If R does not accept, both groups fight. Both players face the same probability of winning, returns to fighting and costs of fighting as before. War continues indefinitely until one party emerge victorious.

Figure 5.1 shows the model in extensive form.

I make the additional assumption that G's beliefs over R's cost of fighting is sufficiently dispersed, relative to G's cost of fighting:

$$c_H - c_L > c_G + c_H$$

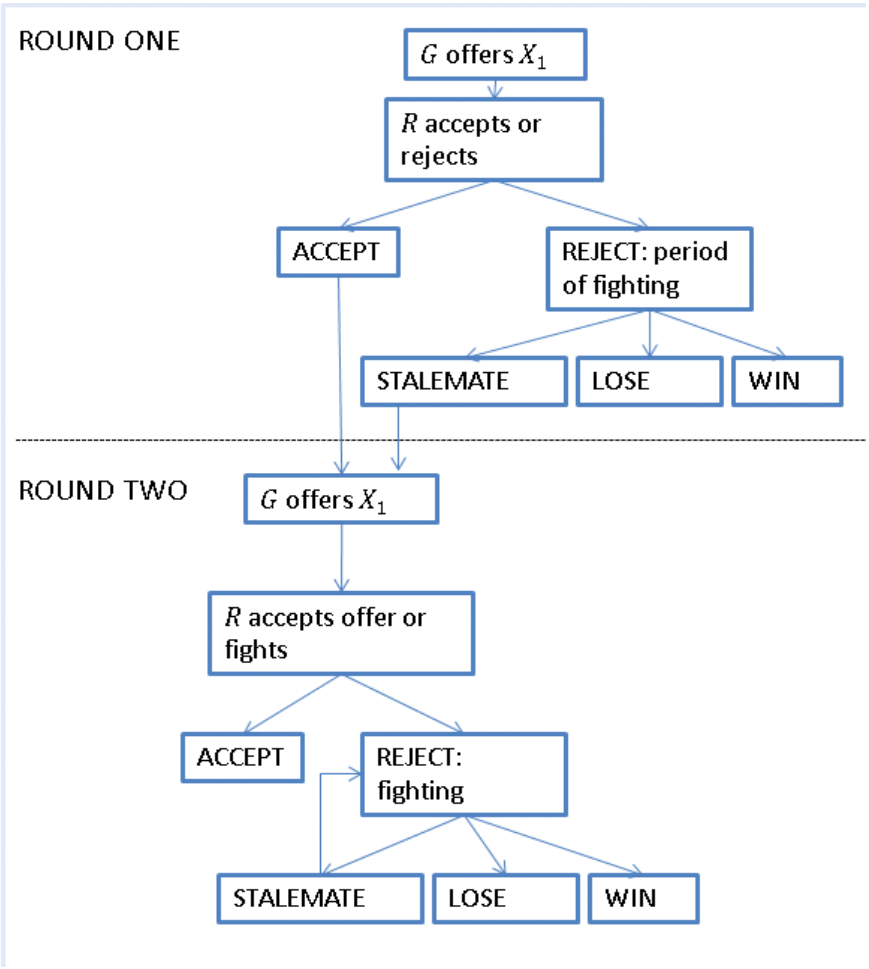


Figure 5.1: Timing of the Game

5.2.2 Some definitions

It is useful to define the flow variables in terms of their net present discounted values. B and X are the NPV's of the total rents from office and an accepted offer respectively:

$$B = (1-\delta)^{-1} \text{ and } X_t = (1-\delta)^{-1} x_t$$

The values to R and G to fighting are respectively:

$$V_R = (1-\alpha\delta)^{-1} (P_R \delta B + q(1-\phi\kappa) - c_R)$$

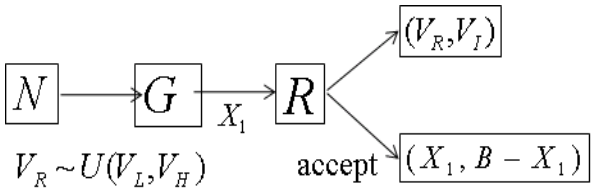


Figure 5.2: The *one-offer-game* in extensive form

$$V_G = (1 - \alpha\delta)^{-1} (P_G \delta B + (1 - q)(1 - \phi\kappa) - c_G)$$

The expected value to fighting one period can thus also be written as $V_R(1 - \alpha\delta)$ and $V_G(1 - \alpha\delta)$. G's prior beliefs over the value of fighting to R can now be defined as:

$$V_R \sim U(V_L, V_H)$$

where V_L is the type with the highest cost of fighting, c_H, and therefore the lowest value to fighting. I further define G's maximum value to an accepted agreement: $v = B - V_G$.

5.2.3 Ability to renegotiate

The full model assumes that G can renegotiate on two dimensions—he can revise both an accepted and rejected offer. In order to separate different mechanisms, it is informative to discuss simpler versions of the full model, based on G's ability to renegotiate. In section three, the *one-offer-game*, G can only make one offer. In section four, the *repeated-bargaining-game*, G can make second offer if the first offer was rejected. In section five, with no *non-binding agreements*, G can revise both an accepted and rejected offer.

5.3 One offer Game

In the benchmark game, G makes one offer, X_1. If R accepts the offer, the game ends. If R rejects the offer, conflict continues until one party is victorious. Figure 5.2 shows the game in extensive form, with payoffs defined in terms of their net present value. The "one offer" game demonstrates how asymmetric information can preclude a power sharing agreement. The players' outside option are their expected values to fighting. R will only accept the offer if it is larger than or equal to his value to fighting: $X_1 \geq V_R$. In turn, G will only make this offer if he can do no better from fighting, $v \leq X_1$.

Under complete information, an agreement will always be reached, since:

$$B - V_R - V_G < 0 \tag{5.1}$$

$(B - V_R - V_G)$ is the bargaining range: the set of mutually beneficial bargaining outcomes that both parties prefer to war.

With the introduction of asymmetric information, G does not know R's true value to fighting. The probability that an offer is accepted is the proportion of types that have a value to fighting less than X_1:

$$\Pr(V_R \leq X_1) = \frac{X_1 - V_L}{V_H - V_L}$$

If the offer is accepted, G receives $B - X_1$. If the offer is rejected, G fights and receives a payoff of V_G . G's expected payoff from an offer, X_1, is therefore:

$$U(X_1) = \Pr(V_R \leq X_1)(v - X_1) + V_G$$

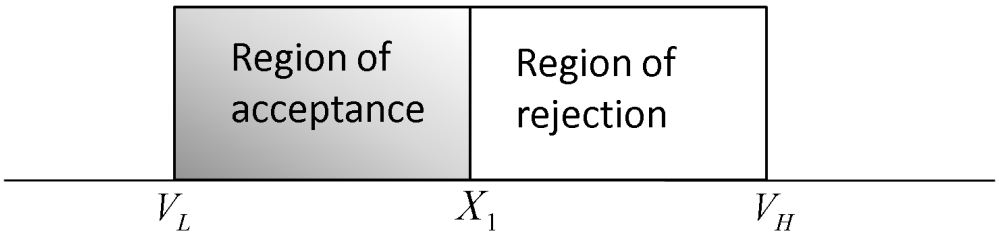


Figure 5.3: Probability of acceptance

and G's optimal offer is:

$$X_1 = \arg\max_{X_1} U(X_1) = \frac{1}{2}(v + V_L) \tag{5.2}$$

Equation 5.2 shows that G's optimal offer is a function of the size of the pie and the respective values to fighting. If G's value to fighting increases, he is willing to make a lower offer, since fighting is no longer that costly. If R's value to fighting increases, G needs to make a higher offer, since he anticipates that R will be less likely to accept.

Proposition 5 *Under asymmetric information, there is a positive probability of a breakdown of agreement*

Proof. Probability of acceptance is:

$$\Pr(V_R \leq X_1) = \frac{1}{2}\left(\frac{c_H + c_L}{c_H - c_L}\right) < 1$$

■

The *one-offer-game* demonstrates how asymmetric information can lead to a positive probability of war. G faces a trade-off between a more favourable agreement (lower offer) and a higher probability of reaching an agreement. The larger share he demands, the less likely that R will accept. Given assumption one, G is willing to risk a breakdown of agreement, in order to end up with a larger share of the pie.

However, the "one offer" game cannot explain the persistence of civil wars, since a large component of the asymmetric information is resolved after one period of fighting. G knows that only an R with a low value to fighting would accept the first offer. If R rejected the offer, G's updated beliefs of the distribution of types has therefore shifted up. Learning takes place through fighting. The probability of war would decrease exponentially.

A rapid decrease in the probability of reaching an agreement is not observed in the real world. Civil wars last for long (Fearon, 2004). It is for this reason that many authors (Fearon, 2004; Powell, 2006; Walter, 2009) dismiss asymmetric information as a credible explanation for prolonged conflict.

5.4 Repeated bargaining with binding agreements

In this section I present the "repeated bargaining" game, where G can make a second offer after a breakdown of agreement. The repeated bargaining model demonstrates how asymmetric information can lead to the persistence of conflict. Key comparative static results are that agreements are harder to reach when the probability of stalemate is higher or the time between bargaining rounds shorter.

The equilibrium concept used for a dynamic game of asymmetric information is Perfect Bayesian equilibrium (PBE). Solving the model requires using the logic of backward induction, but restricting oneself to beliefs of G that are consistent with optimal play of R. I therefore start at the continuation game: G's optimal offer in the second round of bargaining.

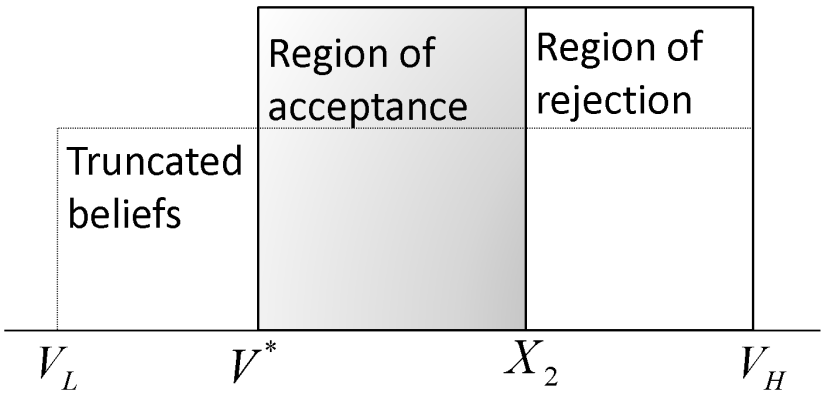


Figure 5.4: I's posterior beliefs are revised upwards

5.4.1 The continuation game

The continuation game is equivalent to the one offer game, expect that G's beliefs are updated. At the second round of bargaining, G faces a truncated distribution of types, since he knows that only R with a higher value to fighting would have rejected his first offer. G's updated beliefs are therefore: $V_R \sim U(V^*, V_H)$, where V is the type which is indifferent between fighting or not fighting in the first period. G chooses X_2 to maximise the following objective function:

$$U(X_2) = \Pr(V_R \leq X_2 | V_R > V^*)(v - X_2) + V_G$$

Which yields:

$$\hat{X}_2 = \frac{1}{2}(v + V^*) \tag{5.3}$$

Note that the optimal second offer is higher than in the "one offer" game. This is because G knows that only "tough" types would choose to fight, and he is therefore more willing to make a higher offer the next period.

5.4.2 R's first period strategy

I turn next to R's decision to fight or accept the first offer. Since R's strategy is binary and monotonically increasing in V_R, it can be uniquely defined by one parameter value, V^* : the value to fighting of the rebel group that is indifferent between fighting and accepting in the first period.[5] If $V_R > V^*$, then R rejects the first offer. If $V_R \leq V^*$, then R accepts the first offer.

Substituting the inequality from equation 5.1 into equation 5.3, gives us: $V^* < X_2$: The V^* type's returns to fighting is always less than the second offer, since G's optimal second offer shifts up according to the new lowest value to fighting, V^*. The V^* type would therefore always accept in the second period. As a result, V^* is defined by the following indifference condition:

$$V^*(1 - \alpha\delta) + X_2 = X_1 \tag{5.4}$$

Furthermore, R knows what his second period offer will be, conditional on G's beliefs. In turn, PBE requires that G's beliefs are consistent with R's optimal play. We can therefore substitute equation 5.3 into equation 5.4 and uniquely define V^* in terms of X_1 :

$$V^*(X_1) = \frac{2X_1 - \alpha\delta v}{(2 - \alpha\delta)} \tag{5.5}$$

Equation 5.5 is the same as equation 5.4 except that R now anticipates how his strategy will influence his future payoffs through changing of G's beliefs. R is more willing to fight, but cannot be too willing to fight, since his second offer is lower if G knows that even soft types will fight. The only fixed point between optimal strategy of R and consistent beliefs of G is equation 5.5.

Note that $V^* < X_1$. There is thus a range of types, $V_R \in (V^*, X_1)$, that

[5] During the rest of the paper I refer interchangeable between "V^* type" and "R with value to fighting of, V^*"

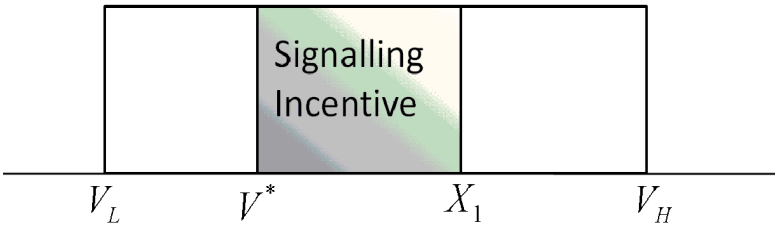


Figure 5.5: R's reputation concern: the range of types for which R would reject an offer, even if it is more than his value to fighting.

would prefer to fight even though their expected returns to fighting are smaller than the offer. This is in contrast to the *one-offer-game* where R would accept whenever $V_R < X_1$. This range of types represents R's reputation concern, or signalling incentive: R fights to signal he has a low cost of fighting, in anticipation of a higher future.

Finally, since we know how G's beliefs change with his first offer, we can now also define his second offer as a function of his first offer. Substituting equation 5.5 into equation 5.3:

$$X_2(X_1) = \frac{(1-\alpha\delta)\,v + X_1}{(2-\alpha\delta)} \tag{5.6}$$

5.4.3 G's optimal first offer

In the first period, G chooses X_1 to maximise his payoffs in both periods. He can anticipate from equations 5.5 and 5.6 how R will respond to his first offer and what his expected payoffs in the continuation game would be, given X_1.

Solving for X_1, yields the following lemma:

Lemma 2 *(Equilibrium Characterisation) Equilibrium exists and is unique. G makes a first offer of $\hat{X}_1$ and a higher second offer of $\hat{X}_2$. R only accepts the*

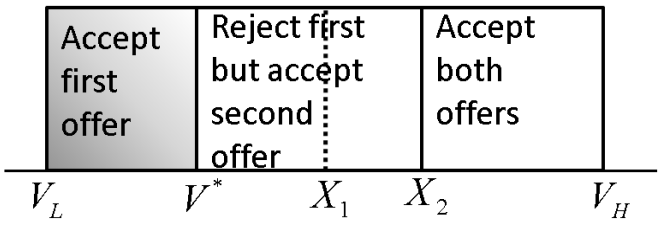


Figure 5.6: Optimal first and second offer

first offer if $V_R < \hat{V}$ *and accepts the second offer if* $V_R < \hat{X}_2$*, where*

$$\hat{X}_1 = \frac{1}{2}(v + V_L) + \alpha\delta(1 - \alpha\delta)\Phi(v - V_L)$$

$$\hat{V} = \frac{1}{2}(v + V_L) + \alpha\delta\Phi\,(v - V_L)$$

$$\hat{X}_2 = \frac{1}{2}(v + V_L) + 2(1 - \alpha\delta)(v - V_L)$$

$$\Phi = (8 - 6\alpha\delta)^{-1}$$

The probabilities of R accepting an offer by the first and second period respectively are:

$$\Pr\left(V \leq \hat{X}_2\right) = 4\,(1 - \alpha\delta)\,\Phi\frac{c_H + c_G}{c_H - c_G} \tag{5.7}$$

$$\Pr\left(V \leq \hat{X}_2\right) = (6 - 5\alpha\delta)\,\Phi\frac{c_H + c_G}{c_H - c_G} \tag{5.8}$$

Proof. Proof. In appendix ■

5.4.4 Discussion

In equilibrium, G makes increasing offers over time, as he revises upwards R's true value to fighting. The probability of reaching an agreement increases over time, since since there is a range of types of R that would not accept the first offer, but would accept the next offer (the shaded area in figure 5.5).[6]

[6]This is a the "skimming property" shared by all repeated bargaining games with one-sided private information, as proved by Fudenberg Levin and Tirole (1985).

Comparison with the single offer bargaining game provides the following insights.

Proposition 6 *Compared to the one offer game:*

1. *The probability of reaching an agreement in the first period is lower.*

2. *Information is resolved more slowly*

Proof. $X_1 > \hat{V}$ ■

The probability of war in the first period is higher compared to the case where there is no opportunity for future bargaining. R anticipates that if he fights, G will believe he is strong and thus make a better offer the next period. R is therefore more willing to fight, due to a reputation concern to "act tough". G anticipates this and makes a higher offer in the first period, but not by enough to offset R's increased willingness to fight.

Furthermore, since even weak types are willing to fight in the first period, the decision to fight is less informative of R's type. With future opportunities for bargaining, R's decision to fight becomes a noisy signal, since even R with a low value to fighting would prefer to fight. Information is thus resolved more slowly as would be the case where there is no opportunity for future bargaining. These results imply that asymmetric information could also explain a continuation of conflict. The probability of reaching an agreement over time depends on the speed of learning; yet learning slows down due to R's incentives to "act tough" in anticipation of future offers.

Proposition 7 *The probability of reaching an agreement in either period is decreasing in both α and δ*

Proof. By inspection of equations 5.7 and 5.8 ■

The probability of reaching an agreement in either period decreases with both α and δ, due to a strengthening of R's reputation concerns. A more patient R cares more about the future and is thus more willing to invest in his reputation by fighting now in order to secure a higher offer in the future. Similarly, if probability of stalemate is higher, then R cares more about future offers, since he is more likely to receive a second offer. This result leads to a depressing conclusion that agreements are harder to reach when war is expected to last longer. If war is less decisive, agreements are more difficult, because agents expect future bargaining opportunities in the process of war.

My model thus provides an explanation for the observation that civil wars last longer when rebel groups have access to a source finance (Collier *et al*, 2004): not only does a source of financing lead to a more durable rebel group, but it makes agreements less likely, since the reputation concerns of the rebel group are stronger.

Proposition 8 *Total welfare and G's welfare can be increased by increasing the number of periods between bargaining rounds, provided that:*

$$\alpha\delta > \frac{2}{3}$$

Proof. In appendix ■

Due to reputation concerns in repeated bargaining, agreement is more likely if the time between bargaining rounds increase. This is because multiple periods between bargaining rounds decrease the effective discount factor and probability of reaching a stalemate by the next bargaining round. R's reputation concern is therefore weaker, since future agreements are distant and less likely. In addition, G benefits from delay between bargaining rounds, since R's bar-

gaining power is reduced: G can both make a lower first period offer and faces a lower probability of rejection.

Furthermore, when the probability of stalemate, or discount factor, is high then the socially optimal length of fighting between offers is long. It is true that a longer period of conflict makes disagreement more costly. However, delay between bargaining rounds also means R has weaker reputation concerns. The net benefit of increasing the time between bargaining is therefore larger when R's has strong reputation concerns: when $\alpha\delta$ is high.

The policy implications from these results are somewhat counter-intuitive. The interests of a social planner (who wants to decrease the probability of war) and the uninformed party (who also wants a higher share of the pie) are aligned. When it is unlikely that any party will emerge victorious, then the international community could improve welfare if they favour the incumbent, in helping him create credible delays between bargaining rounds.

One way of creating credible delays is to delegate decision-making to a hawk after a breakdown of agreement. The optimal design is therefore involves a "good-cop-bad-cop", where a dove makes decisions during bargaining, but a hawk makes decisions during fighting. Declaring a state of emergency when agreement breaks down could be such a mechanism, provided that the military is more willing to fight than the executive.

5.5 Repeated bargaining with no binding agreements

This section looks at the case of non-binding agreements where G can revise both a rejected and accepted offer. I show that, if R is sufficiently patient, there exists an equilibrium with no war in the first period, but a positive probability

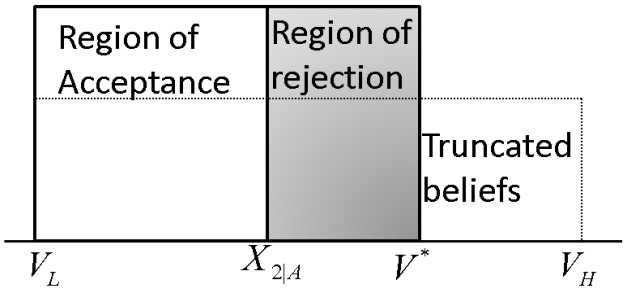


Figure 5.7: I's posterior beliefs are revised downwards

of fighting in the final period.

5.5.1 The continuation games

The only difference to the previous section is that G can now also revise an offer after it was accepted. I again define V^* as the cost of fighting at which R is indifference between rejecting or accepting the first offer. As before, if R accepts then G's posterior beliefs are $V_R \sim U\left(V^*, V_H\right)$; and the optimal second offer conditional on R rejecting the first offer is: $\hat{X}_R = \left(v + V^*\right)/2$.

If R accepts in the first period, then G's beliefs are revised downwards to $V_R \tilde{} U\left(V_L, V^*\right)$. There is now possibility of a corner solution, since G will never offer more than V^*, the minimum offer to assure 100% probability of acceptance. G chooses X_A to maximise:

$$U(X_A) = \Pr(V_R \leq X_A | V_R < V^*)(v - X_A) + V_G \text{ subject to } X_A \leq V^*$$

which yields

$$\hat{X}_A = \min\left\{\left(v + V_L\right)/2, V^*\right\}$$

5.5.2 R's decision to accept or not

R's decision to accept or reject the first offer is uniquely identified by V^*, R's cost of fighting which will make him indifferent between accepting or rejecting an offer:

$$X_1(1-\delta) + \delta V^* = V^*(1-\alpha\delta) + \alpha\delta X_R \tag{5.9}$$

The right hand side of equation 5.9 is the payoff to rejecting the offer, which is the same as in the case of binding agreements in the previous section. The left hand side of equation 15 is the payoff to accepting an offer. $X_1\,(1-\delta)$ indicates the one period payoff from accepting the offer; and the V^* type will always receive his payoff to fighting in the next period. This is because, G will never offer more than V^*, but R will fight and receive V^* if he receives a lower offer.

All results can be derived from equation 5.9:

Lemma 3 *When $\delta \geq \delta^*$, there exists an "all accept" equilibrium where G makes an offer, $X_1 = V_H$, in the first period that all types of R would accept, where:*

$$\delta^* = \frac{1}{1+\alpha\kappa} \text{ and } \kappa = \frac{1}{2}\frac{v - V_L}{V_H - V_L} < 1 \tag{5.10}$$

Proof. Consider the following equilibrium:

- G believes that $V_R = V_L$ if R accepts any offer, $X_1 < V_H$;
- G's beliefs remain unchanged if R accepts $X_1 = V_H$; or if R rejects $X_1 < V_H$.
- R only accepts if $X_1 = V_H$ and G offers $X_1 = V_H$

If R accepts an offer $X_1 < X_H$, he receives no more than: $V_H(1-\delta)+\delta V_R$. If R rejects, there is no updating of information, so he receives $X_R = (v+V_L)/2$ in the second period. He therefore receives no less than $V_R(1-\alpha\delta)+(v+V_L)/2$. When $\delta > (1+\kappa\alpha)^{-1}$, then no R would accept $X_1 < V_H$, since $V_H(1-\delta)+\delta V_L < V_L(1-\alpha\delta)+X_R$, for any $V_R \leq V_L$. G is therefore forced to offer V_H in the first period and all types of R accept. ■

5.5.3 Discussion

Proposition 9 *Compared to the case of binding agreements, there exists the possibility of a breakdown of agreement after it has been reached. With $\delta \geq \delta^*$, all R will accept the first high offer, but R with high cost of fighting will reject the second, lower offer.*

Proof. With the "all accept" equilibrium, there is no updating of information, so: $X_2^* = \frac{1}{2}(v+V_L) < V_H$ ■

When R is sufficiently impatient ($\delta < \delta^*$), the relaxation of the assumption of binding agree ments has no impact on the probability of agreement in each period and G's and R's welfare remain the same as before. R anticipates that G will revise his offer downwards if he accepts and as a result he less willing to accept the offer. G anticipates this and makes a higher offer in the first period. His offer increase by just enough to compensate the lower future reduction and so the probability of acceptance in the first period remains the same. If R accepts the offer, G revises the offer down by just enough to ensure that any type of R that accepted the first offer, would still accept the first offer.

However, when R is sufficiently patient ($\delta \geq \delta^*$), there exists an "all accept" equilibrium with zero probability of conflict in the first period, but positive probability of a breakdown the following period. When R is very patient his reputation cost of accepting any offer is prohibitively high. He will only accept

an offer which won't reveal any information. G is therefore forced to make a high enough offer, $X_1 = V_H$, that all types of R would accept. As a result, there is no disagreement in the first period. Furthermore, with a fixed number of bargaining rounds, the final offer is a binding agreement. In this round, G no longer needs to offer $X_1 = V_H$ because R no longer faces reputation concerns. Since there has been no revelation of information in the past, the final offer is low and there is a high probability of rejection.

Note from equation 5.10 that the patience level, δ^*, beyond which the "all accept" equilibrium exists, depends on the probability of stalemate, α. The equilibrium can occur even for an impatient R if the probability of stalemate is very high.

5.5.3.1 Separate armies and the breakdown of agreement

Agreements can never be binding if each group keeps a separate army. This might explain why power sharing was not sustained in Sudan: The North and South each kept their militaries. In Mozambique, in contrast, the peace deal provided for a disarmament of the rebel army, integration with the government army, and sharing of power at the top level of the armed force (Brown & Zahar, 2008). Power sharing was therefore more stable in Mozambique, since demilitarisation meant that the agreement was binding to both groups. Similarly in South Africa, the resistance group, the African National Congress, never had a military power that could compete with the ruling government.

5.6 Conclusion

Power sharing agreements have strong appeal to the international community: they are low cost interventions that do not violate a country's sovereignty.

Yet, they rarely succeed. I address the question of why lasting power sharing agreements are so hard to reach with a simple model of repeated bargaining under asymmetric information.

I find that asymmetric information can lead to persistent conflict, since reputation concerns slows down learning and fighting is more likely if the problem of asymmetric information is more severe. Furthermore, agreements are harder to reach if fighting is expected to reach a stalemate. A rebel group is more willing to "hold out" for a better future offer if future bargaining rounds are likely.

I further find that power sharing agreements remain unstable if it does not account for the integration of armies. With two sources of military power, the incumbent always has the option to renege on agreements.

Further research is required to look under which conditions groups would be willing to relinquish their armies. This decision depends critically on the expected political power within the newly formed government, since military acts as an insurance to low access to resources whilst in office. This question thus requires a better understanding of the political process: bargaining over the sharing of cabinet seats and integration of the military depends, in turn, on expectations on how the allocation of cabinet seats maps into revenues.

5.7 Appendix—Proofs

Proof. Of Lemma 2 (*equilibrium characterisation*)

$$\hat{X}_1 = \arg\max_{X_1} \bar{U}\left(X_1\right)$$

where

$$\bar{U}(X_1) = \Pr(V_R \leq V^*(X_1)) \cdot (v - X_1) + V_G(1 - \alpha\delta) + \alpha\delta \Pr(V_R > V^*(X_1))\bar{U}_2(X_1)$$

and $\bar{U}_2(X_1)$ is his payoff in the continuation game:

$$\Pr(V_R \leq X_2(X_1) | V_R > V^*(X_1))(v - X_2(X_1)) + V_G$$

and $V^*(X_1)$ and $X_2(X_1)$ are defined by equations 5.5 and 5.6 respectively. ■

Proof. Of proposition 5

The returns to fighting for t periods are

$$V_i(1 - \Delta(t)), i \in (R, G)$$

where I define $\Delta = (\alpha\delta)^t$.

So, equilibrium is defined by:

$$\hat{X}_1(t) = \frac{1}{2}(v + V_L) + \Delta(t)\Phi(t)(1 - \Delta(t))(v - V_L)$$

$$\hat{V}(t) = \frac{1}{2}(v + V_L) - \Delta(t)\Phi(t)(v - V_L)$$

$$\hat{X}_2(t) = \frac{1}{2}(v + V_L) + 2\Phi(t)(1 - \Delta(t))(v - V_L)$$

where

$$\Phi(t) = (8 - 6\Delta(t))^{-1}$$

Total welfare is maximised by minimising the probability of fighing in each period, time discounted and weighted by the length of fighting, which simplifies to:

$$\Delta^* = \arg\min_{\Delta} \hat{V}(t)(1 - \Delta(t)) + \Delta(t)\hat{X}_2(t)$$

With some algebra it is easy to show that the optimal length of time, t, between periods is such that:

$$\Delta(t) = \frac{2}{3}$$

The optimal length of time is therefore for than 1 when $\alpha\delta > \frac{2}{3}$ ■

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