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Veto Players and Civil War Duration

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by

David E. Cunningham

Committee in charge:

Professor Barbara F. Walter, Chair
Professor Clark Gibson
Professor Kristian Skrede Gleditsch
Professor Robert Horwitz
Professor David A. Lake

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The dissertation of David E. Cunningham is approved, and it is acceptable in quality and form for publication on microfilm:

Chair

University of California, San Diego

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VITA

Education

B.A. Political Science, Wake Forest University, 1998

M.S. Conflict Analysis and Resolution, George Mason University, 2001

Ph.D. Political Science, University of California, San Diego, 2006

Honors and Awards

- National Science Foundation Doctoral Dissertation Research Improvement Grant, 2005-06
- American Political Science Association Travel Grant, 2005
- Dean's Social Science Fund Dissertation Travel Grant, 2005
- Rohr Chair in Pacific International Relations Dissertation Travel Grant, 2005
- International Studies Association Travel Grant 2005
- Mark Twain Fellowship, Department of Political Science, UCSD 2001-06

Publications

2006. "Veto Players and Civil War Duration." *American Journal of Political Science*, Vol. 50, No. 4. In press.

2006. "Civil War in Chad." In Derouen, Karl and Uk Heo, Eds., *Civil Wars of the World: Profiles of the Most Intense Internal Conflicts Since World War II*, forthcoming.

Conference Presentations

2006. "Dyadic Interactions and Civil War Duration." With Kristian Gleditsch and Idean Salehyan. Presented at the Polarization and Conflict conference, Nicosia, Cyprus, April 24-28. Also presented at the 47th Annual International Studies Association Convention, San Diego, CA. March 22-25.

2006. "Bargaining and Fighting: The Case of Rwanda." Presented at the 47th Annual International Studies Association Convention, San Diego, CA. March 22-25.

2005. "Veto Players and Civil War Duration." Presented at the 101st Annual Meeting of the American Political Science Association, September 1-4. Also presented at the 38th Annual Meeting of the Peace Science Society, Houston, TX, November 12-14, 2004. Also presented at the Annual Meeting of the Western Political Science Association, Portland, OR, March 11-13, 2004.

2005. "The Effect of Intervention Type on the Duration and Outcome of Civil Wars." Presented at the 46th Annual International Studies Association Convention, Honolulu, Hawaii. March 1-5.

2004. "Contracting Out Security: State-Rebel Group Alliances in Central Africa."
Presented at the Midwest Political Science Association Annual Meeting, Chicago, IL,
April 15-18.

Research and Employment

Research Assistant: International Dimensions of Civil War Project Professor: Kristian Gleditsch	2004
Research Assistant Professor: Branislav L. Slantchev	2003
Research Assistant Professor: William Chandler	2002
Intern: Conflict Prevention and Recovery Program, Fund for Peace Director: Anne V. Russell	2000-2001

Teaching Experience

Teaching Assistant

- PS30: Political Inquiry (Fall 2002, Spring 2003, Winter 2004)
- PS11: Introduction to Comparative Politics (Fall 2003)
- Dimensions of Culture Writing Program (Fall 2004, Winter 2005)

Grader: National Security Strategy, Introduction to International Relations; Government and Politics of Middle East; The German Political System; Political Economy of the Drug Trade; Politics of War Crime Tribunals; Politics in Israel; American Political Parties, Political Science Fiction

Languages

- English (native)
- French (proficient)

ABSTRACT OF THE DISSERTATION

Veto Players and Civil War Duration

by

David E. Cunningham

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Professor Barbara F. Walter, Chair

This dissertation explains why some civil wars are resolved quickly and others are not. I argue that the number of actors involved in a civil war directly affects the ability of all parties to reach a negotiated settlement, and therefore how long the war lasts. This explanation stands in contrast to the existing literature which has traditionally assumed that civil wars are fought between only two actors, the state and one rebel group. The main argument is that conflicts with multiple actors who must approve a settlement (veto players) are longer because there are fewer agreements that are acceptable to all parties, greater information asymmetries make it harder for combatants to determine the extent of concessions they must give up, all actors have incentives to hold out to be the last signer and shifting alliances among parties make the formation of negotiating blocs to overcome these problems difficult.

The dissertation contains two empirical tests of this veto player approach. First, I conduct a quantitative analysis of the effect of number of actors on the duration of all civil wars begun since 1945 using a new dataset containing data on all factions involved in civil war. This analysis reveals a strong correlation between the number of players and civil war duration, and this finding is robust to the inclusion of control variables and to different measurements of the types of players involved in a civil war. Second, I use comparative case studies based on a most similar case design of civil war negotiations in a two actor conflict in Rwanda (1990-1994) and a four actor conflict in Burundi (1993-ongoing) to test whether the correlation found in the quantitative analysis is driven by the specific factors identified in the theory. These case studies show that negotiating peace in Burundi was more difficult due to information asymmetries, incentives to hold out, and shifting alliances. This dissertation demonstrates that the assumption in the existing literature that civil wars are two-party hinders our ability to explain the duration of these conflicts and limits our ability to respond to protracted internal wars.

Chapter One

Introduction

All civil wars end—eventually—but the failure to resolve civil wars quickly is costly. Civil wars show a remarkable variation in their duration. Some, such as the war that broke out in Romania in 1989, last just a few days, while others, such as the contemporary civil wars in Somalia, Colombia, and Sudan, rage for decades without resolution. A survey of all civil wars begun in the last sixty years reveals that, while a quarter of the wars end within the first eight months, another twenty-five percent continue for longer than five years. Moreover, eight-percent of all civil wars last more than two decades. What explains this wide variation in the duration of civil wars?

In this dissertation, I seek to address why some civil wars are resolved quickly while others are not. I argue that civil wars are longer when they include more actors that can block settlement. I demonstrate that multi-party civil wars are substantially more resistant to resolution using both statistical analysis and intensive case-studies of civil war negotiations in Rwanda and Burundi.

Why Duration?

Civil war duration is an important topic for two main reasons. Theoretically, understanding how and why civil wars are resolved through negotiated settlement inherently involves an analysis of why it takes so long for settlements to be reached. If you want to understand what causes combatants to fight and what gets them to cooperate, you must understand the duration of these conflicts. The existing literature has

largely ignored the duration of civil war and looked simply at the outcome of these conflicts, which truncates a long and important process.

Practically, the costs of war tend to be directly related to its duration. New data on battle deaths in civil war reveals that internal conflicts that are two years or shorter generate, on average, under 3000 deaths while civil wars that are longer than two years result in, on average, over 44,000 deaths.¹ For each additional month of civil war, on average, an additional 189 people are killed.² Understanding the factors that drive duration can help the international community resolve conflicts early and save lives.

In recent years, the international community has recognized these costs and shown greater interest in trying to resolve or at least put a lid on long-running civil wars such as those in southern Sudan, Afghanistan and Liberia. Much of this behavior is driven by self-interest, as the extreme economic and human devastation wrought by protracted internal conflict can create a security threat for the international community. Additionally, as the international community, under the lead of organizations like the United Nations, has become more involved in peacekeeping and peacebuilding, increasing attention has been paid to resolving long-running internal conflicts. These interventions have produced significant costs for the international community, both in terms of lives lost and the financial cost of long-term peacekeeping operations.

¹ These estimates are from data on battle deaths collected by Lacinia and Gleditsch (2005) for the Peace Research Institute of Oslo/Uppsala Armed Conflict Database. The median duration in the database is 24 months, and reported above are the mean number of battle deaths for conflicts that lasted up to twenty-four months or more than twenty-four months. The difference in median battle deaths among those conflicts is also substantial. The median number of deaths for short conflicts is 264 while for long conflicts it is 7400.

² This estimate is the result of a simple Ordinary Least Squares regression with battle deaths as the dependent variable and duration, in months, as the independent variable.

Understanding what factors make some wars last so long without resolution is important, therefore, because it can help policymakers to design more successful responses to them.

In recent years, a literature has emerged within International Relations that sees the duration of war (both interstate and civil) as driven primarily by information asymmetries.³ This literature sees conflict as a product of one or both parties overestimating their likelihood of victory, fighting as revealing information about this probability, and war ending when enough information has been revealed and a bargain is reached. Theoretically, informational approaches have given important insight into the factors that affect the incentives for parties to negotiate or fight throughout the conflict process. Empirically, however, no data exists to test the effect of information asymmetries on the onset, duration or termination of interstate or intrastate war.⁴ Even if we had measures of information asymmetries, it is doubtful that these approaches could explain the full variation in the duration of civil war. In particular, they could not explain why many wars last long beyond the point where the outcome of war is clear, and why some last so much longer than others. Greater information asymmetries might explain why one civil war lasts two years and another lasts four, but they cannot explain why some last twenty or thirty years.

This dissertation seeks to expand our understanding of the duration of civil wars both theoretically and empirically. The main theoretical contribution is to move the study of civil war duration beyond static two-party models, to incorporate the effects of multiple players. The main empirical contribution is to collect the data necessary to

³ Examples of these approaches include Filson and Werner (2002), Slantchev (2003) and Smith and Stam (2004).

⁴ Gartzke (1999) suggests that empirical measures that allow us to properly measure information asymmetries may never exist.

conduct rigorous qualitative and quantitative testing of whether multiple competing groups matter. This analysis shows that multiparty conflicts are substantially more resistant to resolution.

The Argument

The main argument in this dissertation is that civil wars that contain more actors that have to approve an agreement to end the conflict will be harder to resolve through negotiation and therefore longer. These actors can be viewed as “veto players” that have the ability to block settlement and continue the war even if the other parties consent to end it. I apply a veto player framework to analyze the effect of multiple parties on the duration of civil war.

Veto player analyses have been used in the Political Science literature to examine the effect of different governmental institutional arrangements on policy outcomes. The heart of the theory is that when there are more actors in government (such as political parties or different legislative bodies) that have to approve any new policy, and these actors have divergent preferences, it becomes harder to implement new policy and to move from that status quo. In governmental systems with more veto players, finding agreement on new policy is difficult and fewer new policies are implemented.

When viewed this way, there are similarities between governmental bargaining and civil war negotiations. Civil wars can be thought of as a violent conflict over policy. Insurgent groups revolt in response to dissatisfaction with a policy or set of policies of the government. They seek to push their demands on the government both through fighting and bargaining. Negotiations in civil war, like in governmental bargaining, involve

parties at the table making offers for a new policy to replace the existing policy, and each party must decide whether it prefers the new policy to the status quo.

Governmental bargaining and civil war negotiations are not identical processes, however, for two reasons. First, when civil war negotiations break down, fighting continues and the costs of fighting are borne by all parties. Bargaining breakdown in governmental negotiations can certainly be costly as well, but those costs are less obvious and it is not clear that all actors in government bear those costs. Second, factions in civil war face a constant threat of being defeated militarily and therefore not being able to bargain further. In governmental bargaining this situation only arises when political parties are faced with elections.

In chapter two, I present a theoretical framework for analyzing the effect of veto players on civil war negotiations. In doing so, I use both the similarities and differences between civil war negotiations and governmental bargaining to develop a model of multi-party negotiations. This model leads to three separate hypotheses about the effect of veto players on the duration of civil war.

The main hypothesis in this dissertation is that civil wars with more veto players will be of longer duration because negotiations will be harder and less likely to generate agreement. I identify four specific factors that make reaching agreement in multi-party conflicts more difficult. First, when there are more parties at the table, the range of possible agreements that all would prefer to continued warfare shrinks. Second, information problems are more acute in multi-party conflicts because it is harder for parties to determine how likely they are to win and it is more likely that one party will overestimate their probability of victory. These information problems make it harder for

parties to find a deal that all prefer to warfare. Third, when there are more parties in the conflict who must sign an agreement, the incentives for each to hold out in the hopes of getting a better deal as a late signer increase. Finally, negotiations in multi-party conflicts are prone to breakdown because of shifting alliances among parties who agree on one issue but disagree on others.

In addition to this main hypothesis, the theoretical discussion suggests two further implications about the relationship between the preferences and strength of veto players on the duration of conflict. First, as discussed above, one reason that multi-party conflicts will be longer is that each additional group shrinks the range of agreements that all parties prefer to continued conflict. This problem will be heightened when the various veto players have more extreme preferences, suggesting that conflicts will be longer when there is greater preference diversity among the veto players.

Second, the mechanisms discussed above will be affected by the ability of parties to bear costs from fighting. Stronger veto players can hold out longer than weaker veto players. Additionally, stronger actors can avoid being defeated militarily by the state. This analysis suggests that civil wars are longer when the non-state veto players in the conflict are stronger.

I develop this veto player approach more fully in chapter two. I explain these mechanisms and the logic that leads to the three hypotheses above much further. Additionally, I discuss the factors that lead conflicts to become multiparty and the conditions under which multiparty conflicts can end.

The Existing Literature on Civil War Duration

In addition to the informational models discussed earlier, existing approaches to war duration⁵ generally fall into two broad schools: costs of war theories and approaches that emphasize commitment problems. Each of these schools generates different predictions about what factors will make wars easier to resolve and therefore shorter.

Costs of war approaches are based on the argument that factors that make wars less costly for the actors involved decrease their incentive to end them, making negotiations more difficult and leading to longer conflicts. Collier and Hoeffler (2004) are the primary advocates of this approach, arguing that “greed” rather than “grievance,” is the main factor behind civil war, meaning that groups are more likely to fight the state when it is less costly for them to do so, rather than when their objective grievances are higher. Collier (2000) rejects Fearon’s (1995) assumption that war is always costly and argues that civil war is often a “quasi-criminal activity” and that some parties can actually see a benefit from fighting. Collier, Hoeffler and Soderbom (2004) argue that wars will be longer when there is positive utility from warfare, due to the economic opportunities that exist only as long as the war continues, because the parties do not have an incentive to end them.⁶

⁵ In actuality, there are very few studies that address the determinants of duration directly. However, many theories of civil war highlight factors that make wars easier or harder to resolve and therefore these approaches have direct implications for the duration of conflict. For simplicity sake, I refer to all of the studies mentioned here as analyzing “duration” but in reality several are studies of war onset or termination that have implications for the duration of civil war.

⁶ In this way, this newer literature is similar to Zartman’s (1989) argument that in order for conflicts to be “ripe for resolution” the parties must reach a stalemate that is “mutually hurting.”

In recent years, a number of scholars working in this vein have focused on one particular aspect of the costliness of war, the availability of “lootable” resources.⁷ Resource approaches argue that when actors in civil war have access to resources such as diamonds, drugs or gold they are more opposed to negotiations because they fear losing control over those resources once the war ends. These authors see the presence of lootable resources as a major factor leading wars to be of longer duration. The costliness of war approaches lead to an hypothesis about the duration of civil war:

Hypothesis 1: The more costly the war, the shorter it will be.

Theorists who emphasize commitment problems contend that wars will be longer if parties are unable to commit to uphold the terms of an agreement. Walter (2002) argues that the environment of extreme risk and uncertainty that characterizes a post-civil war state makes it virtually impossible for the parties to commit credibly not to renege on the terms of any agreement in the absence of third-party enforcement. Civil wars without this international enforcement will tend to be longer because the actors will be unable to implement a settlement to end the war, and therefore will continue fighting until one side defeats the other.

Fearon (2004) argues that it is easier for parties to commit credibly in some types of civil wars than in others, and that those wars in which commitment problems are lessened will be shorter. Specifically, he argues that conflicts which have “sons of the soil” dynamics are afflicted by severe commitment problems and will tend to drag on longer than other types of civil wars. Fearon (p. 14) defines sons of the soil conflicts as those in which

⁷ Good surveys of this literature include Berdal and Malone (2000) and Ross (2004).

the civil war involves an insurgent band fighting on behalf of an ethnic minority on the periphery of a state dominated by another ethnic group; against the state's military or paramilitary formations, and/or members of the majority group who have settled as farmers in the minority groups' declared home areas; and involves either land conflict with migrants from the dominant group or conflict over profits and control of natural resources in the minority's home area.

Sons of the soil conflicts last much longer than other types of civil wars, Fearon argues, because the state cannot commit credibly to prevent migration into the minority land in the future no matter how costly the war is in the short-term. Commitment approaches generate the following hypothesis about the relationship between commitment problems and the duration of war:

Hypothesis 2: The easier it is for parties to commit credibly to uphold the terms of an agreement signed, the shorter the war will be.

What These Approaches Leave Out

The veto player approach I advance in this dissertation does not directly contradict either of the approaches discussed above. The costs of war and credible commitments have been shown, both theoretically and empirically, to be important factors in the duration of war. However, the existing literature has missed a major factor that impacts the bargaining environment found in civil war negotiations. Namely, with very few exceptions, the current literature conceptualizes civil war as a two-party phenomenon, waged between a state and a rebel group. The literature has used this simplifying assumption because doing so leads to more comprehensible game-theory analysis, a tool used by many scholars to develop theories of civil war duration and termination.

This conceptualization is problematic for two reasons. First, it does not match the empirical phenomenon of contemporary civil wars. Many modern conflicts such as those in Afghanistan, Colombia, the Democratic Republic of the Congo, Somalia and the former Yugoslavia have contained more than two parties. The Peace Research Institute of Oslo/Uppsala Armed Conflict Database (ACD) identifies 288 internal armed conflicts, of which 90 contain more than two combatants at the same time (30 percent). Additionally, 55 of the civil wars listed in the ACD are, in actuality, coups or attempted coups, which do not fit traditional definitions of civil war. When the coups are removed the ratio of multi-party civil wars is higher, 81 out of 233 conflicts (35 percent). Figure 1 below presents a graph of the non-coup conflicts and the number of parties involved in each.

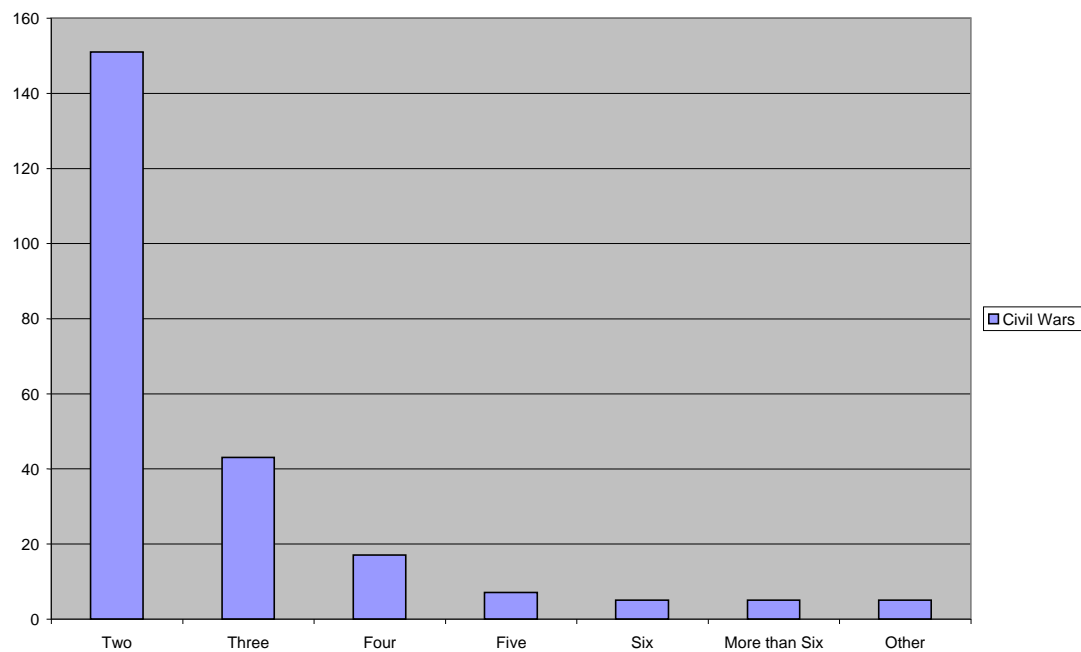


Figure 1: Number of Parties in Civil War (1945-2003)

As can be seen from Figure 1, while a majority of civil wars in the ACD are two-party, a substantial number contain three or four combatants and several are even more fractionalized. Additionally, there is some evidence that the percentage of civil wars that are multi-party is growing. The ACD identified 14 civil conflicts beginning between 2000 and 2003, 7 of which contained more than two parties fighting at the same time (50 percent).

The second, and more troubling, reason that the assumption that conflicts are two-party is problematic is that there are good theoretical reasons to believe that civil war negotiations should be more difficult when multiple combatants have the ability to block settlement. By ignoring the dynamics of multi-party negotiations we may miss a key factor driving the duration of civil wars. I have briefly discussed these reasons above and do so in much more detail in chapter two.

Testing the Determinants of Civil War Duration

In this project, I test the veto player approach described in chapter two as well as the competing hypotheses presented above. I do so through two separate sets of tests. First, I conduct quantitative analysis of the effect of the number, strength, and preferences of veto players, the costliness of war, and the presence of commitment problems on the duration of all civil wars begun since 1945. Second, I use comparative case studies of the civil war negotiations in Rwanda and Burundi to test the specific causal mechanisms presented in the veto player approach.

Quantitative Analysis

I test the five hypotheses using a new dataset containing data on every civil war begun since 1945. The set of civil wars is that identified by the ACD. The dataset developed for this dissertation presents two major advances on our ability to test empirically the determinants of civil war duration.

First, this project collects important systematic data on actors in civil war. While theories of civil war often focus on micro-level factors such as whether parties possess lootable resources, their relative strength, and opportunity costs, empirical studies of civil war duration and termination have tended to operationalize these factors using conflict-level or country-level measures. This mismatch between theory and data has hindered our ability to test fully the theoretical approaches to civil war described above.

Accurately testing the veto player approach requires data on all actors in civil war. Not all actors in a conflict are veto players, only those groups that are strong enough to block settlement by the other parties. To determine which parties are veto players, I have collected data on a wide range of factors for all groups in civil war identified in the Armed Conflict Database. These factors include the number of troops possessed by each party, the terrain that it operates in, whether the group participates in elections, and whether it gains support from a specific ethnic group. I use these data to create a measure of veto players, which is described in more detail in chapter three. The actor-level data are used to test the veto player approach advanced in this dissertation, but they also represent an opportunity for further research to test other theories which focus on actor-level attributes.

Second, this dataset includes comprehensive data that is measured monthly or annually, rather than aggregate data. Theories of the duration of civil war often focus on the timing of events. For example, approaches that emphasize the costliness of war see war as most likely to be resolved when wars are most costly. Commitment approaches see wars as most likely to end when commitment problems are lessened. However, many studies of the duration or termination of civil war have included aggregate measures across the course of the conflict of phenomena such as the number of battle deaths, the presence of an international guarantee, and so on. In this dataset, I have collected data that allows for measuring the timing of these events. The quantitative analysis, for example, includes annual measures of battle deaths and monthly measures of the number of veto players and the presence of international guarantees. In chapter three I discuss the creation of this dataset in much more detail.

This dataset represents an ideal opportunity for testing the approaches to civil war duration found in the existing literature as well as the veto player approach developed in this project. The findings reported in chapter three show that a greater number of veto players is significantly associated with longer civil wars, and this relationship holds when controlling for the costs of war and commitment problems. I conduct robustness tests which show that the relationship identified is not the result of some underlying factor or the way the number of veto players is measured, and that the finding is robust across time and region. Additionally, the results suggest that conflicts are longer when the non-state veto players are stronger. The results are not conclusive with regards to the preference diversity of veto players.

The results in chapter three also show support for commitment approaches. The presence of an international guarantee is found to be associated with shorter civil wars. The sons of the soil variable misses statistical significance, but its coefficient has a sign in the predicted direction. The results in chapter three do not show support, however, for Hypothesis 1, that more costly wars are shorter.

This quantitative analysis provides strong statistical evidence for the central contention that more veto players lead to longer civil war. However, the theoretical approach developed in chapter two argues that four specific mechanisms lead these conflicts to be longer: the shrinking of the bargaining range, greater information asymmetries, incentives to hold out, and shifting alliances. The quantitative analysis in chapter three cannot identify whether these specific factors are, in fact, those driving the correlation between the number of veto players and the duration of war. To test these mechanisms, I conduct comparative case studies of two civil war negotiation process: the 1992-1993 Arusha Peace Process in the Rwandan civil war and the multi-year negotiation process in the Burundian civil war.

Case Studies

The case studies of the Rwandan and Burundian civil wars presented in chapter four are designed to test if the correlation between number of veto players and the duration of civil war identified in the statistical analysis is driven by the four specific causal mechanisms presented in chapter two. This analysis is conducted using detailed process-tracing of the behavior of the actors at the negotiating table and on the battlefield in these two conflicts.

Much of the existing Political Science literature on civil war duration and termination is built on assumptions about the way bargaining works in these conflicts. However, very little systematic analysis of actual negotiations in civil wars has been conducted. It is impossible to fully test theories of bargaining without examining actual negotiations in civil war settings. These case studies, then, will allow me to build on the existing literature by analyzing the negotiation process in these two civil wars.

Rwanda and Burundi are ideal cases because they are as similar on all of the key factors identified in the literature as influencing negotiation as possible. Both conflicts generated low numbers of battlefield casualties.⁸ In neither conflict were lootable resources present in the conflict zone nor were they an issue in the fighting. The two countries have very similar terrain.⁹ In neither conflict were “sons of the soil” dynamics present. The international community did not make a credible guarantee to enforce the terms of a settlement in Rwanda, and did not do so until eight years into the Burundian conflict. Both Rwanda and Burundi had similar levels of development at the time the conflicts began, and the two countries have nearly identical ethnic compositions. The issues at stake in the conflicts are nearly the same.

The main factor on which these two conflicts differ is the number of veto players present in the fighting. The conflict in Rwanda involved two veto players, which is the lowest number of veto players possible and the modal number of veto players for the

⁸ The “low battlefield casualties” designation is relative to other conflicts in the dataset. Additionally, the analysis of the Rwandan civil war examines the period from 1990-1994 prior to the genocide and so those deaths are not counted.

⁹ The terrain comparison is based on Buhaug and Lujala (2004) who have identified the terrain in the area of each country where civil war is fought. They identify the Burundian conflict zone as 67% mountainous and completely unforested, and the Rwandan conflict zone as 93% mountainous and completely unforested.

conflicts in the dataset. The conflict in Burundi contained four veto players, which is in the top decile of cases identified in the dataset. These cases, then, allow me to control for many other factors that could affect the duration of fighting and isolate the independent effect of veto players.

In order to evaluate the causal mechanisms at work, I conduct detailed analyses of the negotiating process in these conflicts. I expect to find four things if my theory holds true. First, in negotiations with multiple veto players, there will be a greater number of issues under discussion by the parties at the negotiating table. There will be more issues because each party will bring a separate set of preferences to the negotiating table, and these preferences will increase the total number of issues at conflict and the likelihood that there will be disagreement between at least some of the parties on any one of those issues. A simple examination of the demands made by each party and the issues discussed will allow for determining whether, in the Burundian negotiations, more issues are at stake.

Second, there should be a clearer relationship between the battlefield and the negotiating table in Rwanda than in Burundi. One barrier to resolving multi-party conflicts through negotiation is that it is more difficult for actors in those conflicts to use their performance on the battlefield to make more realistic assessments of their chances of winning the conflict. Consequently, it is harder for combatants to adjust their demands to more accurately reflect their military position. Because of this difficulty, I expect to find more evidence in the Rwandan peace process of combatants using the battlefield to adjust their demands at the negotiating table than in Burundi. To test this prediction I will

evaluate the battlefield performance of parties over the course of the conflict and how that performance affects the demands they make at the negotiating table.

Third, in conflicts with more veto players, the parties will be more likely to walk away from the negotiating table or to refuse to participate at all in negotiations in the hopes of getting a better deal as the other parties make some progress in the negotiations. This is the hold-out behavior discussed more fully in chapter two. I expect to find a greater occurrence of parties leaving the negotiating table once negotiations have begun in Rwanda than in Burundi. I also expect to see parties refusing to participate in the initial negotiations and joining in later as progress in the negotiations is made.

Finally, the veto player approach predicts that multi-party negotiations will be made more difficult by shifting alliances among parties on different issue areas. In Burundi, then, I expect to see combatants attempt to form negotiating blocs to make reaching agreement easier, but expect these blocs to break down as decisive issues are proposed. Additionally, I expect to see changes over time in the composition of the various blocs.

The data for the case studies has been collected using primary and secondary sources, as well as field interviews with individuals in the countries in conflict and international scholars and policymakers with knowledge of the wars and negotiation processes. The data represent a major advance on existing analyses of negotiation processes because very few models of bargaining are tested systematically on actual negotiations in civil war. These data allow for testing the strategies used by individual actors at the negotiating table in these conflicts.

The qualitative analysis of the Rwandan and Burundian civil war and negotiations reveals that negotiations were in fact more difficult in the latter case. In Rwanda, the two sides were able to reach an agreement that approximated the position of each on the battlefield rather quickly, while in Burundi negotiations dragged on. The greater number of parties in the Burundian conflict clearly led to more issues, and a greater propensity for negotiation breakdown due to an inability to reach agreement. This qualitative analysis suggests that finding a negotiated settlement in Burundi was more difficult due to the presence of multiple parties at the table.

Drawing conclusions from these two cases is complicated by the fact that, while reaching agreement in Rwanda was easier, the Arusha Accords signed in 1993 broke down nine months later into genocide. Given the astonishingly high level of violence (up to 1 million people killed in April-June 1994), the Rwandan peace process can by no criteria be considered a success. If this breakdown was the result of some fundamental aspect of the Arusha process it could suggest that the veto player approach is flawed. However, in chapter four, I will argue that the Arusha peace accord broke down not because of some problem with the process or agreement, but for two primary reasons. First, the international community at Arusha committed to deploy a robust peacekeeping force and then failed to do so. Second, events in Burundi and within political parties in Rwanda led to a polarized political system in 1994 that did not match the power-sharing arrangement created by the Arusha Accords. These two events set the stage for genocide, not the nature of the peace process itself.

Roadmap for the Dissertation

The analysis in this dissertation proceeds through five chapters. Chapter two develops the veto player theory described above in much more detail, addressing five areas in particular. First, I survey the current literature on bargaining and war and discuss how the veto player approach fits into and expands it. Second, I describe veto player approaches in Comparative Political Science and argue for their applicability to studies of conflict bargaining. Then, I examine the conditions that lead conflicts to become multiparty. I use this discussion to build a model for the effect of multiple parties on conflict bargaining. Next, I briefly analyze the conditions under which multi-party conflicts do end. I conclude the theoretical discussion by deriving the main hypothesis that will be tested in this project: hypothesis 3, that civil wars with more veto players will be longer, as well as two additional hypotheses about the effect of the strength and preferences of veto players.

In chapter three I test hypotheses 1-5 quantitatively. The analysis proceeds in three main sections. The first details the creation of the dataset, paying particular attention to issues such as the definition of civil war, and how I determined the duration of conflicts. The second section describes how I measured the number of veto players for each conflict, as well as the other independent variables included in the study. The third section presents the results of a series of quantitative tests designed to isolate the impact of the number of veto players, the costs of war, and commitment problems on the duration of civil war. The results show a strong correlation between the number of veto players and the duration of civil war.

To determine if this correlation is driven by the causal mechanisms discussed in chapter two, chapter four presents comparative case studies of civil wars in Rwanda and Burundi. I use detailed data on the day-to-day events and behavior of parties at the negotiating table and in the conflict to evaluate whether the conflict in Burundi had more issues under negotiation, whether there was a clearer relationship between information revealed by the battlefield and the negotiating table in Rwanda, whether parties in Burundi were more prone to hold out and whether shifting alliances presented a barrier to peace in Burundi. The analysis does not find evidence that more issues were under negotiation in Burundi, but does support the other three mechanisms. The case studies also revealed dynamics not predicted by the veto player approach. In particular, non-combatants played a much larger role in all stages of the peace process in both Rwanda and Burundi, an outcome not predicted by the veto player approach or other existing theories of civil war.

The final, concluding, chapter wraps up the analysis in the first four chapters. I begin by briefly restating the main argument and discussing the results of the statistical and qualitative analyses. I discuss how this dissertation contributes to our understanding of the dynamics of civil war negotiations, and suggest areas where further research is needed. The argument in this dissertation has implications for the field of International Relations and Political Science more broadly, and I discuss how the approach here can inform broader debates within those fields. Finally, I suggest implications of this analysis for practitioners and policymakers seeking to find ways to respond to long-running internal conflicts. International actors dedicated to resolving these conflicts should take into account the number and composition of combatants, and tailor their resolution

efforts accordingly. In particular, policymakers should look for ways to reduce the number of actors that can block agreement, should focus principally on bringing the strongest combatants on board first, and should avoid strategies such as offering public goods for agreement in multi-party conflicts where they may actually increase the incentives for actors to hold out.

Chapter Two

Theory

In this dissertation I am arguing that negotiations are more difficult in civil wars when more actors must consent to an agreement to end the war. Because of this difficulty, negotiations in multi-party conflicts tend to fail more often, leading these civil wars to be longer.

I suggest that one way to think about the effect of multiple parties on civil war negotiations is through a veto player analysis. Veto player analyses have been used in the American and comparative Political Science literature to analyze the effect of different institutional arrangements on policy implementation. In this chapter, I present a theoretical model of civil war negotiations through a veto player framework.

The discussion proceeds in six parts. First, I describe the current state-of-the-art literature on bargaining and war to situate the model in this dissertation in context. Second, I argue that we can conceptualize civil wars as a bargaining process over policy and this conceptualization allows us to use a veto player framework to analyze civil war negotiations. I discuss veto player approaches from American and comparative politics and analyze similarities and differences between governmental bargaining and negotiations in civil war. Third, I discuss why violent bargaining over policy can lead to some conflicts becoming multi-party. Fourth, I present necessary and sufficient conditions that parties to a civil war must meet in order to be considered veto players. In the fifth section, I use the similarities and differences between civil war negotiations and governmental bargaining to develop a theoretical framework for analyzing the effect of

veto players on the duration of civil war. Sixth, I briefly discuss the conditions under which actors in conflicts with multiple veto players can overcome these bargaining difficulties and reach a settlement. I conclude by using this framework to derive three hypotheses about the effect of the number, preferences, and strength of veto players in civil war on its duration.

Bargaining and War

The predominant theoretical approach to war (both inter- and intrastate) within International Relations for the past decade has been the literature on “bargaining and war.” Although this literature builds on insights from many earlier scholars (particularly Bueno de Mesquita 1981 and Wittman 1979), the foundational work in bargaining and war is Fearon (1995). Fearon presented a model to explain why, if wars are costly for all states involved, they still occur. Since his model sets the stage for all future work on bargaining and war, it is worth examining in detail.

Fearon begins with a fundamental puzzle: since war is costly, it is always inefficient *ex post*. That is, all actors would be better off agreeing to implement the outcome produced by the war in advance, without having to pay the costs of fighting. For Fearon, the interesting puzzle is what prevents states from reaching an agreement *ex ante* that gives each a better deal than they would get *ex post* from fighting a war. He uses a simple formal model of two states negotiating over policy which I replicate here for illustrative purposes.

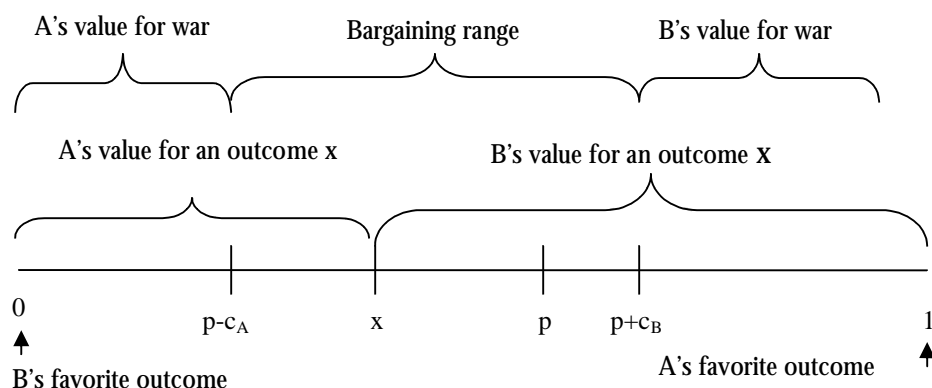


Figure 2: Fearon's Bargaining Model

In the model, state A and state B are arguing over the division of some set of issues bounded by 0 and 1. The states' preferences are diametrically opposed, B would prefer outcome 0 and A would prefer outcome 1, and both prefer any outcome closer to their ideal point. If the states fight, the winner will get to impose its ideal policy. In this model, both states know p , or the probability that state A will win the war. Both states also know that if they fight, regardless of who wins, they will have to pay some costs (represented in the model by c_A and c_B). State A's expected utility from war, then, is p (the probability that it will get to impose its ideal policy) minus c_A , its cost of fighting. State B's expected utility, meanwhile, is $1-p$ (since its probability of victory is the probability that State A will not win) minus c_B . As long as the two cost terms are positive, there is a range of agreements between $p-c_A$ and $p+c_B$ that both states would prefer to war. This range of agreements is referred to as the "bargaining range" in Figure 2.

According to Fearon, rational, risk-neutral states should accept any deal that falls within that bargaining range instead of going to war. In Figure 2, he gives the example of a bargaining outcome “x.” Since x falls within the bargaining range, both states A and B have higher value for outcome x than for fighting, and so each should accept that deal and war should not break out.

The fundamental puzzle that arises out of this formalization, then, is why we ever see wars. If it is true that there is always a range of bargaining agreements that both states prefer to costly conflict, why are they unable to find an agreement in that range? One possibility is that war could be the result of irrational decisions on the part of leaders. Another is that, while war is always costly, the decision-makers involved may not bear these costs directly and so may discount them. However, Fearon argues that there are three conditions under which rational, unitary, risk-neutral states will still have incentives to engage in costly wars.

If the issues at stake in the conflict are indivisible, war is possible. In relation to Figure 2, the “bargaining range” may be empty, there may be no way to divide the issues in such a way that an outcome x falls in that range. Historical struggles over who would be king can be seen as indivisible conflicts—there is only one king, and so dividing that issue is not possible. However, Fearon dismisses issue indivisibility as a compelling explanation for war, because he argues that wars are generally fought over a complex set of issues and that issue linkages and side payments should be available to make virtually all issues divisible.

A more likely situation that would lead states to war is private information about their military capabilities and incentives to misrepresent that information. Private

information alone is an insufficient cause of war because, since war is costly, states have incentives to reveal information that would help them to avoid it. However, in crisis bargaining states are trying to get the best deal possible and therefore they have an incentive to misrepresent their military capabilities in the hopes of getting a better deal.

Finally, states may go to war even if all issues are divisible and they are both completely informed about their relative capabilities if they are unable to commit credibly to an agreement within the bargaining range. Fearon identifies three factors that can limit states' abilities to make credible commitments: an offensive advantage that makes the state that attacks first much more likely to win the war; an anticipation of a change in relative capabilities in the future that would make a current bargain unenforceable; and the strategic nature of territory under dispute that would enhance the military capabilities of the other side.

Fearon's influential argument has generated further theoretical work (although still very little empirical testing). The main advance in the bargaining and war literature is to take what in Fearon (1995) was a static model and make it dynamic. In particular, a number of scholars have moved beyond Fearon's argument that war is the result of bargaining breakdown (due to private information or commitment problems) and instead have shown that war can be part of an ongoing bargaining process.

Scholars who view negotiating and fighting as part of one bargaining process (such as Wagner 2000, Filson and Werner 2001, Slantchev 2003, Smith and Stam 2004) focus on the role of information asymmetries as a cause of war. In these approaches, war breaks out because one or both parties overestimate their probability of victory and, therefore, cannot identify the range of agreements that both prefer to warfare. When

bargaining breaks down, these parties use the battlefield to try to strengthen their position at the negotiating table.

For these “war as process” models, the fundamental theoretical concept is that of “updating.” These models argue that parties use the battlefield to update their estimates of “p” or their probability of victory. For example, a state may think it has a high probability of victory but then lose several influential battles, and come to a more realistic assessment of its chances of winning. Parties then return to the negotiating table, and if enough information has been revealed they are able to identify a bargaining range, and they will sign a deal in that range.

All of the above models have been developed primarily to explain inter-state conflicts. However, the literature on civil war has generally applied the same framework to understanding the outbreak, duration, and termination of internal conflicts. In the next section, I will discuss briefly how civil wars can be thought of as violent bargaining over policy, and argue that the above framework is useful for understanding those conflicts.

The veto player approach that I develop in this chapter fits into this bargaining and war framework. I am not attempting to disprove the theoretical findings discussed above. Rather, I seek to advance this literature by relaxing one of the main (unstated) assumptions that all of these models share: that war is always fought between two parties.

This assumption is problematic for two reasons. First, as demonstrated in chapter one, many civil wars contain more than two combatants. Second, and more importantly, there are good theoretical reasons to believe that negotiations will be significantly more complicated in multi-party civil wars than in the two-party conflicts analyzed in the existing literature.

In the next section, I lay the framework for using a veto player approach to analyze the effect of multiple combatants on civil war duration. I argue that the above models all assume that civil wars represent a form of bargaining over policy, and that this similarity with domestic politics allows us to use a veto player framework to analyze these conflicts.

Civil War Actors as Veto Players

Scholars who analyze internal conflict through a bargaining and war framework see civil war as a form of violent bargaining failure over the policy or set of policies that will be adopted in a country. These wars begin when insurgent groups take up arms because of a failure of the government to respond to some demand for a change in the existing policy—from granting greater local autonomy to increasing the resources available to a specific group, opening up the government, all the way up to the demand that the government step down entirely.¹ Once conflict begins, parties to a civil war use two strategies to try to achieve their ideal policy—fighting and bargaining. They fight because winning the conflict militarily would allow them to implement their ideal policy. At the same time, however, parties bargain to achieve a policy through negotiation that saves them from having to pay the costs of warfare.

Negotiations in civil war, then, involve actors at the table making offers for a new policy (or set of policies) to replace the existing policy. In this conceptualization, negotiations in civil war fit within the same framework of bargaining in civil war

¹ This description of the way civil wars begin is a generalization but holds true empirically almost all of the time. While the government could conceivably initiate a civil war because of its desire to change the status quo, these wars almost never start this way. Theoretically it makes sense to think of wars starting because non-state actors are dissatisfied with the status quo.

presented by Fearon (1995) and the subsequent bargaining and war models. In civil wars, the state and non-state actor(s) each have an ideal policy for the state, and attach some utility to that policy. Military victory would allow them to impose that policy, but fighting in civil wars is costly. Therefore, rational risk-neutral actors in civil war should accept an agreement that gives them more utility than they expect to get from fighting.

The conceptualization of multiple actors within a state bargaining over policy is also similar to models of domestic policy negotiation in governments. In governmental bargaining, a set of actors attempt to reach some policy that all prefer to the existing status quo. Negotiations will be unsuccessful as long as at least one actor that can block agreement does not prefer the new policy to the current status quo.

The similarities between the two allow for a similar framework for analyzing the bargaining that occurs. In both situations, all actors who have the ability to block agreement must decide whether to approve a new policy or to stick to the status quo. A long history of scholarship in Political Science analyzes the situations under which governmental bargaining is more likely to lead to policy change. In recent years, the dominant way of analyzing this question has been to examine governmental bargaining through a “veto-player” framework.

Veto player approaches (such as Tsebelis 2002; Cox and McCubbins 2001) are designed to explain variation among democracies in their ability to reach agreement on policy. The heart of the theory is intuitive: the more actors there are with divergent preferences that have to approve any new policy (veto players), the harder it is to move from the status quo. In governmental systems with more veto players, therefore, policy

stability increases and it becomes more difficult for governments to implement new policies in response to changing societal conditions.

In advanced democracies it is relatively easy to tell who the veto players are. Comparative scholars differentiate between “institutional” and “partisan” veto players. Institutional veto players represent separate institutions—such as the two houses of bicameral legislature—that must approve legislation. Partisan veto players change more often and refer to the number of parties who must approve a piece of legislation for it to come into law.

Several examples of different types of political systems can make this distinction more apparent. In the United States the President, House of Representatives, and Senate must all approve most legislation, and so there are at least three veto players. In the United Kingdom, meanwhile, there could be as few as one veto player since the British parliamentary system is effectively unicameral and single-member districts mean that often one party has a majority in the House of Commons. Parliamentary democracies based on a proportional representation system, such as Israel and Italy, tend to have high numbers of veto players, since reaching a majority in parliament takes the cooperation of many political parties.

Comparative scholars have used veto player analyses to examine the effect of institutional arrangements on macroeconomic policy (Tsebelis 2002), the ability of governments to respond to fiscal crises (McIntyre 2003) and other policy outcomes. In all cases the results indicate that states with more veto players are slower to implement new policies.

I argue that a similar logic can be applied to civil war negotiations. In civil war bargaining, there are a set of actors who must consent to an agreement to end the war. If any of these actors do not prefer the agreement to the status quo, they can block agreement and continue fighting. In a sense, the war as process models described above are based on a veto player logic. In those models, war is a bargaining process which continues until each actor prefers a new policy to the existing status quo (which includes continued warfare). That literature does not refer to this process in terms of veto players, however, because it is based on an assumption that there are only two parties in war. However, once we relax that assumption, the similarity between governmental bargaining and civil war negotiations suggests applying a veto player framework is useful.

I do not mean to imply, however, that bargaining over policy within advanced democracies and negotiating an end to civil war are identical processes. In fact, there are two fundamental differences between those two bargaining environments. First, governmental bargaining takes place within an institutionalized bargaining environment in which it is generally clear which actors will have to consent to policy changes. In the United States example, the President knows he will have to get the approval of the two bodies of Congress to get legislation passed. In civil war negotiations, however, it may not be clear which actors have the ability to block settlement.

Second, in governmental negotiations the costs of bargaining breakdown are generally lower than in civil war negotiations. An inability to implement policy can be costly,² but these costs are not always clear. When bargaining breaks down in civil war,

² An extreme example of this can be seen in MacIntyre's (2003) discussion of the ability of Asian states to implement new policies to respond to fiscal crises.

however, costly conflicts continue and these costs are apparent to all parties. Continued fighting affects strategies in two ways. On the one hand, parties must pay costs of fighting and so their incentive to block agreement will be affected by their ability and willingness to pay costs. On the other hand, since military victory is a possibility in civil war, parties have the option of trying to impose their ideal policy unilaterally by winning the conflict, instead of taking a lesser deal at the negotiating table.

Rather than seeing these differences as making it impossible to apply a veto player framework to civil war negotiations, I use them to develop a theoretical model of multi-party bargaining in civil war. This model emphasizes the strategies that actors in conflict will use both on the battlefield and at the negotiating table to try to get the best deal possible based on the number, preferences, and strength of veto players participating in the conflict. I analyze how these various strategies affect the duration of these conflicts.

Before turning to the presentation of the model there is one remaining question that I need to address. I have argued that, contrary to the predominant assumption in the literature, civil wars often involve more than two parties. In the next section, I discuss what factors lead some civil wars to become multi-party, and I will use that discussion to build the theoretical model of multi-party bargaining discussed next.

Why Some Civil Wars are Multi-Party

A veto player framework is only a useful tool for analyzing civil war negotiations if there is variation in the number of veto players across civil wars. In the previous chapter, I showed that, despite the traditional conceptualization of civil war as a two-

actor phenomenon, many civil wars contain more than two parties at any given time. In this section, I use the discussion of civil war as bargaining over policy to analyze the factors that can lead additional parties to join civil wars, making those conflicts multi-party.

Before doing so, I should point out where this dissertation does not diverge from the existing literature. While I am relaxing the assumption in the existing bargaining and war literature that all civil wars are two-party, I am retaining the assumption that these parties are rational unitary actors. In Fearon's model, State A and State B are treated as unitary actors, even though presumably each state has several decision-makers (or veto players) that have to accept a peace deal. The assumption is that states have some ways to aggregate the preferences of these decision-makers and that we can act as if the state has an ideal policy position that is the product of this aggregation. Similarly, I assume that each veto player (the government and each insurgent group or external intervener) is a rational unitary actor.

This assumption is of course a simplification. Clearly, states and rebel groups have internal factions that affect their ability to bargain. However, bargaining within states and groups is a fundamentally different process than bargaining among them. That is, we can think of conflict bargaining as a two-stage process. In the first stage, each veto player must overcome internal divisions and decide on a coherent set of demands.³ In the second stage, these veto players come to the negotiating table to try to reach an

³ For a theoretical model of the affect of internal divisions on the bargaining process, see K. Cunningham (2005).

agreement that all prefer to continued warfare. I examine the effect of multiple veto players at the table during this second stage of the bargaining process.

Combatants in civil war can be generally categorized into three groups: the government, internal insurgent groups and external actors who have intervened in the fighting. Civil wars generally start when one domestic insurgent group launches a violent challenge against the government. The factors that make civil wars multi-party, therefore, are those that lead to additional internal insurgent groups emerging and/or to external states intervening in the conflict. I will discuss each of these conditions in turn.

Internal Groups

Internal insurgents in conflict can be further divided into two types: “original groups” who emerge independently and pursue an agenda that is separate from the other original groups in the conflict, and “splinter factions” who emerge due to a split within an existing group. The domestic insurgent groups in the 1998-2001 civil war in the Democratic Republic of the Congo can illustrate this distinction. Conflict began in August 1998 when a Congolese insurgent group, the Rally for Congolese Democracy (RCD), based in Goma, on the border with Rwanda, launched an armed challenge against the Congolese government. In November 1998 a second Congolese insurgent group emerged, the Movement for the Liberation of the Congo (MLC), based in Equator Province in North Eastern Congo. The MLC had a completely separate leadership structure from that of the RCD, received support from a separate external patron (Uganda, instead of Rwanda), and represented a different ethnic base of support than the

RCD did. These two groups, then, can both be thought of as “original” groups in that they emerged independently of each other.

The conflict in the DRC also saw the emergence of splinter factions. In January 1999, the leader of the RCD, Ernest Wamba dia Wamba, was replaced at a party congress in Goma. Wamba moved to Kisangani, however, and announced that he was not stepping down. This leadership dispute led to a split in the organization and two main factions, RCD-Goma (the original faction) and RCD-ML (led by Wamba) both fought against the DRC government and battled against each other. RCD-Goma actually saw further splintering across the conflict, although none of these other splinter groups was large enough to have a significant effect on the fighting.

The main factors that lead additional original groups to join a conflict are likely to be the same factors that led the first domestic group to challenge the government: dissatisfaction with the current status quo policy. Separate original groups will emerge if groups are dissatisfied with the current policy (or the government’s position) and also dissatisfied with the policy preference of the other original groups in the conflict. This disagreement over policy preferences between original groups can be generated simply by the fact that each group has a different preference for who would govern the country in the case that the government is defeated. The factors that lead civil wars to include multiple original groups, then, are the same conditions that lead societies to have heterogeneous preferences, and these can include the size of the state, its ethnic composition, and whether the government has employed divisive policies in the past.

It is more difficult to generalize the conditions under which splinter groups will emerge as there are several different factors that can lead them to do so. First, splinter

groups may also emerge due to different policy preferences. Often, umbrella opposition groups form out of a desire to push a strong agenda against the government, but then break down because the internal factions cannot agree on what specific demands to push on the battlefield and at the negotiating table. Second, groups can fracture, as in the case of the RCD, over leadership disputes, which can be thought of as a kind of difference in policy preferences. Finally, groups may splinter out of disputes over which strategy to pursue, for example, whether to negotiate with the government or to hold out and keep fighting.

There are a variety of conditions that can lead civil wars to involve multiple internal actors, therefore. Generally, it makes sense to think of the main factors as being simply that different groups have different preferences over the ideal policy or set of policies that would govern the country. This difference in preferences, however, is not the only factor that can lead conflicts to be multiparty, as sometimes splinter groups can emerge out of disputes over strategy. This distinction will be important for the veto player framework because whether two groups have separate preferences over the outcome of war is an important determinant of whether they both should be considered as veto players.

External Interveners

External states can also become involved in civil conflicts for a variety of reasons. They can intervene to enforce some agreement or try to end the fighting, to provide support to one side or specific actor in its attempts to win the conflict, or to pursue some

independent agenda in the conflict. One way to think about the different types of interventions is whether the intervener brings a separate set of preferences to the conflict.

When external states intervene to enforce some agreement, the intervention is designed to help end the war. Examples of this type of involvement can be seen in a variety of civil wars, such as the 2004 deployment of an African Union peacekeeping mission to western Sudan. External states intervening to support one specific actor can also be seen as an intervention designed to end the war, although in this case the state has a preference about which side will win the war. The United Kingdom's deployment of troops to Sierra Leone in 2000 to back up the government against the RUF can be seen as this type of intervention.

Finally, however, external states may intervene out of a desire to pursue some independent agenda in the conflict. Rwanda and Uganda's interventions in the conflict in Congo/Zaire can be viewed in this way. Although both states supported internal Congolese insurgent groups, they intervened in the Congo to pursue one primary goal: the forceful disarmament of their rebel groups which had based on Congolese territory. Each country was willing to ally with whatever group or government it saw as helping advance that goal and oppose any group that stood in its way. This type of intervention is best viewed not as simply in support of one actor or another but rather as introducing an external actor to the conflict with an independent agenda.

Interventions by external states, then, can be differentiated based on whether they bring separate preferences to the conflict. Interventions to facilitate an end to war clearly do not, and interventions to provide direct support to one side or the other do not bring separate preferences to the conflict. When external states intervene to pursue an

independent agenda, however, they represent an additional actor that has a separate set of preferences and potentially possesses the ability to block agreement.

Identifying Veto Players

The previous section analyzed the factors that can lead civil wars to contain multiple parties. I now turn to a discussion of the conditions these parties must meet in order for them to count as veto players. In this dissertation, I am focusing on armed combatants, whether they be internal parties or external interveners, who have the ability to block settlement. Clearly, however, combatants are not the only actors who may potentially be able to block agreement. In some civil wars, for example, influential political parties or actors in civil society are given a large role in shaping a peace agreement and so have an effect on the bargaining process. In other cases, there are external actors who are not directly involved in the conflict but have an ability to “veto” an agreement they do not view as acceptable. The clearest example of these type of veto players was the role that the United States and Soviet Union played in many internal conflicts during the Cold War. I focus on combatants here primarily because it is among those actors that it should be clearest who has the ability to block agreement, and it is combatants that are most likely to be present in conflict bargaining.

Tsebelis (1995) defines a governmental veto player as “an individual or collective actor whose agreement...is required for a change in policy” (301). Clearly, not every party to a civil war is “required” to agree to a change in policy including an end to civil war. Rather, there are three necessary conditions for a party to a civil war to be a veto player.

Cohesiveness

First, a party must have an internally cohesive organization. Cox and McCubbins (2001) argue that, for a collective group to be a veto player in the policy-making context, it must possess some sort of mechanism for organization to alleviate potential collective action problems associated with exercising a veto. The same logic holds for parties to civil wars: if a party is not internally strong enough to stick to its position at the bargaining table or to carry out any threats or promises made, it is not capable of being a veto player.

The importance of internal cohesion can be seen in reference to the recent civil war in the Democratic Republic of the Congo (DRC) described above. The conflict in the DRC was the most widespread war in the world in the 1990s and involved major military presences from six neighboring countries and several rebel movements. From its outbreak in 1998, there were four parties loosely allied against the Kinshasa government—two external states, Uganda and Rwanda, and two internal rebel groups, the Movement for the Liberation of the Congo (MLC) and the Rally for Congolese Democracy (RCD).

The MLC was closely allied with Uganda and the RCD received support from Rwanda but each party maintained its own military organizations and financial bases. The MLC, in particular, had a strong organization with a leadership structure headed by Jean Pierre Bemba remaining constant throughout the course of the conflict and with that leadership in command of all of the MLC troops. Because of this strong organizational structure, the MLC was able to contain fractionalization, mobilize popular support and

establish a significant resource base by levying taxes on tea, coffee, timber, gold and diamond exports.⁴

By contrast, since its inception the RCD was unable to articulate a coherent set of demands and experienced extreme fractionalization, dividing into two groups, RCD-Goma and RCD-Liberation Movement, within a year of the start of the war and then seeing further splintering of each of those groups, with some breakaway factions of the RCD merging with the MLC. Rwanda continued to back RCD-Goma, but throughout the history of the conflict it was unlikely that, if Rwanda removed military and economic support from the war in the Congo, the RCD would have been able to overcome its coordination problems and present a coherent set of demands at the negotiating table, rendering it unable to veto a settlement that moved from the status quo of continued warfare. For this reason, the MLC would meet the requirement of “coherence” to be a veto player in that conflict while the RCD would not.

Viability

A second necessary condition a party must possess to be a veto player is the capacity to avoid being conquered by the other parties to the conflict if those parties reach an accord. Even if a party has a strongly cohesive internal organization structure, yet is not powerful enough to avoid being soundly defeated by a united army in the aftermath of an agreement, it does not have the ability to unilaterally veto an agreement.

⁴ Although the MLC has often been viewed as a pawn of the Ugandan military in the DRC, the International Crisis Group argued that as of August 2000, the group was paying for 60% of its expenses, with that number expected to rise to 90% by the end of that year (Scramble for the Congo, 2000, p. 37).

A viable option for continued warfare, therefore, is a precondition for a party to be a veto player.

The importance of viability of parties fits in well with existing analyses of civil war duration. Collier, Hoeffler and Soderbom (2004) write that organizations stop fighting civil wars for one of two reasons: they reach an agreement that they prefer to the status quo or they become unviable. This analysis is consistent with the veto player approach because it identifies two conditions under which parties can stop being a veto player—they have an incentive to move from the status quo and so reach a negotiated agreement, or they become unviable and are no longer able to veto agreement.

Viability of parties is difficult to specify but there are a number of factors that affect it. Military capacity is one clear example: parties with larger, better equipped and better trained militaries are likely to remain viable longer than parties that do not possess those attributes. Geographical variables can also contribute: parties that operate in areas of countries that have greater forest coverage or are more mountainous are more likely able to continue warfare because those geographical features make it easier to avoid being defeated. The focus on the importance of geographical variables is similar to Fearon and Laitin's (2003) discussion of the importance of mountainous terrain in facilitating insurgency. I argue that rough terrain also makes insurgencies more difficult to resolve because parties can remain viable longer.

The ability of a party to obtain resources to fund continued rebellion is another important component to viability. This funding can come through the presence of exploitable resources in territory controlled by the rebels (as seen in cases like Sierra Leone, Colombia and Angola) or through the establishment of a tax base among the

population controlled (over the course of the four-year Congolese civil war, the MLC built a significant tax base in the area of the DRC it controlled). There is one caveat for the role of funding, however. Internal rebel groups (or governments) that rely entirely for their viability on funding from external sources should not be seen as veto players, since that reliance removes their ability to act unilaterally. Rather, in those cases, the external party would be a veto player, because that party must be satisfied with the outcome of a conflict in order to pull out its support, and until that support is removed the conflict will continue.

Autonomy

A third requirement for parties to operate as separate veto players is that they must have preferences that are to some extent divergent from the other parties to the conflict. Cox and McCubbins (2001) refer to this dimension of veto player analysis as “separation of purpose” and in the case of civil wars the logic is intuitive. If two parties have exactly identical preferences for the outcome of a conflict, then they do not present two different sets of interests that need to be satisfied, and so only represent one veto player.

Generally, parties that are organizationally cohesive and militarily viable enough to continue a civil war unilaterally have preferences that are at least somewhat different from the other parties to the conflict. Many civil wars see a variety of parties allying to accomplish one goal but dividing after that goal has been completed or new conditions have emerged. A good example is the decades-long civil war in Somalia, where in the late 1980s a number of parties allied to form a coalition to remove Siad Barre from power.

Once Barre was removed, however, differences in preferences between the allied parties emerged and the coalition quickly fell apart. Within days, Mogadishu erupted into more extreme violence and the Somali civil war continues on a smaller scale to the present.

As discussed in the previous section, however, there are conditions under which two or more parties to the conflict can have the same preferences. In particular, this scenario can arise when external states intervene directly to support one actor in the civil war, or when splinter groups break away from an original group out of a dispute over the strategy to be pursued in the conflict. In cases in which two groups have identical preferences, then, it makes sense to only treat one of them as the veto player, since presumably a deal that would satisfy one group would satisfy all others with identical policy preferences.

Analyzing a civil war environment to determine which parties meet all three of these necessary conditions reveals the number of veto players in the conflict. Examining civil wars in this way provides a framework for analyzing the impact of number of veto players on bargaining in conflict.

There are existing arguments in the literature that suggest conflicts with more veto players should be more difficult to resolve. Zartman (1993), for example, argues that conflict analysts have traditionally viewed external states that intervene in a conflict as reinforcing one side of the conflict, either the state or the rebel group. However, he argues that any external actors becoming involved adds a new dimension to the conflict, since the third party will have interests that are not exactly in line with either party. This outside involvement could make the conflict more intractable because after its

intervention there are three sets instead of two sets of interests that need to be satisfied.⁵ Doyle and Sambanis (2000) argue that peace will be less likely to take hold in conflicts with multiple “factions.” Stedman (1997) contends that conflicts are harder to resolve when there are more “spoilers” who are attempting to block the implementation of a peace agreement.

Each of these analyses suggests that multi-party conflicts are harder to resolve peacefully. None, however, provides a clear theory for how that process plays out. In the next section I examine that relationship more closely and argue that the number of veto players present in a conflict has a large effect on the ability of the conflicting parties to reach a negotiated settlement and is, therefore, a major determinant of civil war duration.

Veto Players and Bargaining

Civil war negotiations are more difficult among multiple veto players for four reasons. First, the presence of additional veto players shrinks the bargaining range of agreements that all actors prefer to continued conflict. Second, information asymmetries are more acute when there are multiple combatants, making it harder for parties to identify that bargaining range. Third, each actor in a multi-veto player conflict has incentives to hold out at the negotiating table in the hopes of getting a better agreement as the last signer. Fourth, negotiations among multiple parties can encounter problems of shifting alliances between groups that can lead to bargaining breakdown. These four

⁵ Balch-Lindsey and Enterline (2000) conduct an empirical analysis that supports Zartman’s argument. They find that external states becoming militarily involved in a conflict and targeting the state lengthens, rather than shortens, the duration of civil wars.

factors together make it significantly less likely that multi-party negotiations will generate agreement. I examine each factor in more detail.

The Bargaining Range

In civil war negotiations among multiple parties with divergent preferences, the set of agreements that all parties will accept is smaller. This is true because, by definition, each veto player brings a distinct set of preferences that need to be satisfied to the table (if they did not do so they would not be counted as separate veto players). Much as adding veto players to governmental bargaining shrinks the range of policies that all prefer to the status quo (Tsebelis 2002), more veto players in civil war negotiations shrink the range of bargains that all parties prefer to continued warfare.

The magnitude of the effect of each additional veto player on the size of the bargaining range depends on how different their preferences are from the existing veto players. A new original group joining the conflict with a completely new agenda will have a bigger effect on the size of the bargaining range than a splinter faction that breaks off from an existing group due to some small disagreement over policy. The size of the bargaining range, then, is affected not only by the number of veto players in the conflict, but also by the degree of preference diversity among them.

By itself, the shrinking of the range of acceptable agreements in multi-party negotiations should not matter. Fearon (1995) demonstrates that as long as any potential agreement exists and parties can overcome private information and commitment problems, then they should always be able to negotiate a settlement. However, the smaller

the bargaining range, *ceteris paribus*, the more likely problems such as information asymmetries are to lead to bargaining breakdown.

Information Asymmetries

The size of the bargaining range is particularly important in multi-party conflicts because these conflicts are also more likely to involve information asymmetries.

Informational approaches to bargaining and war (Smith and Stam 2004, Slantchev 2003 and Filson and Werner 2002) are generally in agreement that the process of warfare reveals information that allows parties to form more realistic assessments of their probability of victory. For example, in a two-party conflict, if one party loses several consecutive battles decisively, it can be pretty sure that its chances of military victory are low. As parties become more realistic about their probability of victory, a bargaining range should emerge and agreement should be reached.

In multi-party conflicts, however, the information revealed by the battlefield is more difficult to interpret. Although these wars are multi-party, individual battles are generally dyadic, that is, taking place between two of the parties to the conflict. For example, in the 1980s the government of Somalia faced challenges from several insurgent groups organized by different clans or sub-clans and based in different parts of the country. It was not until the very end of that conflict that several of these groups banded together to take the capital. A similar process can be seen in multi-party conflicts in the civil war in the 1980s in Ethiopia, and the current conflicts in Colombia and Cote D'Ivoire.

The dyadic nature of battles means that the participants in that battle must use its outcome not only to adjust their probability of victory against each other but also relative to other actors in the conflict as a whole. We can think of each actor as having two separate types of probabilities in multi-party conflicts: the probability that they will defeat each other group independently, and the probability that they will win the conflict as a whole. Participating in a battle can certainly reveal information for the first type of probability, but it is not as clear how it reveals information for the second type.

For example, separate insurgent groups in civil war are often based in different parts of the country and do not fight each other directly. These groups may gain an accurate assessment of their probability of victory against the government, then, but not about their ability to defeat the other non-state veto players militarily. This uncertainty could mean that, while each non-state veto player may agree about the level of concessions needed from the government, they collectively may be unable to agree on a division of these concessions among themselves. In this way, the veto player approach is consistent with informational approaches and suggests that the battlefield in multi-party conflicts reveals information less clearly, leading them to be of longer duration.

An additional informational problem posed by multi-party conflicts is that, for bargaining to break down, only one veto player needs to overestimate its probability of victory to the extent that it cannot see the bargaining range. The probability that one party will be overly optimistic, all else being equal, is greater the more veto players there are.

None of the above discussion is meant to suggest that multi-party conflicts are inherently different phenomena than two-party conflicts. There are still good reasons to

expect that a bargaining range would exist in most multi-party conflicts, that the battlefield reveals at least some information to all parties, and that a bargaining range should eventually be visible to all parties in these conflicts. However, the above discussion does suggest that there are also good reasons to believe that these processes will take longer and that bargaining breakdown will occur more frequently in multi-party than in two-party conflicts, leading wars to drag on longer before agreement is possible.

Incentives to Hold Out

The above discussion suggested a change in the bargaining environment when there are multiple veto players in the conflict, namely that it will be harder for all actors to identify the bargaining range. An additional problem that arises in multi-party negotiations is that each individual actor has an incentive to hold out and be the last to sign an agreement. Because unanimous agreement among veto players is required to end the conflict, the last signer has the ability to force all other parties to their reservation point, or the deal that gives them the lowest expected utility possible while still being preferred to continued warfare. The last signer, then, can expect to get a better deal than all other parties at the table. Since all combatants are aware of this benefit, they all have an incentive to hold out and refuse to negotiate or negotiate harder.

These incentives to hold out are available to all parties. In a sense, then, this mechanism contradicts predictions from the existing literature about which actors are likely to “spoil” peace agreements. Stedman (1997) argues that groups will spoil agreements when they stand to lose out from the peace agreement or when they have extreme preferences that do not allow for compromise. This veto player approach,

however, argues that in multi-party negotiations all parties have an incentive to block agreement to try to get a better deal.

Taken to the extreme, this argument would mean that no party would sign a peace deal since they would all have incentives to be the last signer. Clearly, however, parties do sign deals even in multi-party conflicts. Parties are willing to sign eventually because their ability to hold out is directly affected by their ability to bear the costs associated with continued conflict. This is one area where the differences between civil war negotiations and governmental bargaining are important. In civil war negotiations, stronger veto players will be able to hold out longer because they can pay higher costs of fighting.

Holding out and refusing to participate in negotiations is a strategy that many parties to conflicts have used. In the Mindanao conflict in the Philippines, the Abu Sayyaf Group refused to participate in negotiations held in the 1990s between the government and the other main insurgents, the Moro National Liberation Front and the Moro Islamic Liberation Front, and in fact increased its combat activity while the other parties were negotiating. In the conflict in Tripura in India, one insurgent group, the All Tripura Tribal Front (ATTF) signed a peace agreement in 1993. This agreement failed to end the conflict, however, as the National Liberation Front of Tripura refused to participate in negotiations. Eventually, as the conflict in Tripura continued despite the 1993 agreement, many members of the ATTF returned to fighting and the conflict remains unresolved.

Shifting Alliances

Finally, a fourth problem that only arises in negotiations with more than two parties is the problem of shifting alliances between or among parties. In negotiations among multiple combatants a range of issues are brought to the table that must be addressed in a final settlement. Shifting alliances emerge when parties can form different coalitions on separate issues.

Shifting alliances are particularly detrimental to multi-party negotiations because they prevent parties from forming negotiating “blocs” to help them reach agreement. Since war is costly, parties have an incentive to find ways to achieve their goals through negotiation. One way groups could do this would be for them to ally permanently with others who had broadly similar goals (such as a set of insurgent groups battling the government) and form some overarching institution to negotiate. Shifting alliances prevent groups from achieving such alliances, however, because groups that agree with each other on one issue might agree with other actors on other issues.

The negotiations that began in the Cambodian conflict in 1989 and eventually led to the Paris Peace Accords of 1991 show the type of shifting alliances that can arise in these conflicts. Cambodia in 1989 was “ripe for resolution” for a variety of reasons: Vietnam had pulled out of the conflict and all four factions (including the government) involved in the fighting had lost the support of their external patrons. The conflict was incredibly costly for all parties and no party had a realistic chance of winning militarily. The United Nations Security Council had made peace in Cambodia a priority and agreed to deploy a sizeable peacekeeping force to oversee the terms of an agreement reached.

Finally, the three insurgent groups had all formed one overarching institution and were nominally allied.⁶

Despite all of these favorable conditions, the peace process still took two years. A number of factors made negotiation difficult; however, one important factor was that different parties could ally on different issues. Negotiations in Paris in July-August 1989 broke down over two main issues that produced different coalitions. The three insurgent groups (the Khmer Rouge/PDK, FUNCINPEC and the KPNLF) all agreed to be part of a power-sharing government with an equal seat for the four factions, but the government refused to share power with the Khmer Rouge. At the same time, the Khmer Rouge found itself in opposition to all other parties over its demand that Vietnam withdraw completely before any agreement was reached and over the issue of whether the perpetrators of the 1975-1979 Khmer Rouge-led genocide would be brought to justice under the new government.

Two years after the Paris conference the parties were able to reach a comprehensive peace settlement. One of the main reasons they were able to do this is that the United Nations took the issue of the composition of the transitional government off the table by agreeing to temporarily administer Cambodia itself. Even so, the Khmer Rouge did not implement the peace accord that it signed and conflict between the new government and the Khmer Rouge continued until 1998.

These four factors combine to make negotiations in multi-party conflicts very difficult. The shrinking of the range of acceptable agreements makes it harder to find a specific agreement that all parties will accept. Heightened information problems make

⁶ This description of the Cambodian peace process is drawn primarily from Findlay (1995).

parties more likely to overestimate their chance of victory and less likely to identify that bargaining range. All parties have incentives to hold out so that, even once they see a range of agreements they prefer to continued conflict, they will bargain harder. Finally, parties have difficulty forming coalitions to reduce the number of actors at the negotiating table because of the tendency for shifting alliances. In the absence of decisive military victory, wars continue until agreement is found, suggesting that civil wars with more veto players will be of longer duration.

How Multi-Party Civil Wars End

Despite these problems, wars with multiple parties do end when all parties see a greater benefit to signing and implementing a negotiated agreement than continuing fighting and are able to commit credibly to do so. There are several conditions under which that can happen, and I will discuss three of them briefly here. First, the costs of war may be so high that even with a larger number of parties there is still a range of bargains that all parties prefer to an incredibly costly conflict.⁷ The larger the bargaining range, the easier it is to find an agreement that all parties prefer to continued warfare, and so the war may end.

Second, the international community can become involved and help to facilitate an environment where multi-party conflicts are more likely to end. Walter (2002) argues that international actors can play a key role in allowing parties to overcome commitment

⁷ Although as of 2005 it is premature to declare a definitive end to the civil war in the DRC, if the current peace process does stick in that conflict it is likely that high costs of war will be the major factor leading to its end. While some high-ranking officials in the militias of each external state involved in the DRC war profited highly off the conflict, the economies of Uganda, Rwanda, and Zimbabwe in particular were devastated by their participation. These economic costs may have led those parties to accept a resolution that they would find less acceptable in a less costly war.

problems and in facilitating the implementation of a peace agreement. External actors can affect the possibilities for conflict termination in at least two additional ways. Other states can create a bargaining range by imposing an agreement on the parties to the conflict and then enforcing that agreement. This possibility was seen in the Dayton Peace Accord in Bosnia in 1995 that was largely imposed by negotiators from the North Atlantic Treaty Organization countries and then enforced with 60,000 peacekeepers from that organization. Additionally, external states can expand the bargaining range by offering additional economic or other incentives for parties to negotiate and/or by creating additional costs for the parties to stall at the negotiating table or continue fighting. An example of expanding the bargaining range could be seen at the Camp David accords when U.S. negotiators offered substantial foreign aid to Egypt and Israel in an attempt to induce them to sign an agreement. In terms of increasing costs, the international community has utilized a wide range of options from placing sanctions on the export of resources by rebel groups such as UNITA in Angola to bombing campaigns in the conflicts in the former Yugoslavia.

A third way civil wars with multiple parties can end is that parties to the conflict can drop out, reducing the number of veto players that have to approve any agreement. An example of a party dropping out and facilitating an end to a conflict can be seen in the Vietnam conflict, where the war raged for a long time while the U.S. was present, but ended quickly once it left.

Parties may drop out for a number of reasons. A party that was strong enough to act as a veto player early in the conflict may suffer such heavy losses over the course of the conflict that it is no longer able to continue the conflict in the face of unilateral

settlement, and therefore its signature is no longer necessary. Even if a party still has the capacity to continue conflict, the costs of war may become prohibitively high. An unwillingness to continue to pay high costs of war was a major reason that the United States pulled out of the Vietnamese conflict. Finally, a party may accomplish the goals that it has over the course of a conflict and decide not to continue fighting.

Hypotheses

The theory underlying the veto player approach presented in this chapter leads to several empirical implications that will be tested through quantitative and qualitative analysis. This dissertation focuses on civil war duration, and the preceding discussion suggests three hypotheses about the relationship between number of veto players and the duration of civil war. The first is the main hypothesis that will be tested in this study, and is derived from the above discussion of the effect of multiple parties on the ability to resolve conflicts.

Hypothesis 3: The more veto players there are, the longer the duration of the civil war will be.

In addition to this hypothesis about the number of veto players, the above discussion suggested two further implications about the effect of the preferences and strength of the veto players involved in the conflict. These hypotheses are more tentative because, while all of the above discussion led to the hypothesis about the number of veto players, these hypotheses are each based on one specific element of that framework.

The preferences of veto players should matter because when the veto players in conflict have more diverse preferences, the size of the bargaining range shrinks further.

The smaller the bargaining range, *ceteris paribus*, the harder it is to find an agreement within that range. This suggests the following hypothesis:

Hypothesis 4: The more diverse the preferences among veto players, the longer the duration of the civil war will be.

The strength of veto players should affect duration for two reasons. First, the above discussion argued that one reason multi-veto player conflicts will be longer is that in multi-party negotiations all groups have an incentive to hold out to be the last signer. Groups incur costs from holding out and continuing fighting, however. Groups will be able to hold out longer, then, if they are stronger and can absorb more costs. Second, stronger non-state actors can prevent military defeat by the state, and therefore remain in the conflict longer. Both of these factors suggest the following hypothesis:

Hypothesis 5: The stronger the veto players in the conflict are, the longer the duration of the civil war will be.

Conclusion

Multi-party conflicts generate several different dynamics than make them much more resistant to resolution. Combatants in these conflicts have great difficulty finding a range of agreements that all prefer to continued conflict and, even once this range is identified, negotiations are complicated by each party's incentives to hold out and the formation and breakdown of alliances along different issue areas. This difficulty means that these conflicts tend to drag on, increasing the costs paid by the individual, groups, and societies involved and the danger for the international community as a whole.

Because of these high costs, scholars have an interest in understanding the dynamics of these conflicts. In this chapter, I have presented a parsimonious theoretical model that examines multi-party bargaining through a veto player approach. This approach identifies the conditions which parties must meet to be a veto player, allowing for an empirical analysis of the effect of number and type of veto players on the duration of conflict. Additionally, the veto player approach makes clear predictions about the type of behavior that will be observed in multi-party negotiations.

The remainder of this dissertation is dedicated to testing this approach empirically. In the next chapter, I use a new dataset of civil war duration to conduct quantitative analysis of the five hypotheses developed in this and the previous chapter. In chapter four I conduct comparative analysis of the dynamics of multi-party bargaining using detailed data on the civil war negotiations in Rwanda and Burundi.

Chapter Three

Quantitative Analysis

In the previous two chapters, I surveyed current approaches to the duration of civil war and presented a veto player framework for analyzing the effect of number of parties on conflict bargaining. That discussion led to one main hypothesis: civil wars with more veto players will be longer. Additionally, I derived two other propositions, that civil wars will be longer when the veto players have more extreme preferences and when the non-state veto players in the conflict are strong relative to the state. In this chapter, I test these hypotheses, along with those pulled from the existing literature, using a new dataset.

The discussion in this chapter proceeds in three parts. I begin by describing the dataset and discussing how I measure duration and the number of veto players. In the second section, I present the results of quantitative tests which are designed to provide the ideal test of the veto player approach, and discuss their results. Consistent with the veto player approach, I find that conflicts with more veto players are of longer duration. Third, I conduct a series of other tests which show that this result is robust to the inclusion of other variables and to different specifications of the model. Additionally, these tests show that, while commitment problems are good predictors of the duration of civil wars, measures of the costliness of wars perform badly in all tests. I conclude by discussing the implications of this analysis for further theoretical and empirical work on the duration of civil war.

The Dataset

Case Selection

I test the effect of veto players on the duration of all civil wars begun since 1945. The criteria for identifying civil wars are those used by Version 3.0 of the Peace Research Institute of Oslo/Uppsala Armed Conflict Dataset (ACD).¹ The ACD defines an armed conflict as “a contested incompatibility that concerns government and/or territory where the use of armed force between two parties, of which at least one is the government of a state, results in at least 25 battle-related deaths” (Strand, Wilhelmsen and Gleditsch 2004, p. 3). The ACD divides conflicts into four types: interstate wars, civil wars, internationalized civil wars (conflicts that contain both internal insurgents and external states) and extra-systemic wars (which are almost exclusively anti-colonial conflicts). In this analysis I include all conflicts except for the interstate wars.

The ACD is one of the two main datasets used in analyses of civil war. Much of the existing published analysis of civil war duration has been conducted using some variation on the Correlates of War (COW) project.² The COW project is based at University of Michigan and includes a longer timeframe than the ACD, COW has identified a set of civil wars beginning in 1816 and running through 1997. Additionally, Correlates of War has a different coding rule for inclusion, namely, a civil war must have generated 1000 battledeaths across the course of fighting to be included (rather than 25

¹ The Armed Conflict Dataset and codebook can be found at <http://www.prio.no/cwp/armedconflict/>.

² Correlates of War was the first large dataset containing information on interstate and civil wars. The initial version of the dataset is described in detail in Singer and Small (1982). The data has since been updated as described in Sarkees (2000).

battledeaths in a specific year as in the ACD).³ However, a growing number of scholars are using the ACD.

I use the Armed Conflict Dataset because it is a better fit for this empirical study for four reasons. First, much of the data that I need to code the variables included in the empirical analysis is only available after World War II, negating the longer timeframe advantage of COW. Second, while the 25 battledeath-threshold included in the ACD can be problematic, I control for that by including variables that measure whether a conflict ever achieved 1000 battledeaths or generated 1000 battledeaths in a given year. Third, there is a large amount of data currently being collected on the conflicts included in the ACD, and in particular there are more data measuring factors that vary across the course of the conflicts. This availability of time-varying data is crucial because it allows for more accurate analysis of theoretical approaches such as cost-of-war and commitment theories. Finally, and most importantly, the ACD includes a list of participants for every conflict, something that was not available for the COW project at the time I began assembling this dataset. This participant list is the baseline I use to create the variables measuring veto players that I describe in detail below.

Measuring Duration

Despite the advantages of the Armed Conflict Dataset, it cannot be used for analyses of duration without some adjustment. There are two problems with the ACD that I had to address in order to prepare the dataset for a duration analysis. First, I had to

³ The lowered death threshold means that the ACD has a much larger set of civil wars post-World War II than the COW project identifies in that period.

decide how to deal with breaks in fighting. The ACD differentiates between two types of civil war: those over territory and those over government. Each civil war over a distinct piece of territory is treated as separate, so, there can be multiple separate territorial civil wars within the same country at the same time (as is seen in India and Myanmar).

However, all civil wars over government within the same country are coded as one conflict. For example, two coups in Venezuela thirty years apart are treated as the same conflict.

This coding criterion causes major problems for duration analysis. Clearly, it makes sense to treat a 1966 and 1996 coup in Venezuela as separate, short events, rather than as one long civil war. This problem comes up in other contexts as well, when conflicts such as those in Angola and Chad reignite after a period of peace. One potential response to this problem could be to code any break in fighting as a new war. However, doing so would be inappropriate because the veto player theory argues that multi-party conflicts are harder to resolve definitively, not that there will not be temporary lulls in fighting. To strike a balance between treating clearly separate events as the same conflict and treating brief lulls in fighting as new conflicts I code any conflict that occurs after a twenty-four month break in fighting as a new war.⁴ This coding rule generates a set of 288 separate conflicts.

A second problem with the ACD is that it does not identify start and end dates for conflicts, only the years in which the conflict was active. Using calendar year as the unit of analysis presents problems for duration analysis since a conflict beginning in

⁴ An increasing number of scholars who work with the ACD are using a two-year rule, see for example Gates and Strand (2004) and Buhaug and Lujala (2004). In this dissertation, as I describe below, the unit of analysis is civil war-month and for that reason I use a twenty-four month rule.

December of one year and ending in January of the next would be coded as the same length as one lasting two full years. To address this problem, I use more specific start-and-end-date data generated by researchers at PRIO to code the beginning and end month of each civil war.⁵ Splitting the ACD into conflict months produces a dataset with 18529 conflict months with a range in duration of internal conflict from 1 to 659 months. Appendix A includes a list of these 288 conflicts along with start and end dates.

I use the dataset described above to test the hypotheses about factors affecting the duration of war presented in chapters 1 and 2. In the next section, I discuss ways to think about the duration of events, the statistical methodology that will be employed, and the dependent variable used in this study.

The Statistical Model

Analyzing Duration

In order to analyze the duration of civil wars, I use Binary Time-Series Cross-Sectional (BTSCS) analysis using logit. Traditionally, scholars studying the duration of events such as wars have used one of a set of “Event History” models such as the Weibull regression or the Cox Proportional Hazard Model. Those models test the impact of a set of covariates on the duration of events, given some underlying “hazard function” that determines the baseline duration. Event history analysis is a tool that is gaining increasing use in the field of Political Science to test a whole range of phenomena.⁶

⁵ These data are currently unpublished. However, they are used in an empirical analysis described in Gates and Strand (2004).

⁶ See Box-Steffensmeier and Jones (1997) and Bennett (1999) for good arguments for the greater use of Event History Analysis in the field of Political Science.

Event history approaches can be divided into two categories: spell-based and time-varying. Spell-based approaches model the effect of some treatment at the beginning of the observation on the duration of the events from that point onward. Time-varying analyses can incorporate the effect of changes in the value of covariates on the duration of events. For both, the dependent variable is the duration until some event (such as a war end) is observed. However, in the time-varying analysis, where the unit of analysis is time and not spell, the dependent variable can also be thought of as the probability of observing some event at time t , given the covariates at time t and the fact that the observation has survived to time t .

When the dependent variable is conceptualized this way, event history analysis is identical to a binary time-series-cross-section analysis that controls for duration. The advantage of BTSCS is that it allows for the use of commonly understood models, such as logit, that produce coefficients that are more easily interpretable. Beck, Katz and Tucker (1999) provide software that can be integrated into statistical packages such as STATA for the analysis of BTSCS. In this chapter, BTSCS analysis controlling for the duration of wars is used to analyze the impact of time-varying covariates on the probability of a war ending in any given month.⁷

Dependent Variable

The dependent variable is a binary variable coded “1” for the month in which a conflict ended and “0” for every other month. Recall that for this dataset a conflict end is

⁷ Because hazard models are the more traditional way to analyze the duration of events, in unreported tests I also ran the analyses presented in this chapter using a Weibull regression and a Cox proportional hazard model. The results of those analyses were very similar to the results reported here.

identified if there is a twenty-four month break in fighting, so a “1” on the dependent variable means that that month was followed by at least a twenty-four month period in which conflict did not reignite. I will examine the effect of a number of covariates, including those controlling for duration, on the probability of a war ending in a given month through a Logit model.

This dataset includes several civil wars that were ongoing as of the end of 2003. In duration analysis the problem of ongoing observations is referred to as “censoring.” We know that these cases have not ended; however, we do not know how long they will last. Censoring is not a problem for the BTSCS analysis. In these cases, the BTSCS analysis simply treats the final month for which there is data (December 2003) as another in which the war does not end, and it does not affect the analysis that there is not a war end for every conflict.

With this dependent variable, interpreting the effect of the variables included in the model can be a bit confusing. When reading the results, remember that variables with a negative coefficient actually prolong civil wars. For example, if the hypothesis presented in chapter two is correct, there will be a negative association between the number of veto players and the probability of war ending in any given month. This association would mean that wars are lengthier because, if wars are less likely to end in any specific month, they will on average continue longer until they do end.

All of the above discussion has focused on explaining the dataset and statistical model to be used in this study. In the next section, I describe how I have operationalized two measures of number of veto players to test hypothesis 3.

Measuring Veto Players

The main hypothesis that will be tested in this quantitative analysis is that conflicts with more veto players are longer. Creating a measure of veto players to test this hypothesis was an involved process because there currently exists no measure that could be used to proxy that concept. In this section I will describe in detail how I created the measures of veto players that will be used. I go into detail to make the coding as transparent as possible and to increase its applicability to other researchers.

The first step in determining which parties were veto players was to identify the set of participants in each conflict. Version 3.0 of the ACD identifies a list of participants for every year of every conflict. These participants are further divided into two sides: “Side A” actors can be seen as largely on the side of the government and “Side B” actors are opposed to the government. I decided to use this list of participants rather than creating a list from scratch for two reasons. First, it made an already substantial data-collection effort more manageable. Second, and more importantly, it made the collection more objective, I started with an existing list of parties and so had less possibility of influencing the data.⁸

The ACD lists 770 separate conflict participants in the 288 conflicts. I needed a way of determining whether each of these actors was a veto player and, if so, during what time period. I began with several coding rules that made many of the conflicts clear. First, no conflict can have less than two veto players. This rule holds true because veto players are defined as those actors who can block settlement and continue the war unilaterally

⁸ There were some a set of conflicts where the ACD did not identify the participants. For example, there are conflicts listed as “Burma vs. Burmese Communist Party and leftist organizations,” “Burma vs. Arakan insurgents” and so on. For those conflicts, I did use historical sources and case histories to build a list of participants. For all others, I used the participants identified in the ACD.

and if there are not at least two actors who meet that definition, the war would end. For every month of any conflict in which there were only two parties identified in the ACD, both were coded as a veto player.

Second, if there was only one participant identified on either Side A or Side B, that actor was automatically coded as a veto player. Each side must have another actor it is fighting against, and so each side must by definition have at least one veto player.

Third, the government of the state in civil war was automatically coded as a veto player. The government is by definition a veto player because by virtue of being the government it can block settlement until it is defeated militarily.

Once I had applied these criteria, I was left with a set of 304 conflict participants who were potential veto players. Since there was no clear existing criterion for determining whether parties were veto players, I created two separate measures based on separate coding rules which I describe in detail below. The first coding rule involved more strict criteria to be counted as a veto player, while the second rule was more lenient. In order to code whether each of these parties met each coding rule, I needed to determine whether they met the three criteria of autonomy, coherence, and viability identified in chapter two.

Autonomy

Parties were coded as autonomous if they had a separate agenda from the other groups in the conflict. I used two sources to identify the goals of the group in conflict. First, I used historical sources such as Keesing's Record of World Events and news reports obtained through Lexis Nexis to identify the demands that groups made. Second,

I used histories of the cases to examine how they identified the goals of the party in conflict. I used both of these methods in combination because of problems arising from only looking at group demands.

Using group demands is complicated by two factors. First, external states intervening in conflicts rarely state their demands explicitly. To determine whether external states had autonomous preferences in those cases, therefore, I used historical explanations for their involvement in the conflict. For example, it is widely accepted that South Africa intervened in African conflicts in the 1970s and 1980s to oppose anti-apartheid governments or to prevent them from coming to power. In this way, their goals in the conflict were separate from those of the internal actors that they were supporting.

Second, the demands that groups make do not necessarily reflect their actual preferences. In making demands, groups are often trying to gain support from local constituencies and use demands strategically to do so.⁹ For example, virtually every insurgent group is, at least rhetorically, pro-democracy until it wins the conflict outright and does not have to share power. This demand for democracy may have little to do with the actual preferences they need to have satisfied to sign a peace deal.

Examining a combination of demands and historical explanations of combatants' reasons for participating in conflicts allowed me to determine if groups had separate preferences from the other actors in the conflict. For the vast majority of cases, it was clear that the groups were autonomous. Most groups in civil war make at least somewhat different demands from the other groups. There were a small number of cases,

⁹ Jenne (2004) demonstrates that self-determination groups choose their demands strategically to build support from local constituencies or from international actors who have an influence on the conflict.

particularly those involving external states intervening on the side of the government, in which it was obvious that groups did not bring separate preferences and these groups were automatically coded as non-veto players.¹⁰ There remained a set of fourteen cases, however, that were difficult to code.¹¹ Rather than deciding one way or the other whether these groups were autonomous, I left them as unclear and they are included as autonomous in the lenient measure of veto players and not in the strict measure.

Cohesiveness

Parties were coded as cohesive if, for the most part, they were able to avoid fractionalization during their participation in the conflict. This coding was complicated by

¹⁰ For example, the ACD lists all the member states of the North Atlantic Treaty Organization (NATO) as separate participants in the Serbia-Kosovo conflict of 1999. It appears, however, that NATO should be treated as the veto player, rather than seeing Canada and Denmark as separate actors who might block a peace deal. Other than cases of coalitional intervention like that, there were 11 actors that were coded as non-autonomous. They were the Palestinian National Authority in 2000-2002 (Fatah is also listed as a conflict participant in this period and was the leadership of the PNA), the Ugandan Popular Front in Uganda (an ineffective umbrella organization that was unable to coordinate any activity from its member groups); Zimbabwe in Mozambique in 1989-1990 (was directly supporting the government against RENAMO); Senegal in the Gambia (directly supported the government against an insurgency); Zaire in Rwanda (intervened at the direct request of the Rwandan government and left shortly thereafter); the Armenian National Movement in the Soviet Union in 1990-1991 (along with Armenia, wanted independence from the Soviet Union and the Nagorno-Karabakh); Armenia in Azerbaijan (had the same goals at the Republic of Nagorno-Karabakh); the Serbian Irregulars in Croatia and Bosnia-Herzegovina (when Yugoslavia is also listed as a separate participant, the Serbian irregulars were directly controlled by the Yugoslav government); and Botswana and South Africa in Lesotho (intervened only to support the government).

¹¹ These cases were Australia and New Zealand in the anti-colonial conflict in Malaysia (probably did not have separate preferences from the United Kingdom, who they were supporting), the United States in Cuba in 1961 (it was unclear if the US had a different agenda than the Cuban refugees it was backing); the United Kingdom in Oman in 1957 (may have only been backing the government), the United Kingdom in Malaysia in 1958-1960 (may have only been backing the government); Thailand in Laos (it was unclear if they brought separate preferences to the conflict than those of the United States); France in Gabon in 1964 (appears to have only intervened to support the government); the Kampuchean United Front for National Salvation in Cambodia (was directly created and backed by the Vietnamese government and it was unclear if it had separate preferences); Russia and Uzbekistan in Tajikistan (not clear if they had a separate agenda than the government of Tajikistan); Chad in the Republic of Congo (unclear if it had separate preferences from the government); Senegal and Guinea in Guinea-Bissau (again, unclear if they brought a separate agenda to the conflict or fought only to defend the government), and Libya in Central African Republic (probably intervened only to support the government).

the fact that, across protracted conflicts, many groups see factions break off and start fighting independently. In fact, as discussed in chapter two, that is one of the reasons many conflicts are multi-party. Fractionalization only prevents parties from being veto players if they are no longer able to function as a unified group.

In chapter two, I mentioned the Rally for Congolese Democracy (RCD) in the DRC as an example of the importance of organizational cohesiveness. It is worth returning to that example here to make the coding criteria clearer. The RCD was a group formed in 1998 made up of an array of Congolese opposed to the government of Laurent Kabila. The RCD was primarily formed by Rwanda to give its invasion of DRC a domestic face. The various leaders in the RCD, however, had great difficulty agreeing on anything. Additionally, Rwanda and Uganda, both of whom were heavily involved in the DRC, backed separate leaders within the organization. In March 1999, the nominal leader of the RCD, Ernest Wamba dia Wamba, moved his headquarters from Goma to Kisangani, leading to a split in the organization. Two groups then fought the conflict, RCD-Goma, which was backed by Rwanda, and RCD-Liberation Movement (RCD-ML), which received Ugandan support.

The RCD is not coded as cohesive in this project. However, the reasoning for this coding is not that the organization split into two separate factions. Rather, it is not coded as cohesive because the different leaders within the RCD could not agree on anything other than their opposition to Kabila's government. Once the split occurred, the RCD-ML still experienced some fractionalization, but was generally able to operate as a cohesive organization. Therefore, it is coded as cohesive from 1999-2001 (the last year of

the war). RCD-Goma, however, continued to be completely unable to maintain an organizational structure and splintered further. It is never coded as cohesive.

To measure cohesiveness, then, I examined whether the leadership of the group remained relatively constant over the course of the conflict and whether the group had a consistent set of demands that it made through the conflict. Again, for the vast majority of cases measuring cohesiveness was quite clear cut. Insurgent groups tend to maintain the same leadership even over long participation in conflicts. Jonas Savimbi, for example, led the Union for the Total Liberation of Angola (UNITA) for over thirty years until his death in 2002, and the group ceased the armed struggle almost immediately following his death. John Garang (the leader of the Sudanese People's Liberation Movement), Foday Sankoh (head of the Revolutionary United Front in Sierra Leone), and Joseph Kony (leader of the Ugandan rebel group Lord's Resistance Army) are other examples of leaders who stayed at the head of their rebel organizations for over a decade. Additionally, while it is not clear what criteria the ACD used for determining which participants were included, it does seem that more marginal and fractionalized groups were generally not included.

Again, however, as with autonomy, there were a few exceptions. There were five groups that I coded as non-cohesive. One is the RCD, for the reasons described above. A second is the Ugandan Popular Front, an alliance of Ugandan insurgents formed in the 1980s that was never able to exercise any control over its constituent groups (which are listed separately in the ACD). Third, the ACD lists "Eritrean Liberation Front factions" as a participant in the Eritrean self-determination conflict, and those factions cannot be seen as having one coherent leadership structure or set of demands. Fourth, while the

Front for the National Liberation of Chad (FRONILAT) was cohesive during the 1970s, by 1980 it had become so fractionalized it had ceased to function as a coherent organization. Fifth, the ACD identified a military faction in Uganda in 1978-1979, but I could not find evidence that such a faction opposed to Idi Amin actually existed. Amin had frequent purges of the military but it does not appear that there was a coherent opposition organization.

Finally, there were two conflicts for which I could not identify any cohesive Side B actor. The ACD lists a conflict in 1947 between China and “Taiwanese insurgents” and a civil war from 1949-1964 between Israel and “Palestinian insurgents” (this period is prior to the emergence of the Palestine Liberation Organization in 1965). In neither case could I find evidence of groups that appeared cohesive enough to have been potential bargaining partners with the states. Rather, it appears that these periods were characterized by riots and/or sporadic violent acts. Since I could not identify any cohesive groups, I did not code those cases either way and they are excluded from the analysis.

Viability

Viability was the criterion that presented the greatest challenge for coding. A party is viable if it could continue the conflict unilaterally in the face of agreement by all of the other parties. Coding whether or not a party is strong enough to do so, however, is difficult.

In order to determine whether parties were viable, I collected data on a range of factors identified in the literature on insurgency (such as Fearon and Laitin 2003, Collier

and Hoeffler 2004, etc.) as making it easier for parties to carry out an insurgency.¹² These factors included troop strength, the terrain the group operated in, whether the group used forced conscription, whether it committed violence against civilians, whether it had access to lootable resources, and so on. In the end, after examining the data, six main criteria were used to determine whether parties were viable: how many troops the group had relative to the government, whether it operated in mountainous or forested terrain, whether it had access to lootable resources, whether it was the dominant group in the region of the country where it operated, whether it had participated and had a strong showing in elections and whether it received external support.

Once I had this data for each potential veto player, I needed a way to make a composite indicator of viability. I did this by assigning points to each group based on their coding on each of the above measures. The first points were awarded based on troop strength. Groups were given four points if they had a number of troops equal to or larger than the government, three points for 50% or more, two points for 10% or more, and 1 point for 1% or more. The one-percent threshold might seem quite low, however, there were 65 actors who received no points for troop strength because they had less than 1% the troops of the government.

¹² I used three main types of sources to gather this information. The first were a set of online sources with information specifically on conflicts such as Minorities at Risk, the Uppsala Conflict Database and reports from the International Crisis Group. These sites were particularly useful for obtaining information on periods of conflict since 1990. Second, I used news services such as Keesing's Record of World Events, Lexis Nexis, and the New York Times online archives which provided historical information on many conflicts. Finally, I used case-specific sources such as scholarly articles and books to fill in any missing information.

Next, I assigned points to the internal combatants based on the other variables. Groups got one point each if they operated in forested or mountainous terrain.¹³ The logic behind using terrain is that rough terrain makes it easier for groups to evade capture by the state, and therefore smaller parties can remain viable longer if they are based in mountainous or forested terrain. Combatants received one point if they represented an ethnic group that was dominant in the region where they operated, since that would suggest that they had higher popular support.¹⁴ Since “lootable resources” can help groups fund rebellion, those that had access to such resources got a point.¹⁵ If parties had participated in elections and made a strong showing, they received a point.¹⁶ Finally, since external support can enable groups to continue rebellion longer, I gave each actor one point if it received substantial support from external states. This support could not simply be rhetorical, but had to involve actions such as direct military support, a state allowing the group to base on its territory, or significant financial support.¹⁷

The terrain, group dominance, lootable resources, and election performance indicators do not work for external states intervening in conflict. To determine the points awarded to external states, I used the same troop criteria as for the internal groups.

However, I gave each external state one extra point, based on the logic that states that

¹³ To measure this variable, I used terrain data from Buhaug and Lujala (2004) who have mapped the terrain in the zone of each country that is in conflict. I awarded groups points if the conflict zone was more than 50% mountainous and/or forested.

¹⁴ The main source I used to code this criterion was Minorities at Risk (MAR), which generally indicated whether groups were a majority in the area in conflict. If I could not code ethnic dominance from MAR, I used historical sources and case histories to do so.

¹⁵ In order to code access to resources I started with data from Buhaug, Lujala and Gates (2002) who measure whether there are drugs, diamonds or gems in the zone of each country in conflict. I then used historical sources to see if I could find references to which groups controlled resources for each conflict that had them.

¹⁶ To determine if groups had participated in elections, I used Keesing’s Record of World Events, which has good historical reporting on elections throughout the world.

¹⁷ I coded external support using data from historical sources and case histories.

can send their armies to another country are generally better organized relative to domestic insurgent groups. I awarded an additional point if the external state was a major power, since that is an indicator of how strong the military is. The logic here is that the United States sending 1,000 troops to a country would have a bigger impact than Namibia sending 5,000 to that same country, and therefore major power military interventions are weighted higher.

After completing this coding I had a score for each potential veto player (ranging from 0 to 7) that was designed to indicate how strong they were. For example, the Revolutionary United Front (RUF) in Sierra Leone in 1991-1996 has a score of four because it had more than 10% the troop strength of the government (two points), controlled diamonds, and received direct military support from Liberia. The Alliance for Democratic Forces (ADF) in Uganda has a score of two because it had 1% the troop strength of the government and received support from the Sudan. Force Obote Back Again (FOBA) in Uganda in the 1980s, meanwhile, gets a score of zero because it had less than 1% the troop strength of the government and no other attributes facilitating insurgency. The assumption with this coding of viability is that the RUF is more likely to be a veto player than the ADF, which is more likely to be a veto player than FOBA.

After coding this score for each group, I then established thresholds for determining what a group needed to be viable. Since I am creating two measures of veto players, I have two thresholds for the viability measure. To be coded as viable under the strict measure, a group must have 4 points. That means that all groups that have more troops than the government are automatically coded as veto players, as are smaller groups that have other attributes facilitating insurgency. For the lenient measure, I use the

threshold of two. That means groups that have less than 10% the troop strength of the government only meet the lenient criteria if they have other attributes facilitating insurgency.

The above coding criteria are clearly somewhat subjective. Any operationalization of the veto player framework would by necessity be so. Unlike governmental negotiations, there is no definitive way to identify veto players because there is no constitution enshrining who must sign a peace agreement to end a war. However, this subjectivity is not a significant problem for three reasons.

First, determining who is a necessary signatory to a peace agreement for a war to end and therefore who must be a participant in negotiations is a subjective process for parties in conflict as well. Governments, for example, must decide to which armed groups they must grant concessions for conflicts to end. To determine this, governments are likely to rely on the same indicators I use to determine which are strong enough to justify granting concessions. Groups that have more troops, operate in rough terrain, control lootable resources, receive external support, represent a dominant ethnic group or have a strong showing in election, are more likely to end up at the negotiating table and be treated as veto players whether or not they could actually continue the conflict unilaterally.

Second, in the quantitative analysis described below, I can include a measure of number of parties that is not based on these criteria. That measure is not as good an operationalization of the veto player theory presented above. However, it allows for testing that theory with a measure that was constructed differently and if it is found to be

significant as well, it provides support for the veto player argument based on an objective measure.

Finally, I address the issue of subjectivity by making the data I have collected and the coding I have used available for other researchers. That data availability will allow for replication of the results and also gives scholars the ability to recode the viability measure using different criteria to see if the results are robust.

Applying these coding rules allowed me to create two variables that measured the number of veto players using the strict and lenient criteria for each civil war month. The strict measure of veto players includes a count of all actors who were definitely autonomous, definitely cohesive, and had at least four points on the viability measure described above. The lenient measure includes a count of all actors who were possibly autonomous, possibly cohesive, and had at least two points on the viability measure. These two variables will be used to test the impact of veto players on duration.

Analyzing the Effect of Veto Players

The Number of Veto Players

The main proposition in this dissertation is that conflicts with more veto players will be of longer duration. I have described above the dataset, statistical model, and measurement of veto players that I will use to test that proposition. Here, I present the model that is the ideal test of this relationship. The ideal model includes the measurement of number of veto players and any other potential factors that could affect both the number of veto players and the duration of civil wars. If, with the inclusion of these other

factors, veto players achieves significance, that is the strongest statistical support for the theory.

Several factors could potentially be associated with the number of veto players and the duration of civil wars. To begin with, the Armed Conflict Dataset includes several coups or attempted coups as internal conflicts. Coups are virtually always two party, and they are almost always short. Consequently, whether a conflict is a coup is likely to be associated with both the number of veto players and the duration of civil war. To control for this effect, I include a dichotomous variable measuring whether the conflict is a coup.¹⁸

Another factor that could be associated with veto players and duration is the population of the country. Population could drive duration because conflicts remain in the dataset as long as they continue to generate 25 battle deaths a year. It is presumably easier for a conflict to generate 25 battle deaths in a year in a country with a large population. India, for example, has a large number of long-running low intensity conflicts. At the same time, the size of the population could be an indicator for how heterogeneous preferences are in the society. As described in chapter two, diverse preferences are the main factor leading to multiple veto players, and therefore population is likely associated with both the independent and dependent variables. I include a measure of the natural log of each country's population obtained from Gleditsch (2002).

Another factor that is often hypothesized to affect preference heterogeneity is the degree of ethnic division in the country. Additionally, some scholars have argued that

¹⁸ Determining which conflicts in the ACD were coups was relatively straightforward. For some conflicts the Side B actor was listed as "Military faction" and all of those cases were coded as coups. For other conflicts, I used news sources to determine if the conflict was the result of a faction of the military or government seeking to overthrow the government and, if so, I coded those conflicts as coups.

ethnic conflicts are more difficult to resolve than non-ethnic civil wars, and so wars in ethnically divided societies might also be longer. To control for the ethnic composition of the society, I include the Ethnolinguistic Fractionalization Index (ELF) obtained from Fearon (2003).¹⁹

Table 1 presents the results of two logistic regressions including the separate measures of veto players and these three dichotomous variables. Additionally, the models reported include the controls for duration discussed above. Model 1 uses the strict measure of veto players, and model 2 is based on the lenient measure.

Table 1: The Effect of Number of Veto Players

	Model 1	Model 2
Strict Veto Players	-1.163** (0.37)	
Lenient Veto Players		-0.349* (0.148)
Coup	2.155** (0.211)	2.152** (0.213)
Log Population	-0.084 (0.06)	-0.101 (0.064)
Ethnic Fractionalization	0.055 (0.301)	0.133 (0.314)
Warmonths [^]	-0.045** (0.011)	-0.045** (0.011)
Observations	15932	15932

Note: Reported are coefficients with robust standard errors in parentheses.

*significant at the .05 level, **significant at the .01 level (two-tailed test)

[^]Warmonths measure includes cubic splines, I do not report the coefficients for those splines here because they do not have an interesting substantive interpretation.

¹⁹ ELF is a measure of the probability that two individuals drawn at random from the population of a country will be of different ethnic groups. So a score of 0 would mean that the country was completely homogeneous and a score of 1 would mean that the society was completely heterogeneous.

The results in Table 1 show a relationship between higher numbers of veto players and longer civil wars. The signs on both veto players measures are negative, meaning that civil wars are less likely to end in months in which there are higher numbers of veto players. Both variables achieve significance at the .05 level, and the strict measure reaches a substantially higher threshold.

As expected, a conflict being a coup is a predictor of shorter duration, and is highly significant. None of the other variables achieved significance, except for the controls for duration. The population of each country has a negative sign, as predicted, however, it misses traditional thresholds of statistical significance. The ethnolinguistic fractionalization of the country seems to have no relation with the duration of conflict.²⁰

These results show support for the theoretical framework presented in this dissertation. It does appear that civil wars are longer when more veto players are involved. The models reported in Table 1, however, do not allow us to evaluate how big of an effect veto players have on duration, only whether there is a significant correlation. Table 2 reports the results of two models using CLARIFY²¹ to generate predicted probabilities of the effect of changes in the values of the independent variables on the probability of seeing a war end in a given month, holding values constant on the other

²⁰ In addition to this traditional ethnolinguistic fractionalization measure, I ran models which substituted two other measures from Fearon (2003). Fearon has coded an ethnic fractionalization index himself that updates the ELF measure, as well as a cultural fractionalization measure. Neither achieved significance and the results on the other variables were robust to their inclusion. Additionally, I included a squared term of ethnic fractionalization to test if the relationship between ethnic division and duration is curvilinear. The ethnic fractionalization and squared term had a larger coefficient than when simply the linear term was included, however, they still failed to achieve statistical significance at even the .1 level and again the coefficient and standard errors of the other variables did not change much.

²¹ CLARIFY is a program developed by Tomz, Wittenberg and King (2003) that can be used to estimate the effect of changes in the value of one independent variable on the dependent variable while holding all other variables constant at some value. Its application to analyses in Political Science is discussed in King, Tomz and Wittenberg (2000).

independent variables. The results are based on two models including the same variables as in Table 1. Table 2 reports the results of holding every other variable constant²² and changing the value of continuous variables from the 10th to 90th percentiles²³ and for dichotomous variables from no to yes.

Table 2: CLARIFY estimates of Effect of Changes in Variables

When this variable...	Shifts from ... to ...	Change in Probability of Observing a War End in that Month	Change in Probability of Observing a War End in that Month
Strict Veto Players	Two to Three	-0.65% (-0.98% -0.31%)	
Lenient Veto Players	Two to Four		-0.499% (-0.87% -0.12%)
Coup	No to Yes	6.93% (4.02% 10.94%)	7.31% (4.35% 11.15%)
Natural Log of Population	8.365 to 12.276	-0.33% (-0.83% 0.1%)	-0.404% (-1.02% 0.1%)
Ethnic Fractionalization	0.179 to 0.886	-0.05% (-0.35% 0.47%)	0.09% (-0.34% 0.54)

Notes: Effects indicate first differences with all continuous variables held constant at their median values and categorical variables at their modal value. Boldface indicates that the 95% confidence interval around a simulated first difference did not contain zero, signifying statistical significance. Based on a logit model estimated in Stata 8.0, with first differences drawn from 1000 simulations performed by CLARIFY (Tomz, Wittenberg, and King 2003). 15932 observations.

The results in Table 2 show that increasing the number of veto players, using either measure, has a substantial effect on the duration of civil war. Adding one veto player under the strict criterion leads to a 0.65% decrease in the probability of war ending in any given month, give or take about 0.3%. This number may seem low; however, since

²² Continuous variables are held constant at their median and categorical variables are held constant at their mode.

²³ Typically, in analyses such as these, continuous variables are varied by one standard deviation or from the 25th to 75th percentile. However, the veto players variables are skewed to the right, meaning that the 25th and 75th percentile have the same value, 2. For that reason, the 10th and 90th percentile are used.

the mean duration of civil wars in the dataset is 68 months the baseline probability of a conflict ending in any given month is quite low. Going from the 10% to 90% percentile in the lenient measure of veto players has a slightly smaller effect, and with a larger confidence interval.

Further analysis reveals that increasing the number of veto players further has a more dramatic effect. I simulated the difference of going from 2 to 6 strict veto players and 2 to 9 lenient veto players (the maximum for each variable), holding the other variables constant. In both cases, going from the minimum to maximum number of veto players decreased the probability of a war ending by about 0.9%.²⁴ With higher numbers of veto players decreasing the already low probability of a war ending in a month by almost 1%, this shows that multiparty conflicts are substantially longer.

Table 2 shows as well that, as expected, coups are substantially shorter civil wars. A conflict being a coup reduces the probability of it ending in a month by about 7%, give or take about 3%. As in the models reported in Table 1, neither population size nor the ethnic fractionalization of a country has a statistically significant effect on the probability of war ending.

The results in Tables 1 and 2 provide strong support for the veto player approach advanced in this dissertation. They show that there is a statistically significant correlation between the number of veto players and the probability of a war ending, and that adding additional veto players to conflicts leads to substantially longer civil wars. These models

²⁴ Going from 2 to 6 veto players using the strict measure decreases the probability of observing a war end by 0.94%, give or take about 0.3%, going from 2 to 9 veto players using the lenient measure decreases the probability of observing a war end by 0.87%, give or take about 0.4%.

are the best statistical test for the approach because they include all potential factors that we have a theoretical reason to believe could be driving that relationship.

The Strength and Type of Veto Players

The main theoretical proposition arising from the discussion in chapter two was that conflicts with more veto players will be longer. However, the theoretical discussion also led to two additional hypotheses about the effect of the nature of veto players in the conflict. Hypothesis 4 holds that civil wars will be longer when veto players have more diverse preferences because the bargaining range will shrink further. Hypothesis 5 argues that conflicts will be longer when the veto players are stronger (in relation to the state), because that strength will allow them to pay the costs of holding out at the negotiating table longer.

Testing Hypothesis 4 empirically is difficult because there are not clear measures of the preferences of groups. In veto player analyses in American and Comparative politics, the ideological preferences of political parties are often measured using a generally-accepted left-right continuum. However, within the literature on civil war, there is no such standard. For this project, one way to get at some of the effect of preference diversity is by seeing if different types of veto players have different effects. Recall from chapter two that parties in conflict can generally be divided into four groups—the government, “original” insurgent groups, splinter factions that have broken off from original groups, and external states that have intervened in the conflict. Of all of those groups, splinter factions are least likely to bring more diverse preferences to the conflict, because they were originally part of another group.

A crude test of hypothesis 4, then, could be conducted by seeing if different types of veto players have different effects. I used the lenient veto player coding (because it gives a wider range of veto players) to code the type of non-government veto players active for every month of conflict. For example, in Angola from 1975-1977 the veto players were the government, UNITA, the FNLA, Cuba, and South Africa. That period is coded as having two original groups (since UNITA and the FNLA arose independently), two external interveners, and no splinter factions. Based on this coding, I can test if different types of veto players have different effects on duration. If splinter factions prolong civil wars less than original groups or external states, that may give support to Hypothesis 4. Table 3 presents the results of a model that includes the three variables measuring types of veto players along with the other variables from Table 1.

Table 3: The Effect of Type of Veto Players

Number of Original Groups	-0.393* (0.194)
Number of Splinter Factions	-0.604 (0.467)
Number of External States	-0.26 (0.196)
Coup	2.144** (0.214)
Natural Log of Population	-.096 (0.064)
Ethnic Fractionalization	0.116 (0.321)
Warmonths [^]	-0.045** (0.011)
Observations	15932

Note: Reported are coefficients with robust standard errors in parentheses.

*significant at the .05 level, **significant at the .01 level (two-tailed test)

[^]Warmonths measure includes cubic splines, I do not report the coefficients for those splines here because they do not have an interesting substantive interpretation.

As can be seen from Table 3, the only measure of veto players that has a significant association with duration is the number of original groups. As expected, adding more original groups to the conflict prolongs civil wars. Although the other types of veto players miss standard thresholds of significance, on all three variables the sign is negative, meaning that adding veto players of any type to the war leads to longer conflicts. The results do not conclusively provide support for Hypothesis 4, however, because although the splinter factions variable did not achieve statistical significance, its coefficient is actually larger (in absolute value) than the other two coefficients. That suggests that adding splinter factions might actually have a greater effect of prolonging civil wars than the other types of veto players; however, that result must be interpreted

with caution because the standard error on that variable is so large. The large standard error is likely the result of the low number of splinter factions among the actors in the dataset. Of all potential veto players, only 21 are splinter factions, with 96 external interveners and 178 original groups.

Testing Hypothesis 5 is easier given the way the measure of veto players was coded. I have created two variables which measure the strength of veto players in the conflict. The first variable measures the average strength (in terms of the point system used to measure viability) of all veto players in the conflict except for the government. This variable should approximate how strong all the groups are, which could have two effects. Stronger groups can hold out longer at the negotiating table. At the same time, stronger groups can avoid defeat by the state. Both should prolong civil wars. The second variable measures the point total of the strongest veto player. Having even one very strong veto player in the conflict can prolong conflicts because that party, at the very least, can hold out and block agreement. Table 4 presents the results of two regressions that include each of these measures and the covariates from Table 1 and involve all conflict months in which there were more than two veto players (by the lenient measure). I limit the analysis to multi veto-player months since it is in those situations that I expect parties to hold out, and therefore when strength should matter.

Table 4: The Effect of Strength of Veto Players

	Model 1	Model 2
Average Strength	-0.415 (0.253)	
Strongest Veto		-0.405* (0.171)
Coup	1.479 (1.03)	1.128 (1.038)
Natural Log of Population	-0.384 (0.264)	-0.456 (0.262)
Ethnic Fractionalization	0.924 (0.632)	1.078 (0.617)
Warmonths [^]	-0.015 (0.036)	-0.012 (0.036)
Observations	4019	4019

Note: Reported are coefficients with robust standard errors in parentheses.

*significant at the .05 level, **significant at the .01 level (two-tailed test)

[^]Warmonths measure includes cubic splines, I do not report the coefficients for those splines here because they do not have an interesting substantive interpretation.

The results in Table 4 show some support for the argument that stronger veto players lead to longer conflicts. In model two, the presence of a stronger veto player in the conflict leads to longer civil wars. The results in Table 4 suggest that a higher average strength among veto players also prolongs civil wars, however, we cannot say that definitively because the variable does not achieve significance at the .05 level. These results, then, provide some support for Hypothesis 5, but we cannot reach a definitive conclusion.

The above analysis gives us reason to think that the strength of veto players may matter, but cannot tell us about the effect of preference diversity. However, it is not possible to test either of these hypotheses very well using the data as it currently stands. I will examine the effect of preference diversity and veto player strength in greater detail by

analyzing the behavior of groups at the negotiating table in the Rwandan and Burundian civil war negotiations in the case studies presented in chapter 4.

Before turning to that qualitative analysis, however, further examination of the correlation between number of veto players and the duration of civil war identified above is needed. In the next section, I conduct several tests which further analyze this relationship. These tests fall into two types. First, I add additional variables to the models presented in Table 1 to determine if those results are robust to the inclusion of other factors. This analysis also allows us to evaluate the competing hypotheses presented in chapter one. Second, I conduct a series of tests to determine if the relationship identified is the result of the measurement of veto players or some aspect of the dataset used. In all cases, the tests show strong support for the veto player approach.

Further Testing

Competing Approaches

In chapter one, I discussed two alternate hypotheses drawn from the existing literature on the duration and termination of civil war: that more costly wars will be shorter and that wars will be shorter when parties are able to overcome commitment problems. In this section, I conduct multivariate logistic regressions that include measures of the costliness of war and commitment problems along with the number of veto players. These statistical analyses are designed to do two things: to see if the finding in Table 1 that more veto players lead to longer civil wars is robust to the inclusion of more variables, and to evaluate whether war costs and commitment problems are statistically associated with duration when we control for the number of parties. I present these

results in Table 5. First, however, I briefly discuss the variables included to measure war costs and commitment problems.

Identifying empirical indicators of the costliness of war is difficult. War cost theories hold that when wars are more costly, parties have a greater incentive to try to end them and so they will be shorter. Measuring the costliness of war to the individual decision-makers involved in the conflict is difficult, however, there are several measures that could proxy those costs.

I include four proxies of war costs. First, I use a measure of the annual battledeaths collected by Lacina and Gleditsch (2005). The number of people killed in battle is the most commonly used proxy for the costliness of war. Battle deaths are visible and can indicate how costly the war is for parties. Additionally, battle deaths directly impact the ability of groups in conflict to continue to wage war since they decrease their fighting force. Second, terrain is often theorized to affect the extent to which the state can impose costs on insurgents, and so groups that operate in rough terrain should find war less costly. I include measures of the percentage of the zone of the country in conflict that is mountainous or forested collected by Buhaug and Lujala (2004).

Third, scholars have argued that conflicts are less costly when lootable resources are available. The mechanisms specified for the relationship between resources and duration differ, for some theorists they can make war beneficial because parties control resources they would not be able to in peace. For others, lootable resources simply give parties an easy way to finance insurgency, and so offset those war costs. In either case, the expectation is that wars will be longer when parties control lootable resources. To test

this approach, I include a variable measuring whether there are lootable resources in the conflict zone obtained from Buhaug, Lujala and Gates (2002).

A final indicator of the costliness of conflict is the level of development of the country in civil war. Again there are competing explanations for this relationship. Collier and Hoeffler (2004) see gross domestic product (GDP) per capita as a proxy for the opportunity costs to potential rebels of abandoning their livelihood and taking up arms. Fearon and Laitin (2003) see GDP as an indicator of state strength, or the ability of the state to impose costs on those engaging in rebellion. In either case, wars are seen as less costly for insurgents in poorer countries, and so I include a measure of annual real GDP per capita from Gleditsch (2002).

Identifying indicators of commitment problems from the existing literature is easier. There are two main credible commitment approaches to war duration and termination. Walter (2002) argues that the internal parties to civil war cannot overcome commitment problems in the absence of a credible international guarantee to enforce the terms of the agreement. To test that approach, I include a variable, measured monthly, indicating whether there was an international guarantee to enforce a settlement reached.²⁵

Fearon (2002) argues that conflicts with “sons of the soil” dynamics are afflicted by severe commitment problems. Sons of the soil conflicts are those in which the insurgent group represents an ethnic minority faced with in-migration and economic competition from the majority group. Sons of the soil conflicts last much longer than

²⁵ To generate this variable, I began with the dataset in Walter (2002) and her coding notes. I coded every month as “0” for every war that was included in both her and my dataset and that she coded as not having a guarantee. For each war that she did code as having a guarantee, I used her coding notes to code the month in which the guarantee took place and how long it was active. For all remaining conflicts I used the criteria she described in Walter (2002), newsreports, case histories, and online sources to code whether there was an international guarantee and if so when it took place.

other types of civil wars, Fearon argues, because the state cannot commit credibly to prevent migration into the minority land in the future no matter how costly the war is in the short term. To test Fearon's approach, I include a dichotomous variable measuring whether each conflict contained sons-of-the-soil dynamics. I began with a list of conflicts provided in Fearon (2004). However, there were cases in my dataset not included in his analysis and I coded those cases using his definition by reading case histories.

Table 5 presents several models that include these various measures as well as the variables from Table 1. For simplicity, I have only included the strict measure of veto players since that variable had a larger coefficient, higher level of significance, and larger substantive effect in the tests in Tables 1 and 2. In unreported tests, however, I ran the same models with the lenient measure and although the results were not as strong, they were generally comparable.²⁶

²⁶ The lenient measure of veto players achieved significance at the .05 level in model 3 and at the .1 level in models 1, 2 and 4.

Table 5: Other Approaches

	Model 1: War Costs	Model 2: War Costs	Model 3: Commitment Problems	Model 4: All Approaches
Number of Veto Players	-0.907* (0.351)	-1.01** (0.357)	-1.26** (0.389)	-0.963** (0.371)
Coup	2.486** (0.232)	2.545** (0.246)	2.073** (0.221)	2.402** (0.242)
Natural Log of Population	-0.107 (0.072)	-0.091 (0.07)	-0.038 (0.064)	-0.058 (0.074)
Ethnic Fractionalization	0.075 (0.359)	0.017 (0.367)	0.124 (0.302)	0.241 (0.376)
Natural Log of Annual Battledeaths	-0.144* (0.057)			-0.163** (0.059)
Previous Year Natural Log of Battledeaths		-0.063 (0.054)		
Resources	-0.189 (0.196)	-0.157 (0.2)		-0.185 (0.204)
Mountains	0.007* (0.003)	0.007* (0.003)		0.008* (0.003)
Forests	0.0002 (0.003)	0.0006 (0.003)		-0.0002 (0.003)
Log Real GDP	0.071 (0.089)			0.104 (0.09)
Pre-War GDP		0.141 (0.092)		
International Guarantee			1.45** (0.426)	1.652** (0.45)
Sons of the Soil			-0.388 (0.284)	-0.498 (0.335)
Warmonths [^]	-0.038** (0.012)	0.035** (0.126)	-0.047** (0.011)	-0.038** (0.012)
Observations	14864	15094	15932	14864

Note: Reported are coefficients with robust standard errors in parentheses.

*significant at the .05 level, **significant at the .01 level (two-tailed test)

[^]Warmonths measure includes cubic splines, I do not report the coefficients for those splines here because they do not have an interesting substantive interpretation.

The first thing to notice from Table 5 is that the veto players finding is robust to each of the other specifications of the model. The coefficient is relatively stable across the models here and in Table 1. The coefficient on the veto players variable ranges from -0.91 to -1.26, a relatively narrow range. The stability of the coefficient indicates that the result on veto players is robust to the inclusion of all of these different variables, and so that none of them are driving the relationship between veto players and duration. In addition to these results, I ran other tests (which I do not report here) in which I included each of the war costs and commitment variables individually with the model from Table 1. In all those tests, the measure of veto players achieved statistical significance at the .01 level and had a coefficient within the range identified.

The results for the other variables included in the model are mixed. In particular, the costs of war theories fare poorly. Forests, lootable resources, and GDP fail to achieve significance in the models in Table 3. What is more troubling is that the two costliness variables that do achieve significance—battledaths and the percentage of the conflict zone that is mountainous—do so in the opposite direction from that predicted by war cost theories. A higher number of annual battle deaths is actually associated with a lower probability of war ending, meaning that more battle deaths make civil wars less likely to be resolved in a given month. A higher percentage of mountainous terrain, meanwhile, is associated with shorter conflicts, again, a finding at odds with expectations from war cost theories.

These findings could illustrate problems with the measures used to proxy war costs rather than the underlying theory. Each of the measures included is a rather distant proxy of the costs of war to the various parties involved. However, the existing empirical

literature gives us no better indicators of war costs and further work in this area is needed to make a more conclusive determination of the effect of the costliness of war on duration.

Commitment approaches fare better. In particular, Walter's (2002) argument that international guarantees make resolving wars easier receives support. The coefficient on the international guarantee variable is large, indicating that those guarantees have a strong effect. A potential problem with this analysis is that there could be a selection effect driving these results. Namely, the international community might only become involved in conflicts that are "ripe for resolution" and so there may be some underlying features of the conflict that make them both easier to resolve and lead to international guarantees. In her analysis, Walter attempts to demonstrate that selection effects are not driving her findings, but it is a problem to keep in mind when interpreting these results.

Finally, Fearon's (2004) argument that sons of the soil conflicts are harder to resolve does not receive support from this analysis. The sign on the sons of the soil variable is negative, as predicted, but it fails to achieve traditional levels of significance in either test. This result is different from that found by Fearon; however, he uses a different dataset. In particular, Fearon does not require a conflict to continue to generate 25 battledeaths to be counted as ongoing. This may mean that while sons of the soil conflicts are rarely resolved definitively, they lay dormant for long periods of time and then flare up periodically.

The results presented so far have shown strong support for the effect of number of veto players on the duration of civil war. In the next section, I conduct further tests

that examine this correlation. They are designed to determine if there is some aspect of the dataset or the measurement of veto players that is driving this relationship.

Measurement Issues

All of the tests presented in Tables 1 through 5 have used one of the two measures of veto players I generated for this analysis. I described in detail earlier the process that I went through to create those measures, and why that measurement matched the veto player approach best. However, there could potentially be some aspect of the measurement of veto players that is driving the correlation between veto players and duration. An easy way to determine if the measurement is driving the results is to use a separate measure of veto players that I did not collect in the same model presented in Table 1.

Since there is very little existing work on parties to civil war finding an independent measure of veto players is difficult. One option is to include a variable measuring all of the parties identified by the PRIO/Uppsala Armed Conflict Dataset. As described previously, the criteria for parties to be listed in the ACD is not explained and it is not clear why some parties were included and others were not. However, this list gives an independent set of conflict actors that can be used to see if the results are robust to different measures. Table 6 reports the results of a replication of Table 1, except with a variable measuring the number of participants listed in the ACD for each conflict year.

Table 6: Does the Measurement of Veto Players Matter?

Number of parties in ACD	-0.349** (0.115)
Coup	2.052** (0.212)
Natural Log of Population	-0.141* (0.064)
Ethnic Fractionalization	0.035 (0.308)
Warmonths [^]	-0.041** (0.011)
Observations	13815

Note: Reported are coefficients with robust standard errors in parentheses.

*significant at the .05 level, **significant at the .01 level (two-tailed test)

[^]Warmonths measure includes cubic splines, I do not report the coefficients for those splines here because they do not have an interesting substantive interpretation.

As can be seen from Table 6, there is again a highly significant association between the number of parties in the ACD and the duration of civil war. The coefficient on that variable is considerably smaller than on the strict measure of veto players presented in Table 1, which is not surprising given that this measure was not coded based on the theoretical framework presented in chapter two. The statistical association does show that the relationship between veto players and the duration of civil wars is not driven by the measurement of veto players.

Another potential problem that could affect the results so far is the low battledeath threshold used in this dataset. The ACD includes as civil wars all internal conflicts that generate at least 25 battledeaths in a given year, a threshold that may bring in events that do not meet traditional definitions of civil war. The presence of these events may skew the results discussed above. Table 7 presents the results of two tests to

determine if the battledeath threshold is driving the results in this study. The two models contain the same tests conducted in Table 1, except they limit the sample to conflicts that ever reached 1000 battledeaths (model 1) and conflict years with at least 1000 battledeaths (model 2).

Table 7: Does the Battledeath Threshold Matter?

	Model 1: Conflicts with 1000 battledeaths	Model 2: Years with 1000 battledeaths
Number of Veto Players	-0.979* (0.39)	-0.97 (0.605)
Coup	2.41** (0.651)	2.9** (0.868)
Natural Log of Population	0.042 (0.118)	0.203 (0.144)
Ethnic Fractionalization	0.031 (0.518)	-0.78 (0.75)
Warmonths [^]	-0.026 (0.018)	-0.033 (0.03)
Observations	12301	5235

Note: Reported are coefficients with robust standard errors in parentheses.

*significant at the .05 level, **significant at the .01 level (two-tailed test)

[^]Warmonths measure includes cubic splines, I do not report the coefficients for those splines here because they do not have an interesting substantive interpretation.

The number of veto players remains significant when only those conflicts that reach 1000 battledeaths are included. The variable does not achieve significance when the dataset is limited to high battle death years. However, the coefficient is similar to those found in the tests in Tables 1 and 5, the standard error is simply larger. The higher standard error is likely due to the fact that the number of observations has been cut by over two-thirds from those earlier analyses. These results show, then, that the effect of veto players is not simply the result of the inclusion of smaller conflicts in the ACD.

Finally, another way to evaluate the robustness of these results is to examine if the relationship found holds across time and region. It is possible that the effect of veto players on duration is not a general trend, but rather is the result of a South Asia effect or a post-Cold War effect. If that were true, the result would still be interesting, but it would mean that the finding was less generalizable. Table 8 presents a re-analysis of models 1 and 2, with dichotomous variables measuring the decade of the conflict month. Since these variables are mutually exclusive, they cannot all be included in the model, so the excluded variable in Table 8 is the 1940s variable.

Table 8: Temporal Effects

	Model 1	Model 2
Strict Veto Players	-1.099** (0.38)	
Lenient Veto Players		-0.31* (0.152)
Coup	2.197** (0.227)	2.209** (0.228)
Natural Log of Population	-0.096 (0.058)	-0.109 (0.061)
Ethnic Fractionalization	0.167 (0.319)	0.239 (0.331)
1950s	0.232 (0.434)	0.229 (0.437)
1960s	-0.244 (0.398)	-0.177 (0.396)
1970s	-0.506 (0.409)	-0.48 (0.412)
1980s	-0.59 (0.406)	-0.585 (0.405)
1990s	-0.006 (0.37)	0.025 (0.37)
2000s	-0.307 (0.454)	-0.265 (0.453)
Warmonths [^]	-0.044** (0.011)	-0.044** (0.011)
Observations	15932	15932

Note: Reported are coefficients with robust standard errors in parentheses.

*significant at the .05 level, **significant at the .01 level (two-tailed test)

[^]Warmonths measure includes cubic splines, I do not report the coefficients for those splines here because they do not have an interesting substantive interpretation.

Table 8 shows that the relationship between more veto players and longer civil wars holds across time. Both the strict and lenient measure of veto players remain significant and the coefficients are very similar to those found in Table 1. Additionally, none of the temporal variables is significant, suggesting that the results of the analysis are fairly constant across the last sixty years.

To test for regional effects, Table 9 includes dichotomous variables measuring the region of the country in which the civil war took place. The regions included are Africa, Asia, Latin America, Middle East/North Africa, North America and South Asia. Again, these variables are mutually exclusive, so in Table 9 North America is the excluded category.

Table 9: Regional Effects

	Model 1	Model 2
Strict Veto Players	-1.132** (0.379)	
Lenient Veto Players		-0.326* (0.15)
Coup	2.251** (0.233)	2.212** (0.24)
Natural Log of Population	-0.051 (0.09)	-0.048 (0.097)
Ethnic Fractionalization	0.147 (0.464)	0.279 (0.466)
Africa	-0.215 (0.477)	-0.189 (0.482)
Asia	-0.221 (0.424)	-0.237 (0.424)
Latin America	-0.294 (0.439)	-0.168 (0.452)
Middle East/North Africa	0.088 (0.437)	0.065 (0.436)
South Asia	-0.469 (0.491)	-0.592 (0.485)
Warmonths [^]	-0.045** (0.011)	-0.044** (0.011)
Observations	15932	15932

Note: Reported are coefficients with robust standard errors in parentheses.

*significant at the .05 level, **significant at the .01 level (two-tailed test)

[^]Warmonths measure includes cubic splines, I do not report the coefficients for those splines here because they do not have an interesting substantive interpretation.

Table 9 shows that the veto players effect holds in conflicts across region as well. Both the measures of veto players are significant and negative, and none of the regional measures achieve statistical significance. This table shows that we can be confident that conflicts with more veto players are longer wherever they take place, and that the results in the earlier tables are not a product of, for example, a South Asia or Africa effect.²⁷

Conclusion

The quantitative analysis conducted in this chapter provides strong support for the veto player approach. A greater number of veto players was found to be significantly associated with longer wars in almost every test conducted, regardless of which measure of veto players was used. In addition to the statistical significance, increasing the number of veto players was found to substantially decrease the probability of war ending in any given month, and therefore to lead to considerably longer civil wars. Additionally, an independent measure of veto players not collected by the author was found to be associated with longer civil wars, suggesting that the results were not driven by the way veto players were measured in this project. The veto players findings were robust to different specifications of the model, including those controlling for factors identified in the existing literature as influencing the duration of civil war.

These results suggest that the narrow focus on two actors in the existing literature hinders our ability to fully understand the duration of internal conflicts. In this project, I

²⁷ In other tests, I included dichotomous variables measuring whether the civil war took place in Burma or India. Those countries have by far the most conflict months in the ACD and I wanted to make sure those conflicts were not driving the results. In those tests, the veto players variables remained significant and negative. Neither the India nor Burma variables were significant, although the Burma variable just missed traditional thresholds. This suggests that conflicts in Burma may be longer than in other countries for some unexplained reason, but that relationship is not skewing the veto players finding.

have presented a parsimonious model for understanding the impact of multiple parties on conflict bargaining. This model and the analysis herein are just the first step in understanding the dynamics of these conflicts, however. In particular, further examination is needed of the effects of variation in the types of actors involved in multi-party conflicts, as well as of the effect of level of capabilities on the ability of parties to veto resolution and continue civil war. I conducted some initial examination of these factors in this chapter, and will examine them further in the case studies.

While the quantitative analysis conducted in this chapter does show strong support for the veto player analysis, statistical correlation alone cannot conclusively prove the theory correct. In particular this is true because the discussion in chapter two asserted several specific mechanisms that lead multi-party conflicts to be of longer duration. While the statistical analysis has established that there is indeed a correlation between the number of parties and duration, it has shed no light on whether the mechanisms identified are in fact what are driving that correlation.

The next chapter presents comparative case studies of civil war negotiations in Rwanda and Burundi. I use fine-grained data on the behavior of parties at the negotiating table in those conflicts to examine the causal mechanisms identified in chapter two. That qualitative analysis will combine with the statistical tests presented in this chapter to provide a full test of the veto player approach.

Chapter Four

Bargaining and Fighting in Rwanda and Burundi

The statistical analysis in chapter three provided support for the central contention of this dissertation, that civil wars are longer when there are more actors who can block agreement. In this chapter, I use qualitative analysis to further our understanding of the dynamics of multi-party conflicts by conducting comparative analysis of negotiations in a two veto player civil war in Rwanda (1990-1994) and a multi-party conflict in Burundi (1991-ongoing).

Qualitative analysis is an important component of theory testing because it allows for more thorough examination of the mechanisms underlying a statistical correlation. Chapter two presented a theoretical framework that identified four separate mechanisms which lead multi veto player conflicts to be longer: a shrinking of the bargaining range, greater information asymmetries, incentives to hold out and shifting alliances among parties. The statistical analysis in chapter three identified a strong correlation between the number of veto players and the duration of civil war which may be the product of those mechanisms, but there is no way to tell directly from that analysis. This qualitative analysis will allow us to evaluate whether the predicted differences between two-party and multi-party bargaining are seen in negotiations in actual civil wars.

The analysis in this chapter proceeds in three parts. The first section sets up the case studies by presenting the predictions that derive from the theory in chapter two about the differences between two-party and multi-party negotiations in civil wars. It also explains why the conflicts in Rwanda and Burundi present a good test for the veto player

approach and provides a brief background to the conflict. Second, I analyze each of these four mechanisms in turn and evaluate whether the Rwandan and Burundian cases show support for the predictions. A comparison of these two cases indicates that negotiating peace in Burundi was significantly more difficult because of incentives to hold out, information asymmetries, and shifting alliances. The third section discusses the breakdown of the Rwandan peace agreement. The veto player approach argues that it will be easier to negotiate a sustainable agreement in two-party conflicts than in multi-party wars, yet the Rwandan peace agreement broke down in the outbreak of mass genocide. This breakdown does not indicate a problem with the underlying theory, however, because it was not the result of some fundamental flaw in the peace process. Rather, it was caused by three factors external to that process. I conclude by relating the findings from these case studies to the quantitative analysis in chapter three, and discuss what these two empirical analyses tell us about the theory developed in this project.

Predictions

The veto player theory developed in this dissertation leads to four main predictions about the differences between two-party and multi-party negotiations. First, in negotiations with multiple veto players, there will be a greater number of issues under discussion by the parties at the negotiating table. There will be more issues because each party will bring a separate set of preferences to the negotiating table, and these preferences will increase the total number of issues at conflict and the likelihood that there will be disagreement between at least some of the parties on any one of those

issues. This prediction will be supported if there are more issues under negotiation in Burundi than in Rwanda.

Second, in conflicts with less veto players, it will be easier for parties to use their performance on the battlefield to make more realistic assessments of their probability of winning the conflict outright. This dynamic should mean that combatants in two-party conflicts use the battlefield to adjust their demands at the negotiating table. In multi-party conflicts, by contrast, the relationship between the battlefield and the negotiating table should be less clear and there should be less evidence that parties adjust their demands based on their military position. In these conflicts, then, the prediction is that there would be a clearer relationship between performance on the battlefield and demands at the negotiating table in Rwanda than in Burundi.

These two predictions point to the same general conclusion, that it will be harder for the parties to reach agreement on the full set of issues in Burundi than in Rwanda. However, each mechanism affects that difficulty in different ways. The greater number of issues in multi-party conflicts makes finding agreement on the set of all issues under negotiation more difficult. Information asymmetries make it harder for the various actors to assess the strength of the other side and therefore the extent of concessions they have to give up. The combined effect of more issues in the conflict and greater information asymmetries means that reaching a comprehensive agreement in multi-party conflicts will be more difficult.

Third, in conflicts with more veto players, the parties will be more likely to walk away from the negotiating table or to refuse to participate at all in negotiations in the hopes of getting a better deal as the other parties make some progress in the negotiations.

This behavior is caused by the incentives that groups have to hold out and negotiate harder in multi-party conflicts to try to get the best deal as the last signer. I expect to find a greater occurrence of parties refusing to participate in negotiations in Burundi than in Rwanda. Additionally, the veto player approach predicts that in multi-party conflicts it will be the strongest parties who will hold out, rather than the weaker. In Burundi, the strongest insurgent group is CNDD-FDD and the second strongest is Palipehutu-FNL, and both are considerably stronger than all of the other insurgents in the conflict. Therefore, I would expect those two groups to be the most likely to hold out, and expect CNDD-FDD to hold out the longest. I will examine this prediction by analyzing the behavior of the different combatants in the Burundian negotiation process.

Fourth, in multi-party conflicts, negotiations will break down because of shifting alliances among parties. Parties in the conflict will attempt to form negotiating blocs to overcome these problems described above, but these blocs will be prone to breakdown due to shifting alliances on different issue areas. Based on this mechanism, I expect to see negotiations in Burundi break down due to shifting alliances between the various combatants on different issue areas.

These predictions about the differences between two-party and multi-party negotiations stand in contrast to expectations from the existing literature about what factors make negotiation more difficult. Existing approaches to the duration and termination of civil war would predict that negotiating peace will be harder when war is less costly to parties, when information asymmetries are greater and when combatants are less able to commit credibly to uphold the terms of an agreement reached, regardless of the number of actors involved. The predictions from the veto player approach do not

contradict directly these existing approaches. However, they do suggest that the bargaining environment is fundamentally different when more combatants have the ability to block agreement, and that those differences should affect the negotiations in addition to the war costs and commitment problems. Additionally, the theory predicts that a greater number of combatants is a cause of information problems, and therefore can explain the conditions under which parties will have greater difficulty using the battlefield to adjust their demands.

The research design for these cases studies is designed to control for the presence of these other barriers to resolution and isolate the independent effect of veto players. As I describe in detail below, Rwanda and Burundi are ideal tests for the veto player approach because they are as similar as possible on all of the factors affecting the ease of negotiation except for the number of parties. The four predictions arising from the veto player approach, then, can be examined while controlling for factors such as the costs of war and commitment problems that could also affect the ability of combatants to negotiate their way out of conflict. The next section explains more specifically why Rwanda and Burundi are good tests.

Why Rwanda and Burundi?

The Rwandan civil war of 1990-1994 and the Burundian civil war which began in 1991 and continued into 2006 are ideal cases for testing the veto player approach for two reasons. First, they are the best fit cases for a most-similar case research design. Most similar case analyses are designed to control for other factors that could potentially affect the outcome of interest, and therefore isolate the independent effect of the variable the

researcher is interested in. In this analysis, the ideal comparison is between cases that are similar on every potential factor that could affect the duration of conflict except for the number of parties involved. There are several potential pairs of cases within the 288 conflicts in the dataset that could be analysed, however, the conflicts in Rwanda and Burundi are the most similar on all other factors that could affect negotiation as possible, while differing on the number of combatants.

Both conflicts took place in the same region—East Central Africa—and started at roughly the same time, in the immediate aftermath of the Cold War. The societies within which the civil war took place were strikingly similar. Rwanda and Burundi both are divided between two main ethnic groups—the Hutu and the Tutsi—and each conflict was an ethnic conflict fought along those lines. The government and insurgent sides were reversed—in Rwanda, a Tutsi led insurgency was launched against a Hutu-dominated government, while in Burundi the government was Tutsi and the insurgents were Hutu—but the nature of ethnicity was a major factor driving each conflict.

The costliness of fighting was similar in each conflict. Both civil wars generated low numbers of battlefield casualties but had high numbers of civilian deaths. An estimated 5,500 combatants were killed in direct fighting in the Rwandan civil war while the Burundian conflict resulted in just over 5,700 battle deaths from 1991-2002.¹ However, these figures pale in comparison to the number of civilians killed in each conflict. In Rwanda, there was violence against civilians throughout the war, but that violence peaked in the genocide of April-July 1994, when an estimated 500,000-800,000 Tutsi and moderate Hutu were killed. Violence against civilians in Burundi has not

¹ These estimates are from Lacina and Gleditsch (2005).

reached the level of the killing in the Rwandan genocide, however, tens of thousands of Burundian civilians have been killed as a result of the war.

In terms of other factors that affect the costs of war to combatants, Rwanda and Burundi have similar terrain. Rwanda has historically been referred to as “le pays des mille collines” in English, “the country of a thousand hills,” and also has significant mountains that reach more than three miles above sea level. The topography in Burundi is quite similar, although it is somewhat less mountainous than its northern neighbor.² In each country, this mountainous terrain allowed the insurgents to organize outside of the reach of the central government. In addition to the advantageous terrain, the non-state actors in both conflicts had access to external bases. In Rwanda, the RPF formed in Uganda and was able to use Ugandan territory to organize. In Burundi, insurgent organizations formed in refugee camps in Tanzania and used the insecurity in eastern Zaire/Democratic Republic of the Congo as a means to establish bases outside of the control of the government. In neither Rwanda nor Burundi were lootable resources present in the conflict zone nor were they an issue in the fighting.

In relation to commitment problems, in neither conflict were “sons of the soil” dynamics present. The international community did play a significant role in facilitating negotiations in both conflicts but did not make a credible guarantee to enforce the terms of a settlement in Rwanda, and did not do so until eight years into the Burundian conflict.³

² Buhaug and Lujala (2004) provide data on the terrain in the area of each country where civil war is fought. They identify the Burundian conflict zone as 67% mountainous and completely unforested, and the Rwandan conflict zone as 93% mountainous and completely unforested.

³ In 2001, South Africa deployed 1500 troops to monitor and enforce the agreement signed. This force remained until 2003, when it was replaced by a multinational African Union led force.

The main factor on which these two conflicts differ is the number of veto players present in the fighting. The conflict in Rwanda involved two veto players which is the lowest number of veto players possible and the modal number of veto players for the conflicts in the dataset. The conflict in Burundi contained four veto players, which is in the top decile of conflicts in the dataset.⁴

Examining negotiations in Rwanda and Burundi, then, allow me to control for factors such as the costliness of war, commitment problems, the nature of societal cleavages, and any regional and temporal effects that could make negotiation easier or harder. This design should isolate the impact of the number of veto players on the ability of all parties to reach a comprehensive agreement, and provides an ideal environment for testing the implications of the veto player theory.

The second reason I chose Rwanda and Burundi as cases for this project is that these are important conflicts and scholars and policymakers should have a strong interest in understanding their dynamics. The costs of the Rwandan civil war have been well-publicized. The genocide of April-July 1994 left at least 500,000 Tutsi dead and created more than 3 million Rwandan refugees. These refugees were the principal cause of the 1996-2001 war in the Democratic Republic of the Congo (DRC), a conflict that resulted in as many as 4 million deaths. In 2006, the presence of Rwandan rebels in eastern DRC continues to threaten regional stability. Understanding the dynamics of war and

⁴ There is one other potential area where the two conflicts diverge. The RPF in Rwanda had a significant external patron, Uganda. The insurgents in Burundi, meanwhile, did not have external support to nearly the same degree. Although the issue of external support is often brought up in the literature on civil war, there is little theoretical work on how external support should affect negotiations. In either case, a close study of the Rwandan civil war shows that, although Uganda was a supporter of the RPF, it had little influence on the negotiations in the conflict.

negotiation that led to this horrific situation is important for scholars and policymakers interested in finding ways to resolve these conflicts.

The civil war in Burundi has been considerably less costly than that in its northern neighbor and has gained less scholarly attention. Policymakers, however, see the threat of a repeat of the Rwandan genocide in Burundi and have devoted considerable resources to trying to resolve the conflict. Given that Burundi is a country with a nearly identical ethnic composition to Rwanda and has a history of extreme ethnic violence approaching genocide the threat of mass violence there is very real.⁵ Scholars and policymakers have an interest in understanding the dynamics of the Burundian conflict to try to find ways to prevent a repeat of what happened in Rwanda.

Data Collection

The veto player approach is based on specific predictions about the behavior of parties at the negotiating table. Evaluating these predictions requires systematic data on the day-to-day events in negotiations in these conflicts. Obtaining this kind of data is challenging because of the relative lack of scholarly attention that has been paid to these processes. Although the 1994 genocide in Rwanda has generated a substantial body of literature, the civil war that pre-dated that event has received considerably less attention. The conflict in Burundi, meanwhile, has been largely ignored by scholars in the English-speaking community. Collecting the data necessary to conduct these case studies,

⁵ In his preface to the 1994 edition of his book *Burundi: Ethnic Conflict and Genocide*, Lemarchand (1995), began by saying "Nowhere else in Africa has so much violence killed so many people on so many occasions in so small a space as in Burundi during the years following independence." While the 1994 genocide in Rwanda and the 1996-ongoing violence in the Democratic Republic of the Congo have generated more deaths than the ethnic violence in Burundi, that country remains one of the most violent in Africa over the last forty years.

therefore, requires going beyond the current scholarly body of literature. To generate this data, I have used two methods.

First, I built a general outline of the negotiations through existing secondary sources. For the Rwandan peace process, Jones (2001) allows for the creation of a detailed outline of the main events in the different stages of the Rwandan peace process. To compile the outline for Burundi, I used Bentley and Southall (2005) as well as reports from organizations such as the International Crisis Group and the United States Institute for Peace (USIP) that have followed that conflict closely. All of these documents allowed me to create a detailed outline of the different instances of negotiations during these conflicts, who participated, and what the outcome was. Additionally, USIP has an online database of peace agreements which gave me the text of the three peace agreements signed across the two conflicts, indicating which actors were signatories and what provisions were agreed to.⁶ I present the general outline built from these sources in this chapter.

To supplement this general outline, I conducted field work in Belgium, Rwanda and at the International Criminal Tribunal on Rwanda in Arusha, Tanzania in April-May 2005⁷ and in South Africa in January and February 2006.⁸ In that fieldwork, I interviewed more than 15 different scholars, policymakers, and governmental officials who were participants in or knowledgeable of these conflicts. These interviews were used to fill in holes in the outline described above. Also, in interviews I asked people who were participants in or knowledgeable of these negotiation processes direct questions about the

⁶ The USIP online database of peace agreements is available at <http://www.usip.org/library/pa.html>.

⁷ This fieldwork was funded partially through grants from the Rohr Chair in Pacific International Relations and the Dean's Social Science Travel Fund.

⁸ This fieldwork was funded through a grant from the National Science Foundation.

predicted behaviors discussed in chapter three. Additionally, this field research gave me access to documents not currently available in the United States. I use the data gathered from these interviews and documents, as well as the secondary sources listed above, to test the four predictions about these negotiation processes described above. First, however, I discuss the general relationship between veto players and the battlefield in Rwanda and Burundi.

Veto Players and Duration in Rwanda and Burundi

In the quantitative analysis in chapter four, the conflicts in Rwanda and Burundi provide support for the predicted relationship between veto players and duration. The Rwandan conflict involved two combatants. The government, led by President Juvenal Habyarimana, and the Rwandan Armed Forces (FAR) were both dominated by Hutu. The Rwandan Patriotic Front (RPF), meanwhile, was made up primarily of Rwandan Tutsi refugees in Uganda. The war began in October 1990 when the RPF attacked from refugee camps in Uganda into northern Rwanda. It continued the RPF took control of Kigali and won the conflict in July 1994. This two veto player conflict, then, lasted for 46 months.

The Burundian conflict involved multiple actors. The army and government in Burundi was dominated by Tutsi for most of the conflict. The insurgent groups, meanwhile, all represented Hutu interests. Conflict began in November 1991 when the rebel organization Palipehutu launched attacks against the government. By 1994, the conflict involved several insurgent groups, the largest of which were the National Council for the Defense of Democracy (CNDD) and Palipehutu. Another significant group was

the National Liberation Front (Frolina), and those three insurgents, along with the government, are counted as veto players in the quantitative analysis. In 1997, the military wings of CNDD and Palipehutu broke off from the political wings, which abandoned the armed struggle, and continued fighting as CNDD-Forces for the Defense of Democracy (CNDD-FDD) and Palipehutu-Forces for National Liberation (Palipehutu-FNL). In 2000, Frolina signed a peace agreement and ceased its armed activity, and in 2003 CNDD-FDD followed suit. Palipehutu-FNL has largely refused negotiations and continued to fight into 2006. This multi-party conflict, then, is still unresolved more than 170 months since it began.

A first glance at the negotiations in each conflict also supports the contention that negotiations are easier and more expedient in two-party conflicts. In Rwanda, face-to-face meetings between the RPF and the government were held less than one year into the conflict, and comprehensive negotiations began in July 1992. Over a thirteen month period these negotiations led to an agreement signed by both combatants and there was partial implementation of the agreement before it broke down in April 1994. In Burundi, meanwhile, negotiations were not held until 1997, six years into the conflict, and comprehensive negotiations among all of the combatants have never been held. Rather, since 2000 the government has tried a strategy of negotiating with one party at a time, which has led to an expanding coalition of former insurgents participating in the government, but has yet to resolve the conflict definitively.

This broad analysis, then, suggests support for the veto player approach but is not a direct test of the specific predictions arising from that model. In the next section, I will

use fine-grained data on the events, behavior and strategies used by combatants in these conflicts to analyze the four predictions discussed above.

Evaluating the Predictions

The Number of Issues

The veto player approach to civil war negotiations predicts that negotiations will be more complicated in Burundi because a greater number of issues will be under discussion. Finding agreement among all combatants on this expanded number of issues will cause negotiations to drag on and to break down more frequently. To evaluate this prediction, I examine the number of issues brought up in the negotiations between the government and the main combatants in those conflicts.

In Rwanda, face-to-face meetings between the government and the RPF began early in the conflict. The combatants met twice in Zaire in 1991—in March in N'Sele and in November in Gbadolite, but these meetings were not designed to address the main issues in the conflict. Rather, the main agenda of those meetings was to reach a ceasefire, agree on the deployment of a small Military Observer Group (MOG) and lay the foundation for a process of comprehensive negotiations.⁹

That process began in July 1992 in Arusha, Tanzania. The Arusha negotiations included the RPF and a governmental delegation. The government delegation included the President's political party—the National Revolutionary Movement for Development (MNRD)—as well as opposition parties that had been incorporated into the government

⁹ Both of these meetings resulted in the RPF and the government signing a cease-fire, however, in each case, that cease fire agreement broke down almost immediately. The N'Sele Ceasefire Accord, reached on March 19, 1991, broke down almost immediately. The Gbadolite Accord, which was basically an amended version of the N'Sele agreement, was signed in September 1991 and broke down in November of that year.

when a multi-party system was introduced in 1991. These parties included the Democratic Republican Movement (MDR), a Hutu-dominated party that was more moderate than the MNRD, the Liberal Party (PL), which drew support from both Tutsi and Hutu and generally supported the interests of upper-class urban Rwandans, and the Coalition for the Defense of the Republic (CDR), a hard-lined Hutu power party. The CDR was the most stringent anti-RPF party and many CDR members would be influential figures in the April-July 1994 genocide.¹⁰ Although these opposition parties were officially part of the governmental delegation, with the exception of the CDR they had a long history of opposing Habyarimana. It is more accurate to treat the Arusha negotiations, then, as a tripartite process involving the RPF, MNRD/CDR, and a set of opposition parties that were generally more moderate than either of the armed combatants.

The prediction from the veto player approach refers to the number of issues between combatants, so the analysis here will focus on the areas of contention between the RPF and the MNRD. Jones (2001, p. 79) writes that the agenda under discussion in the Arusha negotiations was set by the Tanzanian facilitators, and consisted of “the issues of a cease-fire agreement, the rule of law, powersharing, the integration of armies, and the repatriation of refugees.”

While all of these issues were discussed in Arusha, the central areas of disagreement between the RPF and the government were political powersharing and military integration. In an interview in Kigali in March 2005, Tito Ruteramara, the main

¹⁰ One member of the governmental delegation to the Arusha negotiations from the CDR was retired Colonel Théoneste Bagosora. Bagosora would later become one of the main leaders of the government formed during the Rwandan genocide, and has received much attention for his role in that genocide. He is currently undergoing trial at the International Criminal Tribunal for Rwanda in Arusha.

political organizer of the RPF prior to and during the civil war and current Rwandan ombudsman, said that the RPF's main agenda in the conflict was to take power away from the MNRD. "We wanted to weaken the MNRD for ourselves and the other political parties," he said. This statement indicates the importance that the RPF placed on gaining power in the transitional government. Ruteramara also said that military integration was less important for the rebels because they believed that the international community would monitor that process and the post-conflict transition. A close analysis of the Arusha negotiations, however, shows that the RPF negotiated hard on the issue of military integration as well. The RPF wanted as large a role in the transitional government and military as it could get, and the MNRD struggled to hold onto as much power in both the government and army as possible.

In addition to these two overarching issues, the role of refugees was an important issue in the conflict. The war started because of the presence of hundreds of thousands of Tutsi refugees in Uganda and the "right of return" of these refugees was an important element of the RPF's agenda. Finally, there was much discussion at Arusha over the issue of what kind of international observer mission would be deployed, so this issue should be seen as important as well. These four issues—political powersharing, military integration, the repatriation of refugees and the composition of an international observer mission—were the major issues of contention in the conflict.

Determining which issues were issues under discussion in the Burundian conflict is more difficult because of the lack of comprehensive negotiations involving all of the combatants. The main negotiations in the Burundian conflict took place in Arusha from 1998-2000 and included the government, unarmed opposition parties, and several of the

smaller combatants but did not include the two main insurgents, CNDD-FDD and Palipehutu-FNL. The Arusha process, then, does not allow for an examination of the issues of contention between the main combatants. However, an evaluation of the attempts at negotiation between the government and the CNDD-FDD and Palipehutu-FNL can give an indication of the issues important to those main combatants.

Initial talks between the government and CNDD-FDD began in 2001. In late 2002, the group signed a ceasefire accord and agreed to participate in further negotiations. These negotiations continued into 2003 and in November of that year, the government and the leader of the CNDD-FDD, Jean-Pierre Nkurunziza, signed a comprehensive agreement in Pretoria on power-sharing and military integration. The agreement gave CNDD-FDD four cabinet ministries, as well as designated posts in the parliament and provincial governorships. Additionally, the Pretoria accord gave CNDD-FDD 40% of the slots in the army, 35% of those in the police force and 35% in the Ministry of Intelligence and called for the officer corps of these various security forces to be divided 50-50 between Hutu and Tutsi (The Pretoria Protocol on Political, Defence and Security Power Sharing in Burundi, 2003).

In 2003, as negotiations were proceeding between the government and the CNDD-FDD, Gasana and Boshoff (2003) listed these two parties' positions on the major issues under discussion. They identify those issues as a cease-fire; the composition of a Joint Cease-Fire Commission (JCC); the cantonment of the two armies while they await demobilization; the deployment of an African peacekeeping mission in Burundi; a framework for negotiating a "Forces Technical Agreement" (as called for under the Arusha agreement); the division of power within the new government; the transformation

of the various armed parties into political parties; provisional immunity for members of CNDD-FDD; and how to deal with violations of the cease-fire accord.

Upon first glance, this list looks longer than the issues under discussion in Rwanda. However, while these various issues were important, like in Rwanda, the primary areas of disagreement between the government and CNDD-FDD were political powersharing and military integration. It was those issues that took the longest to resolve and, once agreement was reached on them, the two parties were able to sign a comprehensive accord. The composition of a peacekeeping force was also important, as in Rwanda. The Burundian refugee problem was less acute, but that was an important issue in the conflict as well. The issues of contention between CNDD-FDD and the government, then, look strikingly similar to those between the RPF and the government.

Since there have been few negotiations between the government and Palipehutu-FNL it is harder to reach a definitive list there. However a close study of the Burundian conflict suggests that the issues between those combatants are similar to the list above, with one possible exception. Jan van Eck, who has very close connections with the leadership of Palipehutu-FNL, argues that group “has a deeper analysis of the Burundian conflict, they identify an historical animosity between (the Hutu and Tutsi) and argue that until that issue is addressed, there can be no durable government.”¹¹ This issue of addressing latent ethnic tension can be seen as an additional issue brought to the negotiating table by the Palipehutu-FNL that was not present with the CNDD-FDD or in Rwanda, but it is unclear to what extent that is a central issue to their continued participation in the conflict.

¹¹ Telephone interview, February 2, 2006.

Based on this comparison, the prediction that a larger number of issues will be under negotiation in Burundi than in Rwanda does not hold up to close analysis. At best, there is one more significant issue in the former case, but even that cannot be determined conclusively. However, the failure of this prediction may be a function of the extreme similarity between the Rwandan and Burundian societies and conflicts, rather than an indication of a problem with the veto player theory.

In chapter two, I argued that the major factor that leads some conflicts to be multi-party is that they occur in societies with more diverse preferences. The demands that armed factions make are generally a function of underlying social preferences of the groups from which they draw support. Consequently, separate insurgent groups in fractionalized societies should bring a more diverse set of issues to the negotiating table. Rwanda and Burundi, however, are very similar societies and so it is not surprising that the main issues of contention in the two conflicts are similar.

The ethnic breakdown of Rwanda and Burundi is virtually identical, generally given as 84% Hutu, 15% Tutsi and 1% Twa. Despite these similarities, politics in the pre-colonial period in the two societies was quite different. In Rwanda, although ethnicity was less politicized before the colonial era, the Tutsi were still in a dominant position politically and economically and the Rwandan monarchy was clearly in the hands of the Tutsi. In Burundi, the monarchy was controlled by the Ganwa, a group seen as Tutsi by some and as a separate ethnic group by others. During colonialism, the Germans (who ruled Rwanda and Burundi until 1917) and subsequently the Belgian colonial authorities elevated the Tutsi over the Hutu in Rwanda, but ruled more through the traditional

monarchy structure in Burundi. Ethnic relations in Burundi were more fluid and less politicized than in Rwanda in both the pre-colonial and colonial periods.

Consequently, when Burundi gained independence in 1962, the transition was relatively peaceful and the country had a government that was multi-ethnic and had some features resembling multi-party democracy (Lemarchand 1995). The end of colonialism in Rwanda, meanwhile, was violent, as a number of educated Hutu led a “Hutu revolution” designed to overthrow both Belgian colonial rule and Tutsi domination. That revolution led to hundreds of thousands of refugees fleeing Rwanda (including those that went to Uganda and would later form the RPF) and resulted in the creation of a Hutu dominated government at independence. The Hutu would remain in control of the country straight through the RPF victory in 1994.

While ethnicity was less polarized in Burundi at independence, events in that country were heavily influenced by the 1957-1962 Hutu Revolution in Rwanda. That revolution had two effects. First, ethnicity among Hutu and Tutsi in Burundi became more politicized as they saw the potential rewards and threats of majority democracy. Second, thousands of Rwandese Tutsi fled to Burundi as refugees, and these refugees contributed to the politicization of ethnicity in Burundi. In the 1960s, political crisis and increasing crystallization of ethnic tensions eventually resulted in a Tutsi takeover of power and the emergence of Tutsi hegemony.

Despite the substantial differences between Rwanda and Burundi in the pre-colonial and colonial period, then, by the time of the 1990s conflicts ethnicity was highly politicized in both countries. Ethnic politics dominated the agenda in Rwanda and Burundi, and identity issues were the main ones under contention in both conflicts.

Consequently, those conflicts appear to revolve around a similar set of issues, even though the Burundian conflict contains more parties.

It is likely, then, contrary to the argument in chapter two that preference diversity is the main factor leading conflicts to involve more actors, that the multi-party nature of the Burundian conflict was caused by something other than more heterogeneous preferences within that society. This study does not examine systematically the reasons that the conflict in Burundi became multi-party and that in Rwanda did not, but two separate reasons are suggested by close analysis. First, the Rwandan state prior to the outbreak of conflict was a stronger state by virtually any measure than the Burundian state. This state strength meant that there was less room for organizing opposition to Rwanda than in Burundi. Consequently, while there were likely many aggrieved groups in Rwanda, the RPF was only able to organize because it was based in exile in Uganda, outside of the reach of the state. In Burundi, meanwhile, groups were able to organize both in Tanzania and within Burundi, giving greater space for organized opposition to grow. Second, in Rwanda, the armed opposition to the government came from within the Tutsi, a relatively small percentage of the population. Meanwhile, in Burundi, the opposition was from the Hutu, a much larger group. Consequently, there may have been greater preference diversity within the Burundian Hutu than the Rwandan Tutsi, leading to the proliferation of rebel groups.

Information and the Battlefield

An additional reason arising from the veto player approach that finding agreement in multi-party conflicts is more difficult is that, in those conflicts, the

battlefield reveals information less clearly. Informational approaches to conflict bargaining (Filson & Werner 2002, Smith and Stam 2004, Slantchev 2004), argue that conflict breaks out due to information asymmetries, that parties use military outcomes on the battlefield to update their beliefs about their probability of victory, and that when sufficient information has been revealed, an agreement is reached. These models all assume two parties, however, in multi-party conflicts the battlefield reveals information less clearly, making this updating more difficult. In these conflicts, then, the veto player approach would predict a clearer relationship between information revealed by the battlefield and demands made at the negotiating table in Rwanda than in Burundi.

The predictions made by informational approaches receive strong support in the Rwandan case, particularly in regards to one of the main issues under negotiation, military integration. As mentioned above, military integration was a key area of disagreement between the RPF and Habyarimana's government during the Arusha negotiations. It was a particularly difficult issue for the government, since each officer position given to the RPF would mean one less position for a current Rwandan officer. This factor was compounded by the certain prospect of a demobilization program which would reduce the total number of officers and soldiers in the army, meaning that any agreement on military integration would lead to a large number of unemployed former officers. Several officers within the Rwandan Armed Forces (FAR) held a powerful position in Habyarimana's inner circle, making the government very hesitant to give in to RPF demands on this issue. Consequently, the Government of Rwanda's (GoR) initial offer was for the RPF to have 15% of the executive positions in the army, reflecting the Tutsi percentage of the population in Rwanda (Jones 2001).

The RPF, meanwhile, insisted on an even split of the post-conflict military between its forces and the government. This demand was a major stumbling block in the negotiations. Ruteramara argued that the reason the RPF was able to make substantial demands at Arusha was that was the only party to bring new ideas to the negotiating table. He argued that the RPF had agreed on a set of principles, written them down, and used that to guide their demands at the negotiating table, and the governmental delegation never brought ideas to the table.¹²

It is clear that the RPF was a more effective negotiating team than the government. The RPF negotiators¹³ were more senior than the governmental delegation, better organized, and better equipped. One clear example of the difference in organization is that the RPF negotiating team had cellular telephones, allowing them to call back and forth to RPF headquarters and to communicate with their Ugandan supporters in Kampala easily. The governmental team, meanwhile, had to use the payphone in the lobby of the Mount Meru hotel in Arusha to call back to Kigali (Jones 2001, p. 73).

This organizational capacity clearly affected the ability of the RPF to negotiate effectively. However, their ability to demand significant concessions was also driven by two other factors. First, the costs of continued conflict were disproportionately borne by the government, so the RPF was in a stronger position to negotiate harder. Jones (2001, p. 73) writes:

...it is important to note that the RPF, according to several Western participants, gained important leverage over time from the sense given by the delegation that,

¹² Author interview, Kigali, Rwanda, May 11, 2005.

¹³ The RPF negotiating team was Pasteur Bizimungu, Patrick Mazimpaka, and Théogène Rudasingwa, who was general secretary of the RPF (Jones 2001).

although they were serious about negotiations, they were also prepared to return to the battlefield if they could not achieve what they wanted in talks. Having spent their lives in exile and ten years in armed camps in the bush...they could afford a few more years' fighting more than they could afford to lose in the peace process. This was in stark contrast to the GoR position: Two years of war had savaged what was already an economy in deep retraction; domestic pressures for change were mounting; international pressure for change, from Western donors and from the international financial institutions, were equally intense; and the government was on its last legs and could ill afford a long, drawn-out peace process or a return to fighting.

Second, during the Arusha negotiations the RPF was in a strong military position and its demands at the negotiating table reflected that strength. When the RPF attacked in October 1990 it achieved initial military success, capturing several important outposts in northern Rwanda and advancing sixty kilometers into the countryside (Prunier 1995). The invasion had been unexpected and the element of surprise gave the insurgents initial tactical advantage. However, a combination of missteps by the RPF and international support to the Rwandan Armed Forces (FAR) allowed the government to recapture most territory controlled by the RPF in the subsequent weeks.¹⁴

Over the course of 1991 and 1992, the government and RPF battled for control of the Rwandan countryside. Both increased the size of their forces and, despite subsequent ceasefire agreements, fighting continued. As the conflict continued, the RPF proved to be better organized and more disciplined, and thus a more effective fighting force. By the middle of 1992, the insurgents had gained control of significant parts of

¹⁴ Immediately after the invasion, the Rwandan army received military support from three states—France, Belgium, and Zaire. The French support was the most robust, as France sent 600 troops to Rwanda by October 5. These troops were not sent to engage the RPF, but rather to provide defensive support for Habyarimana. Belgium sent 400 paratroopers primarily to protect the 1,700 Belgian ex-pats living in Rwanda. Zairean president Mobutu Sese Seko, a longtime ally of Habyarimana, sent troops that immediately engaged the RPF, however, the Zairean troops were incredibly poorly trained and committed massacres against the Rwandan population, and Habyarimana asked Mobutu to recall them shortly after they were deployed (Prunier 1995).

territory in northern Rwanda and by the time the Arusha negotiations began in earnest, the RPF had at least reached parity with the government and its position continued to strengthen.

This strong military position enabled the RPF to make substantial demands on the issue of military integration. In late 1992, *Africa Confidential* reported that the RPF's demands at the negotiating table were driven by its strong position on the battlefield. The journal reported, "The (RPF's) hardline position can be explained by its undeniable military advantage over the government" (October 9, 1992, p. 7).

Despite the strong position of the RPF, the government continued to offer a much lower share of the military than that demanded by the insurgents. This disagreement, along with some other areas of contention, led negotiations to stall in late 1992 and early 1993. In response to the breakdown in negotiations, as well as FAR massacres against Tutsi civilians in northern Rwanda, the RPF abandoned a cease-fire and returned to the battlefield on February 8, 1993. The RPF won a decisive victory, taking Ruhengiri and Byumba in northern Rwanda and forcing the FAR to withdraw. RPF troops advanced to within 30 kilometers of Kigali by February 20 when it declared a unilateral cease-fire.¹⁵

In the aftermath of the February RPF offensive, the combatants returned to the negotiating table. The government almost immediately increased the concessions it offered the rebels on military integration. The two sides continued to bargain until

¹⁵ Prunier (1995) writes that the RPF stopped short of taking Kigali because its renewed offensive and allegations of RPF massacres against Hutu had strengthened Habyarimana's support in the capital, particularly among the Hutu opposition parties that had previously been sympathetic to the RPF. This increased support meant the RPF would face strong opposition in trying to take the capital. Additionally, French troops stood between the RPF and Kigali and the group did not want to engage in direct combat with the French army.

agreement was reached. The final deal gave the RPF 40% of the soldiers in the army, but an even 50-50 split of the officer corps. Jones (2001, p. 84-85) writes:

What allowed the RPF to win this major victory in the negotiation process (over military integration)? To return to the point made earlier, the central issue was strength on the ground. Although the sides appeared to be in a military stalemate when Arusha began, the February offensive proved decisively that the RPF had a considerable military advantage on the ground and was poised to continue scoring victories should the negotiations break down. It was, therefore, in a position of strength and could effectively dictate the terms of this final issue.

The negotiations around this central issue of military integration, then, show strong support for the contention that actors in two-party conflicts use their performance on the battlefield to adjust their demands at the negotiating table. The RPF was able to demand a large stake of the post-conflict military because of its relatively low costs of fighting and because of its strong military position. Prior to the battles of February 1993, the government was unwilling to offer the RPF anything close to the proportion of the military it was demanding because it did not recognize that the RPF had the edge militarily. The RPF's decision to abandon a cease-fire and return to the battlefield and its subsequent decisive victory, however, revealed to the government its weakened military position and that its prospects in a return to full-scale civil war were not good. This realization led directly to greater concessions on military integration. A final, comprehensive, peace agreement was signed four months later.

The veto player approach predicts a less clear relationship between the battlefield and the negotiating table in Burundi. For the Rwandan case, the approach suggests that actors should adjust their demands in response to battle outcomes and, as discussed above, there is good support for that argument. In the Burundian case, however, the prediction is that parties should have difficulty using battlefield outcomes to update their

beliefs, and therefore that the relationship should be less clear. Evaluating this prediction empirically is difficult, since it is essentially a prediction about what should not be observed. However, there is some initial support for the contention for the Burundian case, particularly in regards to the latter stages of negotiations aimed at incorporating the Palipehutu-FNL. This analysis is complicated, however, by several different factors.

One complicating factor is that serious negotiations between the government and the main armed combatants did not begin until quite late into the conflict. In Rwanda, comprehensive negotiations between the GoR and the RPF began in Arusha approximately 21 months after the first shots were fired. In Burundi, by contrast, the main round of negotiations (the Arusha process) did not start until six years into the conflict, and then did not include either CNDD-FDD or Palipehutu-FNL (the reasons for their absence will be discussed in detail below). The Burundian government did not enter into serious negotiations with the CNDD-FDD until 2001 and, at the time of this writing, still has not done so with Palipehutu-FNL, when the former had been a significant combatant since 1994 and the latter since 1997. This delay in negotiations means that, even if the battlefield revealed information less clearly in Burundi than in Rwanda, by the time negotiations between the government and the insurgents in Burundi began more information had been revealed, making negotiations easier.

A second factor complicating the analysis is that when negotiations were finally held between the government and the main insurgents, they took place in a context in which a “peace agreement” had already been signed. The Arusha negotiations of 1998-2000 were contentious but did generate a partial peace agreement eventually signed by the 19 participants of those negotiations, including the representatives of the political

organizations CNDD and Palipehutu. The agreement called for the establishment of a thirty-month transitional government made up of representatives of the 19 parties present at Arusha as well as greater ethnic balance in the military. The Arusha agreement did leave many important questions unanswered and so was by no means a comprehensive agreement. At the time of the signing, Africa Confidential reported (September 1, 2000, p. 7):

The clearest thing about the accord signed in Arusha on 28 August is its lack of finality and substance. None of the key issues—such as the new constitution, structure of power-sharing or reform of the military and security services—were agreed by all or even a majority of Burundi's quarrelling military and political factions.

Despite the partial nature of the peace accord and the lack of participation of CNDD-FDD and Palipehutu-FNL, the agreement did have an effect on the subsequent negotiations with those groups. The existence of the agreement meant that the Burundian government, as well as some regional heads of state, insisted that CNDD-FDD and Palipehutu-FNL stop fighting and incorporate within the Arusha framework. Those groups, meanwhile, rejected the agreement outright because they had not participated in its creation, and wanted to negotiate a deal with no pre-existing conditions.

These complicating factors mean that it is impossible to make a definitive statement about the relationship between information and the battlefield in regards to the negotiations between the Burundian government and the CNDD-FDD. A close reading of the negotiations that began in 2001 and led to the Pretoria Protocol of 2003 shows that by that point in the conflict the relative position of those two combatants was not in dispute. The government forces were clearly strong enough to block the CNDD-FDD

from winning the war outright, however, the insurgent force was strong enough to prevent being conquered. Rather, the debates in the negotiations revolved more around the extent to which CNDD-FDD would incorporate into the existing Arusha framework. Additionally, the main uncertainty involved seems to have been to what extent regional actors would put pressure on the two sides to negotiate, rather than about the relative positions of each party on the battlefield.¹⁶ In fact, it was pressure on CNDD-FDD, particularly from Mandela, that was one of the major factors that led that group to sign an agreement.¹⁷ Debates about the relative strength of the CNDD-FDD versus the Burundian army did not emerge in the negotiations, therefore, there is no way to evaluate whether those actors used the battlefield to update these beliefs.

There is more support for the prediction about multi-party conflicts and information in the government's attempts to negotiate an agreement with Palipehutu-FNL from 2003-2005. In the aftermath of the Pretoria Accord, regional leaders turned their attention to incorporating the last remaining insurgent group into the power-sharing government. Given that the larger insurgent group, CNDD-FDD, was going to incorporate into the army, it could be expected that Palipehutu-FNL would be in a weaker position. However, determining exactly how strong the FNL was against the new army would prove a very difficult task.

¹⁶ The regional politics surrounding the Burundian insurgent groups are extremely complicated. Both CNDD-FDD and Palipehutu-FNL based in territory in the Democratic Republic of Congo during that country's 1996-2001 civil war. When a peace agreement in the Congolese conflict was signed in Lusaka, Zambia, in 1999, the Burundian government was a signatory. The Lusaka agreement designated external rebel groups such as CNDD-FDD and Palipehutu-FNL (as well as Rwandan, Ugandan and Angolan rebels) as "negative forces" that should be forcibly disarmed. At the same time, these Burundian groups were not participants in that country's peace process, but clearly would need to be. So, in 2001, when negotiations between the government and these groups started, the groups were on a difficult footing because of their status as "negative forces." The international community has continued to use that designation to try to force Palipehutu-FNL to abandon the armed struggle, but that move has so far proven unsuccessful.

¹⁷ Telephone interview with Jan van Eck, February 2, 2006.

In July 2003, as the government and CNDD-FDD were making progress on agreement, the FNL troops launched a mortar attack in Bujumbura, killing 300 and driving thousands from their homes (Bentley and Southall 2005). In the immediate aftermath of the agreement in late 2003, FNL forces attacked FDD troops in cantonment camps within Burundi. These attacks were surprising given that the FDD and FNL forces had previously collaborated, but illustrated Palipehutu-FNL's continuing rejection of the peace process.

As of early 2006, it remains difficult to determine how strong the military position of the FNL is against the new, integrated, army. In February 2005, Jan Van Eck, a South African former member of parliament who has followed the Burundian conflict and peace process closely since 1995 and is in direct contact with the leadership of Palipehutu-FNL, wrote (p. 6):

It remains the opinion of the author that there is no military solution for dealing with the FNL. In spite of the fact that the combined forces of the Burundian Army and the FDD were deployed against the FNL for more than one year (in one of the largest military operations ever), the FNL—while obviously weakened—seems to remain far from defeated.

This statement by someone who has followed the Burundian conflict as closely as anyone indicates the difficulty in determining the military position of the FNL in the post-Pretoria Accord environment. Clearly, the group is weaker than the new army but determining how much weaker is difficult even after a full year of fighting. This difficulty is important because a major barrier to incorporating the FNL in the peace process is determining how much of a new, integrated, military should be composed of its forces. This process will be particularly difficult since any agreement which gives positions to the FNL will take them away from the former Burundian army (which is exclusively Tutsi),

the FDD, or the small percentage drawn from the other armed groups, and threatens the delicate ethnic balance in the security forces.

This comparison of negotiations in Rwanda and Burundi shows support for the prediction that the battlefield reveals information more clearly in two-party than in multi-party conflicts. In Rwanda, there is clear evidence of the combatants using their performance on the battlefield to adjust their demands at the negotiating table. In Burundi, there is not clear evidence of this, and the difficulty of determining the military position of the FNL illustrates one of the hazards involved in multi-party conflicts.

Incentives to Hold Out

A third prediction that comes out of examining multi-party conflicts through a veto player framework is that actors in these conflicts will hold out at the negotiating table to try to get the best deal as the last signer. In Rwanda, therefore, I expect to see the combatants come to the negotiating table early while in Burundi I would expect to see groups refuse to participate. The veto player approach also predicts that in multi-party conflicts it will be the strongest non-state actors that hold out, since it is these groups who can pay the costs of continued fighting. In Burundi, then, the theory would predict that the stronger insurgent groups hold out the longest.

In Rwanda, there is evidence that both the government and the RPF were willing to negotiate fairly early in the conflict. When the conflict began in October 1990, the government was initially opposed to negotiations. This opposition was somewhat surprising, given how costly the conflict was even in its earliest phases. On November 23,

1990—less than two months after the conflict began—Africa Confidential reported, “The crisis has already resulted in a 40 per cent devaluation of the national currency” (p. 5).

Prunier (1995) argues that the government’s initial opposition to negotiations was the result of two different factors. First, Habyarimana used the RPF invasion to suppress internal political opposition which had been mounting against his regime. In the early months of the conflict, Habyarimana jailed many political opponents and cracked down on political opposition. Second, the Rwandan government was less inclined to negotiate because it anticipated military support from France to block the RPF attack. This support did come, but was not as robust as the government had expected.

The RPF, meanwhile, was more open to negotiations. Part of this openness was strategic as the RPF openly espoused a pro-democracy position, and was attempting to build political support both among Tutsi and Hutu within Rwanda. Opposition to talking to the government would have appeared contradictory to the group’s pro-democracy platform. In any case, the RPF agreed to sit down with the government early in the conflict to attempt to negotiate a cease-fire.

Despite the initial opposition on the part of the government to negotiations, face-to-face meetings between the two combatants happened early in the conflict. By March 1991, less than six months after the RPF invasion, the groups met in Zaire and negotiated a ceasefire. Several more meetings were held over the next year. None of these initial meetings produced anything substantive, but their cumulative effect was that the parties sat down to negotiate a comprehensive agreement in Arusha beginning in July 1992, 21 months into the conflict. As discussed earlier, these negotiations were by no means easy,

but they continued with few interruptions over the course of a year, indicating a general willingness by both sides to negotiate.¹⁸

In Rwanda, therefore, both combatants participated in face-to-face meetings from fairly early in the conflict and showed a general willingness to negotiate on the main issues. In Burundi, by contrast, getting the principal combatants to the negotiating table was one of the major barriers to resolving that conflict. While conflict began as early as 1991, and full-scale civil war came to Burundi by 1994, comprehensive peace negotiations did not begin until 1998. The difficulty in getting negotiations started was due to a general unwillingness to talk by all sides, as predicted by the veto player approach.

Once comprehensive negotiations did start, they did not include either of the two main combatants. Determining if this lack of participation meets the prediction about hold-out behavior is difficult, however, because it is not clear whether CNDD-FDD and Palipehutu-FNL would have participated in negotiations had they been allowed in. Julius Nyerere decided to bar CNDD-FDD, led by Jean-Pierre Nkunrunziza, and Palipehutu-FNL, led by Agathon Rwasa, from participating in the Arusha process because CNDD and Palipehutu were already participants in the negotiations, and he saw the two splinter factions as illegitimate.¹⁹ Nyerere gave Nkrunziza and Rwasa three options for participating: reconcile with the CNDD and Palipehutu (respectively) and join the negotiations under their current leaderships; use the process laid out in CNDD's and

¹⁸ It is debatable to what extent either the RPF or the government intended to implement any agreement they signed. However, the veto player theory predicts that combatants in multi-party negotiations would refuse to negotiate or sign an agreement, and so here the focus of discussion is on the willingness to participate in serious negotiations, not the intention to implement the agreement.

¹⁹ Jan van Eck (telephone interview, Feb. 2, 2006), argues that Nyerere was particularly opposed to including CNDD-FDD and Palipehutu-FNL because the groups had thrown out the political leadership through internal coups. Van Eck argues that Nyerere was very opposed to coups and that this opposition affected his decision about whether to include the groups.

Palipehutu's constitutions to become the legitimate head of the organization and represent the group at Arusha; or, form a new political party that would participate in the negotiations. Nkurunziza and Rwaswa refused all three of these options. Bentley and Southall (2005, p. 64-65) explain the logic for excluding these groups as follows:

Tanzanian Judge Paul Bomani, chosen by Nyerere as his chief aide, and other Tanzanian officials insist that the FDD and FNL effectively excluded themselves by declining Nyerere's three options. Inclusion in the talks had to be by consensus, and to include the rebels whilst they were still claiming to represent their original parties would have invited a walkout from the talks by the established leaderships, as well as inviting the proliferation of other splinter groups. The risks of including the rebel groups unless they made the requested concessions were therefore greater than those of excluding them.

Jan Van Eck argues that Nkurunziza and Rwaswa would have participated in negotiations had they been given a real option of joining the Arusha process. He describes the decision to engage in negotiations that excluded these principal combatants as the "fatal flaw" of the peace process. Even so, however, he indicates that at the beginning of the Arusha process there was little political will on the part of any of the participants to negotiate.²⁰

Even though the CNDD-FDD and Palipehutu-FNL did not participate in negotiations in Arusha, therefore, this is not clear evidence of "hold-out behavior" as predicted by the theory, since they were not allowed in. However, their latter behavior toward negotiations, particularly on the part of Palipehutu-FNL, does show them holding out and refusing to negotiate.

When Mandela became facilitator of the Burundian process following Nyerere's death, he stated a desire to incorporate the rebel groups into the negotiating process. He

²⁰ Jan van Eck telephone interview, Feb. 2, 2006.

was able to make some initial progress when small factions broke away from Palipehutu-FNL and CNDD-FDD and negotiated an agreement with the government to incorporate into the Arusha framework. However, Mandela had much more difficulty convincing Nkurunziza and Rwasa to negotiate in good faith with the government.

Mandela took over in late 1999 and made his first visit to Burundi in January 2000. He made early overtures to the rebel groups, and the South African facilitation team, later lead by Jacob Zuma, was able to persuade breakaway factions of CNDD-FDD and Palipehutu-FNL to negotiate quickly. The CNDD-FDD faction, led by Jean Bosco Ndayikengurukiye had broken away in 2001, and the Palipehutu-FNL faction, led by Alain Mugabarabona, broke away in 2002. Each faction was substantially smaller than the remaining organization. These factions reached agreement in 2002, stopped fighting, and incorporated into the government.

Additionally, the South African team coordinated face-to-face meetings between the transitional government and Nkurunziza's CNDD-FDD in January, April and September 2001, February 2002 and May-June 2002. However, these meetings were largely unproductive and it appears that CNDD-FDD had little willingness to negotiate sincerely at this point in the conflict. The group did finally enter comprehensive negotiations with the government in 2003, resulting in the Pretoria accord and the end of its participation in the conflict.

The CNDD-FDD, then, showed opposition to entering negotiations when Mandela first proposed them in 1999, and indeed, it took more than a year to get their leadership to meet with the government. Even once these meetings did happen, it took several years for the group to agree to a deal. This is not clear evidence of hold-out

behavior as predicted by the veto player theory. However, it does show that CNDD-FDD was less open to negotiations with the government than the RPF had been in the Rwandan example described above.

The behavior of Palipehutu-FNL, meanwhile, fits better the prediction that combatants in multi-party conflicts will hold out. Despite Mandela's openness to including the group in the peace process, Palipehutu-FNL has continued to refuse to participate in serious negotiations with the government into 2006. There have been some face-to-face meetings, but these have been completely unproductive and the group has shown a general unwillingness to negotiate. Additionally, the FNL increased its combat activity when the government and CNDD-FDD entered into comprehensive negotiations and when those combatants signed an agreement. This behavior is evidence of the type of hold-out behavior predicted in multi-party conflicts.

The veto player theory also predicts that in multi-party conflicts it will be the strongest non-state actors that hold out the longest, because these groups can pay the costs of continued fighting the longest. A comparison of the behavior of CNDD-FDD and Palipehutu-FNL does not support this prediction, since the former was clearly the stronger combatant, and the latter has held out the longest. However, CNDD-FDD and Palipehutu-FNL were not the only rebel groups in the conflict, they were the strongest, and the weaker combatants showed a greater openness to negotiations both during and after Arusha.

In 1998, when the Arusha negotiations began, there were three main Hutu insurgent groups, CNDD, Palipehutu and Frolina. Frolina was a group that had broken away from Palipehutu in 1990 and had formed an armed wing. The group had

participated in the conflict and, although the smallest of the three main insurgents, had grown to a point in 1997 when it was a noticeable participant in the conflict. In that year, several clashes between government and FROLINA troops in the Makamba province on the border of Burundi resulted in several hundred deaths between the two sides. While CNDD-FDD and Palipehutu-FNL were not allowed to participate in the negotiations, Frolina did go to Arusha and negotiated as part of a bloc of Hutu parties. Frolina was a signatory to the Arusha Accords in 2000 and implemented the agreement.

Meanwhile, smaller factions in the Burundian conflict continued to show a greater willingness to negotiate after the Arusha accords. In 2000-2001, when Mandela tried to incorporate the CNDD-FDD and Palipehutu-FNL into the Arusha framework, he was able to convince factions of each group to break off and sign a deal. In both cases, it was the smaller factions that were willing to negotiate earlier, and it was the militarily strong factions, led by Nkurunziza and Rwasa, that continued to hold out and participate in the conflict.

In Burundi, then, it was the two strongest insurgent groups that held out the longest. The relationship between military strength and holding out was not linear, since CNDD-FDD was both stronger and more willing to negotiate than Palipehutu-FNL. However, the smaller groups like Frolina and the minor factions of CNDD-FDD and Palipehutu-FNL participated in negotiations much earlier than the stronger insurgent groups.

The prediction that combatants in multi-party conflicts will hold out and refuse to participate in negotiations receives support in a comparison of the Rwandan and Burundian conflicts. In Rwanda, both parties came to the negotiating table early and

showed a willingness to negotiate in good faith on a range of issues. In Burundi, by contrast, the actors did not enter comprehensive negotiations until later in the conflict and the two main combatants did not participate in the main three-year-long negotiation process. This lack of participation was at least partly caused by onerous pre-conditions for their participation. However, their unwillingness to meet these pre-conditions and their behavior once they were given the option by Mandela to negotiate suggests that both CNDD-FDD and Palipehutu-FNL were generally less willing to negotiate than the RPF in Rwanda or the weaker insurgents in the Burundian conflict.

Shifting Alliances

A final prediction that arises from the theoretical discussion in this project is that negotiations in multi-party conflicts will be plagued by problems of shifting alliances. That is, the various parties at the negotiating table will form negotiating blocs around some issues that will break down along others. For this final mechanism, there is only a prediction about the Burundian case, since the possibility for shifting alliances only comes up when there are more than two parties at the negotiating table.

Close analysis of negotiations in Burundi shows support for the effect of shifting alliances on civil war negotiations. In Burundi, much like Rwanda, the conflict broke out heavily along ethnic lines. In Burundi, there were a set of Hutu insurgent groups opposed to a Tutsi-dominated government and military. The conflict negotiations were made more complicated by the presence of a large number of unarmed opposition parties, but most of those parties were also organized around ethnic lines. Given how politicized ethnicity

was in the country, a likely focal point of organization in Burundi, therefore, would be along ethnic divisions.

In the Arusha process negotiating blocs did form around ethnic issues. The 19 parties present in those negotiations coalesced into three blocks—the G2, which consisted of Buyoya’s government and the national assembly, the G10, made up of the Tutsi parties present, and the G7, which consisted of the 7 Hutu parties including Frolina. Although key differences existed between the parties within these various groups, for the most part they negotiated in ethnic groupings, making a 19-party negotiation somewhat feasible.

Additionally, the insurgent groups cooperated along ethnic lines. While CNDD-FDD and Palipehutu-FNL had separate leadership structures, the groups often coordinated their military actions and provided support to each other. This cooperation continued for years until the CNDD-FDD entered into serious negotiations with the government. When the CNDD-FDD signed the Pretoria accord, however, the dangers posed by shifting alliances among groups in multi-party conflicts became clear.

The signing of the Pretoria Accord revealed cracks both in the coalition of parties that had signed the Arusha accord and between the CNDD-FDD and Palipehutu-FNL. Van Eck (p. 1) argued in October 2003 that “A significant new feature of the Burundian peace process is the fact that two clear and opposing alliances are developing within the Burundian body politic. Broadly speaking, they can be described as ‘The Arusha Alliance’ and ‘The Anti-Arusha Alliance.’” The Arusha Alliance consisted of the G2, G7, some of the parties in the G10, the smaller factions of CNDD-FDD and Palipehutu-FNL which had signed agreements in 2002 and, after the Pretoria Protocol, Nkurunziza’s CNDD-

FDD. The Anti-Arusha Alliance, meanwhile, consisted of Rwasas's Palipehutu-FNL, some of the Tutsi parties in the G10, a handful of political parties that had been excluded from Arusha, and "a growing cross-section of Tutsi opinion within civil society" (p. 1) This shifting of the axis of conflict within the country produced some very interesting effects.

The first effect is that the Tutsi hard-line parties that were opposed to Arusha made active attempts to work together with Palipehutu-FNL (van Eck 2003). These attempts were surprising given the history of ethnic politicization in Burundi. Palipehutu was notorious as a strong Hutu-based party formed in Hutu refugee camps in Tanzania and dedicated to overturning the monopoly the Tutsi held on power for most of post-independent Burundi. These Tutsi political parties, meanwhile, have a completely separate view from Palipehutu (and the members of Palipehutu-FNL) about virtually every aspect of politics and society within Burundi. However, the dominance of CNDD-FDD and FRODEBU in the government has pushed them into a temporary alliance with a group with completely separate views about the future. This alliance has led to a strengthening of Palipehutu-FNL's military position.

The second effect is that the current, CNDD-FDD led, Burundian government has taken a harder-line stance against negotiating with Palipehutu-FNL than the transitional government of 2001-2004 and even than the Tutsi-led government of Buyoya. This dynamic is surprising given that CNDD-FDD and Palipehutu-FNL had been strong allies before and have quite similar platforms. However, Nkurunziza's government has pushed the international community to declare Palipehutu-FNL as a terrorist organization and the new, partially integrated, military has increased military

activity against the group.²¹ The hard-line stance presents a serious barrier to incorporating Palipehutu-FNL into a comprehensive peace, and is likely motivated by CNDD-FDD's desire to hold onto as much power as possible.

The combination of these two effects—the emergence of an “Anti-Arusha Alliance” made up of hard-line Tutsi and hard-line Hutu groups and increased opposition to negotiations on the part of the government—shows the difficulty of negotiating an agreement among multiple groups. While the 1997-2000 Arusha process and the 2001-2003 negotiations between CNDD-FDD and the government resulting in the Pretoria Protocol could be seen as substantial progress toward resolving the Hutu-Tutsi dimension of conflict, they have also resulted in a new axis of conflict. This new axis revolves around the issue of inclusion or exclusion from government and presents a significant barrier to reaching peace in the country. Additionally, if the government can make progress in negotiations with the Anti-Arusha alliance, that negotiating bloc is almost certain to break down along Hutu-Tutsi lines when issues of power-sharing and minority rights are raised since the various groups included have entirely different views about those issues.

Shifting alliances between the different combatants and political parties in Burundi, therefore, have been and continue to be a major barrier to resolving that conflict. This discussion illustrates a major problem in multi-party bargaining—progress

²¹ Interview with Henri Boshoff, Pretoria, South Africa, Feb. 2, 2006. Boshoff also indicated another result of the falling-out between CNDD-FDD and Palipehutu-FNL—closer relations between the government of Rwanda and Nkurunziza's government in Burundi. The Rwanda-Burundi ties are surprising given that the government in Kigali is dominated by Tutsi and the government in Bujumbura is dominated by Hutu. However, both Rwandan president Paul Kagame and Nkurunziza perceive threats from insurgent groups and find common-ground in their attempts to get the international community to put pressure on these groups.

in negotiations between two parties can actually harden divisions among others and make negotiations there more difficult. This issue does not arise in two-party conflicts and is a major factor making negotiations in multi-party conflicts more complicated.

The Breakdown of the Rwandan Agreement

The above discussion used detailed analysis of negotiations in Rwanda and Burundi to test four specific predictions about the differences between two-party and multi-party bargaining. These predictions are direct implications from the veto player approach presented in this project. This comparison shows good support for three of the four predictions derived from that theory. While there were not a larger number of issues under negotiation in Burundi than in Rwanda, there is good evidence that the parties in Rwanda were more able to use their performance on the battlefield to make more realistic demands, that combatants in Burundi were more likely to hold out and not participate in negotiations, and that shifting alliances among groups on different issue areas has been and remains a significant barrier to a negotiated peace in Burundi. The combined effect of greater information asymmetries, parties holding out and refusing to negotiate, and shifting alliances has been that despite a nearly decade-long peace process, a comprehensive agreement among all combatants to the Burundian conflict has proven elusive.

Beyond these four specific predictions, the general expectation arising from the veto player approach is that the parties should find it easier to reach a sustainable agreement in Rwanda than in Burundi. At first glance, this general prediction holds up. Serious negotiations in Rwanda began less than two years after the conflict broke out and

in thirteen months generated a comprehensive peace agreement signed by both parties. In Burundi, meanwhile, the negotiations started much later and have yet to produce a comprehensive agreement almost ten years later.

However, there is an important distinction here between the signing and the implementation of an agreement. It is true that in Rwanda an agreement was reached fairly early in the conflict. However, it is impossible to call that negotiation process a success for two reasons.

The first reason the Arusha process was not successful is that the agreement of August 1993 broke down in April 1994 in the outbreak of mass genocide. The nine months between the signing of the Arusha accords and the outbreak of the genocide were characterized by partial implementation and frequent violations by both sides and the international community. A transitional government was formed and some small moves were made toward military integration. Additionally, a small international observer mission was deployed, but members of the United Nations Security Council stalled and it was given a less robust mandate than that called for in the Accords. Meanwhile, however, the political climate in Kigali became more charged as assassinations mounted and each side accused the other of not implementing the agreement.

The Arusha agreement was completely abandoned by both sides when President Habyarimana was killed on April 6, 1994, when his plane was shot down landing at Gregoire Kayibanda International Airport in Kigali. There is much debate about who was responsible for shooting the plane down. Some argue that hard-line Hutu assassinated Habyarimana out of dissatisfaction with his concessions to the RPF. Others blame the RPF and believe that the group was unhappy with the speed of implementation of the

Arusha accords and wanted to return to war. In either case, the assassination proved the spark for the genocide. For months, hard-line members of the army and Habyarimana's party had laid the groundwork for a mass-extirmination campaign targeted against political opponents and the Tutsi ethnic group. Within hours after Habyarimana's death, roadblocks were set up and killings begun. In the initial hours, these killings targeted moderate Hutu politicians who might stand in the way of the extermination campaign, but by the next day systematic killing of Tutsi civilians had begun. Over a period of 100 days, from April to July 1994, at least 500,000 Tutsi were killed, representing as much as 75% of the pre-war Rwandan Tutsi population.²²

The outbreak of genocide also saw the return of Rwanda to civil war. The RPF abandoned the Arusha accords when the first massacres started, and instead sought to consolidate its control over the country. Again, the RPF proved a more effective fighting force than the army, but its progress toward achieving military victory was slow. Finally, in July, the RPF moved into Kigali and took control of the country. The genocide and civil war ended together as the RPF defeated the Rwandan army, much of which fled into refugee camps in Zaire

The presence of these refugee camps in Zaire is the second reason the Arusha accords cannot be considered a "success." While the Rwandan civil war ended officially when the RPF took Kigali, the underlying Hutu-Tutsi conflict and the presence of Hutu refugees in eastern Zaire was the primary cause of a bloody six-year civil war in that country. In 1994, the former Rwandan Armed Forces (ex-FAR) gained control of the

²² The Rwandan genocide is a well-researched topic that has been analyzed in a range of scholarly books and articles. The most complete description of the genocide is Des Forges (1999).

refugee camps on the border and used them as a way to regroup and to organize to launch attacks back into Rwanda. They did so throughout 1995 and into the summer of 1996 when the Rwandan army (now consisting of the RPF) attacked the refugee camps. This attack drove more than a million refugees back into Rwanda and hundreds of thousands of others deeper into Zaire. The Rwandan army (joined by Uganda and Angola) marched all the way to Kinshasa and overthrew Zairean President Mobutu, who had historically been an ally of the Habyarimana government and who had supported the ex-FAR in eastern Zaire. The overthrow of Mobutu led to a temporary peace, however, by the summer of 1998 relations between Rwanda and the new government of the now-renamed Democratic Republic of the Congo (DRC) had soured, again over the issue of the Hutu guerillas. In August 1998, a new conflict broke out when Rwanda and Uganda again launched attacks into DRC, and that war lasted until 2001 and cost the lives of an estimated four million people. Although that war officially ended, there are still thousands of Hutu militants operating in the jungles of eastern DRC and they represent a continued security threat for the region.

Despite the disastrous consequences of the breakdown of the Arusha accords, their failure does not directly contradict the veto player approach. Walter (2002) argues that we should think of conflict resolution as a three-stage process: a negotiation phase in which combatants decide whether or not to initiate negotiations, a bargaining phase in which groups negotiate a specific agreement, and an implementation phase in which the groups must implement that agreement. She demonstrates that different factors affect the decision to negotiate, the ease of reaching a negotiated agreement, and whether or not an agreement will be implemented. The veto player approach makes specific predictions

about the effect of number of parties on the first two phases of conflict resolution, but says less about the implementation phase.

In the Rwandan case, however, the breakdown of the agreement is important. The prediction from a veto player analysis is that combatants in two-party conflicts should be able to negotiate an implementable agreement easier than participants in multi-party conflicts. In the next section, I will argue that, despite the breakdown of the Rwandan Arusha accords, these cases show support for that prediction.

The Breakdown of the Rwandan Peace

The viability of the Arusha accords for resolving the conflict in Rwanda has been a subject of much debate among academics and policymakers interested in understanding the causes of the Rwandan genocide. In broad terms, this debate can be categorized into two camps. The first group argues that the Arusha accords were doomed from the start and could not possibly have resolved the conflict. The second set of scholars argue that the Arusha accords was a workable framework that could have built a sustainable peace, but that factors outside of the Arusha process doomed the agreement. In this section, I will argue that the Arusha accord was a viable agreement which could have resolved the Rwandan conflict but that three separate factors led to its breakdown.

The argument that Arusha was a fundamentally unworkable agreement is based on the perception that the RPF gained too much in the agreement. Lemarchand (1994, p. 591-592) writes:

...the transition bargain in Rwanda emerges retrospectively as a recipe for disaster: not only were the negotiations conducted under tremendous external pressures, but, partly for this reason, the concessions made to the (RPF) were

seen by Hutu hard-liners as a sell-out imposed by outsiders. For the Tutsi 'rebels' to end up claiming as many cabinet posts in the transitional government as the ruling MNRD...as well as half of the field grade officers and above, was immediately viewed by extremists...as a surrender to blackmail.

Lemarchand and others argue that the Arusha accord left the MNRD in such a bad position that Hutu extremists in the ruling party's inner circle were able to use it to gain further support to sabotage the peace. For them, the Arusha accords could not possibly have been implemented because it gave so much to the insurgents.

There are two problems with the argument that the Arusha accord was unfeasible because it gave the RPF too much political and military power. First, by the time the Accords were signed the RPF had an undeniable military advantage on the ground. Habyarimana may have been under external pressure to sign an agreement giving the RPF an equal share of the military and interim government, but there is no reason to think the RPF would have signed any agreement giving them less. This was particularly true after the February 1993 offensive when it was clear to all sides that the government could not win a military campaign, and consequently the RPF had the upper hand at the negotiating table. An accord that gave less to the RPF was not feasible.

Second, while it is true that Hutu extremists were able to sabotage the peace, there is no evidence of a mass rejection of the agreement by supporters of Habyarimana. Rather, a core group of Hutu in the president's inner circle who stood to lose big in the implementation of the agreement began working to plan genocide almost immediately once the agreement was signed. The infrastructure developed for this mass extermination campaign was elaborate, with a Hutu power radio station that used hate speech to drum up anti-Tutsi sentiment, the collection of names of Tutsi in regions across Rwanda, and

assassinations of Hutu politicians who might stand in the way. However, the number of Hutu that participated in the core development of this infrastructure for genocide was small, perhaps numbering in the low hundreds. The fact that these few individuals were able to undermine the agreement does not demonstrate a fundamental problem with the Arusha accord, but rather illustrates the failure of the international community to deploy a minimal amount of force to stop them from doing so.

This discussion of the role of the international community leads to a second school of thought on the collapse of the Arusha accords and the outbreak of genocide. In contrast to Lemarchand, a number of other scholars and experts on the Great Lakes region of Africa have focused on factors outside of the Arusha process that led to the failure to implement peace in Rwanda.

One of the main arguments these scholars make is that the Arusha accords collapsed because the international community failed to send a credible peacekeeping mission to monitor and enforce implementation of the agreement. Walter (2002) argues that the implementation phase of peace agreements in civil war is inherently prone to commitment problems because both sides have incentives to renege on their commitments once the other side begins disarming. One of the cases she examines in detail is Rwanda and she writes (p. 158):

Rwanda failed to end its civil war peacefully because neither the Rwandan government nor the RPF believed the UN would be able to ensure the peaceful transfer of power, and it was the fear of exploitation in the immediate aftermath of signing the accords that ultimately paralyzed the process and opened the door for abuse. A close examination of the peace process shows that the Rwandan government and RPF rebels sought the UN's involvement, that they pushed for the rapid deployment of UN forces, and that they pressed for additional assistance as hard-liners gained strength. The less credible the UN became, the

more empowered the extremists' forces became, and the less likely either side was to proceed with implementation.

Other analyses of the Rwandan peace process support this argument. Romeo Dallaire (2003), force commander of the UN Assistance Mission for Rwanda in 1993-1994, writes that in August 1993 his team called for an ideal mission of 5,500 peacekeepers in Rwanda. Instead, the UN agreed to send 2,217 peacekeepers, and these soldiers arrived slowly and with limited resources. Additionally, as evidence mounted that a group of extremists was working actively to undermine the Arusha agreement, the UN soldiers were prevented from taking action. Des Forges (1999, p. 172) writes:

The preparations for violence took place in full view of a U.N. peacekeeping force. The commander of that force reported evidence of the worsening situation to his superiors who directed him to observe the narrowest possible interpretation of his mandate. He was in effect to do nothing but keep on talking with the authorities while they keep on preparing the slaughter.

For Walter, Dallaire, and others, then, the principal reason the Arusha accords collapsed is that the international community failed to deliver on its promise to deploy a credible peacekeeping mission. The lack of this mission meant that Hutu extremists were able to organize a mass execution campaign even with the UN presence in Rwanda and while the two combatants were supposed to be implementing the agreement.

Filip Reyntjens, a Belgian academic who has closely analyzed the politics of the former Belgian colonies for decades, disagrees with both the arguments that the Arusha accord was fundamentally unworkable and that the agreement broke down due to the failure of the international community to deploy a credible peacekeeping mission. In an interview in Antwerp Belgium in April 2005, Reyntjens argued that the Arusha accord

was a workable agreement that could have led to a peaceful resolution of the conflict, but that two related factors led to its breakdown.²³

The first factor was the assassination of Burundian President Ndadaye in October 1993 by the Tutsi military. For most of Burundi's independent history, the government had been dominated by Tutsi. In 1988, however, following massacres committed by the Tutsi-dominated military against members of the Hutu population, international pressure increased on Burundian President General Pierre Buyoya to open up the political process. In 1989, Buyoya agreed to implement reforms that included integrating more Hutu into the government and establishing a national commission made up of both Hutu and Tutsi to investigate ethnic relations in the country. These transformations led to multi-party elections in 1993 which brought a Hutu President, Melchior Ndadaye, to power and gave the Hutu-dominated FRODEBU party a majority in parliament.

In October 1993, Ndadaye was assassinated by hard-line Tutsi in the army opposed to Buyoya's reforms. That assassination touched off violence against Tutsi and massive reprisals by the army against Hutu civilians. Ndadaye's assassination and the subsequent ethnic violence increased fears in Rwanda that the RPF was planning something similar at the same time that group was supposed to be integrating into government and the military. The reaction of some Hutu politicians to the Burundian putsch can be seen in a speech on October 23, 1993, by Froduald Karamira, second vice president of the Democratic Republic Movement. Karamira accused Kagame of being one of the people behind Ndadaye's assassination. Additionally, he said (quoted in Des Forges 1999, p. 138):

²³ Author interview, Antwerp, Belgium, April 21, 2005.

We cannot sit down and think that what happened in Burundi will not happen here, since the enemy is among us.... We have clarified what we must avoid. Avoid fighting against Hutu. We have been attacked, so let us not attack ourselves. Let us avoid the invasion of the enemy who may steal our government.

In essence, the events in Burundi convinced some Rwandan Hutu of the hard-line Hutu Power position that the RPF could never be trusted as a legitimate negotiating partner and widened the ranks of extremists opposed to the implementation of the Arusha Accords.

A second and related factor that Reyntjens discussed is a shift in the balance of power in the transitional government. The Arusha accord effectively created a transitional government based on a tri-partite arrangement: the RPF, the MNRD and the unarmed opposition parties each had one-third of the seats in government. This tripartite arrangement meant that the RPF and the MNRD had to compromise with these middle groups to accomplish anything in government, and could not impose anything unilaterally. However, in the aftermath of the Arusha accords, two of the main unarmed opposition parties, the Democratic Republican Movement (MDR) and the Christian Democrat Party (PDC) split into two factions, a Hutu-power faction (which allied itself with the MNRD) and another faction that generally allied itself with the RPF. Additionally, the parties that did not split were generally internally divided in this fashion. The effect of this transition meant that there was a bi-polar balance of power on top of an institutional tri-partite framework, and Reyntjens argues that this arrangement led to polarization.

It is difficult to determine which of these three factors was more important. However, the combined effect of the failure of the international community to deploy a

peacekeeping mission, the increase in support for hardline Hutu politicians after the Burundian putsch and the polarization of the transitional government increased the opportunities for individuals in Rwanda to spoil the peace agreement. The growth in the position of spoilers was particularly problematic because there were individuals on both sides with clear incentives to undermine the agreement. On the government side, members of the military that faced demobilization stood to lose everything in a post-Arusha government, and were largely opposed to the agreement. In the RPF, meanwhile, there were legitimate reasons to fear total marginalization from politics once a transition to democracy was made. The RPF got a preview of its electoral prospects in mayoral elections in late 1993, they competed for 13 elections and won zero.

The above discussion suggests that the Arusha accord was a workable agreement to end the conflict but that three different factors undermined it and gave extremists on both sides the opportunity to spoil the agreement. If this argument is correct, then the breakdown of the Arusha accords does not suggest a problem with the veto player approach. The argument in this dissertation is that it is easier for combatants to reach a sustainable agreement in a two-party than a multi-party conflict. However, the theory does not make a specific prediction about the implementation of that agreement, since that phase is affected by a variety of factors that are different from those affecting the negotiation and bargaining phases. A close analysis of the Rwandan and Burundian conflicts suggests that in those cases negotiating a workable peace was easier in the two-party conflict, but that the breakdown of that peace was due to factors outside of the bargaining process.

Conclusion

A comparison of negotiations in Rwanda and Burundi illustrates the difficulties inherent in multi-party bargaining. In Rwanda, both combatants showed a willingness to negotiate fairly early and comprehensive peace negotiations took place early in the conflict. These negotiations were by no means easy; however, over the course of one year the combatants were able to reach an agreement that gave each a share of political and military power approximate to their position on the battlefield. While this agreement did break down nine months later in the worst example of mass slaughter over the last sixty years, that breakdown did not occur due to some fundamental flaw in the negotiation process, but rather was the result of factors outside of the peace process.

In Burundi, by contrast, getting the combatants to talk to each other at all was a major challenge that took years to overcome. Once negotiations between the government and the main combatants did take place, they were made complicated because the main insurgent groups showed a tendency to hold out and because determining how strong the different insurgent groups were relative to each other was difficult. Additionally, a series of shifting alliances between the government, insurgents, and unarmed opposition parties made finding one comprehensive settlement that incorporated all parties very difficult. The combined consequence of these barriers to settlement is that the Burundian conflict continues into 2006, more than a decade after it began.

This qualitative analysis of these two cases, combined with the quantitative analysis presented in chapter three, provides strong support for the veto player argument developed in this dissertation. The theoretical discussion in chapter two suggested that civil wars will be harder to resolve, and therefore longer, when there are more actors that

can block agreement. The quantitative analysis in chapter three illustrated that there is in fact a correlation between the number of veto players and the duration of civil war for all wars begun in the last sixty years. The discussion in this chapter built on that finding by testing specific predictions arising from the veto player approach about differences between two party and multi party negotiations in Rwanda and Burundi and found support for those predictions.

This analysis shows the usefulness of detailed analysis of conflict negotiations as a way to test theories of civil war bargaining. Analyzing negotiations is an underutilized tool given that much of the current international relations literature on conflict duration and termination is based on assumptions about bargaining. These assumptions, however, are almost never tested empirically on cases of civil war negotiations in actual civil war. Doing so can help to improve theory testing by more directly investigating the causal logic presented.

While these case studies do support several elements of the veto player approach, they also suggest areas where refinement is needed. First, in chapter two, I argued that the major factor leading some conflicts to be multi-party is that they occur in more diverse societies. Rwanda and Burundi are nearly identical in terms of population size, ethnic fractionalization, colonial history and other factors generally thought to influence preference heterogeneity, yet, the conflict in Rwanda was dyadic while the Burundian conflict was multi-party. This suggests that, at least in these cases, some other dynamic is driving the variation in the number of combatants. Further theoretical work is needed on the conditions that lead some civil wars to involve multiple actors.

Second, in both Rwanda and Burundi the external parties facilitating the negotiations played a substantial role in shaping the peace agreement reached. The veto player approach, along with most other theories of conflict bargaining, views negotiations as a process where the combatants seek to find agreement over a set of issues and try to overcome problems of information asymmetries, credible commitments, incentives to hold out and shifting alliances. However, in both conflicts the facilitation teams had a major role in setting the agenda and in determining who was at the table. The extreme example of this is Julius Nyerere's decision to bar the two main combatants from participating in the 1998-2000 Burundian peace process. That decision created an additional barrier to resolving that conflict outside of all of those suggested in the veto player approach because a peace agreement excluding CNDD-FDD and Palipehutu-FNL could not possibly have ended the conflict. This dynamic suggests that theories of conflict duration and termination need to address more fully the role of international facilitators since, in the Burundian case in particular, the nature of the conflict resolution effort was a major barrier to resolving the conflict.

Finally, the veto player approach argues that the traditional focus on only two-parties in conflict is too narrow, and develops a theory of the effect of multiple combatants on civil war. However, these cases show that non-combatants can also have a significant effect on negotiations in conflict. In both Rwanda and Burundi, unarmed opposition parties were at the negotiating table, got concessions in a peace agreement, and had a significant role in the implementation phase of that agreement. While the veto player approach is based on a more accurate analysis of variation in the numbers of

combatants across civil wars, these cases suggest that it is still too narrow to incorporate all actors that influence the duration and termination of conflict.

Chapter Five

Conclusion

This dissertation began with an empirical question: why are some civil wars resolved quickly while others drag on without resolution? I have shown that a major factor affecting how amenable conflicts are to resolution, and therefore how long they last, is the number and composition of combatants who have the ability to block settlement. While scholars of International Relations have focused narrowly on civil war as a two-party phenomenon, in reality there is wide variation across conflicts in the number of actors that must consent to end a war. The more of these veto players there are, the harder conflicts are to resolve and the longer they last.

Civil wars with more veto players are harder to resolve due to four dynamics that arise in multi-party conflicts. First, the bargaining range of acceptable agreements shrinks when additional actors with diverse preferences must consent to any agreement. Second, the battlefield reveals information less clearly in multi-party conflicts, making it harder for combatants to become more realistic about their chances of winning the conflict outright and adjust their demands at the negotiating table accordingly. Third, all actors in multi-party conflicts have incentives to hold out in the hopes of getting the best deal as the last signer, and so parties negotiate harder, leading negotiations to drag on. Finally, parties are unable to overcome these barriers to settlement by forming overarching negotiating blocs because of shifting alliances among parties on different issue areas. The combined effect of these four dynamics is that negotiating a sustainable agreement is substantially more difficult when there are more veto players involved in civil war.

A comparison of negotiations in Rwanda and Burundi reveals the difficulties of bargaining in multi-party conflicts. In Rwanda, the two main combatants had face-to-face meetings within six months of the outbreak of fighting and initiated comprehensive negotiations 21 months after war began. These negotiations were by no means easy, but over a period of a year the government and the insurgents were able to reach an agreement that addressed all major issues in the conflict. Although this agreement collapsed in the outbreak of genocide, the breakdown of the accords was primarily the result of factors outside of the peace process.

In Burundi, by contrast, serious negotiations did not begin until at least four years into the civil war. Even so, these negotiations did not include the two main combatants and over a two-year period produced only a partial peace agreement. Convincing the strongest combatant to lay down its arms and join a transitional government took another three years, and the conflict continues today as the last remaining rebel group refuses to incorporate into the government. Achieving a sustainable peace in Burundi has been significantly complicated by the inability of combatants to use their performance on the battlefield to adjust their demands at the negotiating table, the tendency of the main combatants to hold out and refuse to participate in negotiations, and shifting alliances on different issue areas that prevent the emergence of an inclusive agreement.

This difficulty in resolving multi-party conflicts is not limited to Central Africa. Over the last sixty years, civil wars that have contained more actors who could block settlement have been substantially longer than those that contained less. Multi-party conflicts such as those in Colombia, Afghanistan, Somalia, Liberia, and Burma have lasted

for decades and have been incredibly costly for the individuals, communities, countries and regions involved, and for the international community as a whole.

This dissertation has shown that analyzing civil wars through a veto player framework contributes significantly to our understanding of the duration and termination of these conflicts. In this chapter, I conclude that discussion by showing the implications of a veto player approach in three areas. First, I revisit the findings of the empirical analyses to analyze what they contribute to our understanding of war duration and termination and to suggest areas where further theoretical and empirical work is needed. Second, I extend these findings beyond the study of civil war by suggesting implications for the field of Political Science more generally. Third, viewing the duration and termination of civil war through a veto player approach leads to direct implications for policymakers interested in designing responses to these conflicts and I suggest strategies that are likely to be effective in addressing the dynamics of multi-party civil wars.

Findings

Quantitative Analysis

The quantitative analysis in chapter three identified a strong correlation between the number of veto players and the duration of all civil wars begun since 1945. Civil wars with more veto players last longer, and this finding remains when controlling for factors that could affect both the number of parties and the duration of conflict. Further analysis revealed that the relationship between veto players and the length of civil wars is not limited to one region or period in time but is rather a general trend across conflicts over the last sixty years. Additionally, the findings in the statistical analysis are consistent when

different measures of the veto player concept are used, including an independent measure not collected by the author.

The veto player approach also argues that the type and strength of veto players should affect the duration of civil war. Statistical analysis revealed that conflicts are longer when the veto players involved are stronger; but did not show support for the argument that conflicts are more resistant to resolution when the combatants have more diverse preferences. However, the measure of veto player preferences was crude, and better data on the type of actors present in multi-party conflicts is needed to more fully test this argument.

Chapter three also presented tests of two prominent arguments about civil war duration in the literature—that civil wars are longer when they are less costly for the actors involved and when more extreme commitment problems are present. Commitment approaches were generally supported in the tests and, in particular it was shown that civil wars are substantially shorter when the international community makes a credible guarantee to enforce a settlement. None of the tests showed support for war cost approaches, however, the measures used to test these variables in this and other studies represent distant proxies of the theoretical arguments underlying these approaches. Better data on the costliness of war to individuals and groups is needed to more fully examine the plausibility of these theories. Finally, the analysis in chapter three did not include a test of informational approaches to war duration, since there currently exist no measures of information asymmetries in conflict. Our ability to fully test explanations of the duration of war would be improved with data collection focused on the outcomes of

battles in conflicts, since those data would allow for direct testing of the relationship between information revealed by the battlefield and the duration of civil war.

Case Studies

The statistical analysis showed a correlation between the number of combatants and the duration of civil war that supported the veto player argument developed in this dissertation. The theoretical argument leading to that predicted correlation, however, is based on four specific arguments about the dynamics of multi-party bargaining. There is no way from the statistical analysis to know if those dynamics are driving the correlation between veto players and duration. To test for these specific causal mechanisms, I conducted detailed comparative analysis of civil war negotiations in Rwanda and Burundi.

This qualitative analysis showed evidence of three of the four mechanisms predicted by the veto player theory. Contrary to the argument in chapter two, the negotiations in Burundi did not revolve around a larger set of issues than those in Rwanda despite the presence of more actors at the negotiating table. However, consistent with the veto player approach, combatants in Burundi have had greater difficulty using the battlefield to adjust their positions at the negotiating table, been more likely to hold out and to refuse to participate in serious negotiations, and shifting alliances have led to the breakdown of existing blocs and emergence of new ones. The combination of these dynamics has meant that almost eight years of continual negotiations in Burundi have failed to produce a conclusive end to the war.

While these cases do show support for the veto player approach, they also reveal some interesting dynamics that are surprising given existing theories of conflict

negotiations. First, in both conflicts non-combatants had a significant role in the negotiation process, were given concessions in the peace agreement, and were instrumental in the implementation phase of the agreement. The literature on civil war has focused almost exclusively on the attributes of combatants and how those attributes affect the duration and termination of conflict and there is little theoretical and empirical work on the effect of non-combatants in civil war. If the role of non-combatants in Rwanda and Burundi is a dynamic that is not unique to those conflicts, but rather a more general trend, this lack of attention is problematic because their presence in a peace process should affect the incentives that combatants have to negotiate, sign, and implement an agreement.

The analysis in this dissertation shows that viewing conflict as exclusively a two-party phenomenon hinders our ability to explain the duration and termination of civil war and has presented a theoretical framework for analyzing variation in the number of actors in conflict. Within this framework all of these actors are still combatants, however, the case studies suggest that civil war is an even more complex phenomenon in which non-combatants have a role in negotiating and implementing a sustainable peace. This dissertation advances our understanding of conflict by developing and testing a theory that more accurately captures the realities of civil war, but the significant presence of non-combatants in the Rwandan and Burundian conflicts suggests that even further work in this area is needed.

Second, in recent years scholars who study civil war have placed significant focus on the implementation phase of peace agreements, and this focus is warranted. In general, this literature has examined the implementation of an agreement as an extension

of the bargaining process that occurred earlier in the conflicts by, for example, concentrating on the incentives that participants in the negotiations have to implement the agreement signed. A reading of the Rwandan and Burundian cases, however, shows that the signing of a peace agreement can create a new axis of societal conflict that involves new constellations of groups supporting or opposing the government. In Rwanda, this dynamic played out as political parties that had opposed both Habyarimana's government and the RPF split into Hutu-power and non-Hutu-power factions. Some of these parties had their own militias and these groups became instrumental in the genocide.

In Burundi, the signing of the Arusha accord and the implementation of CNDD-FDD into government created a division between two broad coalitions: a pro-Arusha coalition that included CNDD-FDD and an anti-Arusha coalition that included Palipehutu-FNL and several Tutsi parties. This shift in the axis of conflict strengthened the position of Palipehutu-FNL by giving them support from Tutsi. Additionally, the emergence of a significant Tutsi opposition to the Arusha framework threatens long-term stability in Burundi as it presents the possibility of a Tutsi-led insurgency against a CNDD-FDD led government in the future.

These examples show that the focus on implementation of agreements is warranted, but that more attention should be paid to how the implementation phase differs from earlier phases of conflict. In chapter two, I argued that it is useful to think of civil war as a process of bargaining over policy, in which actors decide whether they prefer a new policy to the existing status quo. The signing of a peace agreement can introduce a new status quo policy and change the decision calculus of various actors—

both combatants and non-combatants—for whether they prefer that new status quo. Scholars have correctly recognized that implementation of agreements is a difficult process due to the incentives combatants have to undermine the agreement, but this analysis suggests that an agreement can also create new actors that have an incentive to challenge the status quo, and lead to further conflict.

Implications for Political Science

The theory presented in this dissertation views civil war as a form of violent bargaining over policy in which a set of actors can block the implementation of a new policy through continued warfare. This argument is focused specifically on explaining the duration and termination of internal conflict, but has implications for broader topics within Political Science.

First, the main theoretical contribution of this dissertation is that it brings domestic politics into the study of civil war. Traditionally, scholars of International Relations have focused on interstate conflict, and a dominant assumption in this literature has been that states are rational unitary actors. In recent years, there has been a shift of focus toward studying civil war, but scholars have generally continued to examine conflict through the same theoretical framework developed to analyze interstate conflicts. Some scholars, for example, have treated ethnic groups as essentially states operating in anarchy and examined the way security dilemmas and other strategic dilemmas affect the likelihood of conflict or cooperation among them.¹

¹ See, for example Snyder and Jervis (1999) and Posen (1993).

The theory in this dissertation, by contrast, analyzes civil war bargaining as a process akin to governmental bargaining. Civil wars involve a set of actors with preferences over the outcome of the conflict and these actors constantly make decisions about whether they prefer the status quo to any particular negotiated settlement. Governmental bargaining and civil war negotiations are not identical processes; in fact, there are important differences between those types of bargaining. The analysis does show, however, that using the analogy of governmental bargaining to analyze civil war negotiations leads to useful predictions about the duration and termination of these conflicts.

The primary implication for scholars studying international relations is that states and rebel groups are not unitary actors, which is a dominant assumption in the literature. This assumption, in fact, limits our ability to fully understand the dynamics driving these conflicts. Rather, civil war is a phenomenon that involves a range of different actors. Moving beyond the assumption of two parties and examining the effect of the number and type of actors involved can expand our understanding of these conflicts.

The second implication from this dissertation is that, even if conflict is costly for all parties, it may not be inefficient for individual actors in multi-party conflicts. The idea that conflict is inefficient and that there should always be a bargain that all parties prefer to continued conflict is another dominant assumption in the theoretical literature on rationalist approaches to war. In multi-party conflicts, however, each group has an incentive to hold out to get the best deal as the last signer. If the costs of conflict are low enough and the benefits of holding out are high enough, individual combatants may not

sign a deal even if, in the absence of those incentives, they would prefer it to continued conflict.

This implication comes from looking systematically at the strategies that groups will use in conflict based on the other actors involved. In this dissertation, I have examined how the number and type of actors that can block agreement has affected the incentives that all groups have to negotiate or fight. This theory has shown that groups may act in ways that are individually rational—holding out to get the best deal—but that these actions may lead to sub-optimal outcomes for the country as a whole.

These implications show the importance of scholars moving beyond the narrow assumption that conflicts are two party and focusing on how the number and composition of actors involved in civil wars affect their dynamics. Conflicts with more parties represent a fundamentally different bargaining environment than the two-party conflicts traditionally analyzed in the literature, and scholars interested in studying these processes must take that environment into account to understand fully the factors driving these conflicts. This dissertation has focused specifically on the duration and termination of conflict, but the number and type of combatants involved is likely to have an effect on other dynamics of conflict as well.

In addition to these theoretical contributions for scholars of Political Science, the argument in this dissertation also has direct implications for policymakers interested in designing responses to civil war. In the next section, I describe four implications for the way the international community should respond to multi-party conflicts.

Implications for Policymakers

Over the last fifteen years, the international community has devoted significant attention to resolving protracted internal conflicts around the world. There are increasing resources available for facilitating peace processes and an ever growing number of peacekeeping missions. These conflict resolution efforts are led by official governments, nongovernmental organizations, regional organizations, and prominent individuals. This dissertation suggests several implications for international actors seeking to design responses to these conflicts.

First, since two-party conflicts are easier to resolve than multi-party conflicts, policymakers should look for ways to reduce the number of veto players participating in civil war. In some civil wars, doing so would require dispatching a large international military mission to forcibly disarm combatants, a strategy that is unlikely to be either politically feasible or successful. However, there will be conflicts where the international community can find less drastic ways to reduce the number of actors who can block agreement.

For example, when Chester Crocker was U.S. Assistant Secretary of State for Africa in the 1980s, he developed a two-stage approach to resolving conflicts in Southern Africa such as the civil war in Angola. He argued that these internal conflicts could not be resolved until the external dimension, primarily the involvement of Cuba and South Africa, was removed (Crocker 1999). Crocker and the United States negotiating team committed to addressing the external dimension first and in 1988 a peace agreement between Angola, Cuba and South Africa was signed at the United Nations. That agreement removed a major barrier to negotiation and paved the way for an internal

peace agreement in Angola in 1991. This two-stage strategy illustrates one possible way to reduce the number of veto players in conflicts without having to forcibly disarm combatants.

Another example of a way to reduce the number of actors that can block settlement could be seen in the Lancaster House negotiations in Rhodesia/Zimbabwe in 1979. The Rhodesian civil war was fought between a white settler dominated government and two guerilla organizations representing black nationalists—the Zimbabwean African National Union (ZANU) and the Zimbabwean African Patriotic Union (ZAPU). There were significant differences between ZANU and ZAPU because the groups drew support from different ethnic groups and had charismatic leaders who did not see eye-to-eye. These differences led to military clashes between the two groups. Over the course of the 1972-1979 conflict, however, regional states that supported these groups such as Zambia and Mozambique put pressure on ZANU and ZAPU to form an alliance to present a stronger fighting force against the government. By the time of the Lancaster House negotiations in 1979, ZANU and ZAPU came to the table in a joint delegation as the Patriotic Front. Divisions persisted between the groups, however, their ability to negotiate as a united front was one factor leading to the success of the Lancaster House negotiations and the end of the war.²

Second, international actors interested in resolving these conflicts should seek to identify the set of combatants that are veto players and work primarily to reach a peace agreement that includes all of those parties. As discussed earlier, the peace processes in

² This information on the Rhodesian civil war and peace process is taken largely from Davidow (1984) and Gann and Henriksen (1981).

Rwanda and Burundi both included several unarmed opposition parties as full negotiating partners. In Burundi, the Arusha negotiations were modeled largely on the 1990-1994 South African process, but there is an important distinction between the goals of those two processes. Although there had been violence in South Africa, the negotiations there were designed principally to avert a civil war and shape the political future of the country. Negotiations in Burundi, meanwhile, were designed to end a civil war that had devastated the country for years.

Ending a civil war is likely to be much more difficult when there are unarmed groups included in the negotiations. This dissertation has demonstrated the difficulties inherent in multi-party bargaining, and these difficulties increase as the number of actors increases. Additionally, giving concessions to unarmed groups reduces the incentives that can be provided to combatants to disarm. Every cabinet ministry in the transitional government that goes to an unarmed group is one less inducement for the various combatants to stop fighting. Because negotiations become more complex as the number of actors increases, responses to these conflicts should focus first on getting all the combatants to agree to stop fighting.

The above argument does not mean that there is no role for unarmed groups in a peace process because these groups will likely be very important in the transitional phase. However, it does suggest that convincing combatants to stop fighting and building a permanent political future should be treated as two separate processes. An example of a way to separate these processes can be seen in the Democratic Republic of the Congo (DRC). In that conflict, negotiations in 1998-1999 only included the armed combatants (both internal insurgents and external interveners) and those parties were the signatories

to an agreement in Lusaka, Zambia. The Lusaka Accord called for an Inter-Congolese Dialogue that would include civil society, unarmed political parties, and other relevant political actors and would shape the political future of the country. This separation of conflict negotiations and political dialogue into successive processes, I argue, made each more likely to be successful.

Third, if a comprehensive accord among all veto players is not possible, international actors should seek to reach peace between the government and the strongest domestic insurgents first, and then try to integrate the smaller opposition groups. Often, governments negotiate piecemeal settlements in multi-party conflicts and attempt to bring the various combatants on board one group at a time. Since in these conflicts it is the strongest groups that have the greatest incentive and capability to hold out and block agreement, international actors should focus their attention on convincing those groups to lay down their arms first. Incorporating the strongest groups first will increase the likelihood that conducting piecemeal negotiations is a feasible strategy for reaching a comprehensive peace.

The Philippine government has pursued a strategy of piecemeal negotiations in the Mindinao conflict by trying to incorporate the strongest parties first. Beginning in 1992, the government held negotiations with the Mindinao National Liberation Front (MNLF), the strongest insurgent in the conflict. Those negotiations led to a ceasefire in 1993, and a comprehensive peace agreement in 1996. Conflict in Mindinao continued after this agreement as two other insurgent groups—the Mindinao Islamic Liberation Front (MILF) and the Abu Sayyaf Group—did not lay down their arms. Following the agreement with the MNLF, however, the government attempted to negotiate with each

of those groups. Although there is still not a definitive peace agreement, the MILF reduced its conflict activity in the aftermath of the 1996 agreement and has continued to talk with the government. Additionally, the Abu Sayyaf group indicated a willingness to negotiate after 1996, a shift from its early stance of no negotiations. This example illustrates that piecemeal negotiations can be a workable strategy, but will be most likely successful when the strongest insurgent group is brought on board first.

A final implication for policymakers is that the strategy of “expanding the pie” by offering public goods in return for peace may be productive in two-party conflicts, but is likely to actually make negotiations more difficult in multi-party civil wars. Often, the international community offers incentives such as the promise of greater aid, debt reduction, trade agreements, and so on to induce the combatants to lay down their arms and transition to peace. The logic behind these is that it makes it easier for combatants to see greater benefit from negotiating than from fighting. However, this strategy is problematic in multi-party conflicts because of the incentives parties have to hold out and be the last signer. The bigger the post-conflict pie gets, the greater incentives parties have to negotiate hard to get the biggest piece. These public goods, then, may actually encourage parties to fight harder rather than to lay down their arms.

A more effective strategy to convince groups in multi-party conflicts to stop fighting would be to combine the promise of private goods in a settlement with targeted sanctions or some other method to increase the costs of continued fighting. Examples of private goods to encourage settlement include development projects, aid, or investment targeted in the region of the country where the groups operate. Since these goods would

only be available for a specific group and would be contingent on stopping fighting, they should alleviate the hold-out problem.

There are many tactics that can be used to raise the costs of fighting for these groups. In recent years, the international community has used sanctions targeted against resources exported by insurgent groups in conflicts in Angola, Liberia and the Democratic Republic of the Congo. There is much debate within International Relations about the extent to which lootable resources lead to rebellion. However, regardless of that relationship, it is clear that these resources help groups pay the costs of fighting. Sanctions, if effective, could increase the costs of fighting and therefore make groups less likely to see a benefit from holding out. A similar tactic could be used against the government if it is the intransigent party. The combination of the promise of private goods and increased costs of fighting should make the hold out problem less acute.

Each of these implications flows directly from moving beyond the assumption that civil war is a two-party phenomenon and examining how the number and type of groups in conflict affects combatants' ability and incentives to negotiate. They suggest that policymakers need to think critically about the composition of combatants when designing conflict resolution efforts since strategies that may work in two-party conflicts, such as expanding the pie, can actually be counterproductive in multi-party negotiations. Not taking into account the dynamics of multi-party negotiations can lead to a failure to resolve these costly wars.

In this dissertation, I have argued that multi-party civil wars drag on and are more resistant to resolution through negotiated settlement because of several barriers to negotiation in multi-party conflicts. These barriers are not insurmountable, however, and

international actors can design effective responses to these conflicts by seeking to reduce the number of actors that can block agreement, working primarily to convince all combatants to sign an agreement, starting with the strongest groups, and using a combination of targeted benefits and sanctions to alleviate the incentives groups have to hold out. Given the incredibly high cost of protracted internal conflicts, it is important that international actors design responses that have the most potential for success.

Appendix A Civil Wars Included in the Dataset

War Number	War	Start Date	End Date
1	Bolivia vs. Popular Revolutionary Army	6/1/1946	7/21/1946
2	Bolivia vs. MNR	4/9/1952	4/11/1952
3	Bolivia vs. ELN	3/1/1967	10/16/1967
4	France (Cambodian Independence)	8/1/1946	12/31/1953
5	China vs. People's Liberation Army	1/1/1946	12/15/1949
6	Greece vs. DSE	3/1/1946	10/16/1949
7	Netherlands (Indonesian Independence)	1/1/1946	12/31/1949
8	Iran vs. Republic of Kurdistan	1/1/1946	12/15/1946
9	Iran vs. KDPI	1/1/1966	9/30/1968
10	Iran vs. KDPI	3/15/1979	12/31/1990
11	Iran vs. KDPI	1/1/1993	11/15/1993
12	Iran vs. KDPI	1/1/1996	12/31/1996
13	Iran vs. Republic of Azerbaijan	1/1/1946	12/15/1946
14	United Kingdom vs. IZL (Israel)	1/1/1946	12/31/1946
15	France (Laos Independence)	3/1/1946	12/31/1953
16	Philippines vs. Huk	7/1/1946	9/15/1954
17	Philippines vs. CPP	10/5/1972	ongoing
18	Soviet Union vs. Forest Brothers (Estonia)	1/1/1946	12/31/1948
19	Soviet Union vs. Latvian Partisans	1/1/1946	12/31/1947
20	Soviet Union vs. Lithuanian Partisans	6/6/1946	12/31/1948
21	Soviet Union vs. Ukrainian Partisans	1/1/1946	3/5/1950
22	France (Vietnamese Independence)	11/20/1946	12/31/1954
23	China vs. Taiwanese Insurgents	2/28/1947	12/31/1947
24	Hyderabad vs. CPI	6/1/1947	9/17/1948
25	France (Madagascar Independence)	3/29/1947	12/31/1947
26	Paraguay vs. Opposition Coalition	1/1/1947	8/21/1947
27	Paraguay vs. Military Faction	5/5/1954	5/5/1954
28	Paraguay vs. Military Faction	2/3/1989	2/3/1989
29	Burma vs. Karen Insurgents	1/1/1948	12/31/1992
30	Burma vs. Karen Insurgents	1/1/1995	ongoing
31	Burma vs. Leftists Insurgents	3/28/1948	12/31/1994
32	Burma vs. Arakan Insurgents	1/1/1948	9/18/1988
33	Burma vs. Arakan Insurgents	1/1/1991	12/31/1994
34	Burma vs. Mon Insurgents	1/1/1948	11/15/1963
35	Burma vs. Mon Insurgents	3/1/1990	12/31/1990
36	Burma vs. Mon Insurgents	12/23/1996	12/31/1996
37	Costa Rica vs. National Liberation Army	3/3/1948	4/15/1948
38	India vs. Communist Groups	9/18/1948	12/31/1951
39	India vs. Communist Groups	5/25/1967	7/19/1972
40	India vs. Communist Groups	1/1/1990	ongoing
41	United Kingdom (Malaysian Independence)	6/19/1948	12/31/1957
42	South Korea vs. Leftist Groups	4/1/1948	6/25/1950
43	North Yemen vs. Opposition Coalition	3/1/1948	3/1/1948

War Number	War	Start Date	End Date
44	North Yemen vs. Royalists	10/1/1962	3/15/1970
45	North Yemen vs. National Democratic Front	5/1/1980	5/31/1982
46	Burma vs. Kachin Insurgents	11/15/1949	12/31/1949
47	Burma vs. Kachin Insurgents	1/1/1961	12/1/1992
48	Guatemala vs. Military Faction	7/16/1949	7/18/1949
49	Guatemala vs. Forces of Carlos Castillo Armas	6/18/1954	6/28/1954
50	Guatemala vs. Leftist Insurgents	1/1/1965	3/13/1995
51	Israel vs. Palestinian Groups	1/1/1949	ongoing
52	China vs. Tibet	10/7/1950	10/9/1950
53	China vs. Tibet	5/1/1956	12/31/1956
54	China vs. Tibet	3/10/1959	3/22/1959
55	Indonesia vs. Republic of South Moluccas	8/5/1950	11/15/1950
56	USA vs. Puerto Rican Nationalist Party	10/30/1950	12/31/1950
57	Thailand vs. Military Faction	6/30/1951	7/1/1951
58	Thailand vs. Communist Party of Thailand	10/1/1974	4/15/1982
59	United Kingdom (Mau Mau Rebellion in Kenya)	10/22/1952	12/31/1956
60	Cuba vs. Military Faction	7/26/1953	7/26/1953
61	Cuba vs. 26th of July Movement	1/1/1957	12/15/1958
62	Cuba vs. Cuban Refugees	4/17/1961	12/31/1961
63	Indonesia vs. Darul Islam	9/20/1953	12/31/1953
64	Indonesia vs. Darul Islam	2/15/1958	12/31/1961
65	France (Moroccan Independence)	11/7/1953	12/31/1956
66	France (Tunisian Independence)	4/1/1953	12/31/1956
67	France (Algerian Independence)	11/1/1954	12/31/1962
68	Argentina vs. Military Faction	1/16/1955	9/19/1955
69	Argentina vs. Military Faction	4/2/1963	12/31/1963
70	Argentina vs. Leftist Groups	3/1/1973	12/31/1977
71	United Kingdom (Cyprus Independence)	6/2/1955	12/31/1959
72	South Vietnam vs. Communist Groups	4/1/1955	12/31/1964
73	India vs. Naga Groups	1/1/1956	6/15/1968
74	India vs. Naga Groups	7/1/1992	7/22/1997
75	India vs. Naga Groups	1/1/2000	12/31/2000
76	Burma vs. Karenni Insurgents	1/1/1957	12/31/1957
77	Burma vs. Karenni Insurgents	1/1/1992	12/31/1992
78	Burma vs. Karenni Insurgents	1/1/1996	10/15/1996
79	France (Cameroon Independence)	9/1/1957	12/31/1960
80	France (Moroccan Independence)	1/12/1957	12/31/1958
81	Spain (Mauritania Independence)	11/23/1957	12/31/1957
82	Muscat and Oman vs. State of Oman	7/1/1957	12/31/1957
83	Iraq vs. Military Faction	7/14/1958	3/15/1959
84	Iraq vs. Military Faction	11/18/1963	11/18/1963
85	Iraq vs. SCIRI	8/1/1982	12/31/1984
86	Iraq vs. SCIRI	1/1/1987	12/31/1987
87	Iraq vs. SCIRI	1/1/1991	12/31/1996
88	Lebanon vs. Independent Nasserite Movement	5/15/1958	10/15/1958
89	Lebanon vs. Various Groups	4/13/1975	10/13/1990

War Number	War	Start Date	End Date
90	Malaysia vs. Communist Party of Malaysia	1/1/1958	7/31/1960
91	Malaysia vs. Communist Party of Malaysia	1/1/1974	12/31/1975
92	Malaysia vs. Communist Party of Malaysia	1/1/1981	10/15/1981
93	Laos vs. Pathet Lao	11/12/1959	2/22/1973
94	Laos vs. LRM	12/1/1989	12/31/1990
95	Portugal (Angolan Independence)	2/4/1961	12/31/1974
96	Burma vs. Shan Groups	1/1/1960	12/31/1970
97	Burma vs. Shan Groups	1/1/1976	9/18/1988
98	Burma vs. Shan Groups	1/1/1993	12/31/2002
99	Congo/Zaire vs. Katanga	7/1/1960	12/31/1962
100	Congo/Zaire vs. South Kasai	8/1/1960	12/31/1962
101	Ethiopia vs. Military Faction	12/14/1960	12/24/1960
102	Ethiopia vs. Various Groups	1/1/1976	5/15/1991
103	Nepal vs. Nepali Congress	2/1/1960	8/15/1962
104	Nepal vs. Communist Party of Nepal	7/13/1996	ongoing
105	France vs. OAS	4/22/1961	6/30/1962
106	Iraq vs. Kurds	9/10/1961	3/11/1970
107	Iraq vs. Kurds	1/1/1973	12/15/1993
108	Iraq vs. Kurds	1/1/1996	12/31/1996
109	United Kingdom (Brunei Independence)	2/8/1962	12/31/1962
110	Ethiopia vs. Eritrean Groups	1/1/1962	5/29/1991
111	Venezuela vs. Military Faction	6/2/1962	6/3/1962
112	Venezuela vs. Military Faction	2/4/1992	11/29/1992
113	Portugal (Guinea Bissau Independence)	2/1/1963	12/31/1973
114	Malaysia vs. North Borneo	1/1/1963	8/11/1966
115	Sudan vs. Anya Nya	1/1/1963	1/31/1972
116	Sudan vs. SPLM	5/17/1983	ongoing
117	Congo/Zaire vs. CNL	5/1/1964	12/31/1965
118	Congo/Zaire vs. Opposition Militias	7/11/1967	11/30/1967
119	Congo/Zaire vs. FNLC	8/17/1977	6/15/1978
120	Congo/Zaire	10/18/1996	12/31/2001
121	Gabon vs. Military Faction	2/18/1964	2/18/1964
122	Portugal (Mozambique Independence)	11/19/1964	12/31/1974
123	South Yemen vs. NLF and FLOSY	11/25/1964	12/31/1967
124	Burundi vs. Military Faction	10/18/1965	10/18/1965
125	Burundi vs. Hutu Groups	11/26/1991	ongoing
126	Chad vs. Various Groups	11/8/1965	12/31/1994
127	Chad vs. Various Groups	1/1/1997	12/31/2002
128	Colombia vs. Leftist Insurgents	1/1/1965	ongoing
129	Dominican Republic vs. Military Faction	1/1/1965	9/3/1965
130	Indonesia vs. West Papau	1/1/1965	12/31/1965
131	Indonesia vs. West Papau	1/1/1976	8/31/1978
132	Peru vs. Leftist Groups	10/1/1965	1/15/1966
133	Peru vs. Leftist Groups	1/1/1980	12/31/1999
134	Ghana vs. Military Faction	2/24/1966	2/24/1966
135	Ghana vs. Military Faction	12/31/1981	6/19/1983

War Number	War	Start Date	End Date
136	India vs. Mizo Groups	9/1/1966	10/15/1968
137	Nigeria vs. Military Faction	1/15/1966	1/15/1966
138	South Africa vs. SWAPO (Namibia)	8/26/1966	12/22/1988
139	Syria vs. Military Faction	2/23/1966	2/23/1966
140	Syria vs. Muslim Brotherhood	6/16/1979	2/15/1982
141	Cambodia vs. Leftist Groups	4/2/1967	4/17/1975
142	Cambodia vs. Leftist Groups	12/1/1978	12/31/1998
143	Nigeria vs. Republic of Biafra	7/6/1967	1/12/1970
144	Guinea vs. Military Faction	11/22/1970	11/22/1970
145	Guinea vs. RFDG	9/1/2000	12/31/2001
146	Philippines vs. Mindinao Groups	8/20/1970	12/31/1990
147	Philippines vs. Mindinao Groups	1/1/1993	ongoing
148	Sudan vs. Sudanese Communist Party	7/22/1971	7/22/1971
149	Sudan vs. Islamic Charter Front	7/2/1976	7/11/1976
150	Sudan vs. Darfur Groups	4/9/2003	ongoing
151	Madagascar vs. Monima Independence Movement	1/1/1971	4/3/1971
152	Morocco vs. Military Faction	7/10/1971	7/11/1971
153	Pakistan vs. Mukti Bahini	3/26/1971	12/16/1971
154	Sri Lanka vs. JVP	4/5/1971	6/9/1971
155	Sri Lanka vs. JVP	1/1/1989	12/31/1990
156	Uganda vs. Military Faction	1/25/1971	12/31/1972
157	Uganda vs. Various Groups	1/1/1977	12/31/1991
158	Uganda vs. Various Groups	1/1/1994	ongoing
159	United Kingdom vs. Irish Republican Army	8/1/1971	12/31/1991
160	United Kingdom vs. Irish Republican Army	8/15/1998	8/15/1998
161	El Salvador vs. Military Faction	4/25/1972	4/25/1972
162	El Salvador vs. Leftist Groups	10/1/1979	12/31/1991
163	Oman vs. PFLOAG	1/1/1972	12/31/1975
164	Rhodesia vs. African Groups	12/1/1972	11/18/1979
165	Uruguay vs. MLN	1/28/1972	11/15/1972
166	Chile vs. Military Faction	9/11/1973	9/11/1973
167	Bangladesh vs. Shanti Bahini	1/1/1974	11/15/1992
168	Pakistan vs. Baluchi Separatists	1/1/1974	7/15/1977
169	Eritrea vs. Eritrean Islamic Jihad Movement	4/30/1997	12/31/1999
170	Eritrea vs. Eritrean Islamic Jihad Movement	1/1/2003	ongoing
171	Angola vs. UNITA	11/11/1975	12/31/1995
172	Angola vs. UNITA	1/1/1998	2/22/2002
173	Ethiopia vs. Somali Groups	1/1/1975	12/31/1983
174	Ethiopia vs. Somali Groups	1/1/1996	12/31/2002
175	Indonesia vs. East Timorese Groups	12/7/1975	12/31/1989
176	Indonesia vs. East Timorese Groups	1/1/1992	12/31/1992
177	Indonesia vs. East Timorese Groups	1/1/1997	12/31/1998
178	Morocco vs. POLISARIO	1/20/1975	12/31/1989
179	Mozambique vs. RENAMO	1/1/1976	12/31/1992
180	Afghanistan vs. Various Groups	4/27/1978	ongoing
181	India vs. Tripuri Groups	1/1/1978	12/31/1988

War Number	War	Start Date	End Date
182	India vs. Tripuri Groups	1/1/1992	ongoing
183	Nicaragua vs. Sandanistats	2/3/1978	7/17/1979
184	Nicaragua vs. Contras	12/1/1981	10/15/1989
185	Somalia vs. Military Faction	4/9/1978	4/9/1978
186	Somalia vs. Various Groups	1/1/1981	11/1/1996
187	Somalia vs. SSRC	5/12/2001	12/31/2002
188	Equatorial Guinea vs. Military Faction	8/3/1979	8/3/1979
189	Iran vs. Mujahideen Khalq	1/1/1979	12/31/1982
190	Iran vs. Mujahideen Khalq	1/1/1986	12/31/1988
191	Iran vs. Mujahideen Khalq	1/1/1991	12/31/1993
192	Iran vs. Mujahideen Khalq	1/1/1997	12/31/2001
193	Iran vs. APCO	10/3/1979	12/31/1980
194	Saudi Arabia vs. Juhayman Movement	11/25/1979	12/15/1979
195	Liberia vs. Military Faction	4/12/1980	4/12/1980
196	Liberia vs. Various Groups	12/24/1989	12/31/1995
197	Liberia vs. LURD	1/1/2000	12/31/2003
198	Spain vs. ETA	2/1/1980	12/31/1981
199	Spain vs. ETA	6/19/1987	12/31/1987
200	Spain vs. ETA	1/2/1991	10/25/1992
201	Tunisia vs. Tunisian Armed Resistance	1/27/1980	1/27/1980
202	Gambia vs. SRLP	7/30/1981	8/6/1981
203	South Africa vs. African Groups	1/30/1981	12/31/1988
204	India vs. Manipur Groups	7/1/1982	12/31/1988
205	India vs. Manipur Groups	1/1/1992	12/31/2000
206	India vs. Manipur Groups	1/1/2003	ongoing
207	Kenya vs. Military Faction	8/1/1982	8/1/1982
208	India vs. Sikh Insurgents	8/20/1983	9/12/1993
209	Sri Lanka vs. Tamil Groups	5/11/1983	ongoing
210	Cameroon vs. Military Faction	4/6/1984	4/9/1984
211	Turkey vs. Kurdish Groups	8/15/1984	ongoing
212	Surinam vs. SLA/Jungle Commando	7/1/1986	12/31/1988
213	Togo vs. MTD	9/23/1986	9/24/1986
214	Togo vs. Military Faction	11/28/1991	11/28/1991
215	South Yemen vs. Faction of Yemenite Socialist Party	1/13/1986	1/23/1986
216	Burkina Faso vs. Popular Front	10/15/1987	10/15/1987
217	Comoros vs. Presidential Guard	11/27/1989	11/29/1989
218	Ethiopia vs. Afar Groups	1/1/1989	5/28/1991
219	Ethiopia vs. Afar Groups	4/26/1996	12/31/1996
220	India vs. Kashmir Insurgents	12/11/1989	ongoing
221	India vs. Assamese Groups	5/29/1990	12/31/1991
222	India vs. Assamese Groups	1/1/1994	ongoing
223	Indonesia vs. Aceh Groups	9/8/1990	12/31/1991
224	Indonesia vs. Aceh Groups	1/1/1999	ongoing
225	Panama vs. Military Faction	10/3/1989	10/3/1989
226	Papau New Guinea vs. BRA	1/1/1989	6/15/1996
227	Romania vs. National Salvation Front	12/23/1989	12/25/1989

War Number	War	Start Date	End Date
228	Mali vs. Azawad Groups	6/1/1990	12/31/1990
229	Mali vs. Azawad Groups	1/1/1994	12/31/1994
230	Niger vs. Air and Azawad Groups	10/1/1992	12/31/1994
231	Niger vs. Air and Azawad Groups	1/1/1997	12/31/1997
232	Rwanda vs. Rwandan Patriotic Front	10/1/1990	7/19/1994
233	Rwanda vs. Hutu groups	5/25/1997	12/31/2002
234	Senegal vs. MFDC	9/1/1990	ongoing
235	Soviet Union vs. Republic of Armenia	1/19/1990	8/15/1991
236	Soviet Union vs. Azerbaijani Popular Front	1/19/1990	1/20/1990
237	Trinidad and Tobago vs. Jamaat al-Muslimeen	7/27/1990	8/1/1990
238	Djibouti vs. FRUD	11/12/1991	12/26/1994
239	Djibouti vs. FRUD faction	7/24/1999	12/31/1999
240	Georgia vs. Various Groups	12/22/1991	10/15/1993
241	Haiti vs. Various Groups	4/11/1989	12/31/1991
242	Sierra Leone vs. RUF	3/23/1991	12/31/2000
243	Turkey vs. Devrimci Sol	7/13/1991	10/7/1992
244	Yugoslavia vs. Republic of Slovenia	6/27/1991	7/15/1991
245	Yugoslavia vs. Republic of Croatia	6/26/1991	12/15/1991
246	Algeria vs. Islamist Groups	7/1/1991	12/31/2003
247	Angola vs. Cabinda	6/1/1991	12/31/1991
248	Angola vs. Cabinda	1/1/1994	12/31/1998
249	Angola vs. Cabinda	1/1/2002	12/31/2002
250	Azerbaijan vs. Republic of Nagorno-Karabakh	1/1/1992	7/27/1994
251	Bosnia and Herzegovina vs. Serbs	4/7/1992	12/14/1995
252	Croatia vs. Serbs	1/1/1992	12/14/1995
253	Egypt vs. al-Gamaa al-Islamiyya	3/17/1993	12/31/1998
254	Georgia vs. Republic of Abkhaz	8/14/1992	12/1/1993
255	Georgia vs. Republic of South Ossetia	6/8/1992	6/24/1992
256	Moldova vs. Dniestr Republic	3/1/1992	7/29/1992
257	Tajikistan vs. UTO	5/1/1992	12/31/1998
258	Azerbaijan vs. Military Faction	6/4/1993	3/17/1995
259	Bosnia and Herzegovina vs. Western Bosnia	10/3/1993	12/14/1995
260	Bosnia and Herzegovina vs. Croats	4/1/1993	3/18/1994
261	Russia vs. Parliamentary Forces	10/3/1993	10/4/1993
262	Mexico vs. Zapatistas	1/1/1994	1/12/1994
263	Mexico vs. EPR	9/22/1996	12/31/1996
264	Russia vs. Republic of Chechnya	12/11/1994	8/15/1996
265	Russia vs. Republic of Chechnya	8/7/1999	ongoing
266	Yemen vs. Democratic Republic of Yemen	4/27/1994	7/7/1994
267	Pakistan vs. MQM	6/1/1990	12/31/1990
268	Pakistan vs. MQM	1/1/1995	12/31/1996
269	Ethiopia vs. al-Itahad al-Islami	8/6/1996	12/31/1999
270	Niger vs. Eastern Niger groups	2/6/1996	11/29/1997
271	Comoros vs. MPA	9/3/1997	9/7/1997
272	Congo-Brazzaville vs. Various Groups	11/3/1993	12/31/1994
273	Congo-Brazzaville vs. Various Groups	6/5/1997	11/16/1999

War Number	War	Start Date	End Date
274	Congo-Brazzaville vs. Ntsiloulous	1/1/2002	12/31/2002
275	Guinea-Bissau vs. Military Junta	6/7/1998	5/7/1999
276	Lesotho vs. Military Faction	9/4/1998	9/24/1998
277	Yugoslav vs. Kosovar Albanians	2/28/1998	6/3/1999
278	Ethiopia vs. Oromo Liberation Front	1/1/1989	12/31/1991
279	Ethiopia vs. Oromo Liberation Front	1/1/1999	ongoing
280	Russia vs. Dagestan	8/10/1999	12/31/1999
281	Uzbekistan vs. IMU	8/17/2000	12/31/2000
282	Central African Republic vs. Military Faction	6/8/2001	12/31/2002
283	Macedonia vs. Albanians	1/22/2001	8/12/2001
284	USA vs. Al Qaida	9/11/2001	12/31/2002
285	Ivory Coast vs. Various Groups	9/19/2002	11/30/2003
286	India vs. Bodo Groups	3/16/1989	12/31/1990
287	India vs. Bodo Groups	1/1/1993	ongoing
288	Burma vs. Wa	3/16/1997	12/31/1997

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