

## ABSTRACT

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LEADER INCENTIVES AND THE  
TERMINATION OF CIVIL WAR

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This dissertation examines the influence of leaders' incentives on civil conflict termination and outcome. Building upon principal-agent framework and insights from credible commitment theories of civil war, I argue that culpable leaders – those viewed as responsible for the war by their constituencies and opponents – are more likely to be punished following poor war performance than non-culpable leaders, who can more easily avoid responsibility for the war. As a result, culpable leaders will have incentives to 'gamble for resurrection', extending a losing war in the hope of turning the tide, achieving victory, and avoiding punishment. The culpable leader's incentive to gamble for resurrection thus influences the dynamics of war termination, making wars less likely to end when culpable leaders are in power. Culpability is also hypothesized to increase the likelihood of extreme war outcomes – total defeat or major victory – and to decrease the likelihood that the leader makes concessions to end the war. These propositions are tested using both quantitative and qualitative

evidence. First, using an original dataset of rebel and state leaders of a global random sample of civil wars between 1980 and 2010, I test the influence of leader culpability on civil war termination and outcome. The results provide strong support for my theoretical expectations; culpability decreases the likelihood of conflict termination and concessions, while increasing the likelihood of extreme war outcomes.

Additionally, I test the mechanism underlying the theoretical argument using quantitative and qualitative evidence. Original data on each leader's culpability, war performance, and post-tenure fate demonstrate that culpable leaders are, in fact, more likely to be punished following poor war performance than their non-culpable counterparts. Within-case comparative analysis of settlement attempts during the civil war in Angola provides additional support for the theoretical argument, demonstrating that leader vulnerability to punishment played a critical role in undermining settlement attempts in Angola during the 1980s and 1990s.

LEADER INCENTIVES AND THE TERMINATION OF CIVIL WAR

By

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## **Dedication**

To Nick and Maya

## **Acknowledgements**

Writing a dissertation is simultaneously extremely frustrating and incredibly rewarding. Making it through the dissertation-writing process has required a tremendous amount of self discipline, dedication, and hard work, but even with all of that, it would not have been possible without the help and support of my family, friends, colleagues, and mentors.

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## **Chapter 1 : Introduction**

### **I. Central Question**

All civil wars eventually end. When and how they end, however, varies greatly across conflicts. Some end quickly, within days or months of their initiation. The 1946 Kurdish rebellion in Iran, for example, ended within 7 months, while Hezbollah's war with Israel in 2006 lasted only 30 days. Some conflicts, on the other hand, drag on for years or even decades. The civil wars in Colombia and the Philippines, for example, both began in the 1960s and are still ongoing today.

Civil conflict outcomes also vary significantly from case to case, ranging from comprehensive negotiated settlement to total military victory to truce or ceasefire. State and rebel forces in Burundi, for example, signed a series of peace agreements between 2000 and 2008, ending their decades-long civil war through mutual concessions and extensive military and political power-sharing. Conflicts in Sri Lanka and Rwanda, on the other hand, ended through decisive military outcomes rather than negotiated settlement. In Sri Lanka's fight against the LTTE, government forces achieved a military victory in May 2009 after more than 25 years of conflict, while the FPR's fight against the Rwandan government ended in victory for the rebels, who overran the country as government forces fled. Finally, many conflicts end indecisively, through ceasefires, unofficial truces, or a gradual decline in armed activity without an explicit decision to end the fighting. Pakistan's conflict with the

MQM, for example, tapered off when the rebel organization decided to pursue a political strategy.

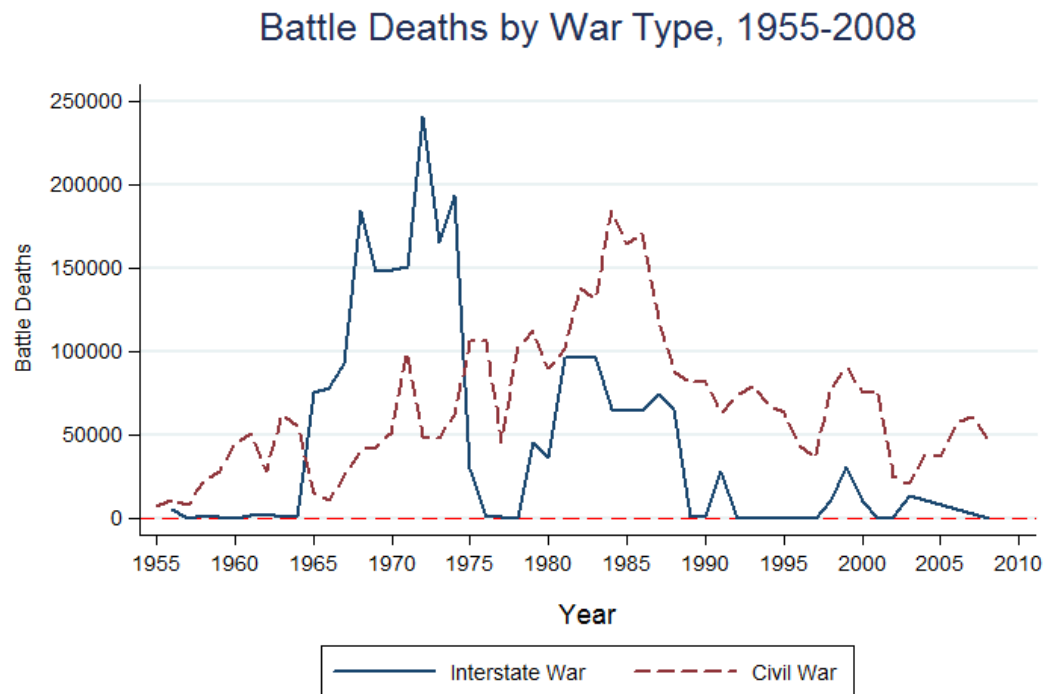
More generally speaking, both civil conflict termination and outcome exhibit significant variation. In the dataset used for analysis in this project, civil wars range in duration from a minimum of one year to a maximum of 48 years, with an average of 8.65 years. Conflict outcomes are similarly varied: 43 percent end through formal peace agreement or a formalized ceasefire, approximately 20 percent through victory for one side or the other, and 37 percent through other means, such as low activity.

This observed variation in war termination and outcome raises two important questions. First, what explains when conflicts end? Why do some end quickly, while others drag on for years or even decades? Second, why do some combatant groups agree to compromise outcomes while others fight to a military end? Stated more generally, what explains variation in the termination and outcome of civil war? Answering these questions constitutes the central aim of this dissertation.

## **II. The Importance of Studying Civil Conflict Termination and Outcome**

Developing clear, accurate explanations for how and when civil wars end is important. The prevalence of interstate war has declined over the past half century, leaving civil conflict as the most common form of organized violence in the international system. Not only is civil war common, but it is deadly. As global battle death trends presented in Figure 1.1 demonstrate, civil wars have consistently caused more battle-related fatalities per year than interstate conflicts since 1975.

**Figure 1.1 Battle Deaths by War Type, 1955-2008**



Source: PRIO Battle Deaths Dataset Version 3.0

Furthermore, the costs of civil conflict extend well beyond the battlefield. The social, economic, and political consequences of civil war are wide-ranging and often severe. Noncombatant populations, in particular, often bear the brunt of these consequences, suffering from direct targeting, forced migration, and the destruction of social, economic, and health infrastructure and services. Collier et al (2003), in fact, argue that by the 1990s, 90 percent of war victims were civilian. Additionally, the UN estimates that over 43 million people worldwide are currently suffering from forced displacement due to conflict or major human rights violations, including 15 million refugees, who have fled across borders to escape conflict, and 27 million internally displaced persons (IDPs), who remain within their homeland territories



(“Global Issues at the United Nations” 2013). Civil conflict also negatively impacts a variety of health outcomes, including HIV/AIDS transmission, infant mortality, life expectancy, fertility rates, and vaccination rates, depressing both immediate and longer-term health outcomes for civilian populations (Ghobarah, Huth, and Russett 2003; Hoddie and Smith 2009; Iqbal 2010).

In addition to these human costs of civil war, the economic consequences of internal conflict are often severe. Sometimes referred to as ‘development in reverse’, civil war not only destroys infrastructure necessary for economic production, but it also qualitatively changes the composition of a country’s economy, reducing investment and increasing subsistence-based economic activity (Collier et al. 2003). The overall impact of civil war, therefore, is to deflate economic growth, reduce per capita income, shrink food production, and decrease export growth, while increasing external debt as a percentage of GDP (Collier 1999; Collier et al. 2003; Stewart, Huang, and Wang 2001). Perhaps equally troubling, the economic consequences of civil war often extend beyond the borders of the warring country. Internal conflict displays economic spillover effects, reducing economic growth in neighboring states, particularly in the short term (Murdoch and Sandler 2002), and reducing bilateral trade (Bayer and Rupert 2004).

The regional consequences of civil war are not limited to economic issues. Rather, civil conflict itself is contagious, often spilling over the borders of one state into neighboring countries, thereby destabilizing entire regions (Gleditsch 2007; Sambanis 2002). This diffusion of conflict across borders has been attributed to a variety of factors, including cross-border issue linkage and transnational rebel

movements, destabilization caused by the diffusion of economic and health impacts across borders, and upheaval due to massive refugee flows as people attempt to escape conflicts in their own countries (Buhaug and Gleditsch 2008; Gleditsch 2007; Salehyan and Gleditsch 2006). The spread of conflict is also often purposeful, as neighboring states with a vested interest in the issues at stake become involved, thereby increasing interstate militarized activity in association with civil war (Gleditsch, Salehyan, and Schultz 2008).

Finally, of particular concern is growing evidence that civil conflict creates a ‘conflict trap’ in which countries emerging from war often cannot consolidate peace (Collier et al. 2003). Over one third (36%) of countries immersed in civil wars between 1945 and 1996 returned to war shortly after termination (Walter 2004). Emerging research suggests that despite international emphasis on peaceful resolution through negotiations, conflicts ending in military victory are less likely to recur than those ending in negotiated settlement (Mason et al. 2011; Quinn, Mason, and Gurses 2007; Toft 2009). Thus, understanding the determinants of conflict outcome and why settlement attempts are prone to collapse is a critical first step towards better understanding conflict recurrence and to devising policies that can offset its risks.<sup>1</sup>

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<sup>1</sup> The international community has become increasingly involved in trying to resolve ongoing civil wars as their costs have become more apparent. The UN alone has, since 1948, launched 67 peacekeeping missions, and is currently directing 15 missions globally. With a budget of over 7.3 billion dollars for the current fiscal year and over 92,000 personnel in the field, these efforts to resolve ongoing wars and prevent a return to war in post-conflict states are costly. And this is to say nothing of the costs incurred by individual states involved in non-UN interventions. The 2011 US intervention in Libya, for example, cost the defense department alone 1.1 billion dollars, a modest sum in relative terms, as the mission involved no ground troops (Rettig 2011).

Given the prevalence of civil war and these myriad costs of conflict for civilian populations, national and global economies, regional stability, and post-conflict peace-building, understanding how and why wars end, as a precursor to devising ways to facilitate rapid and stable termination of civil conflicts, should be a priority for scholars and policy-makers alike. Better understandings of the factors influencing civil war termination and outcome are needed to inform policy that more effectively resolves and mitigates the serious consequences of internal conflict.

### **III. Existing Explanations**

Political scientists have developed a variety of theoretical arguments to explain variation in civil conflict termination and outcome. One major vein of research in this field focuses on conditions that make conflicts ‘ripe for resolution’ (Zartman 1989) by decreasing the expected utility of war and thus encouraging combatants to seek negotiated settlement. Scholars in this tradition argue that combatants decide whether to continue fighting in pursuit of victory or settle on compromise terms based upon a careful utility calculation that takes into account the chances of victory, the costs of war, and the relative payoffs of winning versus accepting a settlement. Wars, they argue, end in settlement when the utility of compromise is greater than the utility of fighting (Mason and Fett 1996; Mason, Weingarten, and Fett 1999; Walter 2002; Wittman 1979). In particular, combatants’ utility calculations are influenced by the costs of conflict and relative power. Higher costs of war promote settlement by decreasing combatants’ expected payoffs for continued fighting (Mason and Fett 1996; Mason, Weingarten, and Fett 1999), while

relatively evenly-matched military capacity promotes settlement by reducing the likelihood of either side achieving a military victory (D. E. Cunningham, Gleditsch, and Salehyan 2009; DeRouen and Sobek 2004; Gent 2008; Hegre 2004).

More recently, many scholars have taken the bargaining model of war as their theoretical starting point, specifically focusing on information.<sup>2</sup> This approach explains conflict as a result of actors having private information about their relative strength or resolve, and incentives to misrepresent that information. If one or both sides are overly-optimistic about their prospects for victory, armed conflict may result. Once fighting breaks out, battlefield performance reveals private information about combatants' relative strength, allowing them to update expectations regarding the likely outcome of the war. As this information is revealed, state and rebel combatants revise their demands and the concessions they are willing to offer, leading to a negotiated settlement that both sides can agree upon to end the conflict.

Using this information revelation theory of conflict as a jumping off point, scholars seek to explain why combatants in civil war often continue to fight even when the outcome of the war seems apparent; that is, when it is clear that neither side can win. One common explanation focuses on the economic incentives for continued conflict. Economic theories of civil war focus primarily on 'lootable resources', arguing that fighting can be profitable when these resources are present, and thus difficult to terminate due to the economic incentives to continue the conflict (Lujala 2010; Ross 2004b). Economic theories treat rebellion as a quasi-criminal activity (Collier 2000); leaders in these models care more about economic profit through war

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<sup>2</sup> See Filson and Werner (2002) and Slantchev (2003) for information approaches.

than about often ill-defined political platforms, and behave more like criminal gangs than revolutionary organizations. The critical incentive in these rebellion-as-business models, therefore, is the economic payoff during conflict, rather than the post-conflict political payoff (Collier, Hoeffler, and Söderbom 2004).

As such, these theories problematize the ‘war is costly’ assumption underlying the bargaining model of war and earlier theories focused on the costs of conflict. The implication is that when wars are profitable for those with the authority to end them (i.e. leaders), conflict will drag on because key actors have no incentive to stop fighting. This effect can be large; Lujala (2010) finds, for example, that gemstones and hydrocarbons located in a conflict zone double, on average, the duration of war due to the incentives and opportunities they create for rebel groups.<sup>3</sup>

A second bargaining model-based explanation for why civil wars continue even after private information is revealed focuses on the difficulty in achieving and implementing negotiated settlements during civil war. Fearon (1994b, 2004) argues that commitment problems represent the primary barrier to bargaining in civil war, influencing both the duration of conflict and the likelihood that the war will end via negotiated settlement.<sup>4</sup> Commitment problems arise, in a domestic context, when state authority breaks down, leaving two political communities without a governing authority that can guarantee adherence to agreements made between them (Fearon 1994b). With no higher authority to enforce agreements, the stronger party in a civil

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<sup>3</sup> As the economic approach to civil conflict has gained prominence among scholars and policymakers, international organizations have spearheaded efforts to reduce the profitability of conflict. The United Nations has, for example, implemented sanctions against rebel groups using looted resources to finance their war efforts in Angola, Liberia, and Sierra Leone.

<sup>4</sup> See Powell (2006) for an overview of commitment problems in interstate war.

war cannot credibly promise not to exploit its position of power once a settlement is in place. Knowing this, the weaker party, fearful of exploitation, will be unwilling to agree to a negotiated settlement. Thus, government and rebel forces fight on, despite little hope of military victory and despite the fact that bargains exist that both sides would prefer to continued war. Fluctuations in combatants' relative power ensure that at least one side will have incentives to renege in the future, and commitment problems thus prevent conflict termination (Fearon 2004).

Extending this logic to the settlement phase of conflict, Walter (1997, 2002) argues that distrust between combatants and the high risk and uncertainty associated with post-civil-war states critically undermine peace. Combatants cannot make credible commitments to uphold the terms of settlement because the process of disarmament, demobilization, and reintegration (DDR) – essential for conflict termination – simultaneously provides opportunities for exploitation. DDR renders former fighters unable to defend themselves should their adversary renege on a settlement agreement. It thus represents a critical node in any peace process; turning in weapons and cantonment of troops by a former combatant group significantly alters the balance of power in favor of its opponent. This relatively large and rapid shift in the balance of power can be easily exploited by the actor whose relative power has just increased in order to gain more favorable terms of settlement. The incentives to renege generated by demobilization, and the resulting commitment problem, thus undermine the stability of settlement agreements.

Scholars focused on the commitment problem in civil war argue that certain provisions can prevent the breakdown of peace and a return to war. According to

Walter (2002), for example, commitment problem can be overcome through third party monitoring and enforcement of agreements. Third party verification of compliance with the terms for demobilization and disarmament provides combatants with prompt, reliable information to ensure that reneging does not occur without their knowledge. Further, strong third party missions with the mandate to use force can enforce the terms of settlement by reassuring the weaker side that surprise attacks will be defended against by the guarantor, and by deterring such surprise attacks by making them more costly for the stronger actor (Walter 2002).

In addition to third party guarantees, the design of settlement agreements can offset commitment problems and thus increase the likelihood of successful conflict termination. Agreements are more effective at ending conflict, and more durable, when they include power-sharing provisions, as these provisions ameliorate commitment problems by ensuring that the weaker party to the agreement retains a voice in post-conflict politics. As power-sharing expands across political, military, territorial, and economic dimensions, peace becomes more durable; multiple dimensions of power sharing ensure that neither side can exercise full control over any area of state power, thus reassuring former combatants that they will not become victims of discrimination or renewed violence in the post-conflict state (Hartzell and Hoddie 2003, 2007). In addition, components of a negotiated settlement that increase the costs of renewed fighting, such as the withdrawal of foreign forces, border seals, the separation of troops, and peacekeeping, facilitate durable settlement by decreasing combatants' incentives to renew violence (Mattes and Savun 2009).

#### **IV. Gaps in Existing Explanations**

The expected utility, conflict profiteering, and commitment problems theories discussed above all provide important insights into the dynamics of civil conflict. Each of these theories, however, suffers from certain weaknesses that limit its ability to explain variation in conflict duration and outcome. First, theories focused on the utility of conflict and the costs of war are not well-supported by empirical evidence. While findings suggest that more costly conflicts do increase the likelihood of negotiations, they have no significant impact on the success of those negotiations; that is, whether or not talks actually result in conflict settlement (Walter 2002). This suggests that theories focused on the costs of war have overlooked other factors which, once negotiations are underway, play a more important role in shaping the outcome of those talks.

Second, while economic models of conflict are theoretically intuitive, they have little to say about the dynamics of conflict in countries with few lootable natural resources. For example, they cannot easily explain the duration of civil war in Mozambique, Sri Lanka, and the Philippines, none of which exhibited the characteristics of a rebellion-as-business model, as none was impacted by lootable economic resources that could incentivize actors to prolong conflict for profit. And yet these conflicts lasted 16, 26, and over 40 years, respectively, all well over the average civil conflict duration of approximately 8 years.

Finally, models that focus on commitment problems cannot easily explain a variety of cases in which settlements were reached despite a lack of third party guarantees or power-sharing provisions, and why, on the other hand, a number of



settlements in which these provisions were present have failed to produce lasting peace. Why, for example, did the MNLF sign a settlement agreement with the Philippine government in 1996, despite a lack of third party guarantees backing the agreement? And why, in the Angolan civil war, did peace agreements signed in 1991 and 1994 fail despite a UN peacekeeping presence and relatively robust power-sharing provisions, while a successful settlement was achieved in 2002 despite terms that were arguably less favorable for peace from the commitment problems perspective? In more general terms, scholars focused on facilitating conflict termination by ameliorating commitment problems tend to assume all conflicts are equally plagued by incentives to renege, rather than examining potential variation in the prevalence of commitment problems and identifying conditions that make credibility issues more or less severe.

In addition to these theory-specific limitations, existing explanations share one important characteristic in common which, I argue, limits their ability to provide a comprehensive explanation for civil war termination and outcome. Specifically, while existing theories provide important insights into the country or dyad-level determinants of war termination, they provide little insight into how the personal incentives of rebel and state leaders influence war termination and outcomes. Put differently, existing scholarship on intrastate conflict termination has done relatively little to open the black box of rebel organizations, or to focus attention on the incentives of rebel and state leaders involved in civil wars.

This oversight is surprising, given the importance placed upon personal utility calculations for explaining why individuals join rebellions (Humphreys and

Weinstein 2008; Kocher and Kalyvas 2007; Lichbach 1995). Recent research on the organizational structure of rebel groups (Weinstein 2007) and the composition of self-determination movements (Bakke, Cunningham, and Seymour 2012) has begun to examine variation in the internal structures of rebel organizations, and how those variations influence a variety of outcomes. These studies represent the most recent stage in the theoretical and empirical disaggregation process ongoing in the civil war literature, which has, over the past decade, moved from analysis of country-level characteristics (Fearon 2004; Hegre 2004), to dyad-level and group-level attributes (D. E. Cunningham, Gleditsch, and Salehyan 2009; K. G. Cunningham 2011). Each step in the process of disaggregating civil war studies has provided important theoretical insights, allowing for refinement of theories through the use of more fine-grained empirical evidence. However, these studies have thus far ignored the incentives of individual leaders, instead focusing on more general characteristics of the combatant group or conflict dyad.<sup>5</sup>

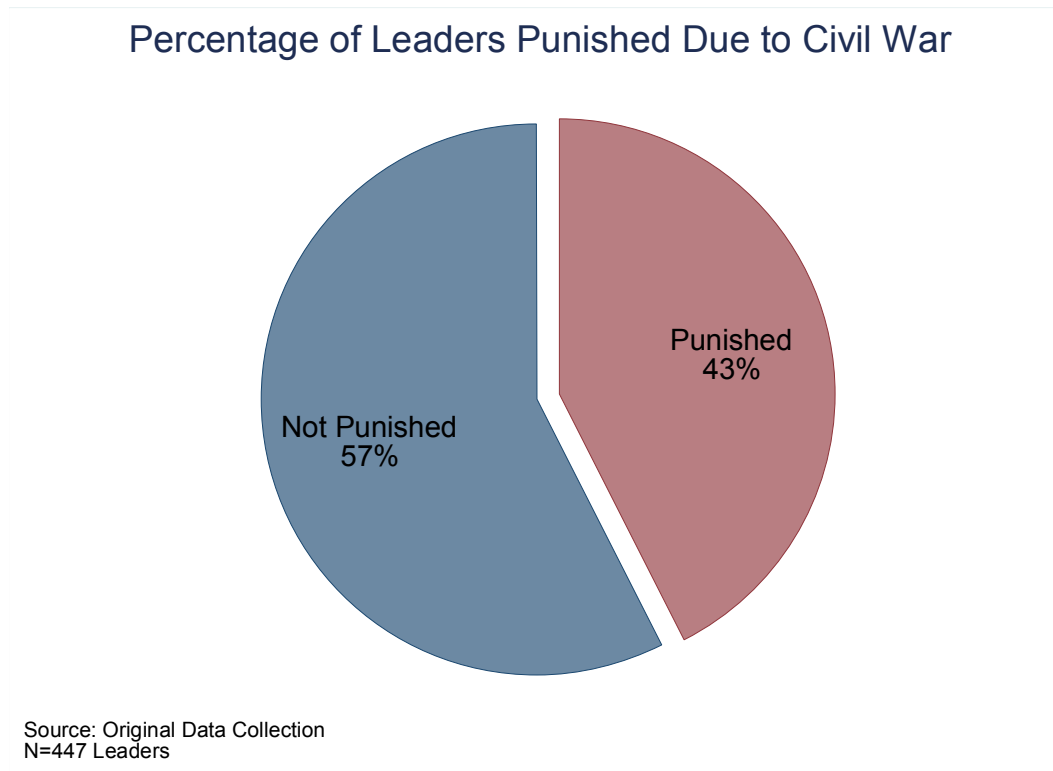
This failure to account for leaders' incentives is a critical weakness in existing literature for several reasons. Most importantly, by ignoring leader incentives, these analyses fail to account for the high-stakes nature of civil war for the state and rebel leaders involved. Figure 1.2 demonstrates that leaders often face punishment as a

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<sup>5</sup> There are two exceptions to this lack of research. First, Thyne (2012) finds that politically stronger, more stable state leaders are more capable of achieving civil war settlements. Thyne's analysis is limited, however, in that it examines only state leaders. Second, research in the terrorism literature finds that leadership decapitation increases the likelihood conflict termination and government victory (Johnston 2012; Price 2012). However, these analyses are limited in two respects: first, they focus only on specific attempts by the state to remove terrorist leaders rather than all types of leadership turnover. Second, they do not theoretically explore variation in the incentives of different leaders, instead assuming that all leader losses affect a group equally.

result of the civil wars they fight: nearly half (43%) of the leaders included in my data experience some type of punishment – i.e. loss of political power, exile, imprisonment, or death – as a result of civil conflict. Given the prevalence of these outcomes, it is reasonable to assume that leaders take the threat of punishment seriously as they make strategic decisions regarding tactics, war aims, and negotiations during civil war.

**Figure 1.2 Percentage of Leaders Punished Due to Civil War**



What does this mean for theories of civil war termination and outcome? If rebel and state leaders take the threat of punishment seriously, and it influences their wartime decisions, then our theories about civil conflict dynamics and termination

should also take a leader's expectation of punishment into account. Failure to account for this important factor influencing conflict behavior in existing literature is a major oversight. It renders these existing explanations incomplete at best, and, at worst, inaccurate. Thus, while existing theories tell us a great deal about how a variety of country, conflict, and dyad-level factors influence conflict termination and outcome, a full understanding of when and how civil wars end requires looking beyond existing explanations and accounting for the personal incentives of rebel and state leaders.

## **V. A Leader-Based Theory of Civil War Termination**

To this end, this dissertation moves beyond existing country and dyad-level explanations and explicitly models the important role played by leaders. The theory developed herein simultaneously accomplishes two things. First, it addresses the serious oversight in existing literature which fails to account for leaders' incentives, and second, in so doing, it provides potential explanations for many of the remaining puzzles and inconsistencies in existing theories of civil war termination that were highlighted in the previous section.

Building upon recent international relations scholarship that examines how domestic political considerations shape foreign policy decisions in general (Bueno De Mesquita et al. 1999a, 1999b; Chiozza and Goemans 2004; Debs and Goemans 2010; McGillivray and Smith 2006; Putnam 1988) and wartime decisions more specifically

(Colaresi 2004; Croco 2011; Goemans 2000; Mattes and Morgan 2004)<sup>6</sup>, I argue that an individual rebel or state leader's incentive to retain political power and otherwise avoid punishment is an important determinant of wartime behavior. The dissertation's central theoretical claim posits that culpable leaders – those viewed by internal and opposition-based audiences as responsible for the war – face a higher expectation of punishment from both internal and opponent-based sources following unfavorable war outcomes than non-culpable leaders, as war losses will be viewed as their own personal policy failures. Non-culpable leaders, on the other hand, can more easily avoid responsibility for poor war performance because they can avoid association with the original decision to go to war and the war aims established at the start of the conflict. Because of their higher expectation of punishment, culpable leaders will have a lower utility for settlement on compromise or losing terms than their non-culpable counterparts, and will therefore face incentives to continue losing wars in a 'gamble for resurrection' (Downs and Rocke 1994) through which they hope to turn the tide, achieve victory, and avoid punishment.

Because of these incentives to gamble for resurrection, I hypothesize, culpable leaders will be less likely to terminate a conflict than their non-culpable counterparts, instead choosing to prolong ongoing wars in order to avoid internal and adversary-based punishment. The heightened threat of punishment faced by culpable leaders will also influence war outcomes; they will forego compromise settlement opportunities, choosing to continue the fight in the hope of achieving a military

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<sup>6</sup> A leader's domestic political concerns and incentives to avoid punishment have been shown to impact a state's war aims, termination behavior, and the outcome of conflict during interstate war (Colaresi 2004; Croco 2011; Goemans 2000; Mattes and Morgan 2004).

victory as a way to forestall punishment. Thus, culpable leaders will be less likely to make concessions on key issues at stake in the war than their non-culpable counterparts, who can more easily disassociate themselves with the war aims established at the start of the conflict and thus avoid punishment for conceding on one or more of those central issues. Additionally, the culpable leader's incentive to gamble for resurrection will lead to more extreme war outcomes. The military gamble will sometimes pay off, allowing the culpable leader to achieve victory where a non-culpable leader would have settled for less. At other times, however, the gamble will fail, and culpable leaders who forego settlement opportunities only to suffer further setbacks on the battlefield will suffer more negative war outcomes than their non-culpable counterparts. Thus culpable leaders are more likely to experience major victories and total defeats than non-culpable leaders.

I employ both quantitative and qualitative methods to test my theoretical expectations regarding the impact of leader culpability on civil conflict termination and outcomes. First, I test these hypotheses quantitatively using an original dataset of rebel and state leaders of a global random sample of civil conflicts ongoing between 1980 and 2010. Results demonstrate, as expected, that civil conflicts are significantly less likely to terminate when culpable leaders are in power. Additionally, the results show that culpable state and rebel leaders are more likely to experience extreme outcomes than non-culpable leaders, and are less likely than non-culpable leaders to make concessions on central war aims at termination. These results are robust to a variety of alternative variable measurements, model specifications, and modeling

strategies, and tests to account for potential endogeneity problems indicate that the relationship between culpability and war outcomes is not spurious.

Second, I use both quantitative and qualitative evidence to test the underlying causal mechanism in my theoretical argument, that culpable leaders are more commonly punished for poor war performance than their non-culpable counterparts. To test this mechanism quantitatively, I use an original dataset on the fate of every rebel and state leader in my sample, as well as original data on each leader's war performance. Statistical results show, as expected, that culpable leaders who perform poorly in war are significantly more likely to be punished than non-culpable leaders who perform similarly poorly in war. Again, these results are robust to a variety of alternative specifications and modeling strategies.

Finally, I use qualitative evidence from the civil war in Angola between 1975 and 2002 to provide additional evidence in support of the mechanism underlying my theoretical argument. Using within-case comparison of the periods before and after UNITA leader Jonas Savimbi's death in 2002, as well as process-tracing of the settlement attempts between UNITA and Angolan government forces in 1989, 1991, 1994, and 2002, I examine the role that culpability and the resulting threat of punishment played in extending the Angolan conflict and undermining negotiated settlement attempts. Case evidence from Angola demonstrates that the threat of punishment faced by Savimbi played a critical role in undermining all three major settlement attempts between government and UNITA forces in the 1980s and 90s. Non-culpable UNITA leader Paulo Lukamba Gato, who came to power after Savimbi's death, faced significantly lower threats of both internal and opponent-

based punishment than his predecessor had, and as a result, was able to commit to a final settlement in 2002, ending the Angolan conflict after 27 years.

This strong quantitative and qualitative support for my theoretical argument demonstrates the importance of taking leader incentives into account when examining civil conflict dynamics and outcome. This dissertation has far-reaching implications for scholarly understandings of civil war termination, suggesting that the decision to terminate and the outcome achieved in civil war are affected not only by state and dyad-level factors, but also by state and rebel leaders' expectations of personal punishment, from two distinct sources, following conflict termination.

## **VI. Contributions and Policy Implications**

This project makes several important contributions to existing scholarship on civil war termination, and has implications for policy-makers seeking to facilitate the timely and successful settlement of ongoing conflicts worldwide.

First and foremost, it is the first study, to my knowledge, to identify both rebel and state leaders' incentives as a novel factor influencing civil war termination and outcome. As such, it demonstrates that existing scholarship focused on dyad and country-level factors such as relative strength, costs of war, commitment problems, and natural resources are valuable but incomplete. In moving beyond these dyad and country-level explanations, this project recognizes that leaders have personal incentives independent of the interests of the groups they represent, and, importantly, identifies the conditions under which conflict-level factors will be eclipsed by those incentives.



This project thus has important implications for more well-established theories in the civil conflict termination literature. First, taking leader incentives and culpability into account provides a theoretical explanation for why high costs of conflict do not consistently produce negotiated settlement. By recognizing that leaders have independent interests which, at times, incentivize the continuation of costly conflicts in order to avoid punishment, this project provides a theoretical mechanism to explain why negotiations sometimes fail and conflicts continue, even when war is particularly costly. It suggests that culpable and non-culpable leaders will respond differently to the costs of war, and identifies a potential conditionality in the relationship between the costs of war and war termination that existing theories have failed to account for.

Additionally, the leader incentive-based theory developed herein identifies a potential source of variation in the prevalence or severity of commitment problems, which existing explanations tend to treat as constant rather than variable. Because of their heightened risk of punishment, I argue, culpable leaders will face particularly acute commitment problems. Non-culpable leaders, on the other hand, will be able to make more credible promises and commitments to peaceful settlement of conflict, as they face a lower threat of internal and opponent-based punishment when they compromise on central issues at stake in the war. This suggests that leader characteristics are a potential source of variation in the severity of commitment problems across different conflicts or within the same conflict at different times. This novel insight advances existing theories of civil war termination based on commitment problems by identifying a potential explanation for why some conflicts

seem to be more plagued by commitment problems than others; that is, why some negotiated settlements are agreed and implemented relatively quickly, while others falter numerous times at the negotiation or implementation stage.<sup>7</sup>

Third, by introducing leader-based analysis to the civil conflict literature, this project extends and modifies existing theories from the international relations literature, adapting them to fit the civil war context. In particular, it applies the concept of culpability (Croco 2011) from existing research on interstate conflict to the civil war context, and in so doing, modifies and updates existing theory by accounting for distinct types of punishment and actors relevant in intrastate conflict. Punishment in the interstate war literature is usually conceived of as loss of political power (Bueno De Mesquita et al. 1999a, 1999b). Because more severe forms of punishment are much more prevalent as a result of civil war than interstate war, I extend the concept of punishment, combining loss of power with other more severe forms of punishment, such as exile and assassination, that are less frequently theorized in the interstate conflict literature (though see Goemans 2000). Further, while the interstate literature focuses on punishment by domestic audiences only (i.e. internal

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<sup>7</sup> By focusing on leaders' incentives, this study also contributes to a growing body of research in the civil conflict literature that identifies internal group characteristics as important determinants of conflict dynamics (K. G. Cunningham 2011; Weinstein 2007), and advances emerging work on the importance of leadership change for conflict termination and the defeat of terrorist organizations (Johnston 2012; Price 2012; Thyne 2012). It takes an important step forward from these existing studies, however, by demonstrating that it is not necessarily leadership change or leader tenure per se, but the incentives of culpable versus non-culpable leaders that affect war termination and outcomes.

punishment), the theory developed here conceptualizes punishment as deriving from both internal and opponent-based sources, as both are prevalent during civil war.<sup>8</sup>

Fourth, this project also makes two important data contributions. First, I develop an original dataset on the leaders of a global sample of civil conflicts between 1980 and 2010. As part of this data collection effort, I have identified all leaders of rebel groups included in the sample. This is the first cross-national dataset, to my knowledge, to identify rebel leaders and to include cross-national data on civil war leaders' time in power, culpability, and post-tenure fates. These new data on rebel leaders, and the additional information collected on both state and rebel leaders involved in civil wars, can be used in a variety of ways in future analyses. Second, as part of this project, I have developed a novel coding scheme for war outcomes that identifies the level of favorability of the war outcome for each warring actor, relative to its original war aims. This new measure overcomes two major limitations in existing data on the outcome of civil wars. First, it eliminates the ambiguity of existing coding schemes that group together all outcomes of a particular type (e.g. negotiated settlement or low activity), regardless of their level of favorability. Second, it better accounts for what each actor achieved through the conflict, and thus

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<sup>8</sup> Although testing variations in the impact of culpability in interstate versus civil wars is beyond the scope of the current project, theoretically, I expect its impact to vary between the two categories of conflict. In particular, because punishment is often more severe and derives from multiple sources during civil war, I expect culpability to have a larger impact on conflict duration and outcomes during civil war than during interstate conflict. Further, existing research demonstrates that culpability's impact is contingent upon regime type during interstate war (Croco 2011). In the civil war context, however, I expect culpability to influence conflict behavior regardless of regime type, largely due to the fact that opponent-based punishment is not contingent upon the type of political institutions regulating political competition and leader removal in either rebel groups or state governments. This proposition is tested in Appendix B.

how satisfied each actor is with the post-conflict distribution of benefits. This has important implications not only for how wars end, but also for the stability of the post-conflict peace.

Finally, this project offers important insights for policymakers regarding appropriate timing and strategies for intervention and mediation. Specifically, the theory and results of this dissertation suggest that mediation strategies aimed at facilitating timely settlement of civil wars should aim to address the personal security concerns influencing leaders' behavior. Mediator proposals must address the personal vulnerabilities of the leader, providing a more favorable post-conflict scenario for the leader than he can expect through continued conflict, in order for mediation efforts to result in successful settlement. This may be quite difficult to achieve, however, as conciliatory offers towards culpable leaders will be unpalatable to a variety of domestic and international actors, and culpable leaders will be willing to take on high-risk strategies in the hope of achieving military victory. This suggests, therefore, that the best opportunities for successful mediation – i.e. the ripest moments for resolution – will occur when non-culpable leaders take power, and that more robust military interventions may be necessary to bring conflict to an end when culpable leaders remain in charge.

## **VII. Roadmap for the Dissertation**

The analysis of civil war termination and outcome in this dissertation proceeds through six chapters. Chapter 2 presents the dissertation's theoretical argument on leader culpability, its impact on the expectation of punishment, and its

resulting effect on conflict termination and outcome. It develops a novel theory of civil war termination and outcome, drawing on insights from the international relations literature, principal-agent framework, and theories of credible commitments in the civil war literature. The theory developed in chapter 2 explains how culpability affects the expectation of punishment, which, in turn, affects a leader's utility for termination and the war outcomes he will be willing to settle for.

Chapter 3 tests the main propositions developed in chapter 2 through quantitative analysis. After reviewing the key hypotheses, it provides information on the new dataset developed for this project, discussing the measurement of leader culpability and details on the new coding scheme for war outcomes. The empirical results are then presented and discussed, and provide strong statistical support for the theoretical argument developed in the previous chapter. A variety of robustness checks are also presented in chapter 3 to ensure that the results are not sensitive to alternative coding schemes for the dependent and key independent variables, to alternative model specifications, or to the potential endogeneity of leader culpability.

Chapters 4 and 5 examine the causal mechanisms underlying the leader incentive-based theory of civil war termination developed in chapter 2, the former through quantitative analysis and the latter via qualitative examination of the civil war in Angola from 1975 to 2002. The results of the quantitative analysis presented in chapter 4 provide strong support for the punishment mechanism underlying the dissertation's theoretical argument, and are robust to a variety of alternative modeling strategies and measurement specifications. Chapter 5 provides additional support for the main theoretical arguments presented in the dissertation, using qualitative

evidence from Angola's civil war. Controlled-comparison of the periods before and after culpable UNITA leader Jonas Savimbi died demonstrates the critical role that a culpable leader's fear of punishment plays in prolonging civil conflict.

Finally, chapter 6 concludes by briefly revisiting the main theoretical argument and results, and by examining the broader implications of the project's central findings for theories of civil war termination and outcome as well as the policy community's approach to conflict mediation, resolution, and intervention.

## **Chapter 2 : A Theory of Leader Incentives and Civil War**

### **Termination and Outcome**

#### **I. Introduction**

In the previous chapter, I introduced two related questions central to research on intrastate conflict. First, why do some civil wars end quickly, while others drag on for years or even decades? Second, why do some combatant groups agree to compromise outcomes while others fight to a military end? That is, what explains variation in the duration and outcome of civil wars? A review of existing literature in the previous chapter demonstrated that current scholarly explanations for civil conflict termination and outcome are incomplete. Because they fail to account for the incentives of rebel and state leaders in civil wars, they cannot adequately explain why some civil wars drag on despite fairly clear evidence that neither side can win, while others end in compromise despite the fact that victory may still be possible. Further, they do not adequately address why some attempts at settlement break down in continued fighting, despite robust settlement agreements, while other settlement attempts succeed despite a lack of proven commitment mechanisms.

This chapter develops a novel theory of civil war termination and outcome to address these remaining puzzles. It builds upon existing rationalist explanations, but revises and updates their expectations by allowing for an important modification to their basic underlying assumptions. Specifically, I relax the unitary actor assumption

that prevails in existing civil war termination literature, instead allowing the individual incentives of rebel and state leaders to differ from those of the groups they lead. This chapter thus develops a novel theory of civil war termination and outcome focused on the incentives of state and rebel leaders. It draws upon principal-agent theory and insights from credible commitment models of civil war to explain why some state and rebel leaders pursue policies that are in the best interests of their constituencies, while other leaders make strategic decisions that may serve their own interests but leave the groups they represent worse off. Variation in war termination and outcome are explained as a result of leader-specific incentives to avoid punishment from two sources: winning coalitions and adversary elites.

The chapter's central theoretical claim is that leaders who bear responsibility for a war – that is, culpable leaders – will expect different payoffs for settlement than those who are not responsible for involving their states/groups in the conflict. Culpable leaders will anticipate a higher likelihood of punishment from both internal and adversary-based audiences if they settle for anything less than a win, and will therefore have a lower utility for termination on unfavorable terms than leaders who do not bear responsibility for starting the war. Because of their lower utility, culpable leaders will be less likely to terminate a civil conflict on compromise or moderately losing terms, instead continuing the conflict in the hope of achieving a better outcome that will diminish the risk of internal and adversary-based punishment. The theoretical argument developed herein has far-reaching implications for our understanding of civil war termination and outcome, suggesting that the decision to terminate and the outcome achieved are affected not only by state and dyad-level



factors, but also by state and rebel leaders' expectations of personal punishment, from two distinct sources, following termination.<sup>9</sup>

This chapter elaborates the dissertation's theoretical argument in the sections below. I first relax the unitary actor assumption and introduce assumptions on the theory's three central actors: leaders, their winning coalitions, and adversary elites. Drawing on principal-agent framework, I then develop a theory of civil war termination that focuses on the relationship between leader (agent) and constituency (principal), which is characterized by information asymmetries and the potential for goal conflict, which can cause leaders to act as unfaithful agents, pursuing policies and strategies that serve their own interests rather than those of their constituencies.<sup>10</sup>

I draw upon insights from the interstate war literature, which identifies culpability as an important source of variation in leader wartime behavior (Croco

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<sup>9</sup> The theory developed in this chapter focuses specifically on domestic-level sources of punishment. There are, in many cases, additional constraints on leaders that derive from the international community. One explicit source of international punishment likely to affect leaders' incentives is investigation or indictment by the International Criminal Court (ICC). The impact of this international source of punishment is controlled for in the empirical analysis.

<sup>10</sup> The principal-agent framework provides a useful tool for understanding how leaders' incentives to avoid punishment affect their wartime behavior. According to principal-agent theories, an agency relationship exists "between two (or more) parties when one, designated as the agent, acts on behalf of another, designated the principal" (Leventhal 1988, 155). Consider, for example, a firm in which stockholders delegate the daily running of the firm to a manager. In this situation, the manager (agent) acts on behalf of stockholders (principal) by handling the daily operations of the firm. In the application of the principal-agent framework to rebel and state actors in civil war, the leaders of these organizations are considered the agents, while the groups they represent are the principals. This use of principal-agent theory builds upon similar applications in the interstate conflict literature (Davies 2002; Downs and Rocke 1994; Feaver 2005; Goemans 2000; Richards et al. 1993). While studies of civil war termination and outcome tend to treat combatant groups as unitary actors, some recent studies use the principal-agent framework to examine the behavior of rebel groups during civil war (Gates 2002; Weinstein 2007). These analyses, however, model the rebel leader as the principal and the individual fighters as his agents.

2011). I modify and adapt this concept for the civil war context, arguing that a leader's perceived responsibility for the war provides information to two relevant audiences: the leader's constituency and his adversary.<sup>11</sup> First, culpability helps mitigate information asymmetries between leader and constituency by providing constituents an important cue regarding the leader's likely type. Using this cue and information provided by war performance, internal audiences will update their beliefs regarding the leader's level of competence, and will decide whether or not to punish the leader based upon that determination. Second, a leader's culpability also provides information to adversary elites regarding the likelihood that that leader will be able to commit to peace. Culpable leaders, because they face a high expectation of punishment internally, have incentives to extend losing conflicts in a gamble for resurrection, and will be viewed by adversary elites as less capable of making credible commitments to peace than their non-culpable counterparts. Adversary elites will therefore have incentives to attempt to remove the opponent leader in the hope that a more credible negotiating partner takes his place. This threat of opponent punishment, however, has the paradoxical effect of increasing the culpable leader's incentives to continue the war as a means to avoid punishment.

Thus, both internal and opponent-based threats of punishment increase the culpable leader's incentives to gamble for resurrection, which in turn affect his expected termination behavior and war outcomes. Hypotheses are developed based upon these theoretical expectations, and the chapter concludes by examining the

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<sup>11</sup> Only the former is theorized in the interstate war literature. Examining opponent-based punishment is therefore a unique contribution of this study.

implications of this theory for our understanding of civil war termination and outcome.

## **II. Internal Politics and Civil War Termination: Actors and Assumptions**

The theory developed below makes several important assumptions about the incentives of and relationships between leaders, their winning coalitions, and adversary elites during civil war. This section introduces those assumptions, beginning with the central assumption that leaders are rational, self-interested actors whose primary goal is to maintain political power and avoid other forms of punishment. In addition to this rational actor assumption, I assume that leaders enjoy an informational advantage relative to their constituents with regard to their competence and wartime decision-making, that constituents are more risk-averse than their leaders, and that opponents prefer to face a leader who can credibly commit to peace. These assumptions and their implications are discussed below.

### **2.1 Winning Coalitions and Opponent Elites**

Before discussing these assumptions in detail, however, it is useful to clearly identify the two actors who are capable of influencing leaders' behavior during civil war. A leader's winning coalition (WC) is the first relevant actor, and the first potential source of punishment for the leader (Bueno De Mesquita et al. 1999a).<sup>12</sup> Specifically, a leader's winning coalition includes those individuals or groups within

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<sup>12</sup> I use the terms winning coalition, internal audience, and constituency interchangeably throughout this chapter and the remainder of the dissertation.

the state or rebel organization who can coordinate to hold the leader accountable (Weeks 2008). The makeup of the winning coalition varies for both state and rebel leaders depending upon the type of governing structures or rules of participation that determine to whom the leader is accountable. For state leaders, the size and composition of the relevant coalition is determined by the type of political system in place.<sup>13</sup> In democratic states, the coalition includes members of the general public, while in autocratic states it is limited to regime elites on whom the leader is reliant for his survival in office. There is similar variation in the composition of internal audiences for rebel leaders, as rebel groups, like governments, vary in their rules of participation and the level of concentration of power (D. E. Cunningham, Gleditsch, and Salehyan 2009; Weinstein 2007).<sup>14</sup> While the size of the winning coalition varies, for nearly all rebel and government leaders, there exists at least a small inner circle of individuals that has the power to remove or otherwise punish the leader.

The second relevant actor and second potential source of punishment for a civil war leader is the opposition or adversary elite. This actor is comprised of those

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<sup>13</sup> As noted in the earliest articulations of audience costs theory, relevant audiences vary in size and makeup, having included, at various times, “kings, rival ministers, opposition politicians, senate committees, politburos, and, since the mid-nineteenth-century, mass publics informed by mass media” (Fearon 1994a, 581). While the majority of research on audience costs focuses on the difference between democratic and nondemocratic regimes in terms of their abilities to generate audience costs (Schultz 2001), more recent research provides convincing evidence that the mechanisms driving the generation of audience costs are present in non-democratic systems as well (Brown and Marcum 2011; Gandhi and Przeworski 2007; Weeks 2008). As Weeks (2008, 36) notes, the “crucial question ... is whether the relevant domestic audience can and will coordinate to sanction the leader”, regardless of the size or composition of that audience.

<sup>14</sup> A rebel group leader’s winning coalition is more often limited to a small number of high-level commanders or elites within the rebel organization who, if they are able to overcome the coordination problem and decouple their own fates from that of the rebel leader, can punish the leader for poor policy decisions.

individuals within the political/military leadership of the wartime *adversary* who hold positions with decision-making authority on issues of security and justice both during the war and at its termination. The exact composition of this group of adversary elites will vary depending upon the adversary's political system or organizational structure, but it generally consists of the group of military strategists and political leaders who determine whether they will focus on fighting their opponent as a whole, or will instead pursue a strategy that directly targets their opponent's leadership. A leader-targeted strategy can be pursued either during the conflict or after; attempts by the adversary elite to decapitate their opponent's organization through killing, capturing, or imprisoning its leader can be pursued throughout the course of the war, while retributive justice through formal or informal legal institutions can be pursued at the conflict's end if the adversary has retained or gained control of the state apparatus.

## **2.2 Rational Self-Interested Leaders**

Existing models of civil war termination and outcome treat states and rebel groups as unitary actors. As such, leaders are assumed to embody the interests of the groups they represent. The theory developed in this section relaxes this assumption, allowing leaders' interests to differ from those of their winning coalition members.<sup>15</sup>

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<sup>15</sup> International Relations (IR) scholars over the past two decades have devoted increasing attention to the interconnections between domestic politics and the foreign policy decisions of states. By opening the 'black box' of the state, this new tradition has generated important insights into how domestic political considerations shape foreign policy decisions (Bueno De Mesquita et al. 1999a, 1999b; Chiozza and Goemans 2004; Croco 2011; Debs and Goemans 2010; McGillivray and Smith 2006; Putnam 1988). Building upon these recent contributions to the interstate conflict literature and new research in the civil conflict literature that examines the internal characteristics of rebel groups (K. G. Cunningham 2011; Weinstein 2007), the theory

More specifically, I assume that state and rebel leaders are rational actors whose primary goal is to maintain political power and avoid more severe forms of punishment such as exile, imprisonment, or death.

The first part of this assumption, that leaders seek to maintain political power, is a well-established theoretical assumption in the international relations literature (Bueno De Mesquita and Siverson 1995; Bueno De Mesquita et al. 1999a; Caselli and Cunningham 2009; Colaresi 2004; Croco 2011; Gandhi and Przeworski 2007; McGillivray and Smith 2006). State leaders value their positions of political power, as leadership grants them tremendous influence over the resources of the state, as well as the distribution of those resources through the setting of domestic and foreign policy. The same is true for rebel leaders. Leadership of a rebel organization grants the individual extensive control over any political or economic resources under the group's control, and generally provides the leader tremendous influence over the group's agenda and the strategies and tactics it uses to publicize its grievances and pursue its goals. Leadership of the rebel organization, furthermore, carries with it the promise of future national-level political power, should the group succeed in toppling the government. Because of their authority positions, rebel and state leaders can set policy for their groups, pursuing strategies designed to maximize the benefits accruing to their winning coalition members and themselves.

The second aspect of this assumption, that leaders want to avoid more severe forms of punishment such as exile, imprisonment, and death, is equally pertinent in the context of civil war. As some scholars recognize, the loss of political office is just

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developed below builds from the assumption that leaders have interests independent of the groups they represent.

one possible – and relatively mild – consequence of failed policies (Debs and Goemans 2010; Goemans 2000, 2008). Particularly in the context of civil conflict, loss of leadership is often associated with more severe punishments, such as imprisonment, exile, or even death. Thus, state and rebel leaders who choose to forego nonviolence in favor of violent strategies to defeat their opponents take on significant personal risk. While the payoffs from victory are high – control of government, economic resources, and the ability to shape political, military, economic, and social policy – the costs of defeat are daunting.

Severe forms of punishment can occur either internally or at the hands of the civil war opponent. Karrim Kassem of Iraq and U Nu of Myanmar, for example, were both removed from power in internal coups in the early 1960s, largely due to failure to defeat rebellions in their respective countries. Kassem was killed during the coup, and U Nu was imprisoned for several years following his removal.

Punishment by an opponent is also often severe, as adversary elites may see an opening to permanently remove the threat posed by a political foe. Examples abound of unsuccessful state and rebel leaders and the grim fates they met at the hands of opponent forces as a result of their failures. The recent conflict in Libya, for example, resulted in the capture and brutal killing of Muammar Qaddafi by opposition forces. High-profile rebel leaders such as Abimael Guzmán of Sendero Luminoso and Vellupillai Prabhakaran of the LTTE have faced similarly grave fates, the former being imprisoned by Peruvian military officials and the latter being killed by government forces after admitting defeat.

Given the threat of punishment internally and by opponent forces, both state and rebel leaders will be primarily concerned with the retention of political power and avoidance of more severe forms of punishment in the context of civil war. As rational actors, leaders are expected to take the threat of punishment into consideration when deciding whether to continue or terminate a conflict. Importantly, while the goal of maintaining power and avoiding punishment sometimes promotes policy decisions that simultaneously serve the interests of the leader and the group he represents, at other times, a leader's desire to avoid punishment generates incentives for policies at odds with the interests of the group. This potential for goal conflict is discussed further in the theory developed below.

### **2.3 Private Information**

The second theoretical assumption underlying the current argument also focuses on leaders. Specifically, leaders are assumed to enjoy an informational advantage over their winning coalitions during civil war. Two forms of uncertainty characterize the relationship between leaders and group members of both rebel organizations and states during civil war. The first relates to the winning coalition's inability to costlessly observe and monitor the decisions of its leader. The second refers to the constituency's uncertainty regarding the leader's type – that is, his leadership ability or level of competence.

First, rebel and state leaders enjoy an informational advantage with regard to their own decisions, the effects of their policy choices, and overall war performance. The leader's informational advantage in this respect stems from the inability of



internal audience members to directly observe or monitor the leader's behavior and decisions. Political/military strategy is generally decided behind closed doors during civil war, where the high-stakes nature of inter-actor relations increases the need for secrecy to limit the threat of exposure and infiltration. In large-WC states or groups with relatively democratic selection procedures, the vast majority of WC members (i.e. ordinary citizens or group members) are not privy to the high-level discussions that set war-time policy. Even in small-WC organizations, where the leader relies upon the support of a relatively small group of officials for his maintenance in power, information asymmetries exist. Under these circumstances, organizational decentralization and specialization within government will ensure that no winning coalition members are privy to all strategy decisions made by the leader. That is, individual coalition members may be involved in strategic decisions in their areas of expertise, but the vast majority will not be fully informed on all relevant issues. Thus, information asymmetries will exist even in small-WC states or rebel groups.

The leader's informational advantage is exacerbated by the limited flow of battlefield and negotiating table information during civil conflicts. First, information from the battlefield is often sparse due to poor infrastructure, remote conflict centers, decentralized organizational structures, particularly within many rebel organizations, and media control by states. While individual commanders will likely have intimate knowledge of the state of affairs in their regions of operation, organizational structures often prevent anyone but top leadership from gaining a full picture of events on all fronts. Even high-ranking commanders that direct operations on a particular front, for example, may have little sense of the state of affairs in another

region. Lack of established communication structures or the disruption of existing modes of communication in many conflict zones increases this informational disadvantage. Battlefield information will be reported back to the leader, who can strategically control its dissemination to the rest of the winning coalition. Second, negotiations with the opponent generally take place behind closed doors, and the specifics of any offers made often remain private information until well after deals are agreed and signed. Thus, the leader's negotiating strategy and prowess remain unobservable by all winning coalition members who are not themselves involved in negotiations.

Leaders thus enjoy an informational advantage with regard to both battlefield information and political developments during the war. Because the flow of information is limited, group members cannot easily gain an accurate, real-time understanding of the state of affairs. This information asymmetry between leader and audience generates the potential for a moral hazard problem to arise (Leventhal 1988). Because constituents cannot directly observe many of the leader's decisions or their repercussions, leaders are free to pursue their own interests rather than those of the group/state without detection. That is, the leader can behave as an unfaithful agent with little fear of immediate detection.

Leaders also enjoy an informational advantage with regard to individual characteristics that affect their competence or ability-level. This source of uncertainty is recognized as an important factor in the economics literature on principal-agent

relations (Leventhal 1988).<sup>16</sup> It is also explicitly linked to competence in studies on the diversionary use of force, where a leader's level of competence is treated as private information that the public makes inferences about based on domestic and foreign policy outcomes (Richards et al. 1993). Here, I assume that a leader's level of competence is private information, and that constituents prefer a competent leader, as leader competence is expected to improve policy outcomes and future bargaining effectiveness. Existing literature commonly identifies a leader's perceived competence as a central concern of domestic audiences and winning coalitions because of its impact on current and future outcomes (Croco 2011; Fearon 1994a; McGillivray and Smith 2006; Tomz 2007; Weeks 2008).<sup>17</sup>

Given that a leader's competence is private information and that constituents prefer a high-competence type, low-ability leaders have incentives to portray themselves as high-ability types, and constituents have no decisive way, *ex ante*, to distinguish the two. This problem is exacerbated by the first informational advantage enjoyed by leaders. Because group members or citizens cannot easily observe the leadership decisions that lead to observable political and military developments during war, they cannot easily update their beliefs on the leader's type. While poor

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<sup>16</sup> More specifically, this source of private information is referred to as an adverse selection or a self-selection issue, which arises specifically because of uncertainty over the personal characteristics of an individual (Leventhal 1988).

<sup>17</sup> Leader incompetence is one of the central mechanisms used to explain leader behavior in a variety of settings, including interstate war. Domestic publics are expected to punish a leader who backs down or performs poorly in war because it signals a general incompetence on the part of the leader. While most do not explicitly identify a leader's level of competence as private information (though see Richards et al 1993), this assumption is implicit in theories that argue war outcomes reveal information about leader competence, which, in turn, affects the likelihood of punishment by domestic audiences.

decisions and poor management of the war effort will, in theory, produce observable unfavorable political and military outcomes, there are a number of factors beyond the leader's competence in executing the war effort that affect outcomes. These exogenous factors, which are beyond the leader's control, may lead to unfavorable developments or poor war outcomes, despite competent leadership decisions. As a result, internal audiences cannot know with certainty whether observed losses are a result of poor leadership or bad luck.

## **2.4 Risk Averse Constituencies**

In line with existing research, I assume that constituencies are, on average, more risk averse than their leaders. This divergence in risk propensities derives primarily from the fact that constituents unequally bear the costs of war, most notably casualties and economic costs, whereas leaders are likely to be largely insulated from these wartime costs (Chapman and Reiter 2004; Croco 2011; Richards et al. 1993). This cost burden differential is particularly acute in modern, interstate wars where the leader is almost never directly involved in battle, but instead sets strategy and policy from the safety of the state capital. It should also, however, characterize the leader-constituent relationship in civil wars. State leaders, as in interstate conflict, generally do not bear direct costs of battle in intrastate war. And while rebel leaders are sometimes less removed from the front lines than their state leader counterparts, in the vast majority of cases, even rebel leaders are more insulated against the costs of battle than the members of the groups they lead. This is particularly true for groups such as the Free Aceh Movement (GAM) in Indonesia, whose top officials led the

rebellion from the relative safety of Sweden, where they lived in exile with their families.

The unequal costs of war therefore affect both state and rebel group leaders' winning coalitions. In large WC states or groups, individual citizens, as members of the winning coalition, bear the costs of fighting both directly, as fighters, and indirectly, through the loss of family members, friends, and neighbors who are injured or killed in battle. They also unequally bear the economic and social costs of war, as they are the first to suffer from the disruption of services and infrastructure caused by war and higher tax burdens – both official state taxes and unofficial rebel extraction – to fund the war effort. Even in small-WC states and groups, a leader's winning coalition members, who often include mid and high-ranking field commanders, will bear greater costs and risks because they are actively engaged on the front lines or in forward bases.

Because constituents bear the brunt of the costs of war, they will be more risk averse than their leaders, and will become increasingly risk averse as costs mount. Thus, as recent research demonstrates, popular support for a war declines as the human costs of that war increase (Gartner 2008). Importantly, the relative risk averseness of constituencies can result in goal conflict between leader and winning coalition. This goal conflict arises when the constituency's aversion to additional costs and the leader's incentive to avoid punishment prescribe different policy paths. The consequences of this goal conflict are discussed in detail in section III.

## 2.5 Adversary Elites

Finally, I assume that adversary elites are also uncertain regarding the opponent leader's type. Opponent elites suffer the same information asymmetries as their adversary's winning coalition with regard to the leader's competence, as they also cannot directly monitor the decisions or behavior of their adversary's leader. This informational disadvantage is greater than that suffered by most winning coalition members, as adversary elites are generally not privy to any of the internal workings of the opponent, unless they have successfully infiltrated the highest levels of the opponent's military-political organization.

In addition to these information asymmetries, I make one additional assumption about adversary elites. Specifically, I assume that adversary elites prefer to face an opponent leader who can credibly commit to peace. While adversary elites will not be interested in pursuing a peaceful resolution to the conflict at all times, I argue that they will prefer to face an opponent who can, should the opportunity arise, be trusted to uphold promises made during negotiations and carry out agreed upon terms of settlement.

The importance of an opponent leader's ability to make credible commitments derives from the costliness of failed settlement attempts and conflict recurrence, and the insecurity it creates for adversary elites themselves (Toft 2009; Walter 2002). Specifically, opponent elites are likely to suffer political costs if they make settlement overtures that are rejected by their adversary's leader, or if they agree to settlement plans that then break down during the implementation phase. Policy failures such as these are likely to damage the opponent leader's reputation for competence among his

or her own constituents. Opponent leaders who pursue a negotiated settlement only to have their adversary reject the process or any resulting agreement are thus likely to have their leadership questioned internally. Multiple examples exist of leaders losing political power as a result of bargaining failures during civil war. Megawati Sukarnoputri of Indonesia, for example, lost elections in 2004 after negotiations with GAM failed to produce results. Similarly, Belisario Betancur's PCC party lost elections in Colombia in 1986 after peace overtures to the country's rebel groups were rejected. In both cases, the *adversary's* inability to commit to a peace deal undermined the leader's political position and resulted in loss of power.

Adversary elites, therefore, should prefer to face an opponent leader who can make credible commitments to peace when the opportunity arises. A leader's hold on power is likely to be threatened by failed settlement attempts or the breakdown of an ongoing peace process, which imposes a variety of social, economic, and political costs that undermine stability and political longevity. The importance of this assumption is discussed further in the context of opponent-based punishment in section IV.

### **III. An Agency Model of Internal Punishment**

Recall the two-part assumption articulated above that leaders have private information about their competence level, and that internal audiences have incentives to punish an incompetent leader. The means by which the internal audience can punish a leader in the political realm, however, are limited. Constituencies have one available constraining mechanism to check the behavior of state or rebel leaders: the

option of removal of the leader (Downs and Rocke 1994).<sup>18</sup> Sanctioning incompetent leaders through removal from office allows the winning coalition to not only improve its prospects in the near term by removing the incompetent leader, but also allows it to incentivize leader behavior that aligns with its interests and to deter leader adventurism by future office holders. The threat of removal is a deterrent to current and future leaders from exploiting the information asymmetry that characterizes the principal-agent relationship and pursuing policies at odds with the interests of the constituency.

Internal audiences will have incentives to differentiate between competent and incompetent leaders before sanctioning or punishing the leader, however, as improper removal of a competent leader is inefficient (Downs and Rocke 1994). In more concrete terms, constituencies should prefer to maintain leadership continuity, particularly during periods of conflict, *if* the leader is able to execute the war effort competently. This continuity ensures greater stability in strategy, lines of communication, and chains of command, thereby minimizing the possibility that disruptions caused by leadership turnover during war could lead to military gains for the opponent. The benefits of continuity, however, are outweighed by the drawbacks of poor strategic decisions when an incompetent leader is in power during civil war, and internal audiences will therefore have strong incentives to distinguish between the two types of leaders.

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<sup>18</sup> The other, more severe, forms of punishment discussed above are also available forms of punishment, but each is precipitated by removal. That is, removal is a necessary precursor to or component of exile, imprisonment, and execution. These options can therefore be thought of as different forms of the same removal sanction available to internal audiences.



### 3.1 Culpability and War Performance: Identifying Incompetent Leaders

The constituency's incentive to maintain a competent leader in office while dismissing an incompetent one, however, raises an important dilemma. Namely, because leaders have private information regarding their type, internal audiences cannot know, with certainty, whether a leader is competent or not.

Economics solves this dilemma by generating complex contractual relationships between principal and agent with contingencies for performance and effort, which increases the principal's ability to weed out low-ability types *ex ante*.<sup>19</sup> This type of contractual complexity, however, is not possible in the political realm where the constituency's only available sanctioning option, leader removal, is a crude, ungraduated instrument that can only be applied *ex post* (Downs and Rocke 1994). Without the ability to weed out incompetent leaders before coming to power, winning coalition members must use cues provided by 1) observable conflict outcomes and 2) the leader's connection to the decision to go to war, to help determine whether the leader is competent or incompetent, and thus whether he is deserving of punishment or not.<sup>20</sup>

The first source of information on a leader's abilities, observed war outcomes, provides preliminary information to constituents. As discussed above, however, the

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<sup>19</sup> These complex contractual relationships also allow the principal to counteract the agent's incentives to act unfaithfully.

<sup>20</sup> Richards et al. (1993) and Downs and Rocke (1994) identify policy/battlefield outcomes as the only available signal constituencies have to judge the leader's competency or faithfulness. As Downs and Rocke note, "high information uncertainty forces a citizenry to gauge by battlefield success or by the apparent consequences of inaction the extent to which an executive is acting in a manner that is consistent with its preferences" (1994, 363). I argue, on the other hand, that the leader's culpability provides a second source of useful information that allows internal audiences to separate competent from incompetent leaders.

relationship between leader decisions and war outcomes is imperfect, as exogenous factors beyond the leader's control also influence the course of events in war. Because internal audiences cannot directly observe either the leader's decisions or the variety of other exogenous variables that affect war outcomes, they can only make probabilistic inferences about the extent to which the leader's decisions led to a specific war outcome. In fact, a leader may make highly competent strategic decisions that directly reflect the constituency's preferences, but which are unsuccessful through no fault of his own. Thus, the relationship between leader decisions/competence and war outcomes is inefficient and at times inaccurate, sometimes leading constituents to draw erroneous conclusions about a leader's type.

The second source of information constituents use to help identify incompetent leaders deserving of punishment relates to the leader's connection to the original decision to go to war. Specifically, constituents will attribute responsibility for a conflict to leaders who were in charge of the state or rebel group at the start of the conflict, and to those with direct political connections to the decision to involve the group in violent conflict. These leaders will be designated as *culpable* for the war by their internal audiences, as they are directly connected to the start of the war and are therefore perceived as responsible. The concept of culpability was first introduced in the interstate conflict literature (Croco 2011). Whereas previous scholarship assumed that constituents had no ability to distinguish among leaders and would have equal incentives to punish any leader who presided over poor war outcomes, Croco's (2011) analysis demonstrates that constituents do, in fact, make

distinctions between leaders who are connected to the decision to go to war and those who are not.

More specifically, a rebel or state leader's culpability is determined by two factors: (1) whether he/she was in charge of the state/group when the war began (i.e. a first leader), and (2) whether a replacement leader shares political connections with the first leader.<sup>21</sup>

Leaders who preside over the start of the conflict (i.e. first leaders) are considered culpable because they hold power at the start of the war and are thus directly responsible for presiding over the transition from peace to war.<sup>22</sup> Rebel and state leaders who come to power during the war *and* share political or familial connections with the first leader inherit the first leader's responsibility for the war by virtue of these political or familial connections. Existing research demonstrates,

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<sup>21</sup> Specifically, replacement state leaders are likely to be considered culpable if they 1) are members of the first leader's political party (democracies), 2) were members of the first leader's cabinet at the start of the war, 3) are family members of the first leader, or 4) were otherwise members of the first leader's inner circle at the war's start. For rebel leaders, culpability is likely to transfer to replacement leaders who are 1) co-founders of the organization, 2) family members of the first leader, or 3) leaders who held high-level positions within the organization at the start of the conflict. Additional information and coding rules for culpability are provided in chapter 3.

<sup>22</sup> First leaders are considered culpable whether or not they actually initiate violence because they bear responsibility for either firing the first shot (initiators) or failing to prevent the dispute from escalating to violent conflict (non-initiator first leaders). A non-initiator first leader's failure to prevent an attack through either a pre-emptive strike or accommodative policies is likely to be viewed as a policy failure, responsibility for which again rests with the leader, thus ensuring that he faces a similar burden of responsibility as one who actually fired the first shot. Furthermore, it can be difficult for citizenry and even elites within the state to identify 'initiators' in civil conflicts, as both sides may have incentives to portray themselves or the opponent as the aggressor, regardless of facts on the ground. Thus, both rebel and state leaders in power at the start of conflict are likely to be assigned responsibility for its initiation. Therefore, attributing responsibility to all first leaders is a theoretically defensible choice in this context.

along these lines, that constituents tend to equate individual leaders and those with whom they share political connections, particularly in attributing blame or responsibility for poor policy outcomes (Cotton 1986; Erikson 1988). Thus, both internal audiences and opponent elites are likely to assign replacement leaders who share political connections with the first leader responsibility for the war with little or no discounting because membership in a first leader's inner circle or political affiliation with that leader implies intimate knowledge of and influence over the decision-making process that led to conflict. Leaders who inherit an ongoing war upon coming to power but who share *no* political or familial ties with the first leader, on the other hand, will likely avoid responsibility for poor war performance. They will be viewed as non-culpable by constituents because they can concretely avoid affiliation with the first leader's war aims and with the original decision to become involved in the conflict.

Winning coalition members, furthermore, will likely be able to make clear distinctions between culpable and non-culpable leaders, making accurate attributions of responsibility, for several reasons. First, the transition from peace to war is generally identifiable, within a reasonable window or timeframe. Winning coalition members will therefore be able to identify the start of the conflict – the point at which rhetoric transitioned to violent clashes – and link that to the leader in power at the time, as well as to other high-ranking members of the government or rebel organization at the time. This is particularly likely given that winning coalitions are often relatively small in organizations and states that fight civil wars. While information asymmetries on some issues may be exacerbated in these regimes,

winning coalition members should be generally well-informed on basic characteristics of the organization – such as who holds political power and who makes up the regime’s insiders at any given time – and on whether the group is engaged in violent conflict or not. Further, even in large-WC regimes, citizens should be relatively well-informed about a leader’s connection to the decision to go to war because political opponents of the leader have incentives to highlight his connection to the war if the war is going poorly, while the leader himself may want to highlight his role in initiating the conflict if it is going well.

Internal audiences should therefore be relatively adept at attributing responsibility for a conflict to culpable leaders, while sparing non-culpable leaders. Identifying culpable leaders then helps constituents distinguish low from high-quality agents by linking responsibility for war outcomes to leaders viewed as responsible for the conflict. Cues provided by the leader’s connection to the start of the war (i.e. his culpability) help internal audiences distinguish leaders whose bad choices *caused* war losses from those who made the right policy choices but were unable to achieve victory due to the poor decisions of previous leaders.<sup>23</sup>

Specifically, the culpable leader’s clear association with the decision to go to war means that, when confronted with major war losses, internal audience members will conclude that the culpable leader’s decision to go to war was a poor one, or that his execution of the war effort was deficient. The clear association between the

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<sup>23</sup> Existing models in the interstate war literature either assume that constituencies have only the war’s outcome as a tool to identify unfaithful agents (Downs and Rocke 1994; Richards et al. 1993), or that regime type influences the likelihood and extent of punishment (Goemans 2000). Croco (2011) recognizes that leader culpability influences the likelihood of punishment, though does not identify this as a tool to facilitate identifying unfaithful agents within a principal-agent framework.

culpable leader and responsibility for the decision to initiate conflict, coupled with the poor conflict outcome, signals to internal audience members that the leader is likely a low-ability type. Non-culpable state and rebel leaders, on the other hand, can distance themselves from the original decision to engage in conflict. War losses are therefore less likely to be viewed as their personal policy failures; the non-culpable leader can attribute poor outcomes to the poor decisions of his culpable predecessor(s), thereby avoiding a similar low-ability designation.

### **3.2 Internal Punishment and the Gamble for Resurrection**

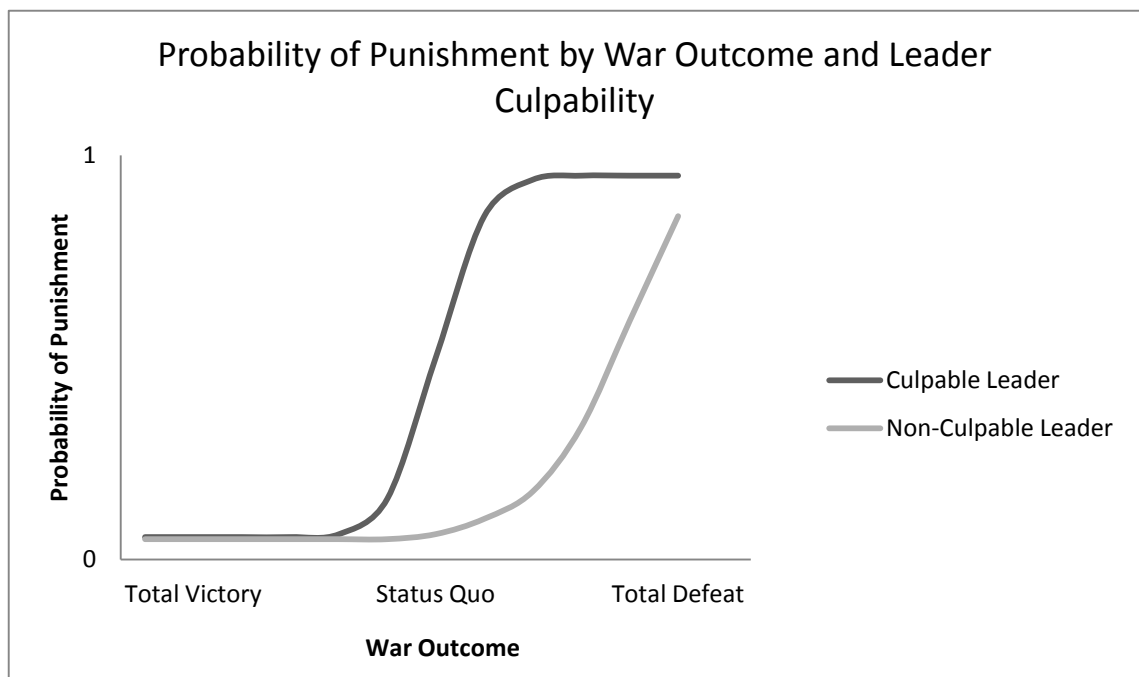
Internal audiences' use of responsibility for the war as a cue to help identify incompetent leaders has far-reaching implications for leaders' expectations of punishment and their subsequent conflict behavior. Culpable leaders who preside over poor war outcomes are more likely to be viewed as low-ability types than non-culpable leaders who preside over similarly poor outcomes. Recall from section 2.3 that constituents prefer a competent leader, as leader competence is expected to be generate better policy outcomes. This suggests that culpable leaders are more likely to be punished for poor war outcomes than their non-culpable counterparts because internal audiences attribute them responsibility for poor conflict outcomes (i.e. judge them as incompetent) while disassociating non-culpable leaders from those poor outcomes. Culpable leaders who preside over poor war outcomes are therefore likely to have a higher expectation of punishment from internal audiences than non-culpable leaders who perform similarly poorly in war. High-ranking members of Palipehutu-FNL, for example, removed FNL founder and culpable leader Kossan Kabura in

February 2001 when rumors of secret negotiations with the government surfaced.

Several years later, his non-culpable successor successfully settled with the government on similar compromise terms, without being punished as a result.

The relationship between culpability and punishment is depicted graphically in Figure 2.1. Neither culpable nor non-culpable leaders will fear punishment when they achieve highly favorable war outcomes, as they will have satisfied internal audiences and achieved either a total or a major victory over the opponent. Culpable leaders, however, are much more likely than non-culpable leaders to face punishment following moderately to severely unfavorable war outcomes. Under these conditions, culpable leaders will be designated incompetent by internal audience members, and they will be left vulnerable to punishment as the removal sanction is implemented.

**Figure 2.1 Probability of Punishment by War Outcome and Leader Culpability**



This suggests that variation in the expectation of punishment for culpable versus non-culpable leaders drives variation in their expected utilities for termination. Responsibility for the war locks culpable leaders into a particular course of action by making compromise and concessions costly, as even moderate losses may earn them the ‘incompetent’ designation. Changing course to a more conciliatory strategy for a culpable leader essentially involves reneging on promises made at the start of the conflict, signals low-ability type, and damages the leader’s ability to bargain effectively in the future. It thus lowers the utility of termination for anything less than a victory, as victory is the only way for a culpable leader to ensure his post-conflict political and physical security.

**Figure 2.2 Utility of Termination by War Outcome and Leader Culpability**

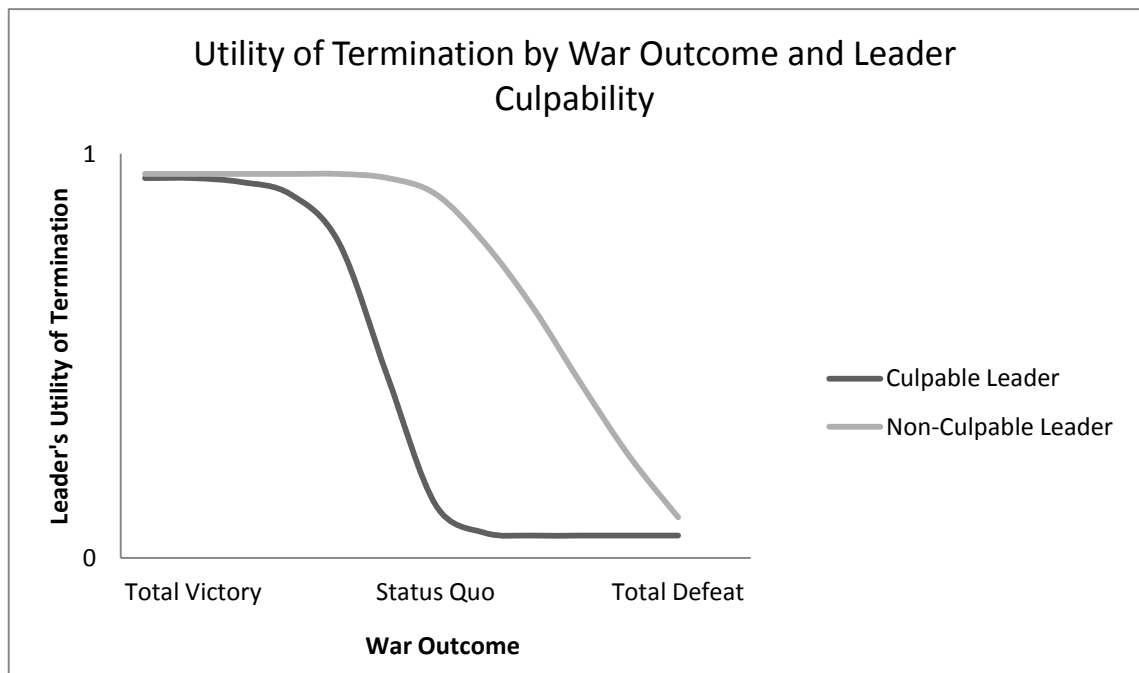




Figure 2.2 depicts the relationship between war outcome and the utility of termination for culpable versus non-culpable leaders, demonstrating that culpable leaders have a much lower utility of settlement for status quo and moderately losing outcomes than their non-culpable counterparts.

### *Incentives to Gamble for Resurrection*

Given this relationship between culpability and the utility for termination, a culpable leader will have incentives to continue a losing war rather than settle on unfavorable terms, as he will anticipate punishment for his role in the conflict and his utility for termination will be lower than his utility for fighting. Continuing the fight provides the culpable leader the opportunity, however remote, to turn the tide of the conflict and achieve a favorable outcome to forestall punishment. In other words, the expectation of punishment creates perverse incentives for culpable leaders to become unfaithful agents and ‘gamble for resurrection’ (Downs and Rocke 1994; Goemans 2000) rather than settle on losing terms that leave them vulnerable to punishment.

This is the heart of the principal-agent problem in civil conflict. Recall from section II that leaders are more risk-acceptant than their internal audiences. Because constituents are more sensitive to the costs of conflict than their leaders, they will prefer to terminate an ongoing conflict when the prospects of victory decline. The leader’s primary goal, on the other hand, is to retain political power and avoid punishment. These divergent preferences do not automatically result in unfaithful leader behavior; a non-culpable leader, because his expectation of punishment is low, can terminate a war on compromise or moderately losing terms, in line with his

internal audiences' preference to end a costly war when the prospects of victory dwindle.

Incentive problems arise, however, when culpable leaders are in power. A culpable leader's high expectation of punishment for poor war performance generates incentives for him to stay in a losing war, hoping to turn the tide to avoid punishment.<sup>24</sup> This conflicts, however, with his internal audience's preference to terminate a losing war. Because acting faithfully in these circumstances ensures the culpable leader faces internal punishment, his incentive is to instead act as an unfaithful agent.

The logic of this strategy is straightforward. Culpable leaders update their beliefs about the likely outcome of the war as new information comes in from the battlefield and the negotiating table. As the likelihood of a favorable outcome decreases, the culpable leader becomes assured of punishment, as his perceived responsibility for the war, in combination with the expected poor war outcome, will lead internal audiences to conclude that he is incompetent. Having passed the threshold of near certain punishment, the culpable leader has nothing to lose by

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<sup>24</sup> Research in the criminology literature suggests that the threat of punishment impacts behavior – in this case the decision to terminate or continue a war – when it meets the following three requirements: it is severe, certain, and swift (Bailey and Smith 1972; Howe and Brandau 1988; Howe and Loftus 1996; Paternoster 1987; Yu 1994). Arguably, each of these conditions is met with regard to the threat of leader punishment during civil war. First, the forms of punishment a leader faces during civil war are severe – even loss of political office, the least severe form – is a relatively serious consequence of failed policies. Second, the prospects of punishment are relatively certain once a culpable leader's prospects for victory decline past a certain threshold. Finally, punishment of civil war leaders often takes place through irregular means and does not occur at specified times through regulated elections or the like. This ensures that some level of imminent threat is always felt by leaders involved in civil war, thereby fulfilling the third and final requirement for the threat of punishment to influence leader behavior.

continuing the fight in the hope of turning the tide and achieving victory. While achieving victory may not be likely, the probability of a favorable outcome is greater than zero if the culpable leader continues the fight, making his probability of punishment less than one. Should he settle on losing terms as the likelihood of victory diminishes, on the other hand, the culpable leader's probability of victory decreases to zero, which ensures punishment with probability one. Thus, the higher variance, higher risk strategy of continuing the fight yields the culpable leader a higher utility than termination on unfavorable terms.<sup>25</sup> Because the attribution of blame for war losses is less likely to pass to non-culpable leaders, they will not face the same incentive to gamble for resurrection when faced with the probability of compromise or moderately unfavorable war outcomes.<sup>26</sup>

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<sup>25</sup> The theory also implies that leaders process information revealed through conflict differently depending upon their individual expected payoffs for continuing versus terminating a conflict. Battlefield gains and losses reveal information about each side's relative strength and resolve and thus the likely outcome of the conflict. That information has different implications, however, depending upon the leader's vulnerability to punishment. Culpable leaders, faced with evidence that they will likely lose the conflict, are likely to increase their war aims and continue the fight. Non-culpable leaders, in contrast, will seek and accept compromise settlements. UNITA leader Paulo Lukamba Gato provides a prime example; as a non-culpable leader, he quickly agreed to a settlement that had been rejected by culpable leader Jonas Savimbi when faced with advancing Angolan government troops.

<sup>26</sup> It may be the case that the behavior I attribute to culpability and incentives to gamble for resurrection is instead a result of leaders who are 'true believers' in the cause and are therefore unwilling to concede. I argue that culpability provides a better explanation for the observed patterns of behavior, however, because it provides a better explanation for why even some hard-liners, upon coming to power, choose to settle on compromise terms. Paulo Gato of UNITA, for example, was considered a militant member of the organization, consistently favoring the military option against UNITA as a member of the group's Political Bureau. Upon taking the leadership role in the organization, however, Gato – a non-culpable leader – agreed to a compromise settlement which he had not advocated previously. The differential in expectations of punishment between culpable and non-culpable leader provides a more convincing

Thus, internal audience members cannot ensure that culpable rebel and state leaders carry out their duties faithfully (i.e. pursue the group's interests rather than their own), due to their inability to monitor the leader's behavior and apply real-time sanctions, because goal conflict between leaders and constituents generates incentives for the culpable leader to take advantage of his informational advantage and pursue his own self interest at the expense of the group he represents (Downs and Rocke 1994; Feaver 2005; Leventhal 1988; Tirole 1988).

#### **IV. Culpability, Gambling for Resurrection, and Opponent-Based Punishment**

The theoretical discussion thus far has focused on a leader's incentives to avoid *internal* punishment – removal at the hands of his winning coalition. During civil war, however, leaders also face an often severe threat of punishment by the adversary. This opponent-inflicted punishment occurs when the wartime adversary, rather than the internal audience, punishes the leader. This type of punishment is thus at least implicitly sanctioned or at most explicitly ordered by the adversary's military or political leadership.

Internal conflicts are characterized by shared national territory, over which a single political system and legal structure extends. Consequently, during and at the conclusion of the conflict, unless it ends in secession by the rebel group and partition of the state, the combatant groups share a single national territory and fall under the jurisdiction of a single sovereign political/legal system. This makes punishment by

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explanation for why wartime hawks such as Gato change course, taking a conciliatory stance, once taking over the group's leadership.

the opponent a much more common occurrence in civil conflict than in the interstate context, where opponent punishment is generally only precipitated by total defeat and occupation.<sup>27</sup> Opponent, or adversary, based punishment is thus a more important consideration for leaders engaged in civil conflict than those fighting internationally. The logic of adversary-inflicted punishment is discussed in this section.

As argued above, leaders who are viewed as responsible for the war *and* who perform poorly in the conflict are likely to be judged as incompetent by their winning coalitions. Because of their culpability, these losing leaders have incentives to continue the fight in order to avoid internal punishment. This phenomenon is called gambling for resurrection, and is driven by the culpable leader's personal incentives.

Importantly, these incentives to gamble for resurrection internally exacerbate commitment problems externally (i.e. with the adversary). Credible commitments to peace are notoriously difficult to make in the context of civil war; conflict recurrence is a common and serious problem for states as they emerge from war (Fortna 2004; Mason et al. 2011; Quinn, Mason, and Gurses 2007; Toft 2009; Walter 2004). The consolidation of peace is fraught with difficulties and challenges, not least of which is the threat that the defeated group will reorganize, regroup, and re-launch its violent challenge. The difficulties associated with agreeing to and implementing settlement agreements that successfully terminate conflict arise because steps taken toward peace – specifically demobilization and reintegration – are inherently threatening to the actor taking these steps. Every move toward peace renders the group less-able to

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<sup>27</sup> Because of the rarity of opponent punishment in the interstate context, this type of punishment is not theorized in the interstate war literature.

defend itself should the opponent decide to attack. This is the classic commitment problem as articulated in the civil conflict literature (Walter 2002).

While existing literature conceptualizes commitment problems as a group-level phenomenon, I argue that leader-specific incentives are an important source of variation in the severity of commitment problems that existing literature largely ignores. Specifically, a culpable leader's incentives to gamble for resurrection, deriving from the threat of internal punishment, make him less able to commit to ending the war or to implementing any agreed upon settlement deal. Similar to group-level insecurities, leader-specific vulnerabilities to punishment will make it difficult for a culpable leader to commit to a process which threatens his political or physical survival. Thus in the current Syrian crisis, for example, international peace proposals that envisage a power transition in which Bashar al-Assad steps down from power without any guarantees for his or his family's personal safety have been continually rejected by the Syrian head of state. Threats of international criminal prosecution or exile, furthermore, only serve to decrease the likelihood that Assad commits to any sort of compromise settlement, as these threats further undermine his prospects for continued power and security.

Recognizing that the culpable leader is unlikely to be able to commit to ending the war because of his personal vulnerability to punishment, adversary elites will have strong incentives to try to remove him from power in the hope that a non-culpable leader takes his place.<sup>28</sup> These incentives follow from the assumption,

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<sup>28</sup> Recent research on leadership decapitation in the counterinsurgency and terrorism literatures provides strong evidence that removing and otherwise punishing adversary leaders is a common component of many states' counterinsurgency strategies (Carvin

outlined earlier, that adversary elites prefer to face an opponent leader who can make credible commitments to peace, and from the expectation that war will be more costly, longer, and more likely to end in defeat when a culpable adversary leader remains in power.

It follows that the counter-productive consequence of internal incentives for culpable leaders to gamble for resurrection is a worsening of the threat of adversary-based punishment. Because the threat of internal punishment increases the leader's incentives to continue the fight, it simultaneously increases the probability that the opponent will seek to punish the culpable leader in order to facilitate termination and deter conflict recurrence. This, in turn, increases the culpable leader's vulnerability, reinforcing his incentive to continue the fight in the hope of achieving victory or extracting personal security guarantees to ensure his personal safety. Because non-culpable leaders have fewer internal incentives to renege on settlement deals or re-launch the conflict, they will also face a lower expectation of punishment from the adversary. Non-culpable leaders will be viewed as more trustworthy negotiating partners, and the adversary will be more willing to envisage a post-conflict scenario in which the non-culpable leader remains an important domestic political figure. The non-culpable leader's distance from the original decision to go to war affords him greater flexibility to negotiate and settle with the opponent on compromise terms

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2012; Johnston 2012; Price 2012). These analyses, however, do not examine variation in the likelihood of a state using this strategy. This theory has important implications for these studies' initial findings, suggesting that combatants will have greater incentives to attempt decapitation strategies when the adversary's leader is culpable.

without the imminent threat of exile, imprisonment, or assassination at the hands of the adversary.

The logic of insecurity due to the threat of adversary-based punishment played out in Angola in the mid 1990s. UNITA founder and leader Jonas Savimbi's personal vulnerabilities played a large role in stalling the implementation processes of both the 1991 Bicesse Accord and the 1994 Lusaka Protocol designed to end the 20 year conflict in the country. Savimbi refused to take up his position in the Angolan government and stayed away from Luanda, out of fear that he would be assassinated or imprisoned should he enter the capital. Savimbi's inability to commit to these peace processes out of personal security fears ultimately spelled disaster for both agreements, and in each case, the country returned to war shortly after the settlement failed.

## **V. Empirical Implications**

The theory developed above argues that culpable leaders will anticipate a higher likelihood of punishment from both internal and opponent-based sources than non-culpable leaders when they experience poor war outcomes. Recognizing this, culpable leaders will respond to battlefield and negotiating table information differently than their non-culpable counterparts.

These varying incentive structures for culpable and non-culpable leaders have important implications for war termination and outcomes. As they update their expectations regarding the likely outcome of the conflict with new battlefield information, culpable leaders will become more likely to extend a losing war, while



non-culpable leaders will be more willing to terminate on compromise or moderately losing terms. That is, culpable leaders, when faced with the likelihood of a loss and subsequent punishment, will adopt the high-variance strategy of continued conflict rather than the low-uncertainty strategy of settlement. Continued fighting buys the culpable leader time to attempt to turn the tide of war, achieve a favorable outcome, and avoid punishment. This logic suggests the following hypothesis:

**Hypothesis 1:** Civil wars are less likely to terminate when culpable leaders are in power.

Because culpable leaders have incentives to redouble the war effort when faced with possible defeat, culpability is also likely to affect civil war outcomes. Specifically, culpable leaders whose gambles pay off – those who manage to hold their ground and make inroads against the adversary – will achieve major victories more often than their non-culpable counterparts, who would have settled on compromise or moderately losing terms when the hope of victory initially diminished.

The decision to gamble for resurrection, however, will not always pay off. Having stronger incentives to win does not mean that culpable leaders will always be able to do so. While all culpable leaders have incentives to try to achieve total victory, other factors such as relative strength, resolve, and the ability to avoid costs will affect which gambles actually succeed. Leaders who pass up settlement offers only to lose additional battles, territory, or troops make their groups worse off than they would have been had the leader agreed to a settlement when in a militarily

stronger position. This suggests that culpable leaders will ultimately preside over more major victories *and* more major defeats than non-culpable leaders.

Non-culpable leaders, on the other hand, have a significantly lower expectation of punishment, as they can distance themselves from much of the responsibility and blame for the war, and thus from charges of incompetent execution of the war effort. This provides non-culpable leaders more freedom to negotiate and compromise as the likelihood of victory diminishes. The following hypothesis follows from this logic:

**Hypothesis 2:** Culpable leaders are more likely to experience extreme war outcomes than their non-culpable counterparts.

Extending this logic suggests that a leader's culpability will affect the likelihood of concessions at termination as well. Specifically, a culpable leader is expected to be more constrained with regard to his ability to make concessions to end the war than a non-culpable leader. Because of their association with and responsibility for the war, culpable leaders' fates are tied closely to the goals they establish at the conflict's start. Any movement toward compromising on central war aims will be viewed as evidence by internal audiences of the leader's incompetent execution of the war effort, due to his close association with the goals established at the start of the war, and will increase the likelihood of punishment.

Non-culpable leaders, on the other hand, can distance themselves from the first leader's original war aims. This distance allows them to more easily

compromise on central war aims without being viewed as an incompetent leader by internal audiences. This reduces the threat of internal punishment, while also mitigating fears of adversary-based punishment for the leader who can avoid responsibility for the war. This suggests the following hypothesis:

**Hypothesis 3:** Culpable leaders are less likely to make concessions with regard to central war aims than non-culpable leaders.

Finally, the theory presented above makes clear predictions regarding the mechanism underlying the relationship between leader culpability and war termination and outcome. Specifically, the threat of punishment is expected to drive culpable leaders' wartime decision-making, and thus the duration and outcome of conflict. Culpable leaders are expected to anticipate a higher likelihood of punishment from both internal and opponent-based sources than non-culpable leaders when they perform poorly in war, as they are viewed as responsible for the decision to go to war, and thus for any setbacks suffered during the conflict. This proposition, underlying the relationship between culpability and war termination/outcomes, yields the final hypothesis:

**Hypothesis 4:** Culpable leaders are more likely to be punished following unfavorable war performance than their non-culpable counterparts.

## **VI. Conclusion**

The goal of this chapter was to develop a theory of civil war termination and outcome that derives from relaxing the unitary actor assumption generally imposed in studies of civil conflict. Drawing upon principal-agent framework and commitment problem theories of civil conflict termination, I presented a theory of civil war termination and outcome focused on the incentives of rebel and state leaders. I argued that the threat of punishment from two sources – internal audiences and adversary elites – affects a leader's utility for termination and therefore the war outcomes he achieves. Constituents use information provided by war outcomes and the leader's culpability for the war to determine whether a leader is competent. Culpable leaders are more likely to be perceived as incompetent should they suffer war losses, and will therefore have a heightened expectation of punishment.

This suggests that culpable leaders will have incentives to gamble for resurrection when the war is going poorly, extending and escalating the conflict in the hope of achieving victory and avoiding punishment. Non-culpable leaders, on the other hand, will have little incentive to extend a losing war because they are less likely to be branded incompetent due to poor war performance, and therefore have a lower expectation of punishment.

The threat of punishment by the opponent also affects leaders' wartime behavior. Adversary elites have incentives to punish their opponent's leader if that leader is unable to make credible commitments to peace. Culpable leaders who preside over poor war outcomes will suffer particularly severe commitment problems because of their incentives to gamble for resurrection internally. Thus, the heightened

threat of adversary-based punishment further reinforces the culpable leader's incentives to continue the fight in the hope of achieving victory and avoiding punishment from both internal and opponent-based sources.

I hypothesized, based on this theoretical argument, that culpable leaders will be less likely to terminate an ongoing war and more likely to preside over extreme war outcomes than their non-culpable counterparts. I also hypothesized that they will be less likely to make concessions at termination than non-culpable leaders. Finally, based upon the underlying logic of the argument, I hypothesized that culpable leaders will be more likely to face punishment for poor war outcomes than non-culpable leaders who perform similarly poorly in civil war. These hypotheses are tested in the next two chapters. Additional hypotheses regarding culpability's potential conditionality on regime type are developed and tested in Appendix B.

## **Chapter 3 : Statistical Analysis of Leader Culpability's Impact on Civil War Duration and Outcome**

### **I. Introduction**

The preceding chapter presented a theory of civil war termination and outcome that identified state and rebel leaders' incentives to avoid punishment as a central factor influencing each warring party's termination calculus. It built upon principal-agent framework and commitment problem theories of civil conflict, arguing that internal audiences and opponent elites are more likely to punish culpable leaders who perform poorly in war than non-culpable leaders whose war performance is similarly poor. This higher expectation of punishment generates perverse incentives for culpable leaders to 'gamble for resurrection', extending a losing war and striving for total victory in order to ensure his own political survival and to eliminate the threat of internal and opposition punishment.

Responsibility for the conflict thus systematically alters a leader's anticipated payoffs for termination on favorable versus unfavorable terms. This, in turn, alters the likelihood of termination as well as the likely outcome achieved by each actor. This chapter presents the research design and data collected to test the hypotheses on civil war termination and outcome developed in the previous chapter, and presents the results of statistical tests.

The empirical analysis presented below demonstrates that factors affecting a leader's anticipation of punishment following poor war performance – specifically the

leader's culpability for the war – critically influence the likelihood of war termination/settlement and the type of outcome the leader is willing to accept. Because of their increased risk of both internal and opponent-based punishment, culpable leaders fight longer than their non-culpable counterparts. They also preside over more extreme outcomes than their non-culpable counterparts, and are less likely to make concessions at termination than non-culpable leaders.

This chapter proceeds as follows. First, I describe the new dataset that was designed and collected, based on original research, for this project. I discuss the coding of the dependent and independent variables in the next sections, describing limitations in existing war outcome coding schemes and how this new dataset overcomes those limitations. Next, I present the results of the main statistical analyses, which provide strong statistical support for the theoretical expectations described in Chapter 2. The fourth section presents the results of a variety of robustness checks using alternative variable specifications, alternative modeling strategies, and tests to account for the potential endogeneity of leader culpability. These robustness checks provide additional evidence in support of the relationship between culpability and war termination/outcomes, and demonstrate that the statistical results are not sensitive to a variety of specification changes. The final section discusses the implications of these results and remaining empirical questions. Statistical tests of the punishment mechanism developed in the previous chapter are left to Chapter 4.

## **II. Dataset and Research Design**

### **2.1 Hypotheses**

The previous chapter laid out three primary hypotheses regarding the impact of a leader's responsibility for the war on its termination and outcome. To review, these hypotheses are the following:

Hypothesis 1: Civil wars are less likely to terminate when culpable leaders are in power.

Hypothesis 2: Culpable leaders are more likely to experience extreme war outcomes than their non-culpable counterparts.

Hypothesis 3: Culpable leaders are less likely to make concessions with regard to central war aims than non-culpable leaders.

### **2.2 The Dataset**

This section discusses the original dataset collected for this dissertation, the operationalization of key variables, and research design. Several innovations in research design and measurement of variables were necessary in order to test the hypotheses developed above. These innovations are discussed in this section.



### *New Dataset of Civil War Leaders*

Based on original research, I developed a new dataset on civil war leaders specifically for this dissertation. The dataset includes information on all state and rebel leaders for a random sample of 102 warring dyads, representing 60 unique civil conflicts, between 1980 and 2010.<sup>29</sup> This random sample was selected from the Uppsala Conflict Data Program's (UCDP) Dyadic Dataset of armed conflicts (Themnér and Wallensteen 2012). It includes both low-intensity (25 to 999 battle deaths per year) and high-intensity (at least 1,000 battle deaths per year) conflicts, and includes conflicts from all regions of the world. The regional distribution of cases is presented in the first column of Table 3.1 below. As Table 3.1 demonstrates, Africa is strongly represented, comprising approximately half of the dyads included in the dataset. Asian civil wars are the next most common in the dataset, representing approximately 20 percent of the conflict dyads in the dataset. The remaining regions comprise approximately 10 percent of the dataset each. While at first glance this case distribution suggests that Africa and Asia may be over-represented in the dataset relative to other regions, a comparison with the UCDP dyadic armed conflict dataset from the post-1980 period indicates otherwise. The distribution of civil conflict dyads for the same time period in the UCDP data, presented in the second column of Table 3.1, closely mirrors the case distribution in the dataset developed for this project.

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<sup>29</sup> Civil wars ongoing at any time between 1980 and 2010 were eligible for inclusion in the sample. Some conflicts selected were ongoing as of 1980. For these cases, information from the start of the conflict is included. Several dyads experience more than one conflict episode; 120 distinct dyad-conflict-episodes are included in the dataset.

**Table 3.1 Distribution of Cases by Region, UCDP Comparison**

<b>Region</b>	<b>New Dataset</b>	<b>UCDP Dyadic Data</b>
	<b>Frequency (%)</b>	<b>Frequency (%)</b>
Americas	8 (7.8%)	22 (7.5%)
Europe	12 (11.8%)	29 (9.9%)
Africa	52 (50.9%)	142 (48.3%)
Middle East	9 (8.8%)	31 (10.5%)
Asia	21 (20.6%)	70 (23.8%)
Total	102	294

Testing hypotheses on the impact of leaders' characteristics on war outcomes requires a data structure that treats the leader as the unit of analysis. While studies of international conflict have used this data structure in the past, to my knowledge, no cross-national quantitative analysis of civil conflict has undertaken an analysis in which the leader is the unit of analysis. It was therefore necessary to collect original data to identify each state and rebel leader involved in each of the conflicts included in the sample. Existing data sources provide information on state leaders (Goemans, Gleditsch, and Chiozza 2009), but no comprehensive, cross-national dataset on rebel leaders exists.

To test the hypotheses developed in chapter 2, therefore, I collected original data on all leaders of each rebel group included in the sample. To identify rebel leaders, I emphasized power over title: those individuals who exert final decision-making authority over broad group policy were identified as rebel leaders. In cases where rebel groups included both political and military wings, research was done to identify which branch of the organization held more power. In some cases, the locus of power within a rebel organization shifts over time. Thus with the Provisional Irish Republican Army (PIRA), for example, the locus of power within the organization

seems to have shifted by the early 1980s from the Dublin-based Army Council to the Northern Ireland branch of the organization headed by Gerry Adams and affiliated closely with Sinn Féin, the political wing of the organization. A variety of sources were consulted to identify rebel leaders, their post-conflict fates, their relationships to first leaders, and their roles in the organization throughout each conflict.<sup>30</sup>

Data on state leaders was adapted from an existing data source, the ARCHIGOS dataset (Goemans, Gleditsch, and Chiozza 2009), which provides information on all state leaders from 1875-2004. ARCHIGOS defines ‘leader’ by emphasizing power over title; figureheads are excluded in favor of individuals who exert real authority over strategy decisions and bear ultimate responsibility for state policy. As such, this dataset corresponds well with the criteria established to identify rebel leaders. Because these data are updated only through 2004, additional research was done to identify leaders in power for those conflicts in the dataset ongoing between 2005 and 2010.

The result of this research is an original dataset in which the civil conflict leader is the unit of analysis. The dataset includes all state and rebel leaders of the randomly sampled civil war dyads between 1980 and 2010, a total of 479 leaders. Of these, 286 (59.71%) are state leaders and 193 (40.29%) are rebel leaders. This indicates that turnover in state leadership is somewhat more common during intrastate

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<sup>30</sup> A sample of sources consulted includes: 1) news databases such as Factiva, Lexis Nexis, and Keesings, 2) secondary sources such as books on specific conflicts or groups (Denov 2010; Martinez 2000; O’Brien 1999), 3) encyclopedic sources on rebellion and terrorism (Mulaj 2010; Szajkowski 2004), 4) online databases such as the UCDP Database and the START Center Database, 5) a variety of primary sources, including UN and government documents, as well as manifestoes, press releases, and other available documents from the rebel groups themselves. Specific sources used are cited in the bibliography.

conflict than is rebel leader turnover, but that turnover is fairly common among both states and rebel groups involved in civil conflict. Indeed, state parties to conflict average 2.4 leaders per conflict, while rebels average 1.6 leaders per conflict. There is significant variation across conflict actors in wartime leadership volatility. While approximately 58 percent of the rebel and state actors in the dataset experience no leadership changes during the course of the conflict, thus having only one leader, the 42 percent of states/groups that do experience leadership change vary in their number of war-time leaders from 2 to twelve. Table 3.2 provides descriptive information on leader volatility for the war combatants included in the dataset.

**Table 3.2 Leadership Volatility during Civil War**

Position of Wartime Leader	All Leaders Frequency (%)	State Leaders Frequency (%)	Rebel Leaders Frequency (%)
First	240 (50%)	120 (42%)	120 (62%)
Second	101 (21%)	57 (20%)	44 (23%)
Third	53 (11%)	36 (13%)	17 (9%)
Fourth	29 (6%)	24 (8%)	5 (3%)
Fifth	17 (4%)	15 (5%)	2 (1%)
Sixth	11 (2%)	9 (3%)	2 (1%)
Seventh	8 (1.7%)	6 (2%)	2 (1%)
Eighth	6 (1.3%)	5 (1.8%)	1 (<1%)
Ninth	5 (1%)	5 (1.8%)	
Tenth	3 (<1%)	3 (1%)	
Eleventh	3 (<1%)	3 (1%)	
Twelfth	3 (<1%)	3 (1%)	
Total	479 (100%)	286 (59.7%)	193 (40.3%)

### *Research Design for War Duration and Outcome Analyses*

Two different research designs are used in the analysis of war duration and outcome. To test the effects of leader culpability on civil conflict duration, I use the

newly collected data on leaders to create a dataset in which the conflict-dyad-year is the unit of analysis. Information on leader culpability is thus incorporated at the dyad level (i.e. one or both leaders can be coded as culpable in any given dyad year). The coding of culpability is discussed further in section 2.4 below. The termination-leader is the unit of analysis in the tests of hypotheses 2 and 3 on conflict outcomes and concessions. Specifically, each leader that is in power at the end of a given conflict is included in the analysis. Leaders not in power at the end of the war are excluded.<sup>31</sup>

## **2.3 Dependent Variables: War Termination, Outcome, and Concessions**

### *War Termination*

The dependent variable used to test the impact of leader culpability on war duration is a dummy variable coded one in the year a given conflict ends. The variable takes on a value of zero for all ongoing conflict years. Of the 120 dyadic conflict episodes included in the dataset, 14 are ongoing as of 2010, the final observation year included in the dataset. One hundred and six terminations, therefore, are coded in the dataset, representing approximately 10 percent of the observation years in the dyad-episode-year dataset used for the war duration analysis (see Table 3.3 below). The conflicts range in duration from one year to 48 years, with the average civil war in the dataset lasting 8.65 years.

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<sup>31</sup> These non-termination leaders are included, however, in the analysis of leader punishment in Chapter 4.

**Table 3.3 War Termination DV Descriptive Statistics**

DV Value	Frequency (%)
Ongoing (0)	932 (89.8%)
Terminated (1)	106 (10.2%)
Total Observation Years	1038

### *War Outcome in Existing Literature*

A survey of the quantitative literature on civil war outcome and recurrence reveals a largely consistent tendency: empirical analyses in this field generally focuses on the determinants of negotiated settlement, settlement versus victory, or the differences among negotiated settlement, government victory, and rebel victory (Brandt et al. 2008; Mason and Fett 1996; Mason, Weingarten, and Fett 1999; Nilsson 2010; Walter 1997). Others build upon this basic categorization, adding additional categories such as truce and treaty (DeRouen and Sobek 2004), low activity (D. E. Cunningham, Gleditsch, and Salehyan 2009; Kreutz 2010), and ceasefire/stalemate (Mukherjee 2006; Toft 2009). While the number and type of categories used to classify civil war outcomes vary, the basic trend is consistent: civil war outcomes are defined (1) at the conflict or dyad level and (2) relative to the opponent, rather than relative to the warring actor's own aims.

These existing coding schemes are not appropriate for the current project for two reasons, one conceptual and one methodological. First on theoretical grounds, the theory developed in Chapter 2 suggests that leaders will be unwilling to settle on terms that leave them open to punishment from internal or opponent-based audiences. Testing this proposition requires identifying the level of favorability of a war outcome for each actor (state and rebel) involved. While broad categories such as victory,

defeat, and negotiated settlement provide some preliminary information on favorability (i.e. a defeat is clearly unfavorable, while a victory is highly favorable and a settlement is often somewhere in between), these broad groupings mask important variations in favorability that may critically affect a rebel or state leader's expectation of punishment.<sup>32</sup>

In particular, conflicts ending in negotiated settlement, low activity, or stalemate/ceasefire show significant variation within each of these categories with regard to their level of favorability. Mauritania, for example, signed a peace agreement with POLISARIO in 1979, ending its war in the Western Sahara. In the agreement, Mauritania gave up all claims to Western Sahara, recognized Polisario as the representative of the Saharawi people, and agreed to withdraw all troops from the disputed territory. While both Mauritania and Polisario achieved an outcome of 'negotiated settlement' in this conflict dyad, the agreement was highly favorable to Polisario, while Mauritania essentially capitulated on all major issues.

This high level of variation is also apparent among conflicts identified as ending through 'low activity'. As Cunningham, Gleditsch, and Salehyan (2009, 587) acknowledge, "there can be real ambiguities as to whether an outcome is 'favorable' or not to a faction, particularly for conflicts ending in 'low activity', without a formal agreement or a clear victory. Governments may be content with a rebel organization

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<sup>32</sup> Fearon and Laitin (2008) identify a related limitation, arguing that wars ending in military victory can have aspects of negotiated settlement, while those ending in settlement may also be characterized by a lopsided military outcome. Further, coding outcomes across different types of conflict (secessionist versus those for central control) introduces additional complications. A government defeat, for example, means different things if the rebels are fighting for secession versus government overthrow (Fearon and Laitin 2008).

that ceases to engage in violence without a decisive defeat. Likewise, a rebel organization allowed de facto control over territory in the periphery without challenges from the government may be quite satisfied with a low activity outcome.” In other words, low activity might be coded for a case in which rebels are largely defeated and on the run, while the same outcome can be coded for a case in which rebel gains on the battlefield have caused the state to give up the fight.

In addition to these conceptual limitations, existing coding schemes are ill-suited for use in the current project on methodological grounds. Because existing data define outcomes for each actor based upon how it fares relative to its opponent rather than its own war aims, the outcomes coded for individual actors involved in the same conflict are not independent. For example, in existing datasets, a government victory necessitates a rebel defeat and vice versa, while a negotiated settlement or low activity outcome for one side is automatically coded the same for the other side. This is not problematic empirically for studies that treat the conflict, dyad, or country as the unit of analysis, as they include only one observation per conflict/dyad. The analysis of war outcomes in the current project, however, uses the combatant leader as the unit of analysis. It therefore includes two observations (one state and one rebel) for each warring dyad at termination. Using existing data to code war outcomes would therefore introduce dependence in the dependent variable between observations from the same conflict dyad, and thus would cause correlation among the error terms. Including only one observation per warring dyad would avoid this statistical dependence problem, but would not address the conceptual problem discussed above with regard to the level of favorability of each outcome. As a result



of these conceptual and methodological issues, therefore, existing data on conflict outcomes is not ideally suited for use in the current analysis.

#### *New War Outcome Variable*

Rather than adopt existing coding schemes which do not meet the needs of this project on theoretical or empirical grounds, I instead develop a new coding scheme for war outcomes that identifies the favorability of the outcome for each actor, *relative to its original war aims*. This new variable measures the degree to which a leader achieved his state/group's original war aims, instead of identifying how he/she fared relative to the opponent.

Specifically, this new variable is coded on a seven point scale from 3 (total victory) to -3 (total loss) with 0 (pre-war status quo) in the middle.<sup>33</sup> A total victory (3) is coded for both state and rebel leaders who achieve all war aims, make no concessions, and overthrow their opponents. For a rebel group, this entails military defeat of the central government, while for state leaders this involves militarily destroying the rebel group. A major victory (2) is coded for leaders who achieve all war aims and make no concessions, but who do not militarily defeat their opponent. A major victory is therefore coded, for example, when a rebel group with secessionist aims achieves independence for their desired territory while the government they fought against remains in control of the rump state. A partial victory (1) is coded in

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<sup>33</sup> This coding scheme focuses on political concessions, territorial gains and losses, and major changes in the composition of the group or state that render it unable to continue the war. It does not take into account casualties or other costs of war in the 'loss' portion of the scale. All conflict actors are expected to suffer losses and to pay other associated costs of war. These costs are controlled for in the analysis, and are not incorporated into the coding of war outcomes because they are anticipated costs of war that accrue throughout the war rather than at its end.

cases where a leader achieves some, but not all, of his group's war aims. A rebel leader with secessionist goals who achieves autonomy, or one who wishes to overthrow the central government but instead is awarded power-sharing, for example, receives a coding of partial victory.

A status quo outcome (0) is coded in cases where the leader achieves no war aims but makes no concessions; essentially where the pre-war status quo remains in place at the war's end.<sup>34</sup> Among the loss categories, a partial loss (-1) is coded when a leader makes minor concessions at termination, while a major loss (-2) is coded for leaders who make major concessions. The former may involve small losses of territory, withdrawal of claims to territory, or changes to the character of the state political system that do not directly/immediately threaten the composition of the winning coalition. The latter, on the other hand, includes major losses of territory, more extreme changes to the character/composition of the government, and military outcomes that severely weaken but do not completely destroy the warring party in question. Finally, a total defeat (-3) is coded for leaders who preside over the total military defeat of their group/state. Table 3.4 provides the distribution of this variable for all termination leaders.

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<sup>34</sup> If the pre-war status quo was the group's goal, however, achieving the status quo would be coded as a major victory. This is more often the case for state leaders than rebels.

**Table 3.4 Distribution of War Outcomes**

Outcome Type	Value	Frequency (Percent)
Total Defeat	-3	26 (12.3%)
Major Loss	-2	40 (18.9%)
Partial Loss	-1	24 (11.3%)
Status Quo	0	20 (9.4%)
Partial Victory	1	35 (16.5%)
Major Victory	2	38 (17.9%)
Total Victory	3	29 (13.7%)
Total		212 (100%)

This coding scheme has two major benefits over existing measures. First theoretically, it more accurately captures the favorability of the war outcome for each leader by identifying the extent to which he achieves his party's war aims. This directly addresses the conceptual issue in existing data and provides a more appropriate test of the hypotheses developed in Chapter 2. Second, this new measure of war outcome introduces variation in war outcomes within each conflict dyad, thereby overcoming the statistical limitations of existing measures. Returning to the Mauritania example from above, for instance, what was previously coded a negotiated settlement for both Mauritania and Polisario under the old coding schemes is now coded as a major victory for Polisario leader Mohammed Abdelaziz and a partial loss for Mauritanian leader Ould Ahmed Louly.

A variation on this war outcome scale is used to test hypothesis 2 (extreme outcome) developed in Chapter 2. Specifically, to measure extreme outcomes, I generate a dummy variable that is coded 1 if the relevant actor's war outcome is either a total or major victory (3 or 2) or a total loss (-3). For all other outcomes – partial victory, partial/major loss, and status quo – extreme outcome is coded 0. This

dummy variable thus captures all instances in which a leader either achieves all of his group/state's war aims, or achieves none of those war aims and suffers a military defeat.<sup>35</sup> Information on the distribution of this variable is presented in Table 3.5.

### *Concessions*

Testing hypothesis 3 requires a measure of whether or not a leader makes concessions at termination. A leader who makes concessions to end the war is likely to be viewed differently by winning coalition members than one who stands firm on his/her war aims, even in the face of defeat. Leaders are identified as making concessions if they explicitly compromise on central issues at stake in the war. Explicit compromise is coded if the leader makes a direct statement conceding on central war aims, or if he signs an official settlement agreement through which he implicitly gives up central goals. Antonio Bento Bembe of FLEC-R, for example, signed a peace agreement with the Angolan government in 2006 through which he officially recognized Angola as a unitary and indivisible state, thereby implicitly renouncing separatist claims for control over Cabinda. Conflicts that end through negotiated settlement generally involve concessions-making by at least one party, while those that end in military victory/defeat or low activity generally do not. In low-activity outcome cases, leaders often maintain their claims, refusing to compromise on central issue at stake, even if such compromise could potentially lead

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<sup>35</sup> As a robustness check, I reran the analysis using alternate coding schemes for extreme war outcomes. In the first alternative model, I code only total victories (3) and total losses (-3) as extreme outcomes. In the second alternative model, I code both major (2/-2) and total (3/-3) victories and losses as extreme outcomes. The results remain consistent with those presented below.

to some concessions that would leave the group better off. The concessions variable is coded 1 if the leader makes concessions on central issues at stake in the war, 0 otherwise. Descriptive statistics on this variable are included in Table 3.5.

<b>Table 3.5 Distribution of War Outcomes Dependent Variables</b>		
Value	Extreme Outcome Frequency (%)	Concessions Frequency (%)
0	119 (56%)	137 (65%)
1	93 (44%)	75 (35%)
Total	212 (100%)	212 (100%)

## **2.4 Key Independent Variable: Leader Culpability**

The primary independent variable of interest in this analysis is the leader's responsibility for the war, or his culpability. As discussed in chapter 2, culpability is hypothesized to influence termination decisions and war outcomes by altering the leader's expectation of punishment. Culpability is defined as a leader's perceived responsibility for the war, or his responsibility for the decision to involve his group in the conflict. Leaders in charge of the state or rebel group at the start of the conflict, and those with direct political connections to the decision to involve the state/group in violent conflict, are considered culpable (Croco 2011).

Culpability is measured as a dummy variable, coded 1 if the leader is culpable and 0 if he/she is non-culpable.<sup>36</sup> A rebel or state leader's culpability is determined

<sup>36</sup> As a robustness check, a four-point culpability scale is also created. The four point scale ranges from high culpability (3), which is coded for first leaders, to non-culpable (0), coded for leaders with no political connections to the first leader. Moderate culpability (2) is assigned to replacement leaders who share close political

by two factors: (1) whether he/she was in charge of the state/group when the war began, and (2) the nature of a replacement leader's political connections with the first leader. First leaders, those who preside over the start of the conflict, are considered culpable because they hold power at the start of the war. As such, they are likely to be held directly responsible for presiding over the transition from peace to war by both internal audiences and opponent elites. First leaders are considered culpable whether or not they actually initiate violence because they bear responsibility for either firing the first shot (initiators) or failing to prevent the dispute from escalating to violent conflict (non-initiator first leaders).<sup>37</sup>

In addition to first leaders, rebel and state leaders who come to power during the war (i.e. replacement leaders) *and* share political or familial connections with the first leader are considered culpable. Culpable replacement leaders inherit the first

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connections with the first leader at the start of the conflict, including state leaders who served in the cabinet of the first leader or who were members of the political inner circle of the first leader. Rebel leaders who were co-founders of the organization and who held high-ranking positions within the organization at the start of the conflict also receive a culpability score of 2. Low culpability (1) is coded for state leaders who share political connections with the first leader but were not part of the first leader's inner circle, while rebel leaders receive a coding of 1 if they were high-ranking members of the group at the conflict's start, but were not founding members of the organization. Analyses using this alternative culpability coding scheme produce results that are consistent with those presented below. The results using these alternative specifications are presented in section 4.2.

<sup>37</sup> A non-initiator first leader's failure to prevent an attack through either a pre-emptive strike or accommodative policies is likely to be viewed as a policy failure, responsibility for which again rests with the leader, thus ensuring that he faces a similar burden of responsibility as one who actually fired the first shot. Furthermore, it can be difficult for constituents and even elites to identify 'initiators' in civil conflicts, as both sides may have incentives to portray themselves or the opponent as the aggressor, regardless of facts on the ground, and because of the information asymmetry characterizing the leader-audience relationship. Thus, both rebel and state leaders in power at the start of conflict are likely to be assigned responsibility for its initiation. Therefore, attributing responsibility to all first leaders is a theoretically defensible choice.

leader's responsibility for the war by virtue of these political or familial connections to the first leader. Both internal and opponent elites will assign them responsibility for the war with little or no discounting because membership in a first leader's inner circle or political affiliation with that leader implies intimate knowledge of and influence over the decision-making process that led to conflict. The clear ties between first leaders and culpable successors will be observed by internal audiences and adversaries alike, and both audiences will transfer blame for the conflict to these successors.

Culpability transfers along political party lines for leaders of democratic states (Cotton 1986), and to leaders who were members of the first leader's inner circle (political elites, members of the ruling family) in non-democratic states (Croco 2011). Replacement state leaders are therefore considered culpable if they 1) are members of the first leader's political party, 2) were members of the first leader's cabinet at the start of the war, 3) are family members of the first leader, or 4) were otherwise considered members of the first leader's inner circle at the war's start. Ali Hussain Kafi, for example, was leader of Algeria from July 1992 until January 1994 and second leader in Algeria's conflict with the MIA/FIS/AIS movement. Kafi is considered culpable because he was one of five members of the High Committee of State (HCS), Algeria's collective presidency, under the conflict's first leader, Mohammed Boudiaf. After Boudiaf's assassination in June 1992, Kafi took over as chairman of the HCS. As a member of Boudiaf's inner circle at the start of the conflict, he is likely to be assigned co-responsibility for the state's involvement in the war by both internal and opponent audiences.

For rebel leaders, culpability transfers to replacement leaders who are co-founders of the organization, family members of the first leader, or who hold high-level positions within the organization at the start of the conflict. Because rebel organizations generally have no regulated political competition analogous to that in democratic states, culpability transfers along the lines of political party co-membership do not apply to rebel groups. As with state leaders, replacement rebel leaders who were members of the first leader's inner circle at the conflict's start are likely to acquire a similar level of responsibility for the conflict in the minds of both internal and opponent-based audiences. Thus leaders like Nicalau Lobato, second leader of FRETILIN during its war against Indonesia, a founding member of the organization, and its Vice President under Xavier do Amaral during the conflict's first two years, inherits the culpability of his predecessor as a result of his path to the leadership position. Similarly, Santu Larma, second leader of the Shanti Bahini in their war with Bangladesh over the Chittagong Hill Tracts and brother of first leader, Manobendro Narayon Larma, is considered culpable by virtue of his relationship with the movement's first leader.

Finally, non-culpable leaders are those who inherit an ongoing war upon coming to power, who share no political or familial ties with the first leader, and who held no position of power within the rebel organization or state government at the conflict's start. This category includes state leaders who do not share political ties with the first leader and who come from outside the first leader's inner circle. It also includes rebel leaders who did not hold high-level leadership positions within the organization at the start of the war, or joined the movement after the start of the



conflict. These leaders are considered non-culpable because they can concretely avoid affiliation with the first leader's war aims and with the original decision to become involved in the conflict.

In addition, replacement leaders are considered non-culpable if the first leader of the organization or state was a personalist leader. Personalist regimes are those in which "access to office and the fruits of office depend much more on the discretion of an individual leader. . . . Neither the military nor the party exercises independent decision-making power insulated from the whims of the ruler" (Geddes 1999, 121–2). That is, the personalist leader is one who successfully limits his "supporters' influence on policy and personnel decisions" (Geddes 1999, 123). This has direct implications for determining leader culpability, as the key to transferring culpability from one leader to the next is the perception among internal and opposition audiences that the new leader played an important role in the decision-making process which led to the initiation of conflict, and thus can be held responsible for the decision to go to war. In regimes and rebel groups organized around a single strong-man (i.e. a personalist leader), the relevant audiences are likely to attribute sole responsibility for policy decisions to the leader himself. Thus, even if a replacement leader held a high-ranking position within the organization or the government at the conflict's start, he is unlikely to inherit the culpability of his personalist predecessor. In these cases, therefore, all replacement leaders, regardless of their political role at the start of the conflict or their connection to the first leader, are coded as non-culpable.

Examples of non-culpable leaders include Pierre Nkurunziza, who was president of Burundi in 2008 when a settlement was reached with Palipehutu-FNL,

and Zau Mai of the KIO in Myanmar. Nkurunziza was himself a rebel leader until the early 2000s, at which point he settled with the government, entered politics, and was elected president in 2005. He thus held no position in government at the conflict's start in the 1990s, and had no political connection to the conflict's first leader. On the rebel side, Zau Mai joined the KIA after the start of the war and did not hold a high-ranking leadership position within the organization for over a decade. He is thus considered non-culpable. Non-culpable leaders are in charge at termination in 50 cases (23.6%).

To test the effects of culpability on war outcomes (H2 and H3), the culpability of each leader in power at the war's end is included in the analysis. Because the analysis of war duration uses the dyad-year rather than the individual leader as the unit of analysis, variations on the basic culpability measure are used. Specifically, for each dyad-year in the dataset, two dummy variables are created. The first, *One Culpable Leader*, is coded 1 if either the rebel or the state leader in that given conflict year is culpable, and zero otherwise. The second dummy variable, *Both Leaders Culpable*, is coded 1 if both the rebel and the state leader in the given dyad year are culpable, and zero otherwise.<sup>38</sup> Thus, for example, in the conflict between the Philippines and the Communist Party of the Philippines (CPP) and its armed wing, the New People's Army, *Both Leaders Culpable* is coded 1 between 1969 and 1985, when culpable leaders Marcos on the state side and Jose Maria Sison (1969-1977)

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<sup>38</sup> In some cases, a change in leadership that occurs during the year brings a leader with a different culpability coding to power, thereby changing the culpability coding for that particular conflict dyad. The dyadic culpability codings used in the analysis are based on the culpability of the rebel and state leaders in power on December 31 of a given year, or, in cases where the conflict ends before December 31, on the culpability of the last leaders in power during the war.

and Rodolfo Salas (1977-1986) on the rebel side were in power. *One Culpable Leader* is coded 1 from 1988 on, after Sison returned to power in the CPP but the state was led by a succession of non-culpable leaders. Both variables are coded zero from 1986 to 1987, when both sides had non-culpable leaders in power. Coding culpability in this way allows me to compare the effects on conflict termination of having just one versus two culpable leaders in power. Table 3.6 includes descriptive statistics on these variables.

**Table 3.6 Distribution of Culpability Variable**

	All Leaders	Termination Leaders Only	Dyad-Year Dataset
<b>Unit of Analysis: Leader</b>			
Non-Culpable (0)	133 (27.8%)	50 (23.6%)	--
Culpable (1)	346 (72.2%)	162 (76.4%)	--
<b>Unit of Analysis: Conflict-Dyad Year</b>			
No Culpable Leaders (0)	--	--	49 (5%)
One Culpable Leader (1)	--	--	349 (34%)
Both Leaders Culpable (2)	--	--	640 (61%)
Total	479	212	1038

## 2.5 Control Variables

A variety of control variables are included in the analysis to account for several other factors expected to influence civil war termination, outcome, and the likelihood of concessions. These control variables are introduced here, and further information on coding rules is presented in Appendix A. Summary statistics for all controls are also included in Appendix A.

### *Relative Power*

First, war combatants' relative military power is accounted for in the analysis. A combatant's relative strength has been hypothesized by a number of scholars to influence the duration of war, the likelihood of government or rebel victory, as well as the probability of a negotiated settlement (D. E. Cunningham, Gleditsch, and Salehyan 2009; DeRouen and Sobek 2004; Gent 2008; Mason, Weingarten, and Fett 1999). While empirical results are somewhat mixed, recent analyses that use dyadic measures of strength demonstrate that a rebel group's military strength relative to the state significantly increases the likelihood of conflict termination, rebel victory, and negotiated settlement (D. E. Cunningham, Gleditsch, and Salehyan 2009). Therefore, to account for the influence of relative military capacity, I control for the strength of each war combatant, relative to its opponent, in tests of all three hypotheses. Greater strength disparity is expected to increase the likelihood of conflict termination and extreme war outcomes, as evenly matched adversaries should be unable to achieve decisive victory militarily, and due to their relatively even bargaining power and evenly matched forces, will likely be unable to achieve a quick military or negotiated outcome. Strength imbalances are expected to decrease the likelihood of concessions, however, as actors at either end of the spectrum will be more likely to achieve a total victory or suffer a total defeat rather than engaging in political negotiations.

### *Third Party Support*

Military support from third party states is also expected to influence a conflict's duration and outcome by altering the relative capabilities of combatants (Balch-Lindsay, Enterline, and Joyce 2008; Gent 2008). I therefore control for the presence of foreign support in the statistical analyses. Specifically, in tests of

hypothesis 1, I control for the presence of balanced interventions, or foreign support for *both* combatants in a given year. Balanced support is expected to lengthen conflict by offsetting any military deficiencies that may allow one side to achieve victory while also increasing the number of actors whose consent must be achieved before a negotiated solution is possible (Balch-Lindsay and Enterline 2000; D. E. Cunningham 2010). In tests of hypotheses 2 and 3, I control for the presence of foreign troops supporting each leader during his/her tenure. Foreign troop support is expected to increase the likelihood of an extreme outcome while decreasing the likelihood of concessions.

#### *Casualties (Costs of War)*

The empirical analyses also account for the costs of war by including a measure of the war's intensity, or the total number of battle-related casualties suffered. Existing research suggests that higher casualty rates increase the likelihood of victory by either side (Gent 2008), suggesting that greater casualties will increase the likelihood of extreme war outcomes (H2). Existing scholarship is more mixed on the influence of casualties on the likelihood of negotiated outcomes, however (Gent 2008; Walter 2002). Walter (2002), for example, finds that increasing the costs of war, measured as casualties, increases the likelihood that actors engage in negotiations to terminate a conflict. Casualties, however, do not increase the likelihood that those negotiations are successful in producing a negotiated settlement agreement. I hypothesize that casualties will, instead, decrease the likelihood of termination (H1) and concessions (H3), as higher costs suffered by the group will

increase the leader's incentives to try to achieve the most favorable outcome possible in order to justify the human costs of war.

*Incompatibility (Stakes of War)*

The issues at stake in the conflict are also expected to influence war outcomes. Existing scholarship recognizes that the two main classes of civil war – secessionist conflicts over territory and center-seeking or governmental conflicts fought over control of the central government of the state – are often characterized by fundamentally different conflict dynamics and thus different types of outcomes (Fearon and Laitin 2008).<sup>39</sup> Some argue, for example, that secessionist conflicts have lower stakes than center-seeking conflicts, as center-seeking conflicts, by definition, represent existential threats to the state, while separatist conflicts allow for the possibility of a rebel victory without necessitating a total governmental overthrow. In other words, the stakes in territorial conflicts are more divisible than those in conflicts over control of the central government (Walter 2002). Others argue, on the other hand, that territory is often indivisible in civil war, leading to bargaining breakdowns and more intractable conflicts (Toft 2002, 2005). Along these lines, existing research demonstrates that negotiated settlements are actually less likely in territorial conflicts, as these wars become so intractable that compromise is untenable (Walter 2002). I therefore expect conflicts over control of the central government to be more likely to produce concessions than secessionist conflicts, while the effect on extreme outcomes is likely to be indeterminate.

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<sup>39</sup> Fearon and Laitin (2008), for example, explicitly design a war outcomes coding scheme that takes into account the differences between secessionist and governmental conflicts.

### *ICC Signature*

The war termination model includes a control for whether the state party to the conflict has signed the Rome Statute establishing the International Criminal Court (ICC). This variable is included to account for an alternative source of punishment – international criminal prosecution – that may influence the behavior of rebel and state leaders involved in civil conflict. Existing research suggests that ICC ratification is associated with the reduction of violence in some states, as it acts as a commitment mechanism, allowing states tie their hands and demonstrate their dedication to achieving peace (Simmons and Danner 2010). Further, leaders hoping to avoid international punishment for their role in a conflict may have incentives to end the conflict in order to lower the risk of indictment by the ICC. ICC signature is therefore expected to increase the likelihood of conflict termination.

### *Leader Conflict Duration/Tenure*

A measure of each leader's war-time tenure is also included in the analysis of war outcomes. Longer conflict duration is expected to increase the likelihood of concessions and to decrease the likelihood of extreme war outcomes as the costs of war mount over time (Walter 2002). Because the model of war duration uses yearly data, a counter and cubic polynomials of time are used instead of this measure of tenure length (Carter and Signorino 2010).

### *Political Institutions*

I control for the political institutions of the state and the rebel group as well. In line with existing findings on democracy, I expect democratic institutions to decrease the likelihood of conflict termination, while increasing the likelihood of

concessions at termination (D. E. Cunningham, Gleditsch, and Salehyan 2009). On the rebel side, I control for the presence of a rebel political wing. Cunningham, Gleditsch, and Salehyan (2009) hypothesize that conflicts are more likely to end in negotiated settlement when the rebel group in question can pursue its political demands through means other than violence. Specifically, when the rebel group has a political wing and can engage the political process as an alternative to violence, the conflict is more likely to end in negotiated settlement. I expect the presence of a rebel political wing to decrease the likelihood of extreme outcomes for both rebel and state leaders, and to increase the likelihood of concessions by both sides.

#### *Mediation*

I control for the presence of third party mediation in the models of war outcome (H2 and H3). Existing literature suggests that mediation can facilitate conflict termination through negotiated settlement (Regan and Aydin 2006). Mediation efforts are therefore expected to increase the likelihood of concessions and reduce the probability of extreme war outcomes.

#### *Population and GDP per Capita*

Finally, I include controls for population size and GDP per capita in the war termination model, as these controls have been shown to influence war termination in a variety of analyses (D. E. Cunningham, Gleditsch, and Salehyan 2009; Fearon 2004). Larger population size is expected to decrease the likelihood of termination at any given time, while wealthier states are expected to fight shorter wars (i.e. increase the likelihood of termination).



### III. Results: War Duration and Outcome

Having discussed in detail the construction of the dataset and key variables used in the analysis, I now present the central findings regarding the effects of culpability on war duration and outcome. Due to the binary nature of the dependent variables used to test hypotheses 1, 2, and 3, the primary results presented below are based on logistic regression analysis.<sup>40</sup> Robust standard errors are clustered on the dyad episode for the duration models and on the conflict for the outcome models, to account for time dependence in the former and dependence across different dyads within the same conflict in the latter.

#### 3.1 Leader Culpability and War Duration

Figure 3.1 presents the results of the civil war termination analysis (H1). Coefficient estimates with 95 percent confidence intervals are presented; confidence intervals that do not cross zero indicate that the relevant coefficient estimate is significant in a two-tailed test at a p-value less than 0.05.

As demonstrated in Figure 3.1, a civil conflict is significantly less likely to terminate in any given year when at least one culpable leader is in power, relative to instances when both the rebel and state leaders in power are non-culpable.

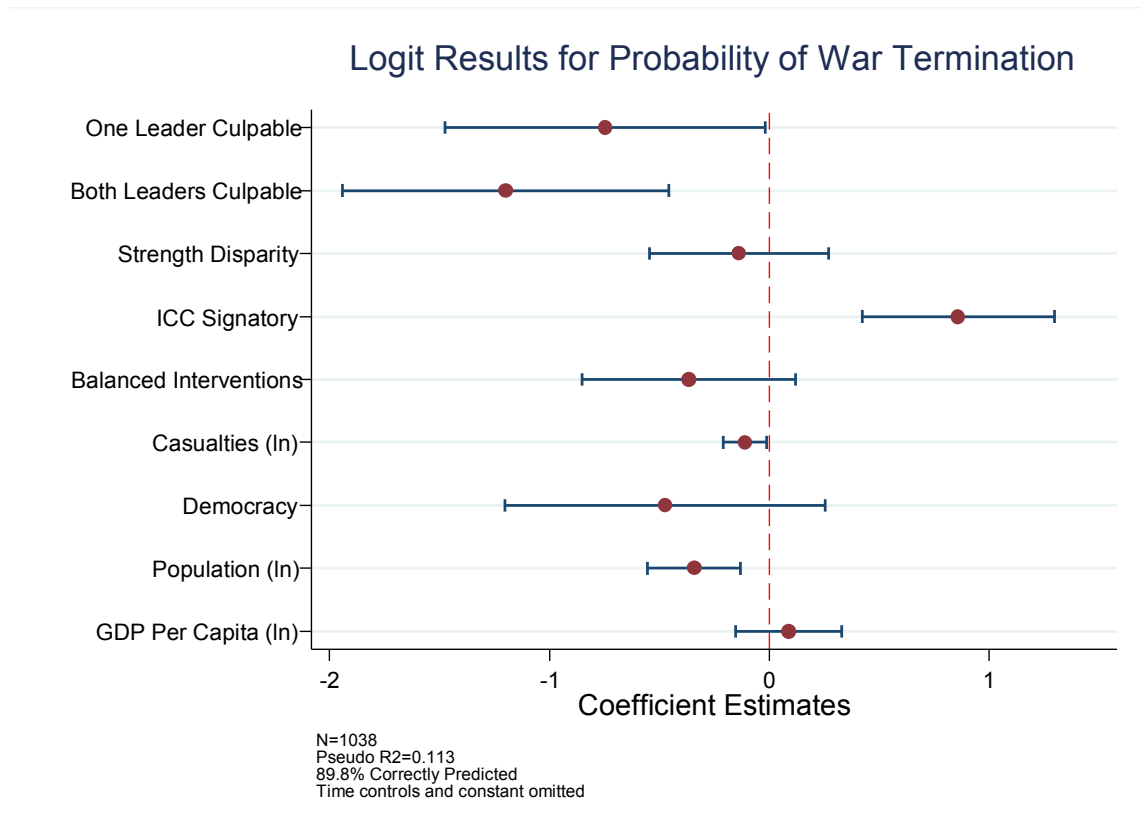
Coefficient estimates for the *One Culpable Leader* and *Both Leaders Culpable* variables are negative and significant, indicating that both variables significantly

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<sup>40</sup> Some robustness checks and secondary analyses presented later in the chapter use categorical dependent variables to examine the effects of culpability on the type of war outcome experienced. Multinomial logistic regression models are used in these additional empirical tests.

reduce the probability of war termination, relative to instances in which both leaders are non-culpable.

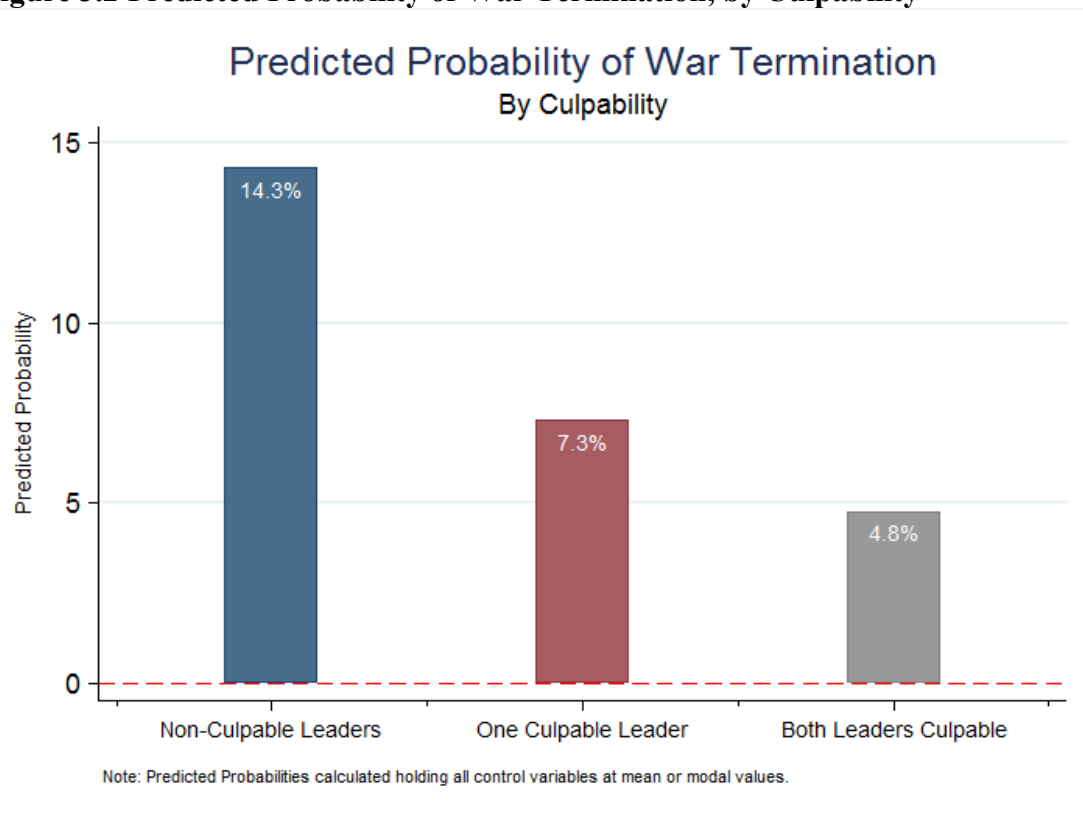
**Figure 3.1 Logit Results for Probability of War Termination**



Examining the predicted probabilities and first differences provides information on the substantive impact of culpability that is not provided by coefficient estimates alone in a non-linear model. I use Clarify (King, Tomz, and Wittenberg 2000; Tomz, Wittenberg, and King 2003) to estimate quantities of substantive interest, specifically presenting predicted probabilities and first differences for the culpability variables. In the empirical analysis of conflict termination, the predicted probability is the probability that a conflict ends in a given

year, given specified values for the explanatory variables. First differences, or changes in predicted probabilities, are calculated by subtracting the predicted probability that the war ends when neither leader is culpable (baseline probability) from the predicted probability that the war ends when one (or both) leaders are culpable (post-change probability).

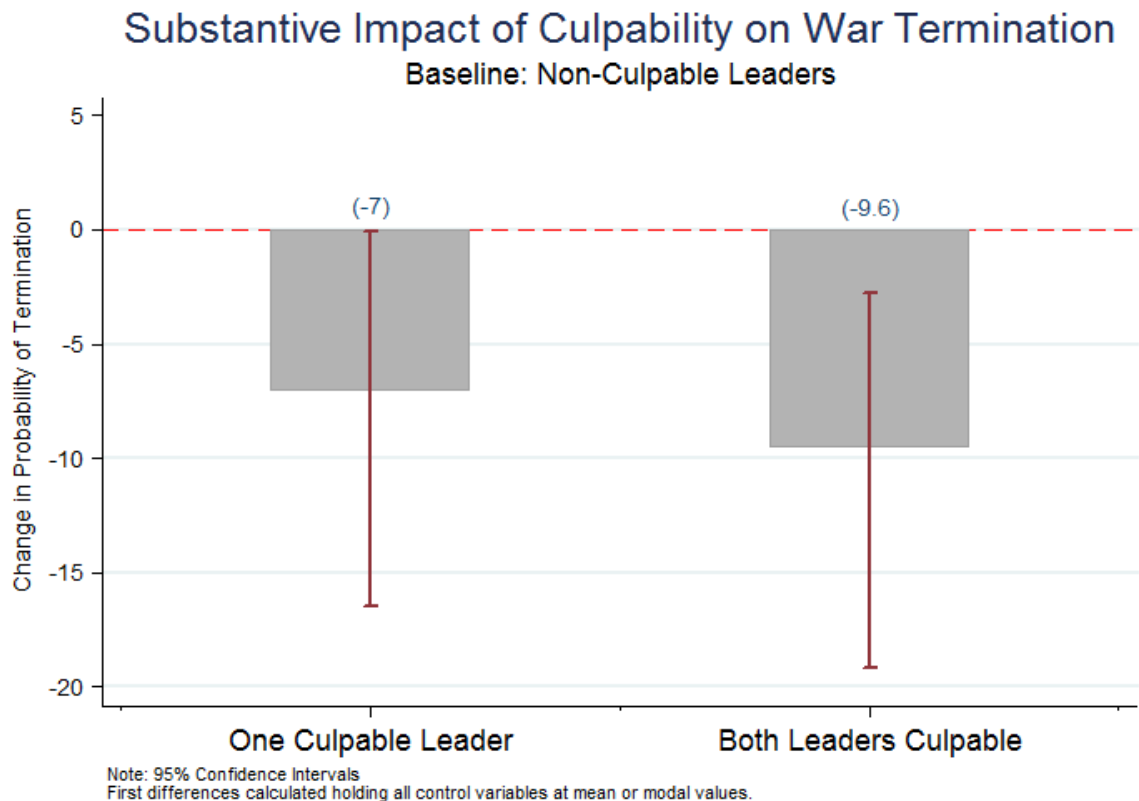
**Figure 3.2 Predicted Probability of War Termination, by Culpability**



As demonstrated in Figure 3.2, the predicted probability of a civil conflict terminating in a given year when no culpable leader is in power is 14.3 percent. This falls to 7.3 percent when one culpable leader is in power, and to 4.8 percent when

both the rebel and state leaders are culpable.<sup>41</sup> This represents an almost 50 percent reduction in the likelihood of termination when moving from zero to one culpable leader, and a full 67 percent reduction in the probability that a war ends when there are two culpable leaders in power, relative to none. Presented differently, Figure 3.3 shows that the moving from zero to one culpable leader leads to a 7 percentage point decrease in the likelihood of termination, while moving from zero to two culpable leaders results, on average, in a 9.6 percentage point reduction in the likelihood that the conflict ends.

**Figure 3.3 Substantive Impact of Culpability on War Termination**



<sup>41</sup> All control variables are held at mean or modal values when calculating predicted probabilities and first differences.

Additionally, a Wald test for equivalence of coefficient estimates between *One Culpable Leader* and *Both Leaders Culpable* results in a Chi squared of 2.80, significant in a one-tailed test ( $p=0.094$ ). This indicates that the null hypothesis that a single culpable leader and two culpable leaders have equivalent effects on the likelihood of conflict termination can be rejected. More substantively, it suggests that conflict termination is significantly less likely when both leaders in a conflict dyad are culpable, relative to instances when only one leader is culpable.

Overall, these results provide strong support for hypothesis 1 regarding the likelihood of war termination when culpable leaders are in power. As expected, when at least one culpable leader holds power, wars are more difficult to terminate. This effect is even more pronounced when two culpable leaders are in power, as both will have incentives to avoid concessions and to hold out for favorable terms of settlement.

### **3.2 Leader Culpability and War Outcome**

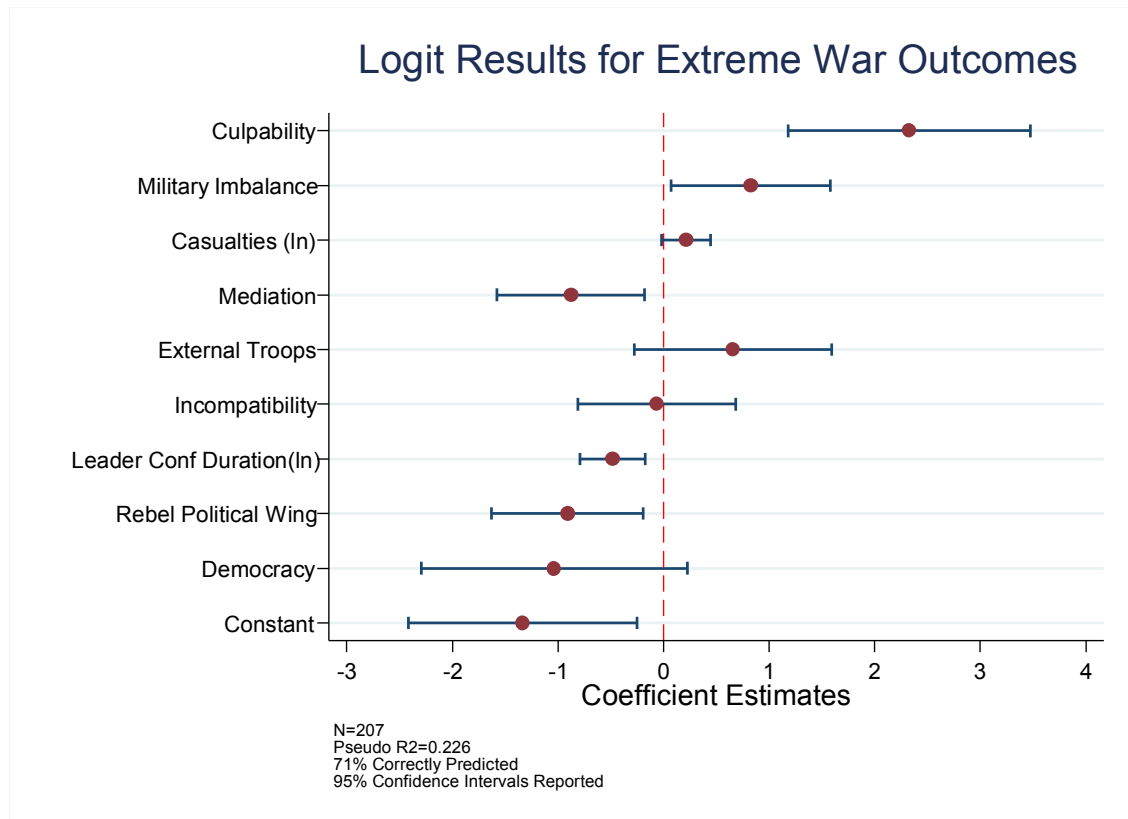
Because hypotheses 2 and 3 focus on war outcomes, only those leaders in power at conflict termination are included in the analysis. The leader-level sample for these empirical tests, therefore, includes 207 leader observations.<sup>42</sup> The results for hypotheses 2 and 3 are presented in Figures 3.4 and 3.5, respectively. As before,

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<sup>42</sup> The total number of termination leaders is 212. However, five observations are excluded from the analysis because the leaders involved settled multiple conflict dyads simultaneously (e.g. Burundi 2000, Cambodia 1991). Because the same state leader settled with multiple rebel groups at once in these cases, his terminations are not considered independent events, and are included in the analysis only once rather than once for each warring dyad. Thus, for example, Hun Sen of Cambodia is included in the dataset only once for the 1991 settlement agreement signed with both FUNCINPEC and the KPNLF.

these figures graphically present coefficient estimates with a point and use line segments to represent the 95% confidence interval surrounding each point estimate. Coefficient estimates whose confidence intervals do not cross zero are significant in a two-tailed test at a p-value of 0.05.

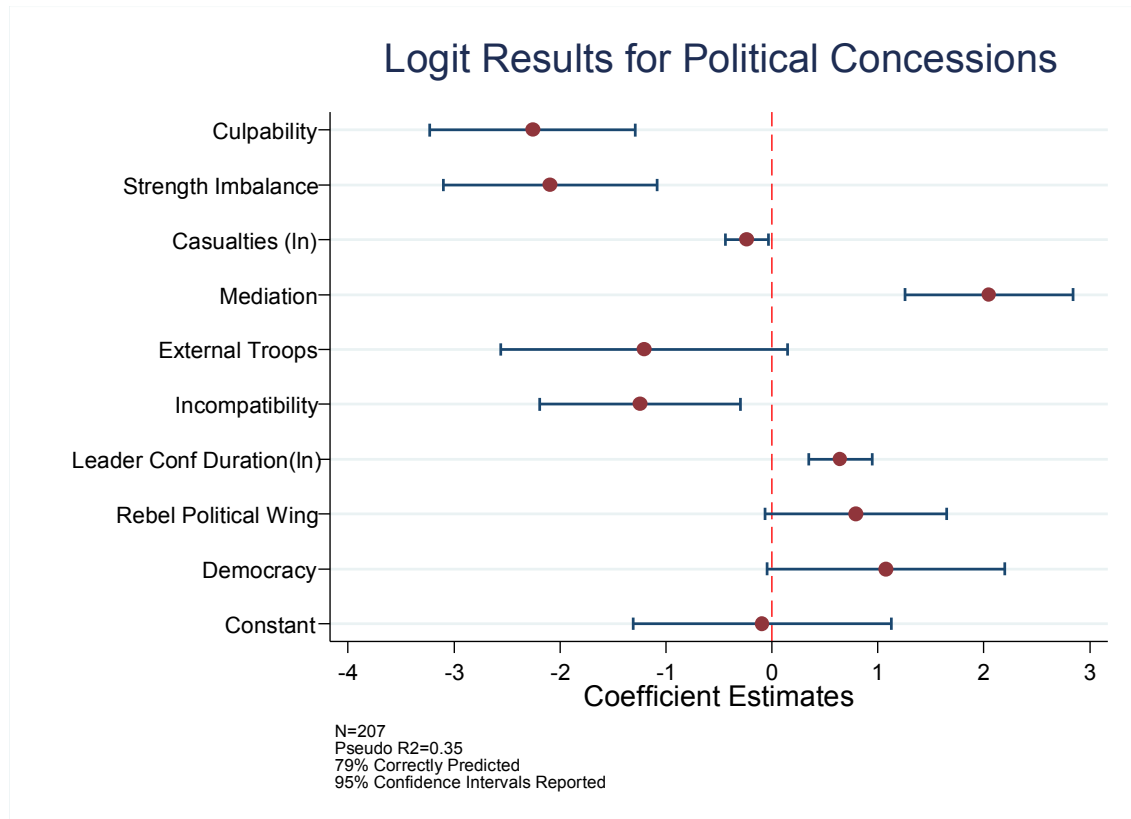
**Figure 3.4 Logit Results for Extreme War Outcomes**



As demonstrated in Figure 3.4, the coefficient estimate for the impact of leader culpability on extreme war outcomes is positive and significant, indicating that culpability significantly increases the likelihood of an extreme war outcome, and supporting the expectations of Hypothesis 2. Culpability, on the other hand, decreases the likelihood that a leader makes concessions to end a civil conflict, as predicted by Hypothesis 3. The coefficient estimate for culpability in Figure 3.5 is

negative and significant, as expected. Initial results therefore confirm expectations regarding the impact of culpability on both H2 and H3.

**Figure 3.5 Logit Results for Political Concessions**



To demonstrate the substantive impact of culpability on war outcomes, Figure 3.6 presents the predicted probability of each outcome (extreme and concessions) when moving from a non-culpable to a culpable leader. All other variables are held at mean or modal values when calculating these probabilities. The left-hand side of Figure 3.6 shows that the likelihood of an extreme war outcome increases from a baseline of 19 percent to 68 percent probability when going from a non-culpable to a culpable leader. The right-hand side of Figure 3.6, on the other hand, demonstrates

that the predicted probability of political concessions at termination falls from a baseline of 61 percent to just 15 percent when moving from a non-culpable to a culpable leader.

**Figure 3.6 Predicted Probabilities of War Outcomes**

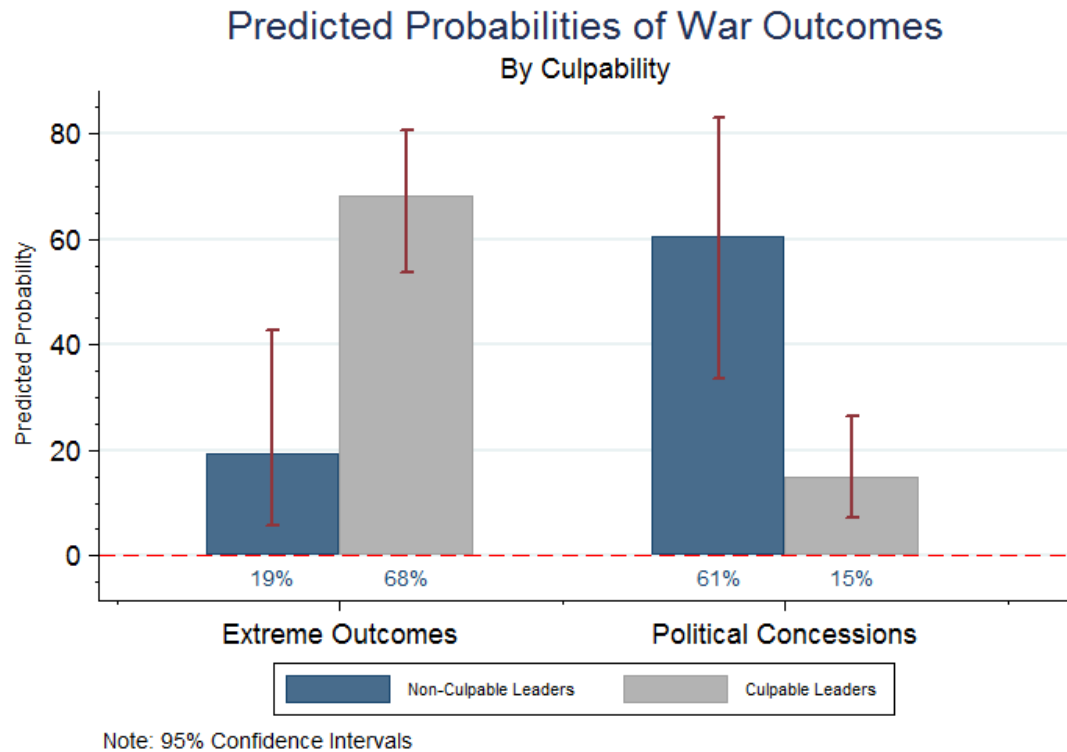
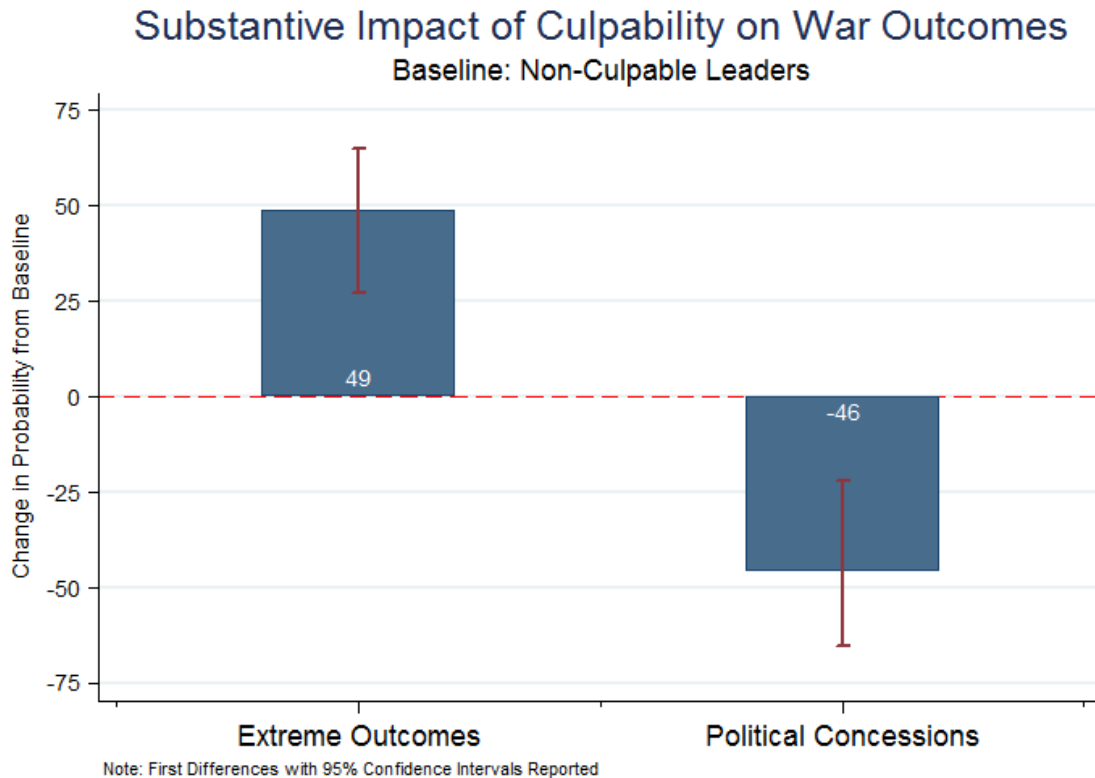


Figure 3.7 graphically represents the change in the predicted probabilities of extreme outcomes and political concessions. The first bar on the left depicts the change in the predicted probability of experiencing an extreme war outcome when moving from a non-culpable leader (baseline category) to a culpable leader, while the bar on the right-hand side of Figure 3.7 presents the change in the predicted probability of political concessions when moving from a non-culpable leader to a



culpable leader. Ninety-five percent confidence intervals surrounding each of these changes indicate that both are significant; that is, there are qualitative differences in the types of outcomes culpable and non-culpable leaders preside over.

**Figure 3.7 Substantive Impact of Culpability on War Outcomes**



More specifically, a culpable leader is over 49 percentage points more likely to preside over an extreme war outcome than a non-culpable leader, holding all other variables constant at mean or modal values. This represents a change from the baseline probability of approximately 19 percent to a post-change probability 68 percent, or an approximately 250 percent increase in the predicted probability of an extreme war outcome. As expected, culpability has the opposite effect on the

likelihood that a leader makes concessions to end the war. While a non-culpable leader has a 61 percent probability of making concessions at termination, a culpable leader will make concessions on central war aims with only 15 percent probability. This indicates that culpable leaders are, on average, 75 percent less likely to make concessions at termination than their non-culpable counterparts. These results, taken together, suggest that culpability has not only a strong statistical influence on the likelihood of both extreme outcomes and concessions-making, but that it also has a strong substantive impact on war outcomes.

### **3.3 Control Variables**

It is useful to compare the impact of culpability to the effects of the control variables included in the models to get a sense of the relative influence of each. This discussion focuses only on the results for the political concessions model (Model 3), but it is important to note that most controls behave as expected in the other two models.

As expected, a military strength imbalance between combatants, the number of casualties suffered during the leader's tenure, and the type of incompatibility at issue in the war all significantly decrease the likelihood of concessions at termination. External troop support also decreases the likelihood of political concessions, though this result just misses conventional levels of significance. Attempts at mediation and increasing the leader's wartime tenure, on the other hand, significantly increase the likelihood of political concessions, as expected. Controls for the presence of a rebel political wing and democratic state-level institutions also increase the likelihood of

concessions, as expected, though both variables just miss standard levels of significance.

**Table 3.7 Predicted Probability and Percentage Change for Control Variables, Political Concessions Model**

	Predicted Probability of Political Concessions		First Difference	Percentage Change
	Low	High		
Culpability	60.54	14.98	-45.56 (-65.27, -22.08)	-75.26
Strength Imbalance	60.54	18.79	-41.76 (-59.79, -22.14)	-68.97
Mediation	60.54	91.70	31.16 (12.52, 52.29)	51.47
External Troops	60.54	34.49	-26.05 (-50.70, 3.10)	-43.03
Rebel Political Wing	60.54	76.20	15.65 (-1.22, 34.28)	25.86
Democracy	60.54	78.92	18.37 (-1.50, 36.48)	30.35
Incompatibility	60.54	32.80	-27.74 (-47.56, -7.17)	-45.82
Ln Casualties	68.13	51.65	-16.48 (-31.46, -2.11)	-24.18
Ln Leader Conf Duration	44.91	76.17	31.25 (14.99, 46.62)	69.59
Note: To estimate the predicted probability, the covariate of interest is moved from low to high (25 <sup>th</sup> to 75 <sup>th</sup> percentile for continuous variables) while holding all other covariates at mean or modal values. First differences are computed by subtracting the baseline predicted probability (covariate at low value) from the predicted probability following the change in the covariate of interest. I then divide the discrete change by the baseline probability and multiply by 100 to get the percentage change. Ninety-five percent confidence intervals are presented in parentheses.				

Comparing the substantive effects of these controls to those for culpability demonstrates that culpability's impact is large in relative substantive terms (see Table 3.7). Military strength imbalance, the number of casualties, leader wartime tenure, external troop support, and the type of incompatibility all decrease the likelihood of

concessions by between 24 and 69 percent, while mediation, rebel political wing, democracy, and leader wartime tenure increase the likelihood of political concessions by between 25 and 70 percent. Strength imbalance and leader conflict tenure have the largest effects, on par with culpability, at approximately -69 percent and 70 percent, respectively. While it is difficult to directly compare the substantive impact of variables, particularly those measured on different scales from one another, these results suggest that culpability's impact on war outcomes is non-trivial; culpability has a significant and substantively meaningful impact on the likelihood of conflict termination, extreme outcomes, and political concessions, and produces an effect on par with or greater than the majority of control variables used to account for existing explanations of war termination and outcomes.

#### **IV. Robustness Checks**

The main results presented above provide strong support for the hypotheses developed in Chapter 2. Leader culpability significantly decreases the likelihood that a conflict ends (H1) and the probability that a leader makes concessions on central issues at stake in the war (H3), while significantly increasing the likelihood that the relevant actor experiences an extreme war outcome (H2). There are a variety of potential limitations and caveats to these findings, however, so in this section I undertake a variety of robustness checks to ensure that these findings are robust to: (1) changes in coding rules for the dependent variables, (2) changes in coding rules for culpability, (3) alternative modeling strategies to account for the potential non-

independence of outcomes across rebel and state actors, and (4) potential endogeneity of leader culpability.

#### 4.1 Alternative Dependent Variables

The first set of robustness checks focuses on alternative measures of the dependent variables to ensure that the results are not sensitive to specifications used in the main analysis. In the first of these robustness checks, I re-run the war termination (H1) analysis using a four-category dependent variable, rather than the dummy version used in the main analysis. Specifically, this alternative measure is coded 0 in any year a conflict is ongoing, 1 in the year a negotiated settlement is reached, 2 for years in which either the government or the rebels achieve a military victory, and 3 for years in which another type of termination occurs (i.e. low activity outcomes or ceasefires in which no formal agreement is reached). The analysis for this alternative dependent variable is run on the same dyad-year dataset used for the analysis of Hypothesis 1, and uses a multinomial logit estimator due to the nominal nature of the DV. The model also includes all controls from the main termination equation, including time controls and clustered standard errors.<sup>43</sup>

Because the theoretical argument developed in Chapter 2 focuses primarily on the difficulty in achieving a negotiated settlement when culpable leaders are in power,

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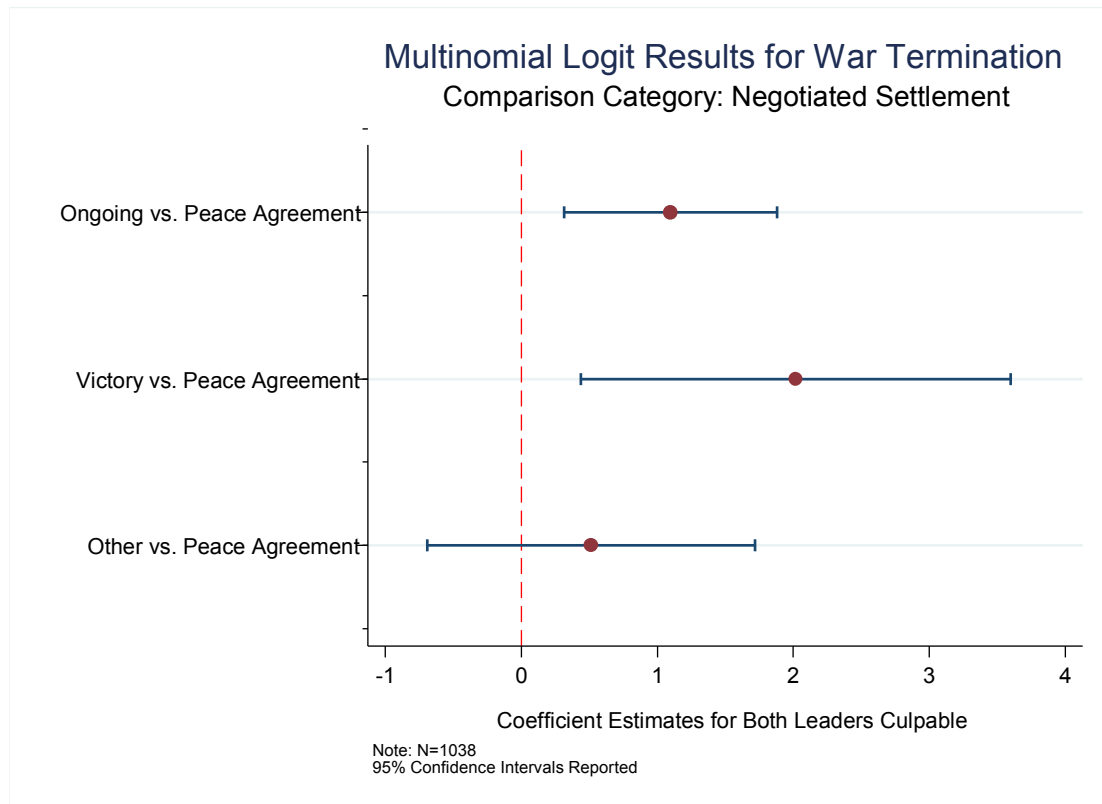
<sup>43</sup> The analysis includes only the *Both Leaders Culpable* variable, rather than both this variable and the *One Leader Culpable* variable. The *One Leader Culpable* variable is excluded because of a low number of observations in some of the outcome categories, making it impossible to run the analysis with this variable included. The effect of the key independent variable, therefore, should be interpreted as the impact when both leaders are culpable, relative to cases in which either one or neither leader is culpable.

this test treats negotiated settlement as the base category for comparison to all other outcome types. The theoretical expectation is that culpability will increase the likelihood that a conflict continues (0), relative to settling peacefully, and that a military victory by one side (2) will be more likely than a peace agreement when culpable leaders are in power. Expectations are somewhat less clear with regard to the comparison between negotiated settlement and the ‘other’ category (3), which includes low activity outcomes and ceasefires. While culpable leaders may hope to avoid this type of ambiguous outcome because it could be viewed by winning coalition members as a failure to achieve wartime goals, these leaders may actually prefer a low activity/ceasefire outcome to a negotiated settlement, as it allows the leader to avoid making explicit or overt concessions and to argue that the current lull in violence is actually an opportunity for regrouping and rearmament. I thus expect culpability to weakly increase the likelihood of ‘other’ outcomes relative to negotiated settlements.

Figure 3.8 presents the coefficient estimate for each comparison category for the *Both Leaders Culpable* variable from this model. As this figure demonstrates, when both leaders in a civil conflict are culpable, continuation is significantly more likely than termination via negotiated settlement. Similarly, victory by one side in the conflict is significantly more likely than a peace agreement when both leaders are culpable. Finally, a low activity/ceasefire outcome is more likely than a negotiated settlement when both leaders are culpable, as evidenced by the positive coefficient estimate for this comparison, but somewhat unsurprisingly, this comparison is not

statistically significant. A full table of results for this model is presented in Appendix C.

**Figure 3.8 Multinomial Logit Results for War Termination**

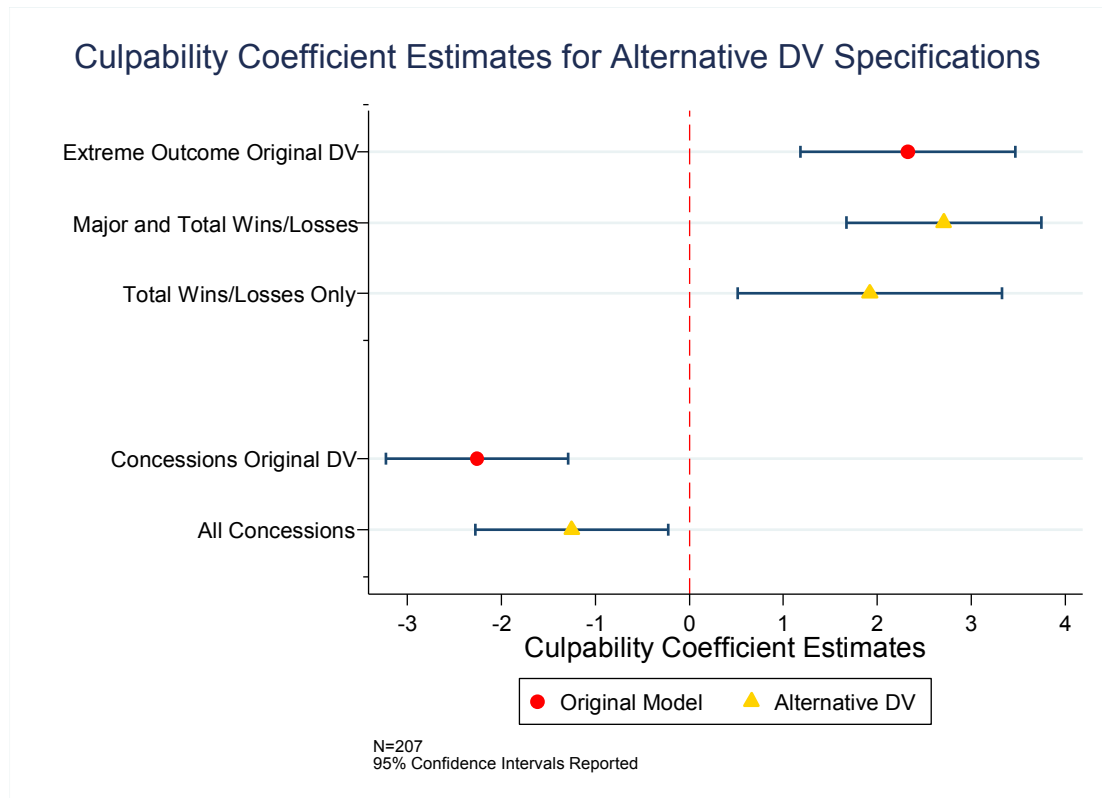


These results indicate, as expected, that settlement of civil conflict through peace agreement is the least likely outcome type when both rebel and state forces are led by culpable leaders. Under these conditions, both leaders will have incentives to avoid concessions that could leave them vulnerable to punishment, and will therefore have incentives to continue the conflict or fight to a military end.

### *Alternative Measurement Rules for Extreme Outcomes and Political Concessions*

The second set of robustness checks in this section uses alternative measures of the dependent variables used to test Hypotheses 2 and 3. In the main results, Extreme Outcomes were coded for leaders who experienced a major victory, total victory, or total defeat. To ensure that the results are not sensitive to changing this variable specification, I reran the analysis using variations on this coding of extreme outcomes. First, I recoded extreme outcomes to include both major and total victories *and* major and total defeats. Second, I recoded this variable more strictly, to exclude major victory/defeat and include only total defeat/victory in the extreme outcome category.

**Figure 3.9 Culpability Coefficient Estimates for Alternative DV Specifications, H2 & H3**





The coefficient estimates for culpability from these alternative models are included in the top half of Figure 3.9 below. The coefficient estimate represented by the red dot is the original coding, while those represented by yellow triangles are those for the alternative DV measures. As shown in Figure 3.9, the results are robust to these changes in measurement of extreme outcomes; culpability continues to significantly increase the likelihood of an extreme outcome, regardless of whether that is measured by including or excluding major victories and defeats.

The bottom half of Figure 3.9 displays the results using an alternative measure of Political Concessions (H3). In the main results, the political concessions variable was coded 1 for leaders who made concessions on *central issues* at state in the war. In this robustness check, I recoded this variable to include *all* concessions, rather than limiting concessions to those made on substantive issues. Thus, this alternative measure of concessions includes all major-issue concessions from the original variable, as well as those related to more procedural or secondary issues in the conflict. The original coefficient estimate for concessions is represented by the red dot in the lower half of Figure 3.9, while that for the alternative measure of concessions is represented by the yellow rectangle in the lower half of Figure 3.9. As these results demonstrate, culpability has a slightly weaker, yet still negative and statistically significant impact on the likelihood of concessions when this variable incorporates all types of concessions. Culpable leaders are less likely to make any concessions to terminate conflict than their non-culpable counterparts, even if those concessions are relatively minor.

### *Alternative War Outcome Variables*

Finally, this section includes two alternative dependent variables to capture some of the more fine-grained effects of leader culpability on war outcomes. First, rather than categorizing war outcomes in terms of the existing categories discussed above, they can usefully be categorized according to two criteria: (1) whether they are militarily imposed or ‘willingly’ negotiated, and (2) whether they are favorable or unfavorable. Conceiving of civil war outcomes according to these two criteria suggests an outcome variable with four distinct categories: (1) military loss, (2) negotiated loss, (3) negotiated win, and (4) military win. This conception of war outcome is particularly salient for the theory developed in Chapter 2, as one important implication of the theory is that culpable leaders will avoid *willingly* conceding to their opponents. That is, culpable leaders should be particularly unlikely to experience *negotiated loss* outcomes in civil war. They should be willing to settle through agreement if they are able to extract concessions from their opponent through negotiations, on the other hand. Additionally, they should be particularly hopeful of achieving a military win, as this is likely the best way to avoid punishment. Finally, based upon the theory developed in Chapter 2, culpable leaders should be more likely to suffer a military loss than to agree to a negotiated loss, as the incentive to gamble for resurrection will lead these leaders to forego making negotiated concessions in the hope that a more favorable outcome can be achieved militarily, but this gamble will not always pay off. I therefore expect culpability to increase the likelihood of all other outcome types, relative to negotiated loss.

To test these propositions, I code a new four-category outcome variable. A leader is coded as experiencing a military loss (DV=1) if his outcome is categorized as status quo, minor loss, major loss, or total defeat, and he does not sign a peace agreement. A negotiated loss (DV=2) is coded for leaders who sign peace agreements *and* whose outcome is coded as status quo, partial loss, major loss, or total defeat. A negotiated win (DV=3) is coded for leaders who achieve partial, major, or total victories through negotiated agreement. Finally, a military win (DV=4) is coded for leaders who do not sign peace agreements and whose outcomes are categorized as partial, major, or total victories. Using this 4-category outcome variable, I run a multinomial logit model, using *negotiated loss* as the baseline category for comparison. The results for leader culpability are presented in Table 3.8 below. A full results table, including all control variables, is included in Appendix C.

**Table 3.8 Multinomial Logit Results for Civil War Outcome, Negotiated Loss Base Category**

	Military Loss vs. Negotiated Loss	Negotiated Win vs. Negotiated Loss	Military Win vs. Negotiated Loss
Culpability	1.053 (0.633) 0.096	1.237 (0.520) 0.017	1.845 (0.683) 0.007
Observations	207		

Standard errors in parentheses, clustered on conflict. P-values in third row. Base Category is Negotiated Loss.

As expected, culpability has a positive coefficient for all three comparisons; that is, culpability increases the likelihood of military losses, negotiated wins, and military victories, relative to negotiated losses. Culpability's impact is statistically significant for both negotiated wins and military wins, relative to negotiated losses.

While the effect of culpability is slightly weaker for the negotiated loss versus military loss comparison, the coefficient estimate is significant in a two-tailed test at a p-value of 0.1. These results indicate, as expected, that culpable leaders are least likely to settle for a negotiated loss. The threat of punishment makes culpable leaders unable to willingly concede on major issues at stake in conflict. They are more likely to forego settlement opportunities, terminating via military means, or to fight to a point where they can extract concessions from their opponent, thereby achieving a favorable outcome.

The final alternative dependent variable robustness check focuses on culpability's influence on favorable war outcomes. In Chapter 2, I hypothesized that culpable leaders would fight harder to achieve favorable outcomes in order to avoid punishment. While I expect this incentive to gamble for resurrection to ultimately result in more extreme outcomes because leaders' gambles will not always pay off, this argument also has implications for the likelihood of favorable war outcomes. Specifically, measureable factors, most notably combatants' relative strength, are likely to influence whether a leader's gamble pays off, resulting in a favorable war outcome, or fails, leading to a military defeat. I therefore expect culpability to have a conditional impact on the likelihood of favorable outcomes. When combatants are relatively weak, the culpable leader's incentive to fight harder and forego settlement opportunities may not be enough to overcome his group's relative weakness. For combatants who are either at parity with or stronger than their opponents, on the other hand, culpability is expected to significantly increase the likelihood of favorable

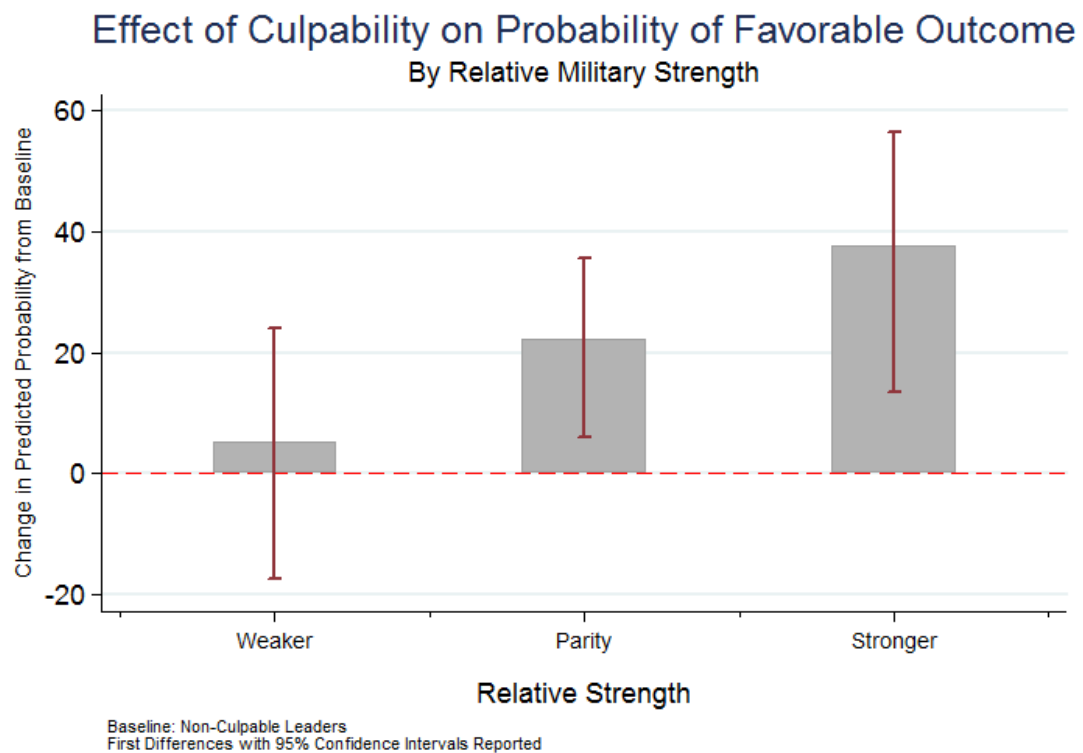
outcomes, as these leaders have both the incentive and the ability to extract concessions from their opponents.

To test this proposition, I create a *favorable outcome* dummy variable coded 1 if a leader achieves a partial, major, or total victory, zero otherwise. I interact the culpability measure with a 3-category measure of the group's relative strength. The relative strength variable is coded 0 if the group is weaker than its opponent, 1 if the group is at parity with the opponent, and 2 if the group is stronger than its opponent. Because this model includes an interactive term in a non-linear model, I move directly to the first differences to determine the statistical significance and substantive impact of the interaction between culpability and relative strength. A full results table for this model is provided in Appendix C.

Figure 3.10 graphically depicts the change in the predicted probability of a favorable war outcome when moving from the baseline non-culpable leader to a culpable leader, at different levels of relative strength. As expected, when a combatant group is weaker than its opponent, culpability has no significant impact on the likelihood of a favorable outcome. While the first difference is positive, the 95 percent confidence interval includes zero. Culpability significantly increases the likelihood of a favorable war outcome, on the other hand, when combatant groups are at parity with their opponents. Under these circumstances, the incentive to hold out for a favorable settlement or a military victory is likely to pay off because culpable leaders have greater military might backing them up. This effect is even stronger for culpable leaders whose groups are stronger than their opponents. When this is the case, culpability increases the likelihood of a favorable outcome by just under 40

percentage points, as opposed to the approximately 20 percentage point increase for culpable leaders at parity.

**Figure 3.10 Conditional Impact of Culpability on Probability of Favorable War Outcomes**



## 4.2 Alternative Culpability Specifications

The second set of robustness checks focuses on the possibility that the results for war outcomes are sensitive to the specific coding rules for leader culpability. To account for this possibility, I run the outcome analyses on three alternative measures of culpability. First, I create an alternative culpability dummy variable that is coded in the same manner as the original, except that no adjustments are made for post-

personalist leaders. Whereas in the original coding scheme replacement leaders who follow personalist first leaders are coded as non-culpable, in this alternative coding scheme, replacement leaders following personalist leaders are only coded as non-culpable if they share no political or familial connections with the first leader. Using these coding rules, the number of culpable leaders in power at termination increases from 158 to 164.

The second alternative culpability coding scheme takes into account the *extent* of a leader's culpability, rather than treating all leaders as either culpable or not. Specifically, the leader's level of culpability is coded on a four-point scale, based upon how closely associated he/she is with the first leader and the decision to go to war. All first leaders are coded as highly culpable (3) because they officially preside over the start of the conflict. Non-culpable (0) continues to be coded for leaders who share no political connections with the first leader or who follow personalist first leaders. The important distinction with this coding scheme is that replacement leaders are coded as either moderately culpable (2) or low culpability (1). Moderate culpability (2) is assigned to replacement leaders who share close political connections with the first leader at the start of the conflict, including state leaders who served in the cabinet of the first leader or who were members of the political inner circle of the first leader. Rebel leaders who were co-founders of the organization also receive a culpability score of 2. Low culpability (1) is coded for state leaders who share political connections with the first leader but were not part of the first leader's inner circle, while rebel leaders receive a coding of 1 if they were

high-ranking members of the group at the conflict's start, but were not founding members of the organization.

The final alternative measure of culpability uses the same four-category scale to measure culpability, but does not code all post-personalist leaders as non-culpable. Instead, it treats replacement leaders who follow personalist first leaders as moderate or low culpability leaders if they share political connections with the first leader. Post-personalist leaders are only coded as non-culpable if they share no political/familial connections with the first leader. The distributions for these two four-category culpability measures are presented in Table 3.9.

**Table 3.9 Distribution of Alternative Culpability Measures**

<i>Level of Culpability</i>	<i>Alternative Three</i>	<i>Alternative Four</i>
High Culpability	206 (43%)	206 (43%)
Moderate Culpability	75 (16%)	86 (18%)
Low Culpability	65 (14%)	65 (14%)
Non-Culpable	133 (28%)	122 (25%)

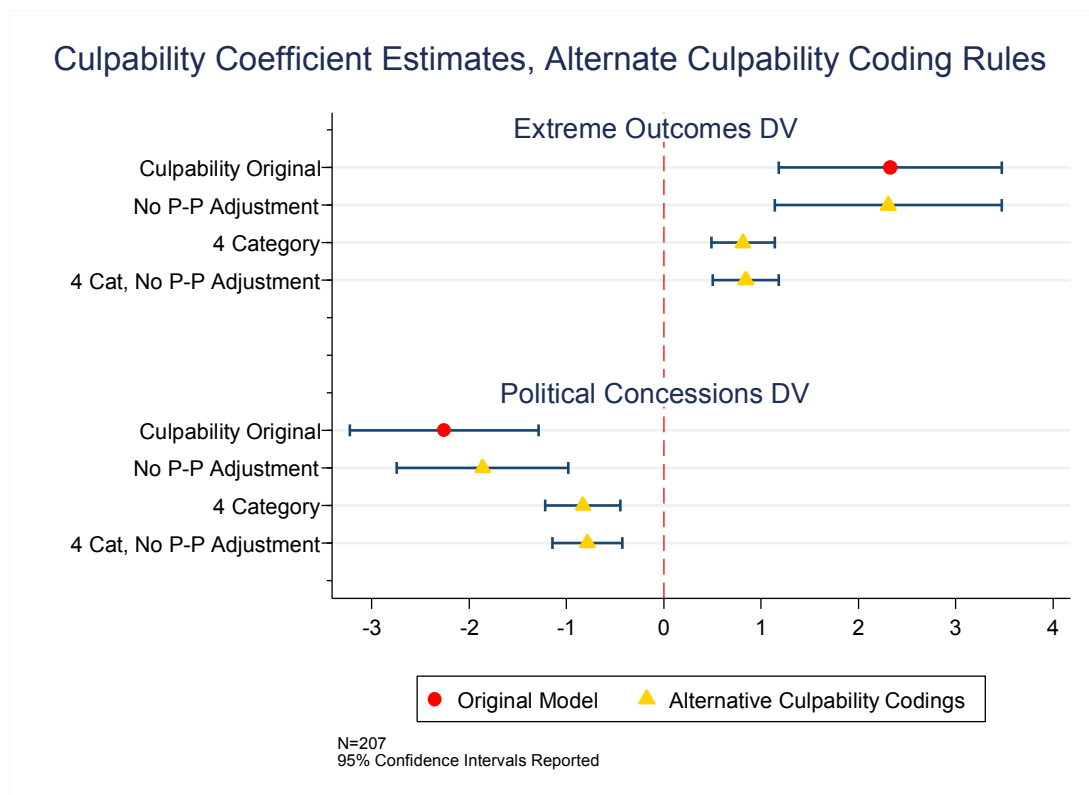
The results of statistical tests using these alternative culpability measures are presented in Figure 3.11. This figure presents the coefficient estimates for the original culpability variable (red dot), as well as all three alternative measures (yellow triangles), for the Extreme Outcomes equation and the Political Concessions equation. As these results demonstrate, culpability continues to have a significant, positive impact on the likelihood of extreme outcomes for all three alternative measures. The impact of culpability using the alternative dummy variable is nearly identical to the original model, and the coefficient estimates for the four-category versions of



culpability demonstrate that as a leader's level of culpability increases, his or her likelihood of experiencing an extreme outcome also significantly increases.

The results for the political concessions equation presented in the bottom half of Figure 3.11 also confirm that the results are not sensitive to changes in the coding rules for culpability. The alternative dummy measure of culpability continues to significantly decrease the likelihood of concessions, similar to the result using the original measure of culpability. Further, the four-category versions of culpability are also significant and negative, as expected. As a leader's level of culpability increases, the probability that he makes political concessions at termination significantly decreases.

**Figure 3.11 Culpability Coefficient Estimates, Alternative Culpability Coding Rules**



### *Culpability over Time*

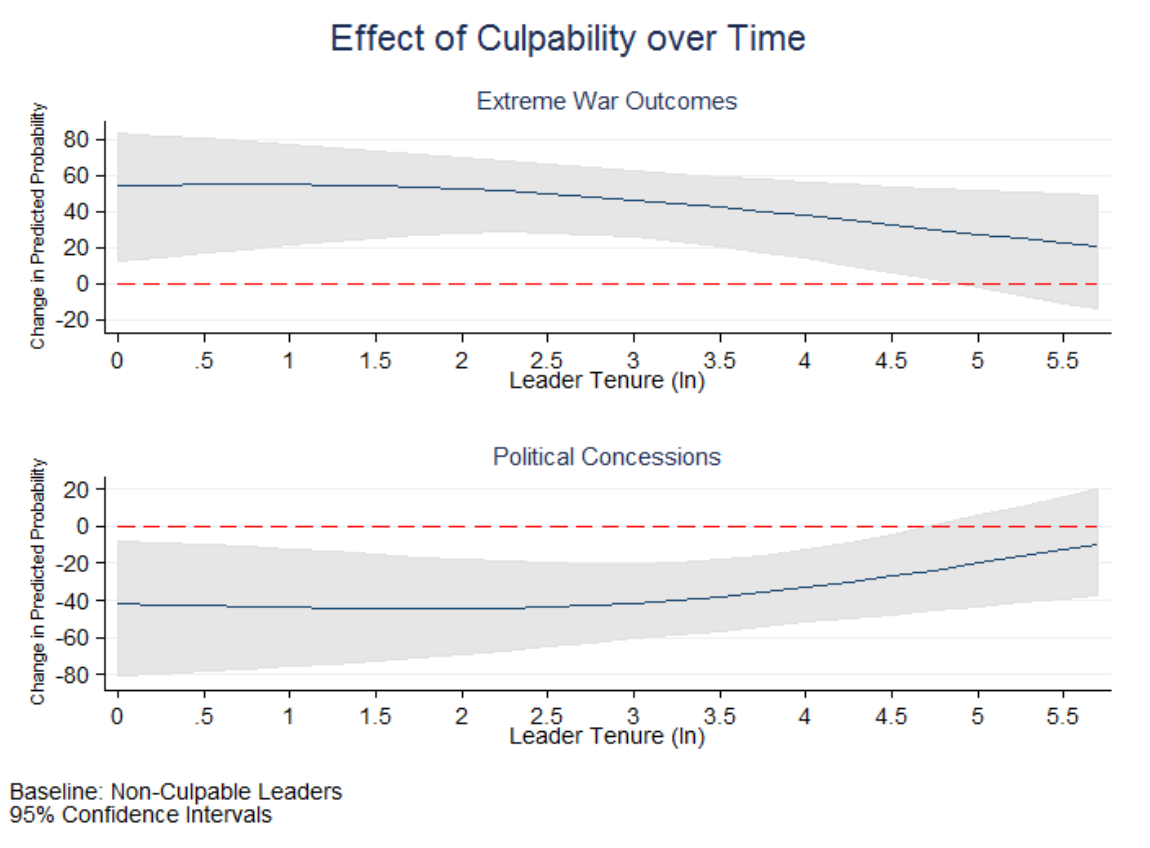
One additional possibility is that culpability's impact changes over time. Specifically, culpability may affect a leader's expectations of punishment most early in his or her tenure. Over time, the culpable leader's expectation of punishment for poor war performance may decrease for two reasons: first, the leader may consolidate power over time, increasing the security of his hold on political power and decreasing the possibility of removal, regardless of war performance. Second, leaders may anticipate that as they build a multifaceted reputation over time, audience members' incentive to punish the leader for the war may be counteracted by an interest in maintaining benefits provided by the leader's prowess in other issue areas. This suggests that culpability's impact on war outcomes and concessions may decrease over time.

To test for the possibility that culpability's impact decreases over time, I interact culpability with the natural log of leader wartime tenure. Figure 3.12 graphs the change in the predicted probability of extreme outcomes (top graph) and political concessions (bottom graph) when moving from the baseline non-culpable leader to a culpable leader, across different values of leader wartime tenure. As the graph for Extreme Outcomes indicates, culpability has its largest impact when leaders have just come to power, and the size of this effect gradually decreases over time. Culpability remains a significant predictor of extreme war outcomes until wartime tenure reaches approximately 127 months ( $\ln \text{tenure} = 4.85$ ), or about 10.5 years.

The conditional impact of culpability on political concessions exhibits a largely similar trend. Culpability has a significant, negative impact on the likelihood

of political concessions at termination for leaders with a short wartime tenure. The size of culpability's impact actually increases slightly as tenure increases, and exerts its largest impact of about a 44 percentage point decrease in the probability of concessions when wartime tenure is about 5.75 months ( $\ln \text{tenure}=1.75$ ). The size of culpability's impact then decreases gradually from this maximum, but remains significant until tenure reaches just over 115 months, or 9.5 years ( $\ln \text{tenure}=4.75$ ).

**Figure 3.12 Effects of Culpability over Time**



Overall, the results for both models demonstrate that culpability's impact is statistically significant and substantively meaningful across the majority of wartime

tenures. Culpability only becomes insignificant for leaders who have been in power for close to or just over a decade. Leaders whose wartime tenures are greater than these cutoffs comprise a small percentage (approximately 10 percent) of the data. This insignificant result, therefore, should be interpreted with caution, as insufficient data rather than a discernible trend in the data may be responsible for this result.

### **4.3 Alternative Modeling Strategies**

It is also possible that the results are sensitive to the specific modeling strategies used in the main analyses presented above. To ensure that this is not the case, this section presents results of statistical tests using alternative model and sample selections. First, I rerun the conflict termination analysis using a Cox proportional hazards model instead of the logistic regression model used in the main analysis. The Cox survival analysis models the time to a given event, in this case conflict termination, without making any restrictive assumptions about the shape of the hazard over time. One important assumption of the Cox model is that of proportional hazards (PH). The model essentially assumes that the survival curves for two strata based on different values of a given independent variable are proportional over time. A test of the PH assumption based on Schoenfeld residuals reveals that one variable in the model, ICC signatory status, does violate the PH assumption. I therefore interact this variable with the natural log of time in the model presented below. All other variables remain the same as the original analysis.

**Table 3.10 Cox Duration Results for Civil War Duration**

	Hazard Ratio	S.E.	P-Value
One Culpable Leader	0.579	(0.166)	0.057
Both Leaders Culpable	0.388	(0.105)	0.000
Strength Disparity	0.958	(0.153)	0.790
ICC Signatory	4.069	(1.108)	0.000
ICC Signatory X Time(ln)	0.591	(0.103)	0.002
Balanced Interventions	0.742	(0.153)	0.148
Casualties (ln)	0.908	(0.036)	0.016
Democracy	0.648	(0.202)	0.165
Population(ln)	0.767	(0.064)	0.001
GDP Per Capita (ln)	1.076	(0.104)	0.448
Observations	1038		

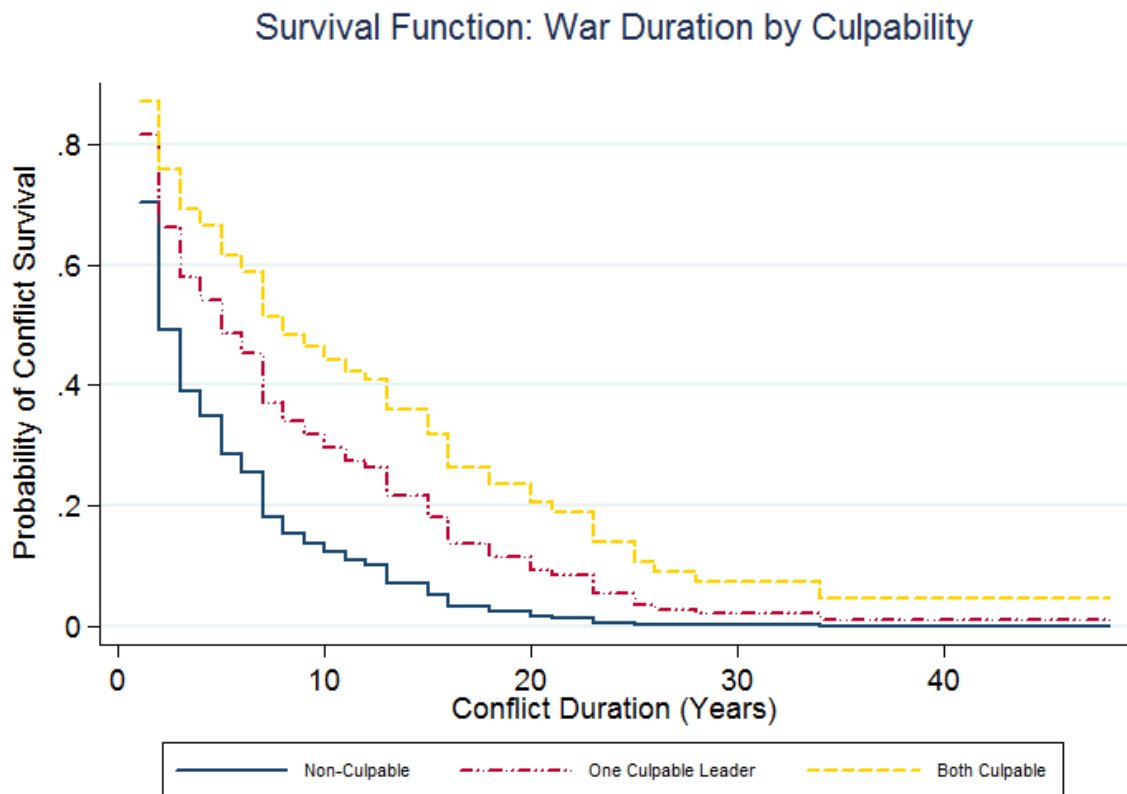
Hazard ratios reported. Standard errors in parentheses, clustered on conflict dyad. P-values in third column.

The results of this model, presented in Table 3.10, are largely consistent with those presented in the main analysis. The hazard ratios for both the *one leader culpable* variable and the *both leaders culpable* variable are less than one, indicating that they reduce the likelihood of conflict termination, relative to instances in which both leaders are non-culpable. Both variables are significant, furthermore, though the significance of only one leader being culpable is slightly weaker than that in the original model. Finally, the hazard ratio for *one culpable leader* is 0.579, indicating that moving from no culpable leaders to one culpable leader decreases the likelihood of termination, on average, by over 40 percent. The hazard ratio of 0.388 for *both leaders culpable* indicates, as expected, that this variable has an even larger, negative

impact on the likelihood of termination, on average reducing the probability of a conflict ending by over 60 percent.

Figure 3.13 graphs the survival function for war duration derived from the results of the Cox model. This graph depicts the probability that a conflict continues over time when no culpable leader is in power versus when either one or two culpable leaders maintain political office. All control variables are held at mean or modal values when calculating these predictions. The dashed, yellow line represents the probability of conflict survival when both leaders are culpable, the red dash-dot line depicts the probability of conflict survival when either the rebel or the state leader is culpable, and the solid blue line shows the probability of the conflict continuing when neither leader is culpable. As expected, the probability that a conflict continues is highest when two culpable leaders are in power across all time periods, while the probability of conflict survival is lowest across all time periods when no culpable leaders are in power. For a conflict in its tenth year, for example, the probability that the war continues is nearly 50 percent if both leaders are culpable. This falls 20 percentage points to a 30 percent chance of continuation when only one leader is culpable, and drops to just a 15 percent probability of the conflict continuing, another 50 percent reduction, when no culpable leaders are in power. Overall, therefore, these results confirm those of the logit model presented in the main results section, and reinforce the result that culpability significantly decreases the likelihood of conflict termination.

**Figure 3.13 Survival Function: War Duration by Culpability**



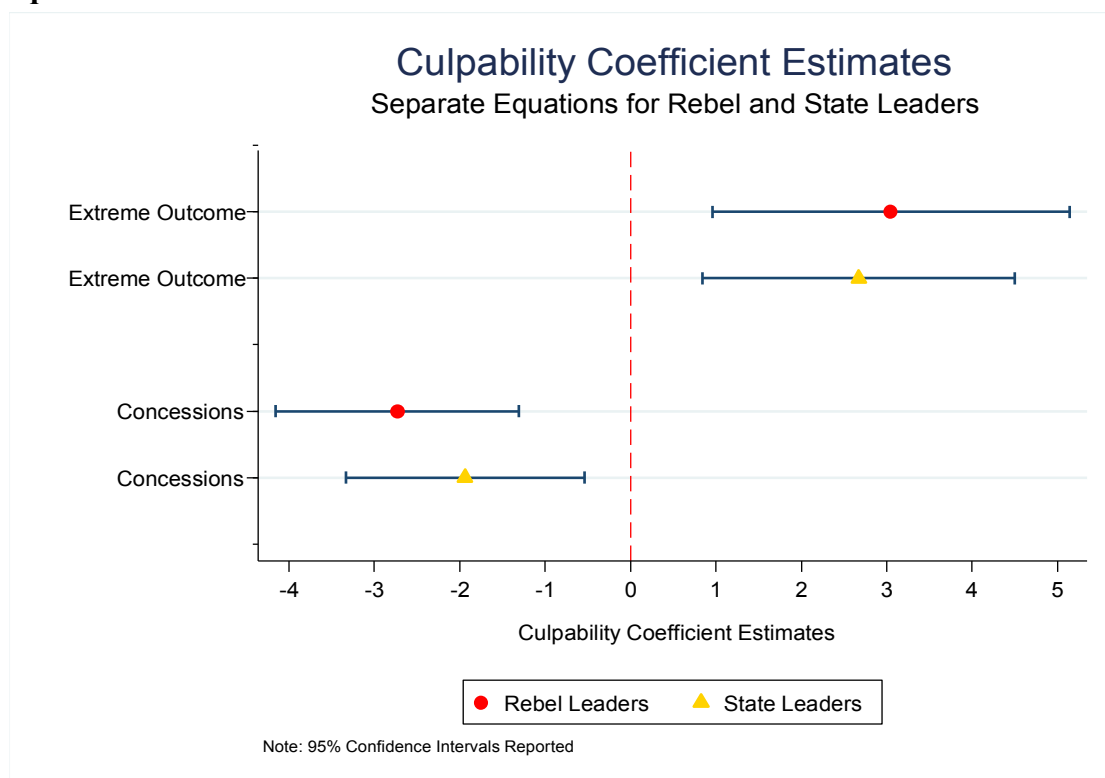
#### *Alternative Models of War Outcomes*

The main results for war outcomes and concessions (H2 and H3) use the leader as the unit of analysis, pooling all rebel and state leaders in one model. This modeling decision raises two possible critiques. First, the results may be driven by one type of leader rather than both; it may be the case, for example that culpable *state* leaders behave as expected, while culpable *rebel* leaders do not, or vice versa. Second, using the leader as the unit of analysis may introduce bias into the statistical results because state and rebel leaders' decisions regarding when and how to end a war are not fully independent. Neither a state nor a rebel leader can unilaterally end a

conflict without at least the tacit agreement of his opponent unless he achieves a total victory over his opponent. Thus, outcomes across leaders involved in the same conflict dyad are, to a certain degree, linked. The tests presented in this section account for both of these possibilities.

First, to ensure that one type of leader is not driving the findings presented above, I rerun the analyses of extreme outcomes and political concessions for rebel and state leaders separately. Figure 3.14 presents the coefficient estimates for culpability when using these two separate samples.

**Figure 3.14 Culpability Coefficient Estimates, Separate Rebel and State Equations**



These results confirm that for both rebel and state leader samples, culpability significantly increases the likelihood of an extreme war outcome. They also show, as



expected, that culpability significantly decreases the likelihood of concessions for both types of leaders. The coefficient estimate for state leaders is slightly smaller than that for rebels, but in both cases, culpable leaders are significantly less likely to make concessions at termination than their non-culpable counterparts. These results demonstrate that the results are not driven by a single leader type, confirming instead that culpability influences war outcomes for both rebel and state leaders.

The second test in this section accounts for the non-independence of outcomes across state and rebel leaders by rerunning both the extreme outcomes analysis and the political concessions analysis using bivariate probit regression models. Statistically speaking, because opponent leaders' decisions on when and how to terminate a war are unlikely to be fully independent of one another, there may be dependence in the error terms across the two relevant samples. If this is the case, failure to account for this linkage can introduce bias into statistical results. The bivariate probit model accounts for this, identifying the level of correlation in the error terms.

The results of these models, presented in Table 3.11, demonstrate two things. First, the rho parameter identifying the level of correlation in the error terms across the two subsets of leaders is relatively small and is insignificant in both models. This suggests that there is not significant correlation in the error terms across rebel and state leaders, and their war termination decisions can be modeled as independent of one another.

**Table 3.11 Bivariate Probit Results**

	DV: Extreme Outcomes		DV: Political Concessions	
	Rebel Leaders	State Leaders	Rebel Leaders	State Leaders
Culpable	1.781*** (0.501)	1.494*** (0.448)	-1.542*** (0.381)	-1.187** (0.393)
Strength Disparity	0.235 (0.336)	0.764* (0.358)	-1.472** (0.457)	-0.952* (0.382)
Casualties (ln)	0.0673 (0.0921)	0.188* (0.0792)	-0.0582 (0.0970)	-0.189** (0.0733)
Mediation	-0.0782 (0.323)	-1.237*** (0.346)	1.114** (0.351)	1.523*** (0.343)
External Troops	0.0712 (0.675)	0.182 (0.555)	-5.801*** (0.679)	-0.235 (0.454)
Incompatibility	0.0570 (0.307)	-0.0264 (0.371)	-0.639 (0.386)	-0.789* (0.363)
Leader Conflict Duration (ln)	-0.275* (0.122)	-0.334** (0.127)	0.360** (0.134)	0.353** (0.117)
Democracy	-0.818 (0.453)	-0.329 (0.543)	0.788 (0.435)	0.294 (0.443)
Rebel Political Wing	-0.155 (0.313)	-0.983** (0.341)	0.230 (0.372)	0.508 (0.304)
Constant	-1.213* (0.566)	-0.614 (0.504)	-0.317 (0.526)	0.225 (0.566)
Rho	0.114 (0.203)		0.379 (0.199)	
Observations	106		106	

Second, even using this statistical model, culpability remains a positive, significant predictor of extreme outcomes and a negative, significant predictor of

political concessions for both rebel and state leaders. These results indicate, therefore, that the modeling strategy used in the main analysis is appropriate, while also demonstrating that the results are robust to this alternative modeling choice.

#### **4.4 Potential Endogeneity of Leader Culpability**

Finally, it may be the case that the results presented above are biased due to endogeneity. That is, a leader's responsibility for the war may not exert an exogenous effect on war outcomes. Specifically, if non-culpable leaders are more likely to come to power when the war is going poorly, and are therefore more likely to capitulate or make concessions that affect war termination and outcomes, the observed impact of culpability may in fact be due to unobserved factors rather than the leader's culpability per se. Statistically, the error term and independent variable of interest (culpability) may be correlated, which can lead to biased and inconsistent results.

It is reasonable to expect leader change to be affected by recent conflict history, with leaders performing poorly in the war more likely removed from office than those whose war performance is positive. It is less clear, however, whether the *culpability* of replacement leaders would be systematically related to the previous leader's war performance. A cursory overview of the data shows that leaders who leave office because of poor war performance are often replaced by culpable successors, suggesting that endogeneity may not be a serious problem for the culpability variable. Of the 237 replacement leaders in the dataset, 106, or 45% are culpable. Seventy-six of the 237 replacement leaders come to power while the war is

going poorly, and of those 76 leaders, 32 are culpable. Thus, 42% of the replacement leaders who come to power while the war is going poorly are culpable, a percentage which is quite close to the average across all replacement leaders. While this initial examination of the data suggests that culpability is likely not endogenous to war performance, further testing is necessary to systematically rule out bias due to endogeneity. This section addresses the potential endogeneity issue using three different strategies.

First, I use matching in order to more directly assess the causal impact of the treatment variable, culpability (e.g. Hill 2010). Essentially, matching allows the researcher to pre-process the data so that for each ‘treated’ observation, there is at least one ‘untreated’ or control observation that is similar on the other covariates. In other words, matching increases the similarity between observations in the treatment (culpable leaders) and non-treatment (non-culpable leaders) groups. This helps address endogeneity to the extent that one can measure the variables influencing treatment assignment, or the likelihood that a given leader is culpable.<sup>44</sup>

Many different matching techniques are available. The specific technique used for this robustness check is coarsened exact matching (CEM), developed by Iacus, King, and Porro (2011, 2012). CEM involves temporarily ‘coarsening’ all relevant variables into substantively meaningful categories, exact matching on these categories, and then retaining the uncoarsened values of matched observations for analysis. More specifically, CEM involves five steps. First, I create a new dataset including the controls and treatment variable from the original models. Second, these

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<sup>44</sup> If unobserved variables systematically influence treatment assignment, matching by itself cannot solve the endogeneity problem.

data are coarsened into substantively meaningful categories. Dummy variables remain two-category variables, while continuous variables are recoded into categorical variables.<sup>45</sup> Third, strata, or all possible combinations of the coarsened variables, are created, and each observation is assigned to a stratum. Fourth, these strata values are assigned to the original, uncoarsened data, and any strata that do not contain at least one treated and one un-treated observation (i.e. one culpable leader and one non-culpable leader) are dropped. Finally, I rerun the analysis using the matched data, excluding unmatched observations.

The results of models run after this CEM pre-processing procedure are presented in Table 3.12. The first column presents results for war termination, the second for extreme war outcomes, and the final column presents the results for the likelihood of concessions. As demonstrated in Table 3.12, culpability remains a significant predictor of all three outcomes of interest when using the matched data. Culpable leaders are significantly less likely to end a war, more likely to experience extreme war outcomes, and less likely to make concessions, than their non-culpable counterparts. Importantly, the extreme outcomes and concessions models include a measure of the battlefield situation at the start of each leader's tenure, coded as negative (-1), status quo (0), or positive (1). Matching on this variable and including it in the analysis help ensure that the observed influence of culpability is not simply a result of how the war is going when a leader first comes to power. These results thus provide preliminary evidence to suggest that the observed effect of culpability is not

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<sup>45</sup> Categories were determined based upon the distribution of the data on these variables.

endogenous. However, to the extent that unobserved variables influence culpability, these results are limited in their ability to mitigate bias.

**Table 3.12 Logit Results using Matching**

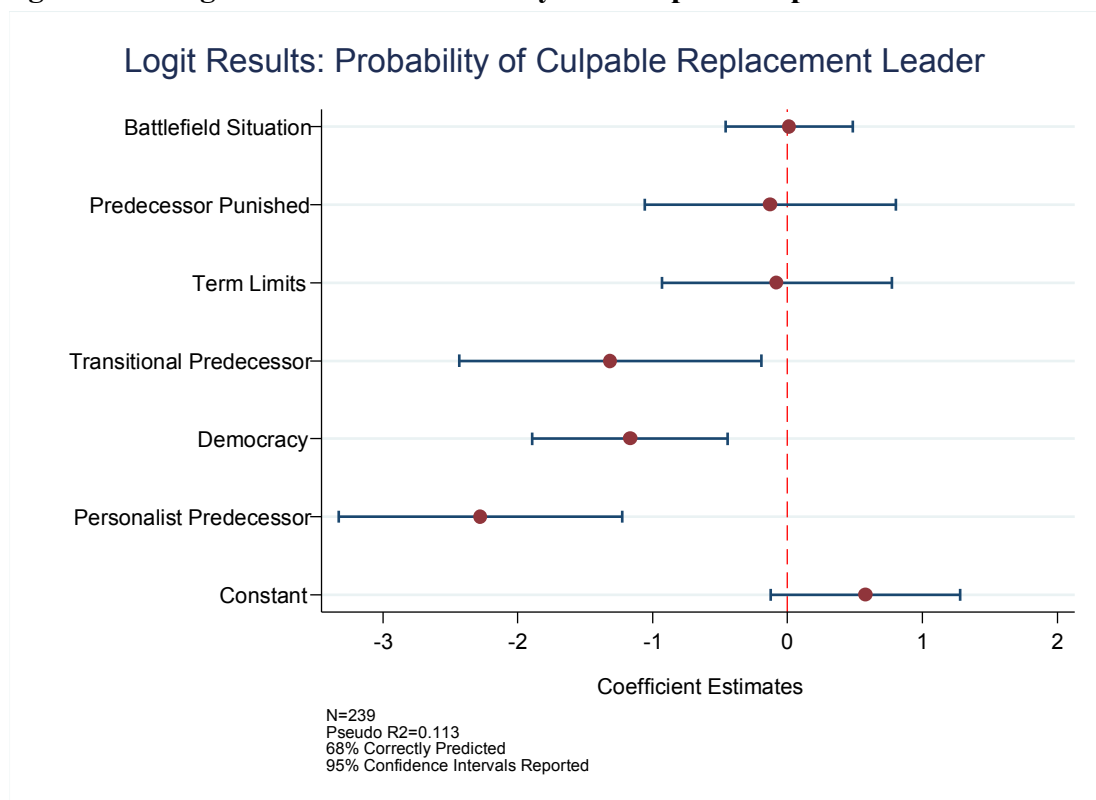
	Termination	Extreme Outcomes	Political Concessions
<b>Leader Culpability</b>	<b>-0.860<sup>**</sup></b> <b>(0.343)</b>	<b>2.659<sup>***</sup></b> <b>(0.683)</b>	<b>-2.659<sup>***</sup></b> <b>(0.806)</b>
Relative Strength/ Strength Imbalance	-0.893 <sup>***</sup> (0.336)	0.606 (0.649)	-1.926 <sup>**</sup> (0.798)
ICC Signatory	1.446 <sup>***</sup> (0.328)		
Balanced Interventions/ External Troop Support	-0.699 <sup>*</sup> (0.371)	1.085 (0.705)	
Democracy	0.225 (0.598)	-2.381 <sup>***</sup> (0.688)	1.678 <sup>***</sup> (0.649)
Population (ln)	-0.359 <sup>**</sup> (0.152)		
GDP per Capita (ln)	0.289 (0.207)		
Mediation		-1.953 <sup>***</sup> (0.575)	1.845 <sup>***</sup> (0.624)
Incompatibility		-0.488 (0.659)	-1.106 (0.820)
Leader Conf Duration (ln)		-0.693 <sup>**</sup> (0.289)	0.642 <sup>**</sup> (0.259)
Rebel Political Wing		-0.660 (0.671)	-0.198 (0.603)
Battlefield Situation		2.343 <sup>**</sup> (0.925)	-1.450 <sup>*</sup> (0.753)
Constant	3.312 <sup>*</sup> (1.820)	0.705 (1.178)	-0.829 (1.030)
Matched Observations	741	113	113

Estimates based on Logit. Robust standard errors in parentheses. \*p<0.10, \*\*p<0.05, \*\*\*p<0.01.

As a second step toward addressing the potential endogeneity problem, I estimate a logistic regression model on the subset of leaders who come to power during war (i.e. all replacement leaders) in order to test for whether non-culpable

leaders are more likely to come to power under unfavorable circumstances that force them to make concessions or end a conflict. The dependent variable in this model is a dummy variable indicating whether or not the replacement leader in question is culpable. The key independent variables in this model are (1) the battlefield situation (i.e. how the war was going before the replacement leader came to power), and (2) whether the last leader was punished as a result of the war. I also include controls for whether there are term limits in place, whether the last leader was a transitional leader, whether the state is a democracy, and whether the last leader was a personalist leader.

**Figure 3.15 Logit Results for Probability of a Culpable Replacement Leader**



If the observed impact of culpability on war outcomes were endogenous, both of the key independent variables – the battlefield situation and the prior leader's fate – should be significant, negative predictors of the dependent variable, replacement leader culpability. Significant results would indicate that culpable replacement leaders are less likely to come to power when states are faring poorly in war or when prior leaders were punished as a result of the conflict. As the results presented in Figure 3.15 indicate, however, this is not the case. Neither the battlefield situation variable nor the predecessor punished variable is a significant predictor of the new leader's culpability. These results suggest, therefore, that replacement leaders are not any more likely to be non-culpable than culpable when the war is going poorly. This result provides additional evidence in support of the exogeneity of leader culpability.

However, this test also has inherent limitations. Most importantly, because the unit of analysis is the leader change, the results tell us only that when there is a leader change, the new leader's culpability is unrelated to the battlefield situation. It does not, however, tell us whether poor battlefield performance in general increases the likelihood that a leadership change occurs in which a non-culpable leader comes to power. This question is at the heart of the potential endogeneity issue, and cannot be answered sufficiently by examining the non-random sample of observations in which a leadership change does occur.

To overcome this limitation, I undertake a final analysis in which I examine the relationship between wartime performance and the likelihood that a leadership change bringing a non-culpable leader to power occurs. To carry out this analysis, I use events-level data on battle fatalities between 1989 and 2010 for a sample of



African conflicts. These data come from the UCDP Georeferenced Event Dataset (GED), version 1.5 (Melander and Sundberg 2013). The GED identifies every battle between a given state and rebel group, and records, for each battle, the number of deaths suffered by each side. The data are available only for conflicts in Africa between 1989 and 2010. The sample for analysis therefore includes 66 distinct combatants across 47 different conflict dyads, all from African conflicts between 1989 and 2010. Using these events data, I created a combatant-month dataset that records the number of battles fought by each combatant in each month, as well as the number of deaths suffered by each combatant in each month. This dataset, therefore, captures each combatant's battlefield performance throughout the entire course of the conflict, post-1989.

The dependent variable for the analysis is a dummy variable recording, for each combatant month, whether a change in leadership occurred which brought a non-culpable leader to power. Specifically, this variable is coded 1 for any month in which a new, non-culpable leader takes power. It is coded 0 for all months in which no leader change occurred, as well as all months in which a leader change occurred, but the new leader was culpable. This dependent variable allows me to predict whether poor wartime performance significantly increases the likelihood that a non-culpable leader takes power, which is the central endogeneity concern.

The key independent variables in this analysis proxy the leader's wartime performance. The first measures whether the group suffered higher or lower than average battle deaths in the last month, relative to its monthly average from the previous 12 months. The second measures whether the group's deaths per battle last

month are higher or lower than its average deaths per battle over the past 12 months. Each of these measures, therefore, gets at how the group is faring in the conflict, relative to recent conflict history. Higher deaths/deaths per battle than recent history indicates higher costs of conflict and poorer war performance, while lower deaths or deaths per battle than recent history suggests stronger war performance and lower costs.

Both measures are logged to reduce the influence of a small number of outliers. A value of 0, therefore, indicates that last month's deaths average (or deaths per battle) is equal to that from the previous year (i.e. status quo war performance). Values less than 0 indicate that the group suffered fewer deaths last month than its average over the last year (i.e. favorable war performance). Finally, values greater than 0 indicate that the group suffered more deaths, or more deaths per battle, last month than its average over the past year (i.e. poor war performance). Based on these measures, if non-culpable leaders are more likely to come to power when wars are going poorly – that is, if culpability is endogenous to war performance – then these variables will have a positive, significant effect on the dependent variable, leader change to non-culpable.

Table 3.13 presents the results of two models using the two different measures of war performance. Model one includes the measure of relative battle deaths, while Model 2 uses the second measure of war performance, based on relative deaths per

battle. Rare events logistic regression is used to estimate these models, as leadership change to non-culpable is a rare event in the data.<sup>46</sup>

**Table 3.13 Rare Events Logit Results for Leader Change to Non-Culpable**

	Model 1	Model 2
Last Month Deaths Relative to Last Year Average	-0.0124 (0.0198)	
Last Month Deaths Per Battle Relative to Last Year Average		-0.00414 (0.0208)
External Support	-0.751* (0.457)	-0.805 (0.493)
Democracy/Rebel Political Wing	0.574 (0.536)	0.712 (0.506)
Last Leader Transitional	1.087* (0.647)	1.115 (0.714)
Time	0.0870 (0.0651)	0.170** (0.0687)
Time Squared	-0.00137 (0.00109)	-0.00306** (0.00121)
Time Cubed	0.00000593 (0.00000533)	0.0000156*** (0.00000600)
Constant	-6.215*** (1.202)	-7.145*** (1.268)
Observations	4088	3631

Estimates based on Rare Events Logit. Robust standard errors in parentheses, clustered on conflict dyad. \*p<0.10, \*\*p<0.05, \*\*\*p<0.01.

<sup>46</sup> The dependent variable is coded 1 in only 18 cases, or just under 0.5 percent of the observations.

As demonstrated in Table 3.13, neither measure of war performance has a significant impact on the likelihood that a non-culpable leader comes to power. In fact, both variables have negative coefficient estimates, which is opposite what one would expect if poor war performance increased the likelihood of leadership change to non-culpable leaders. These results, therefore, provide further evidence that culpability is not endogenous to war performance. Poor war performance, measured as higher deaths and higher deaths per battle than recent conflict history, does not significantly increase the likelihood that a new non-culpable leader comes to power. While these results are based upon a relatively small number of cases, the combined evidence from all three tests for endogeneity provide strong support for the exogeneity of leader culpability.

## **V. Implications and Remaining Questions**

The results presented in this chapter provide strong support for the theoretical expectations developed in Chapter 2. Statistical tests using an original dataset on the leaders of a global random sample of civil wars between 1980 and 2010 demonstrate that a leader's responsibility for the war has a significant and substantively large impact on war termination and outcome. War termination is significantly less likely when culpable leaders are in power, while extreme outcomes are significantly more likely under the same conditions. Additionally, culpable leaders are significantly less likely to make concessions at termination than non-culpable leaders. These results are robust to a variety of alternative variable and model specifications, and hold up under additional tests for the potential endogeneity of culpability.

These results provide important new insights into the determinants of civil war termination and outcome, and suggest an important new avenue for research in the civil conflict literature. While leader-based analyses have become prominent in the interstate conflict literature, the influence of leader-specific incentives has received much less attention in studies of internal war. The results presented in this chapter clearly indicate that leaders' personal incentives significantly influence war termination, outcome, and the willingness to make concessions in order to terminate a conflict. These results thus highlight the importance of leaders in civil conflict processes.

While these results provide strong support for the theory developed in Chapter 2, an important question remains. Chapter 2 argued that a culpable leader's increased fear of punishment was the central factor leading him to gamble for resurrection, extending conflict in the hope of victory. This argument implies that culpable leaders face a higher probability of punishment following unfavorable war outcomes than non-culpable leaders. This remains an open question, however, as the tests undertaken in this chapter do not directly test the mechanism underlying the theoretical argument.<sup>47</sup> This remaining question is examined in the next chapter. I test hypotheses on the likelihood and severity of punishment for culpable versus non-culpable leaders, conditional on war performance. Expectations are that leaders viewed as responsible for the war will be more likely to experience punishment following poor war performance than non-culpable leaders.

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<sup>47</sup> An additional remaining question is whether culpability's impact is conditional on regime type. This possibility is addressed in Appendix B.

## **Chapter 4 : Testing the Leader Punishment Mechanism**

### **I. Introduction**

The previous chapter presented results that provide strong statistical support for the hypothesized relationships between leader culpability and war duration and outcome. However, this statistical evidence, while supportive of the relationships hypothesized in Chapter 2, tells us little about the underlying mechanism driving the correlations between leader culpability and war duration/outcome. This chapter, therefore, presents additional statistical analysis to test the mechanism underlying the relationship between leader culpability and civil war dynamics.

Specifically, I argued in Chapter 2 that a culpable leader has incentives to continue a losing war, fighting to achieve a military victory, because he has a higher expectation of punishment following poor war performance than a non-culpable leader who performs similarly poorly in civil conflict. Because they bear responsibility for the decision to go to war and the original war aims established at the start of the conflict, culpable leaders cannot easily avoid the personal political consequences of defeat. Non-culpable leaders, on the other hand, can more easily shift blame onto prior culpable leaders, and can therefore more easily avoid punishment should they preside over unfavorable war outcomes. It is this underlying relationship between culpability and the expectation of punishment that drives my

expectations regarding the relationship between culpability and war termination and outcomes.

In this chapter, I quantitatively test this theoretical mechanism by examining whether culpable leaders do, in fact, face a higher risk of punishment as a result of poor or non-favorable war performance. This chapter proceeds as follows. In the first section, I review the research design and data used to test this hypothesis, including discussion of original data on leader punishment and war performance collected specifically for this empirical test. I then present the statistical results for the effect of culpability on the probability of leader punishment. Next, I present a variety of secondary analyses to demonstrate the robustness of the results to alternative coding rules and model specifications. Finally, I conclude by examining the implications of these findings for the dissertation's key theoretical argument.

## **II. Research Design and Data**

Testing the relationship between leader culpability and the likelihood of punishment requires additional original data collection, beyond that discussed in the previous chapter. After reviewing the hypothesis that is the focus of this chapter, this section discusses the data structure and research design used for the subsequent empirical tests and discusses the new data collected to test the effects of leader culpability on the likelihood of punishment.

## 2.1 Hypothesis

Chapter 2 presented one secondary hypothesis regarding the impact of a leader's responsibility for the war, or culpability, on that leader's likelihood of facing punishment as a result of the war. To review, this hypothesis is the following:

Hypothesis 4: Culpable leaders are more likely than their non-culpable counterparts to be punished following non-favorable war performances.

## 2.2 Dataset and Unit of Analysis

To test the relationship between leader culpability and the likelihood of punishment for poor war performance, I use the full dataset of 479 leaders – both termination and non-termination leaders – involved in the 120 civil conflict dyad episodes covered by the data.<sup>48</sup> The 25 leaders who remain in power and whose conflicts are ongoing as of the end of the observation period (2010) are excluded from the analysis, as they are not yet 'eligible' for post-tenure or post-war punishment. Seven additional observations are excluded from the analysis because (1) there was no *single* identifiable leader during that period of conflict, and therefore no individual for whom to code punishment (4 observations), (2) the conflict continued under a different dyad but with the same leaders in power (2 observations), or (3) no information was found regarding the leader's post-conflict fate (1 observation). The resulting dataset includes 447 observations with the leader as the unit of analysis.

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<sup>48</sup> Existing research demonstrates that limiting the sample to only those leaders in power at the end of a war may bias results (Croco 2011).



### 2.3 Dependent Variable: Leader Punishment

No existing studies in the quantitative civil war literature examine the fate of leaders involved in intrastate conflict. Thus, no cross-national data sources on leader punishment as a result of civil war exist. To test this hypothesis, therefore, I collected new data through original research on the fate of rebel and state leaders involved in civil conflicts. Information used to code leader punishment came from a variety of primary and secondary sources, including secondary histories of each war, conflict anthologies/encyclopedias that include summaries of each conflict and information on the individuals involved, and a variety of news sources that provide coverage of major events throughout the course of each conflict. A list of sources consulted is included in the bibliography.

As discussed in Chapter 2, leaders involved in civil conflicts face potential punishment from two sources: their internal audiences (i.e. winning coalitions) and their wartime opponents. In addition, punishment can take a variety of forms, ranging from the relatively benign (e.g. loss of political power) to the relatively severe (e.g. death/assassination). More specifically, I code a leader as facing war-related punishment if he or she experiences any of the following *as a result of the war*: 1) major fractionalization of the group he/she represents, 2) loss of political power/office, 3) exile, 4) imprisonment, or 5) death.<sup>49</sup>

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<sup>49</sup> Major fractionalization is included as a form of punishment because it represents a significant reduction in the leader's support base, and as a result, a significant decrease in his or her bargaining and fighting capacity. As a robustness check (see section 4.2 below), I exclude fractionalization, recoding leaders who suffer this fate as non-punished leaders. As described below in Section 4.2, the results are robust to this change in measurement of the dependent variable.

Coding this variable involves two important steps: first, I identify what happened to each leader after his or her wartime tenure ended, and second, if the leader suffered any type of punishment, I determine whether that punishment occurred as a result of the war. This second step represents an important departure from some existing literature (e.g. Goemans 2000), which codes punishment if a leader loses power within a year of the war's end, regardless of the cause of that loss of power. As Croco (2011) notes, it is inappropriate to assume that all leaders who leave office or face any other type of punishment within a given time-period have done so because of the war. Hassan Al-Bakr of Iraq, for example, left office during that country's conflict with the KDP due to ill health, not as a result of internal or opponent-based punishment. Similarly, Ehud Barak, Prime Minister of Israel at the end of the first Hezbollah conflict, resigned in March 2001, just 10 months after the end of the conflict in Southern Lebanon. The majority of sources agree, however, that Barak's resignation had little to do with the Hezbollah conflict and is instead attributable to failures in negotiations with the PLO. In my dataset, therefore, no punishment is coded for Barak as a result of the Hezbollah conflict. For each leader in the dataset, punishment is coded if a majority of sources consulted agree that the war was a major underlying or precipitating cause of the punishment faced.

As mentioned above, punishment can take a variety of forms. First, major fractionalization occurs when a leader remains the head of his group nominally, but the group experiences a major split in which the majority of the group's followers/fighters no longer view the original leader as legitimate. In other words, major fractionalization occurs when the locus of power within the organization shifts

away from the faction of the former leader and toward a new leadership, thereby marginalizing the former leader. Major fractionalization is coded, for example, for Etienne Karatasi, leader of Palipehutu during its struggle against the Burundian government in the early 1990s. After Karatasi's failure to extract any concessions from the government, he remained the nominal head of Palipehutu, but was largely sidelined as an important figure in the movement when the FNL split from his political wing at the end of fighting in 1992. Karatasi continued to claim leadership of Palipehutu from his base abroad in Denmark, but the locus of power among Palipehutu followers shifted to the FNL faction, headed initially by Cossan Kabura.

Second, loss of political power is coded for rebel and state leaders who lose their leadership position as a result of the war but suffer no additional, more severe, consequences. Thus, for example, Nepalese Prime Minister Girija Prasad Koirala resigned in July, 2001, admitting in a national broadcast that his failure to defeat the country's CPN-M insurgency was the reason for his resignation.

Exile is coded for leaders who, in addition to losing political power, are forced to leave the country as a result of their role in the war. Both Momoh and Kabbah of Sierra Leone, for example, were ousted in military coups and forced to flee the country. Momoh was ousted and forced into exile in Guinea in April 1992, when mid-level members of the military overthrew him due to dissatisfaction caused by the conflict with the RUF. Kabbah, similarly, fled to Guinea the day after being ousted in a coup led by Koroma. The latter cited Kabbah's inability to settle the RUF conflict as a major reason for the coup.

Fourth, imprisonment is coded for leaders who lose power and are incarcerated as a result of the war. Michael McKevitt, leader of the Real IRA (RIRA), a splinter group that emerged from the PIRA following the Good Friday Agreement in 1998, was arrested in 2003, convicted, and sentenced to over 10 years in prison for his role in the bombings and attacks carried out by the RIRA. Somewhat more unofficially, Pol Pot, leader of the Khmer Rouge in Cambodia, was captured and imprisoned by one of his top commanders, Ta Mok, after attempting unsuccessful purges within the group. He was put ‘on trial’ internally in July 1997, and sentenced to house arrest for life.

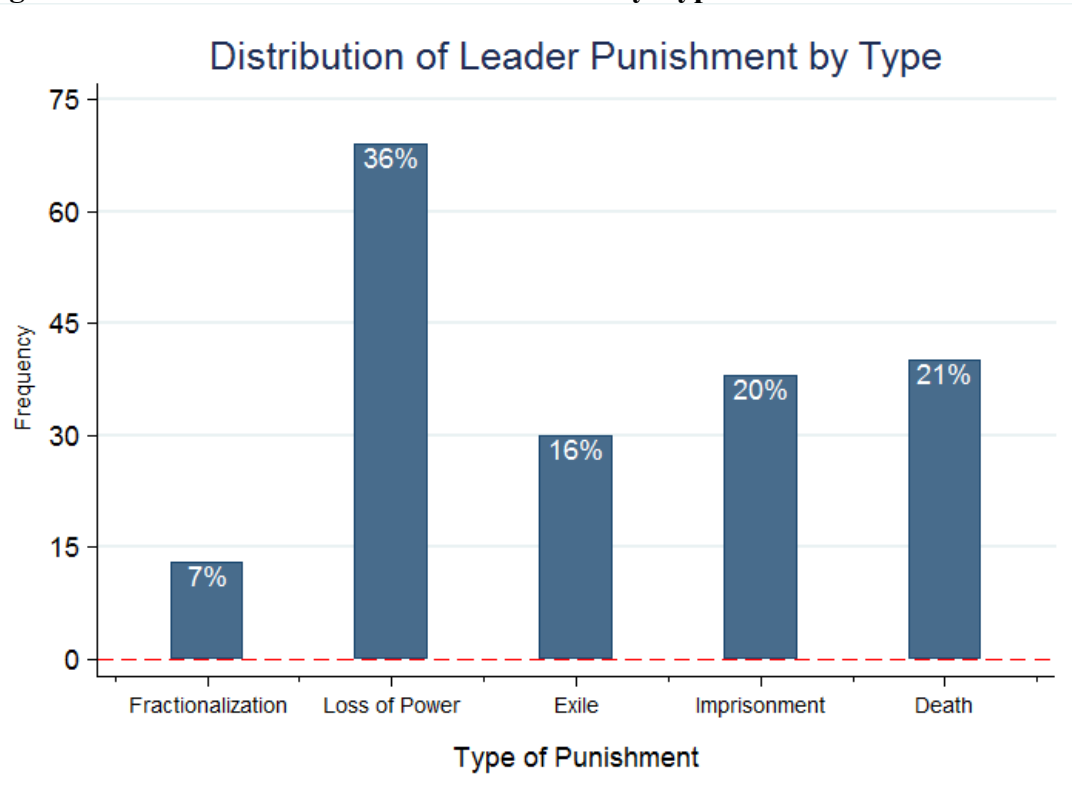
Finally, death is coded for leaders who are killed or assassinated as a result of the war. This can happen internally, as with Karrim Kassem, leader of Iraq in the early 1960s, who was deposed and killed in a coup led by members of the Iraqi military largely because of the Kurdish rebellion. Alternatively, punishment by death can occur at the hands of the opponent. Thus, Sadegh Sharafkandi of the KDPI in Iran was assassinated by Iranian agents while abroad in Berlin in September 1992.

Based on these coding rules, each leader in the dataset is coded according to the *most severe* type of punishment he or she faced.<sup>50</sup> Figure 4.1 provides descriptive information on the distribution of punishment types across the 190 leaders in the dataset who experienced some type of war-related punishment. As demonstrated, loss of political power is the most common form of punishment in the dataset, followed by death and imprisonment.

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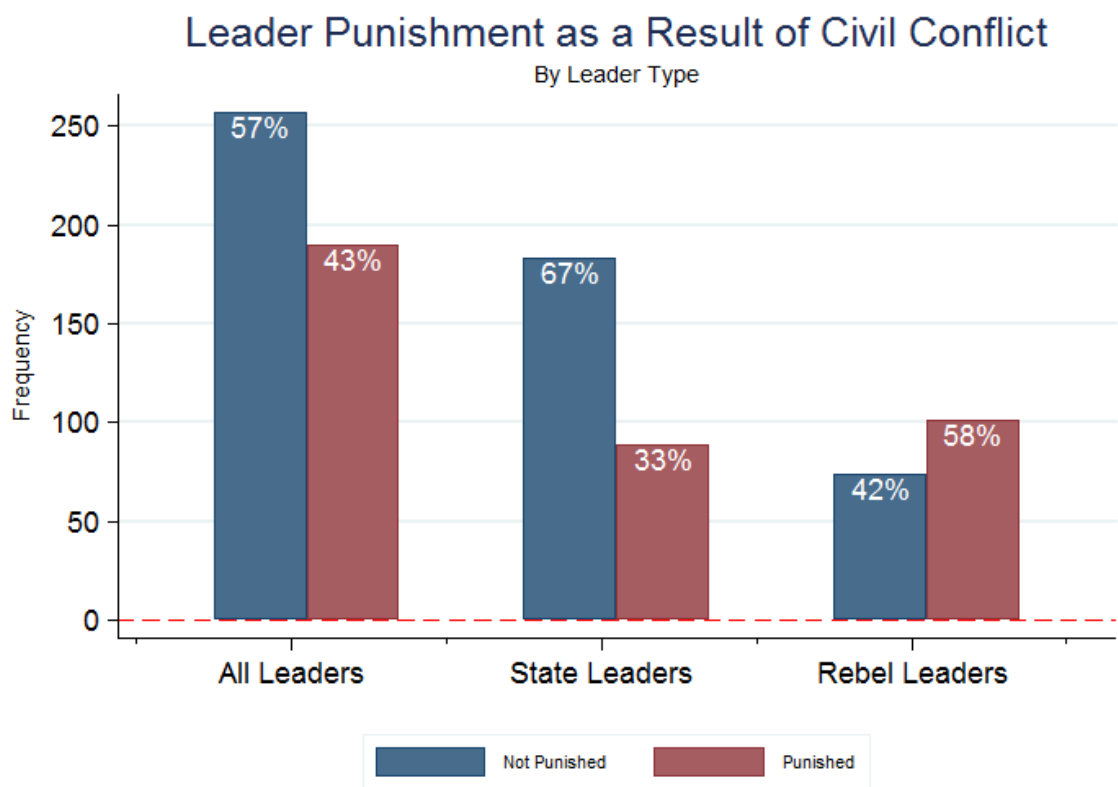
<sup>50</sup> In many cases, a leader experiences more than one type of punishment. For example, a leader who faces exile, imprisonment, or death also faces, by definition, loss of political power. I code a leader’s punishment type as the most severe form of punishment experienced, on a scale from fractionalization to death.

**Figure 4.1 Distribution of Leader Punishment by Type of Punishment**



In the main analysis presented below, these different categories of punishment are collapsed into a single dummy variable, coded 1 if the leader in question faced any type of punishment for his role in the war, and 0 otherwise. As Figure 4.2 below demonstrates, 190, or approximately 43 percent of leaders in the dataset experienced some type of war-related punishment. The other 257 leaders, or 57 percent, were not punished as a result of the war. When breaking this down according to leader type (i.e. state versus rebel), Figure 4.2 shows that punishment is more common among rebel leaders than state leaders. Over 50 percent of rebel leaders experience war-related punishment, while for state leaders, punishment occurs in approximately one third of cases.

**Figure 4.2 Leader Punishment as a Result of Civil Conflict, by Leader Type**



#### **2.4 Theoretical Variables of Interest: Leader Culpability and War Performance**

In Chapter 2, I hypothesized that culpable leaders who perform poorly in war will face a higher expectation of punishment than non-culpable leaders who perform poorly in war. This hypothesis suggests an interactive effect in which culpability's impact on a leader's likelihood of punishment is dependent upon that leader's war performance. Therefore, the key explanatory variable in this section is the interaction between culpability and war performance. Culpability was discussed in detail in the previous chapter, and the same variable is used here. A leader is coded as culpable if

he/she was in power at the start of the conflict (first leaders) or if he/she shares political or familial ties with a non-personalist first leader. See Chapter 3 for more detailed coding rules on this variable.

### *War Performance*

To code each leader's war performance, I collected original data identifying how the war was going at the end of each leader's tenure. Specifically, for each leader in the dataset, I coded his/her war performance as poor (-1), status quo (0), or favorable (1). A coding of 'poor war performance' is assigned to any leader whose combatant group suffered significant setbacks on the battlefield, or who made substantial concessions to the opponent, over roughly the last one to two years of his/her wartime tenure.<sup>51</sup> A leader's war performance is coded as 'status quo' if his group failed to make significant military (or negotiating) progress, while also avoiding significant setbacks on the battlefield or at the negotiating table. Finally, a coding of 'favorable war performance' is assigned to any leader who achieved substantial battlefield success and/or significant concessions from the opponent.

For leaders in power at war termination, I used information provided by the data collected on war outcomes (discussed in Chapter 3) to code war performance. Termination leaders who achieved partial, major, or total victories received a 'favorable war performance' coding. Termination leaders who achieved status quo outcomes were coded as 'status quo' performers on the war performance variable.

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<sup>51</sup> The rough one to two-year window is used code all three categories of war performance. Using this general guideline ensures that the coding captures the leader's recent war performance; it is expected that recent performance has a greater impact than more distant successes or failures. For leaders in power less than one year, the battlefield situation at the start of their time in office is compared to that at tenure end in order to code war performance.

And finally, leaders whose war outcomes are coded as minor, major, or total defeats received a coding of -1, or ‘poor’, on the war performance variable.

For leaders not in power at termination, additional original research was necessary to determine each leader’s war performance, as no existing data sources provide this information cross-nationally. For the 235 non-termination leaders in the dataset, therefore, I consulted a variety of primary and secondary sources to determine the extent of battlefield and negotiating-table progress over the course of each leader’s tenure. A list of sources used to code this variable is included in the bibliography. The resulting war performance variable is distributed as follows: 164 leaders (37%) receive a -1, or poor performance coding, 130 (29%) receive a 0, or status quo coding, and 153 (34%) receive a 1, or favorable performance, coding.

Finally, to account for the hypothesized conditional impact of culpability on punishment, I interact leader culpability with the above described war performance variable. This interaction term takes on values of -1, 0, or 1.

## **2.5 Control Variables**

The model of leader punishment also includes several controls for other factors likely to influence a leader’s probability of facing war-related punishment. Specifically, I control for the severity of the conflict, the leader’s wartime tenure, whether the leader is based locally or abroad, whether he/she faces term limits, and whether that particular leader is a transitional leader. Theoretical expectations for each of these variables are discussed in this section, while more detailed coding information is provided in Appendix A.



First, the severity of conflict is expected to increase the likelihood of leader punishment. As the severity of conflict increases, as measured by battle deaths, the costs of conflict shouldered by the domestic populace also increase. In line with existing research, I expect increasing costs of war to increase the likelihood that a leader faces punishment (Goemans 2000). Second, a leader's wartime tenure is expected to decrease the likelihood of punishment, as a longer tenure may indicate a tighter hold on power, and thus greater resilience to attempted removal from power and a variety of other punishments that go along with loss of office (Croco 2011; Thyne 2012).

Third, I include a dummy variable controlling for whether the leader is based domestically or abroad. While all state leaders are coded as domestically-based, several rebel leaders lead their organizations from bases abroad. Hasan di Tiro and the other top leadership of the Free Aceh Movement (GAM) in Indonesia, for example, led the group for decades from their headquarters in Sweden. I expect leading from abroad to reduce the likelihood of punishment, as leaders are insulated, in particular, against various forms of physical punishment.

Finally, I include controls for term limits and transitional leaders. Leaders with these constraints are expected to be less likely to face punishment, as there are established limitations on their time in office. As a result, both internal and opponent-based audiences may have less incentive to attempt to remove or otherwise punish the leader, as he/she will be expected to step down from power on his/her own. Coding rules for these control variables are included in Appendix A.

### **III. Results**

Having discussed the construction of the dataset and key variables used in the analysis, I now present the central findings regarding the conditional impact of culpability on leader punishment. Due to the binary nature of the dependent variables used to test hypothesis 4, the primary results presented in this section are based on logistic regression analysis.<sup>52</sup> Robust standard errors are clustered on the conflict to account for dependence across different dyads within the same conflict.

#### **3.1 Leader Culpability and Punishment**

Figure 4.3 presents the results for the analysis of the likelihood of leader punishment (H4). Coefficient estimates with 95 percent confidence intervals are presented; confidence intervals that do not cross zero indicate that the relevant coefficient estimate is significant in a two-tailed test at a p-value less than 0.05.

As expected, the coefficient estimate for leader culpability is positive while that for war performance is negative, suggesting that culpability increases the likelihood of punishment while more favorable war performance is associated with less punishment. However, because these variables are included in an interactive term in a non-linear model, neither the direction of impact nor the significance of the coefficient estimates for the interactive term and its constituent parts can be discerned based on the coefficient estimates alone (Ai and Norton 2003). I therefore turn to the

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<sup>52</sup> Some of the robustness checks presented later in the chapter use an ordinal dependent variable to examine the effects of culpability on the severity of leader punishment. Ordered logistic regression is used in these additional empirical tests.

first differences and predicted probabilities to determine the impact of leader culpability on the probability of punishment, conditional on leader war performance.

**Figure 4.3 Logit Results for Leader Punishment**

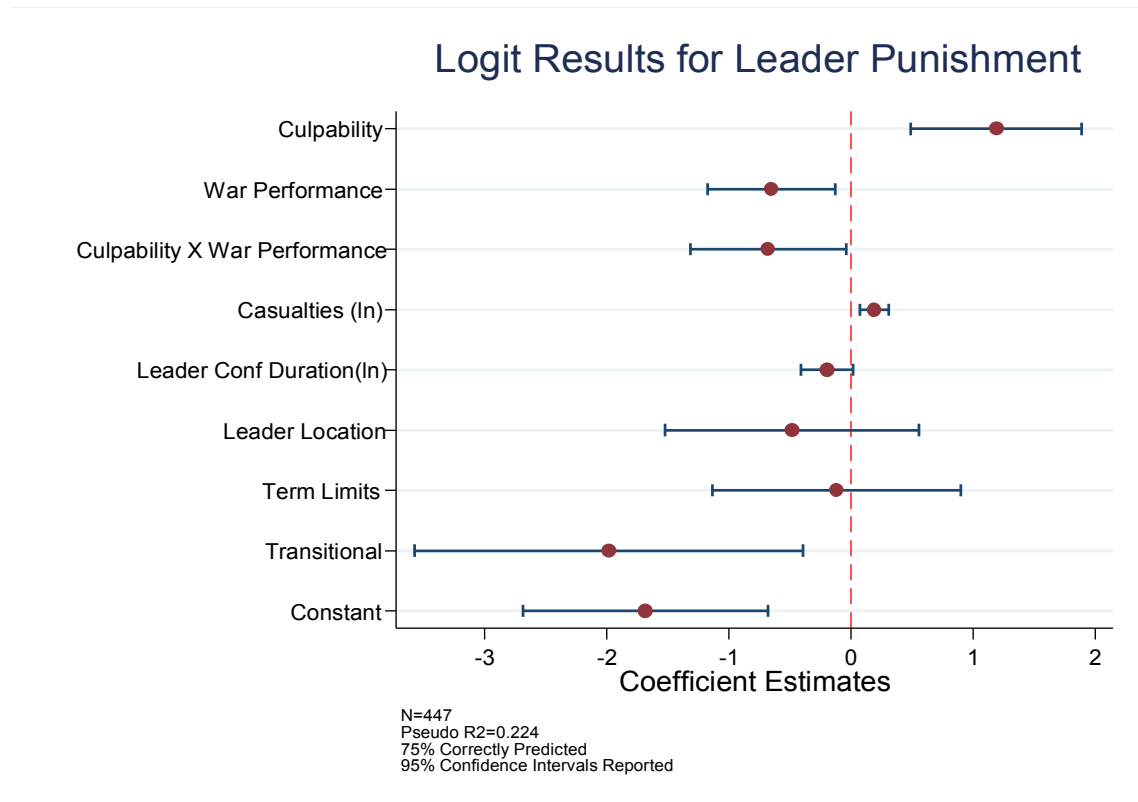
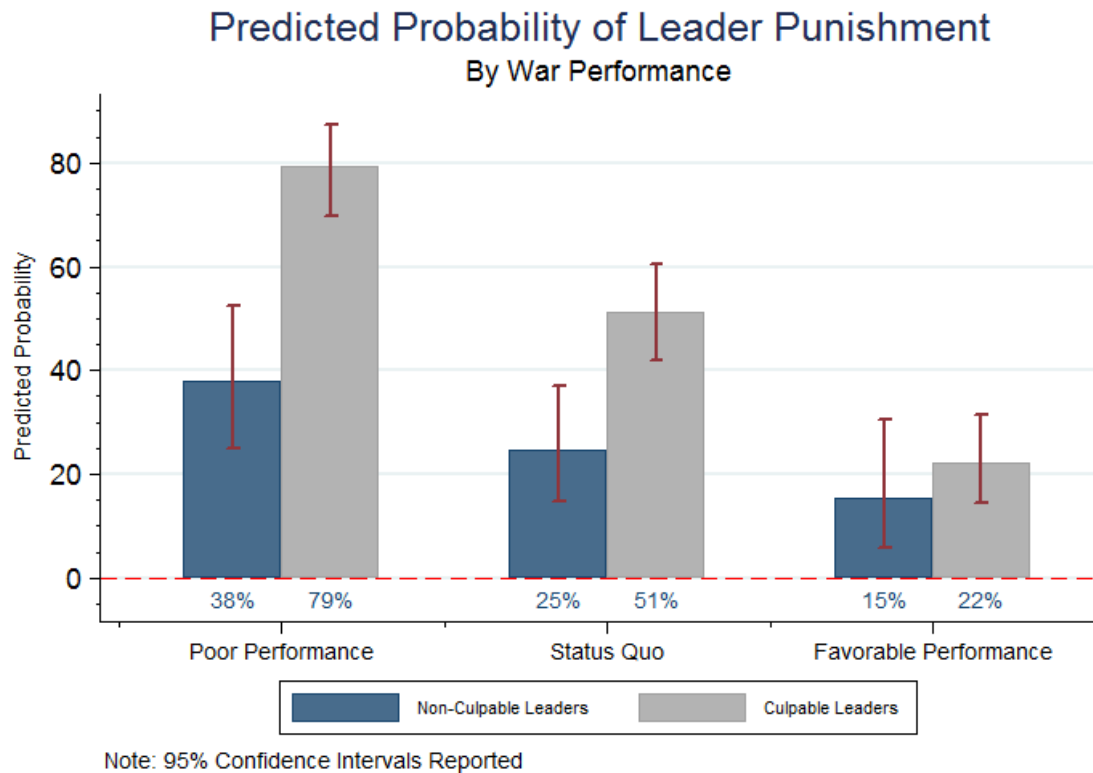


Figure 4.4 graphically presents the predicted probability of leader punishment for culpable versus non-culpable leaders, conditional upon war performance. As this figure demonstrates, culpability increases the likelihood of punishment, regardless of the group's fortunes on the battlefield. The probability of punishment increases from 38 percent to 79 percent when moving from non-culpable to culpable for leaders who suffer setbacks on the battlefield, from 25 to 51 percent when moving from non-culpable to culpable for leaders who maintain the status quo, and from 15 to 22

percent when moving from non-culpable to culpable for leaders who make battlefield progress. These results also demonstrate that the likelihood of punishment decreases as war performance improves for both culpable and non-culpable leaders.

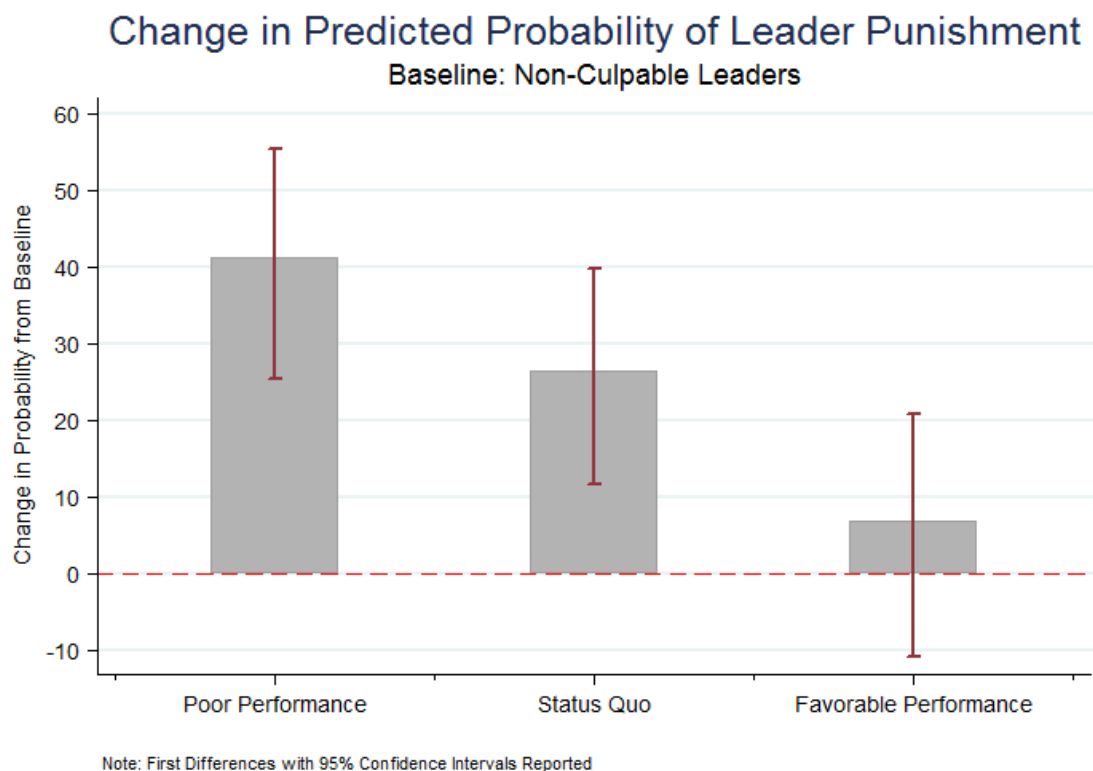
**Figure 4.4 Predicted Probability of Leader Punishment**



The first differences presented in Figure 4.5 provide further evidence in support of Hypothesis 4. This figure presents the change in the predicted probability of leader punishment for non-culpable versus culpable leaders. It clearly demonstrates that culpability significantly increases the likelihood of punishment for leaders who suffer setbacks on the battlefield *and* for those who fail to make battlefield progress (i.e. maintain the status quo). Culpability increases the likelihood

of punishment for leaders who suffer poor war performance by 41 percentage points, or approximately 108 percent, while increasing the probability of punishment by 27 percentage points (also 108 percent) for leaders who experience status quo outcomes.

**Figure 4.5 Change in Predicted Probability of Leader Punishment**



As the far right-hand side of Figure 4.5 demonstrates, on the other hand, culpability has no significant impact on the likelihood of punishment for leaders who are able to achieve battlefield successes. All leaders who preside over favorable war performances/outcomes have a relatively low probability of punishment, and while

successful culpable leaders are 7 percentage points, or about 46 percent, more likely to suffer punishment than their non-culpable counterparts, this change is not significant. Thus, as predicted by H4, culpability significantly increases the probability of punishment for leaders who fail to perform favorably in war, but has no significant impact on punishment for leaders who are successful in conflict.

Overall, these results provide strong support for Hypothesis 4. As expected, culpable leaders who are unable to achieve favorable outcomes or make battlefield progress are significantly more likely to face punishment than non-culpable leaders who perform similarly poorly in war. Leaders who achieve battlefield successes, on the other hand, have a relatively low probability of punishment, regardless of whether they are culpable for starting the conflict or not. By confirming the underlying mechanism developed in Chapter 2, furthermore, these results also provide additional support for the relationship between leader culpability and war outcomes. By demonstrating that culpable leaders are, in fact, more likely to face punishment for poor war performances, these results support the argument in Chapter 2 that culpable leaders are likely to be more sensitive to the threat of punishment, which, in turn, affects their war termination decisions.

### **3.2 Control Variables**

The control variables included in the model discussed above also generally behave as expected. The direction of influence for all controls is as expected; increasing battle deaths (i.e. costs of war) increases the likelihood of punishment, while leader tenure, leader location, term limits, and transitional status all reduce the

likelihood that a leader faces war-related punishment. Battle deaths and transitional status, however, are the only two control variables that have a statistically significant impact on the likelihood of punishment.

**Table 4.1 Predicted Probabilities and Percentage Change for Control Variables**

	Predicted Probability of Political Concessions		First Difference	Percentage Change
	Low	High		
Battle Deaths (ln)	20.52	31.15	10.63 (3.72, 18.64)	51.82
Leader Wartime Tenure (ln)	28.42	21.43	-6.99 (-15.73, 0.53)	-24.59
Leader Location	24.57	17.99	-6.58 (-20.1, 11.01)	-26.79
Term Limits	24.57	23.33	-1.23 (-17.63, 18.21)	-5.01
Transitional Leader	24.57	5.45	-19.12 (-32.15, -6.21)	-77.81
Note: To estimate the predicted probability, the covariate of interest is moved from low to high (25 <sup>th</sup> to 75 <sup>th</sup> percentile for continuous variables) while holding all other covariates at mean or modal values. First differences are computed by subtracting the baseline predicted probability (covariate at low value) from the predicted probability following the change in the covariate of interest. I then divide the discrete change by the baseline probability and multiply by 100 to get the percentage change. Ninety-five percent confidence intervals are presented in parentheses.				

Substantive results for the control variables are presented in Table 4.1.

Moving from low to high battle deaths reduces the likelihood of punishment by approximately 11 percentage points, or just over 51 percent. Being designated a transitional leader, on the other hand, reduces the likelihood of punishment by 19 percentage points, or over 77 percent. The effects of leader tenure, leader location, and term limits are smaller, decreasing the likelihood of punishment by

approximately 25, 27, and 5 percent, respectively. While it is difficult to directly compare the substantive impact of variables, particularly those measured on different scales, these results suggest that culpability's conditional impact on war outcomes is non-trivial; culpability has a significant and substantively meaningful impact on the likelihood of punishment, and produces an effect on par with or greater than the majority of control variables used to account for other determinants of leader punishment.

#### **IV. Robustness Checks**

This section includes a variety of robustness checks designed to ensure that the results presented above are robust to variations in the dependent variable, independent variables, and sample selection. As all models are non-linear and include interaction terms, I forego presentation of coefficient estimates, instead presenting first differences for each model, as these provide a clear interpretation of the statistical significance and substantive impact of culpability, conditional on leader war performance.

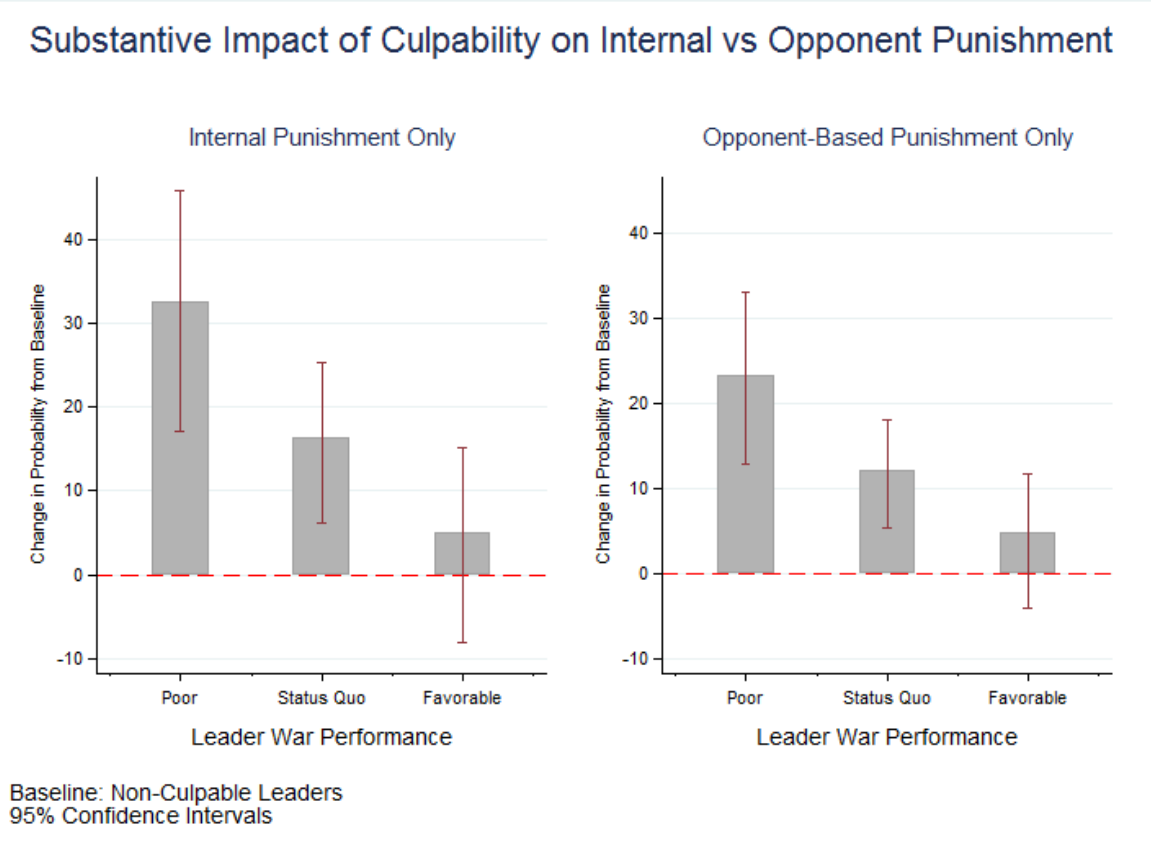
##### **4.1 Sources of Punishment**

The first robustness check accounts for the possibility that the relationship between culpability and punishment is driven by punishment from one particular source, or audience. That is, punishment by winning coalition members may be influenced by culpability and war performance while opponent-executed punishment is not, or vice versa. To demonstrate the importance of accounting for both sources



of punishment, and to show that culpability influences both, Figure 4.6 presents the changes in predicted probabilities of internal and opponent-based punishment separately, rather than pooled together as in the primary results presented above.

**Figure 4.6 Substantive Impact of Culpability on Internal vs. Opponent-Based Punishment**



The left-hand side of Figure 4.6 presents the change in the predicted probability of *internal* punishment when moving from a non-culpable to a culpable leader, at different levels of war performance. The right-hand side of Figure 4.6, on the other hand, provides the change in the predicted probability of *opponent-based*

punishment when moving from a non-culpable to a culpable leader, conditional on war performance.

As demonstrated in Figure 4.6, culpability significantly increases the likelihood of both internal and opponent-based punishment for leaders who either perform poorly in war or who fail to make substantial gains (i.e. those who achieve only the status quo). As expected, on the other hand, culpability has no significant effect on the likelihood of punishment for leaders who achieve battlefield successes, as evidenced by the confidence interval crossing zero in the far right-hand bar of each graph. Further, while the first differences for internal punishment are somewhat larger than those for opponent-based punishment, the baseline probability of opponent-based punishment is also smaller (8 percent baseline probability versus 12 percent baseline probability of internal punishment). Therefore, while the first differences are slightly smaller, the percentage change in the probability of opponent-based punishment is on par with that for internal punishment. These results indicate, therefore, that culpability's impact is consistent across the two sources of punishment, that the main statistical results discussed above are not driven by one source or the other, and that it is important to account for both types of punishment in leader-based theories of civil conflict dynamics and outcome.

## **4.2 Types of Punishment**

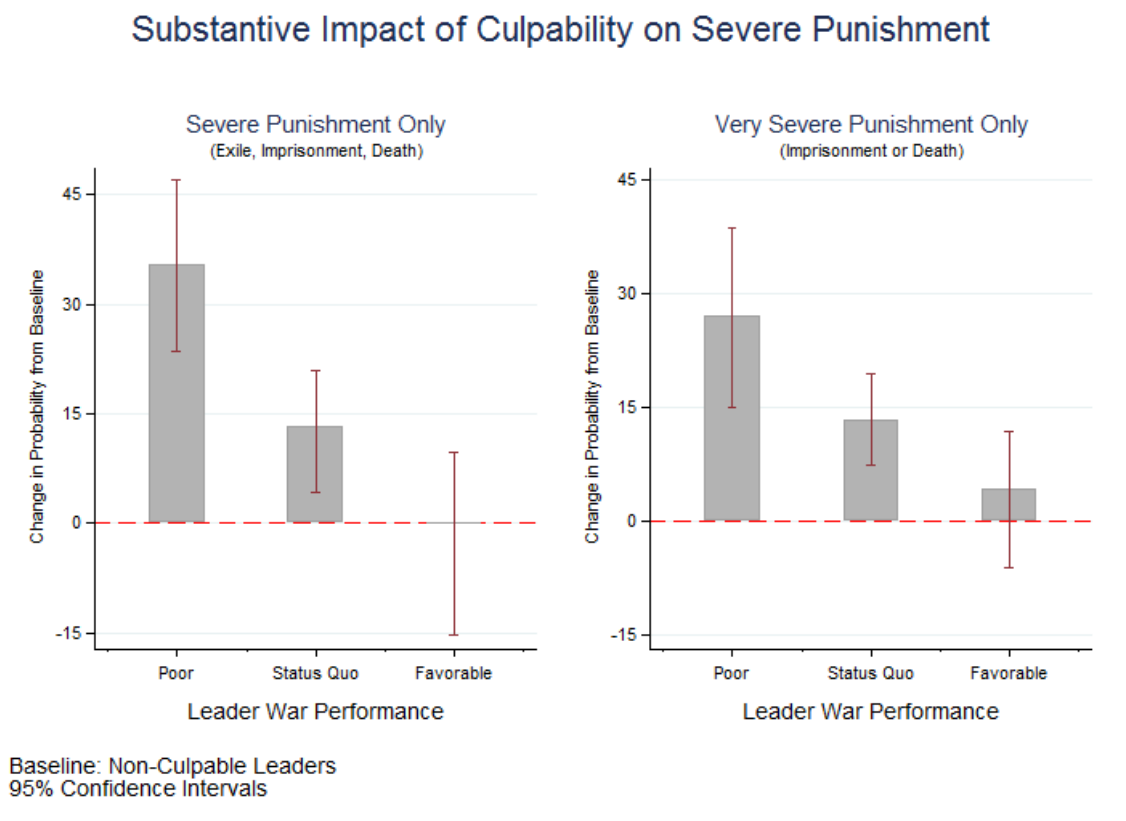
The second set of robustness checks disaggregates different types or severities of punishment in the measurement of the dependent variable. These robustness checks, therefore, account for the possibility that only some types of punishment are

influenced by culpability. It may be the case, for example, that culpable leaders are more likely to face loss of political office than non-culpable leaders, but that more severe forms of punishment, including exile, imprisonment, and death, are simply driven by war performance rather than culpability. That is, severe punishment may be no more likely for poor-performance, culpable leaders than it is for poor-performance, non-culpable leaders.

To test for this possibility, Figure 4.7 presents the results of two models that focus only on the more severe types of punishment. First, the left-hand graph in Figure 4.7 presents the change in the predicted probability of *severe* punishment (i.e. exile, imprisonment, or death) for culpable versus non-culpable leaders, conditional on war performance. The right-hand side of Figure 4.7 presents the change in the predicted probability of *very severe* punishment (i.e. imprisonment or death only) when moving from non-culpable to culpable leaders, conditional on their war performance.

As these graphs demonstrate, culpability's impact on severe forms of punishment is similar to its effect on punishment in general; whether one focuses on exile, imprisonment and death (left-hand graph), or just the latter two categories (right-hand graph), culpability significantly increases the likelihood of severe punishment for leaders who fail to achieve battlefield successes, while having no significant impact on the likelihood of severe punishment for leaders who perform favorably in the war. These results confirm that culpability's impact on punishment is not limited to relatively benign types.

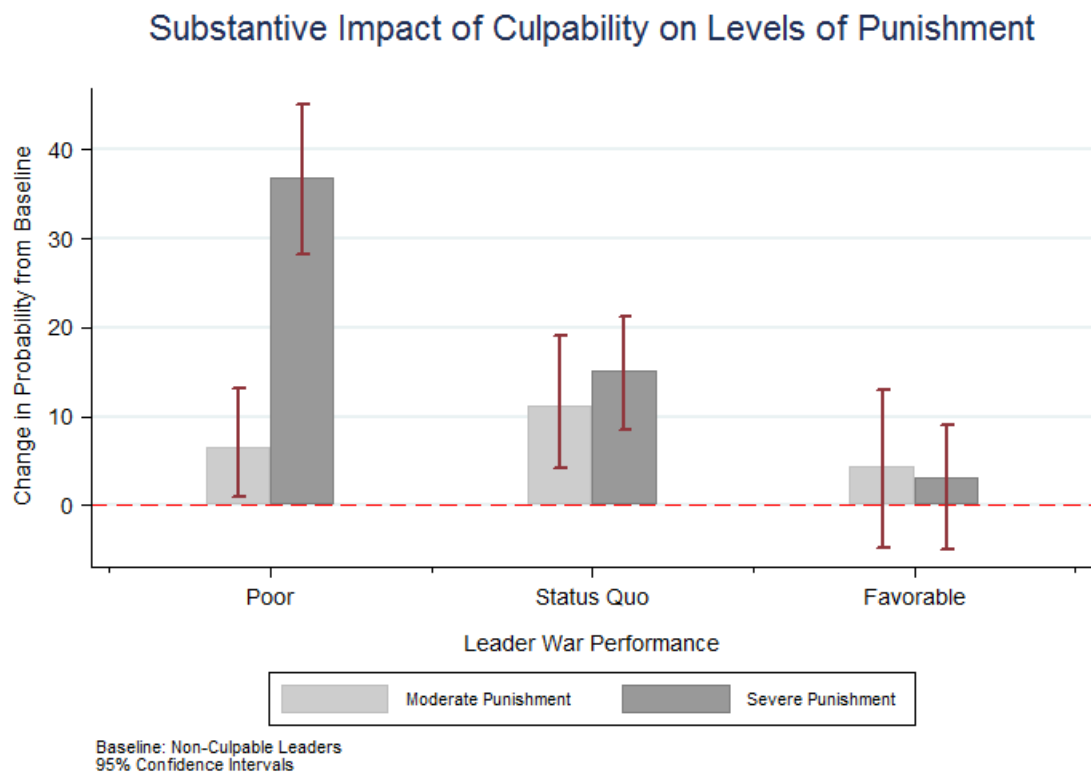
**Figure 4.7 Substantive Impact of Culpability on Severe Punishment**



To create a more complete picture of the potential divergent effects of culpability on different severities of punishment, Figure 4.8 below presents the results of an ordered logit model in which the dependent variable is a three-category variable coded 0 if the leader is not punished, 1 if the leader faces *moderate* punishment (i.e. group fractionalization or loss of political power/office), and 2 if the leader suffers *severe* punishment (i.e. exile, imprisonment, or death).<sup>53</sup>

<sup>53</sup> Ordered logistic regression models assume proportional odds across response categories. Violating this assumption, also called the parallel regression assumption, can lead to biased results. It is therefore necessary to test for violation of the proportional odds assumption. A likelihood ratio test of the proportionality of odds for the entire model produces a  $\chi^2=5.31$  with a p value of 0.38, indicating no violation. Further, a Brant test, which tests the parallel regression assumption for

**Figure 4.8 Substantive Impact of Culpability on Levels of Punishment**



The first differences presented in Figure 4.8 indicate that culpability significantly increases the likelihood of both moderate and severe punishment for leaders who either perform poorly in the war or who manage only to maintain the status quo. Interestingly, culpability has a substantively large impact on the likelihood of *severe* punishment for leaders who perform poorly in the war, while having a much smaller, though still statistically significant, influence on the likelihood of moderate punishment. Culpability actually has a larger impact on

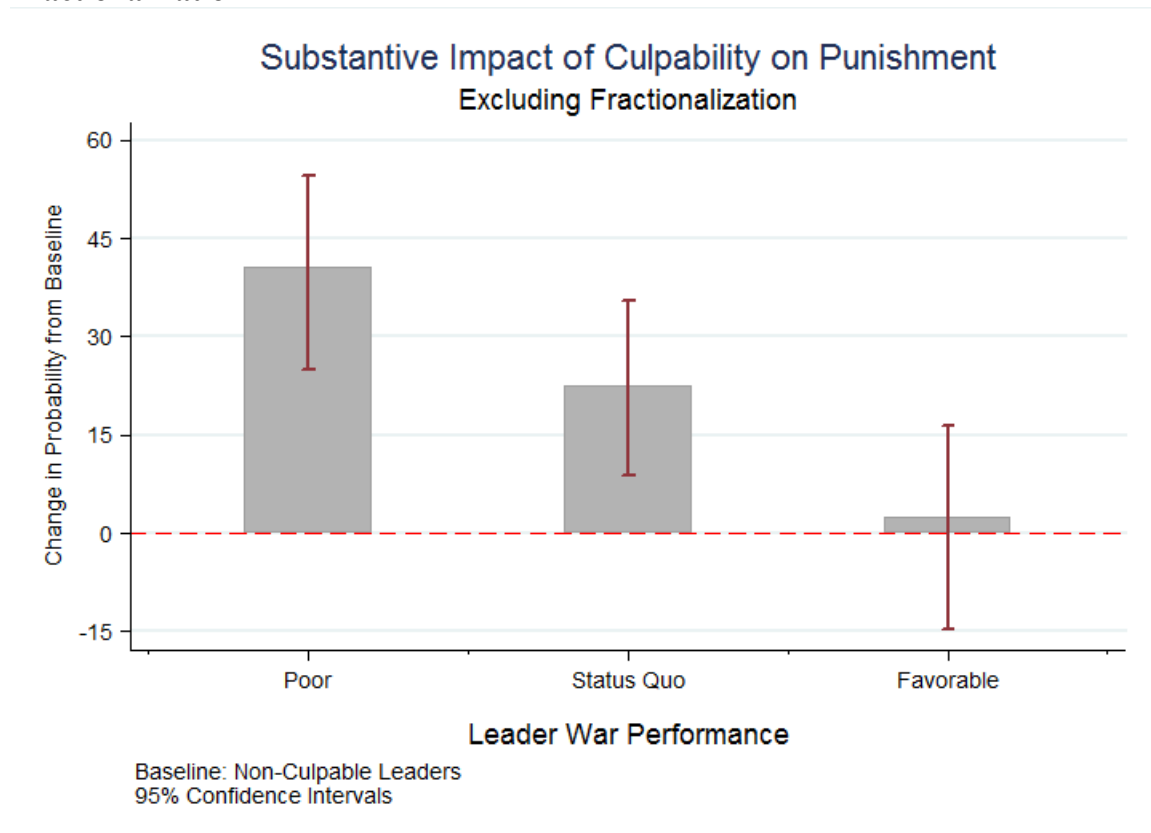
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each variable individually, produces no significant test statistics, indicating that none of the variables in the model violate the assumption.

*moderate* punishment for status quo leader than for poor-performance leaders, while the opposite is true for culpability's impact on *severe* punishment. Importantly, as hypothesized in H4, culpability has no significant influence on the likelihood of either moderate or severe punishment when leaders perform favorably in the conflict.

Finally, to ensure that the results are robust to excluding fractionalization as a form of punishment, I recode the dependent variable to equal one for leaders who lose power, are exiled, imprisoned, or killed as a result of the war, and zero otherwise. This variable specification thus excludes fractionalization as a form of punishment, recoding leaders who suffer this fate as unpunished. The substantive results for the logit model run on this dependent variable are presented in Figure 4.9.

**Figure 4.9 Substantive Impact of Culpability on Punishment, Excluding Fractionalization**



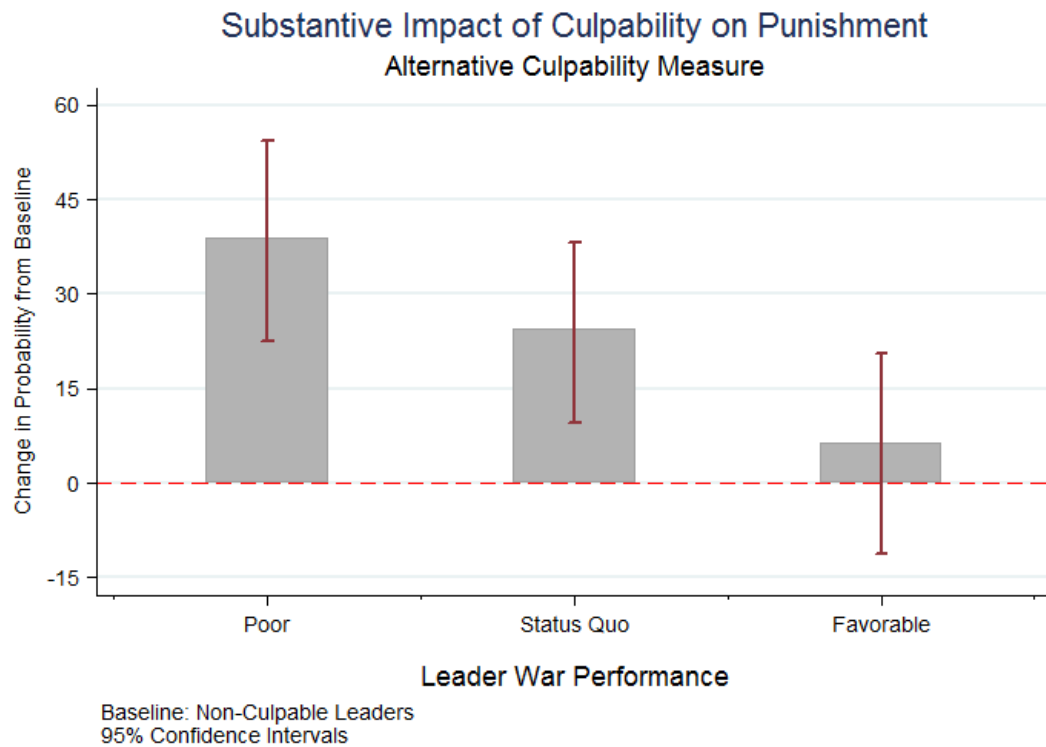
As this graph demonstrates, the results are robust to this change in specification; culpability continues to significantly increase the likelihood of punishment for both poor and status quo performers, while having no significant impact on the likelihood of punishment for leaders who perform favorably in war.

Taken together, these results indicate that the relationship between culpability and leader punishment is not driven by one particular type of punishment. Culpability significantly increases the likelihood of both moderate and severe punishment when leaders are unable to achieve gains on the battlefield during their tenure. Additionally, the results remain consistent when recoding the dependent variable to exclude fractionalization as a form of punishment. These robustness checks indicate, as a whole, that the results are robust to changes in specification of the dependent variable.

#### **4.3 Alternate Measures of War Performance and Culpability**

In this section, I present robustness checks using alternative measures of the key independent variables. First, Figure 4.10 presents result of a model run with an alternative measure of leader culpability. This alternative variable is coded one for all first leaders and for replacement leaders who share political/familial connections with the first leader, as with the original culpability variable, but does not adjust for the regime type of the first leader. That is, it does not code replacement leaders who follow personalist first leaders as non-culpable. Chapter 2 provides additional details on this alternative culpability measure.

**Figure 4.10 Impact of Culpability on Punishment, Alternative Culpability Measure**



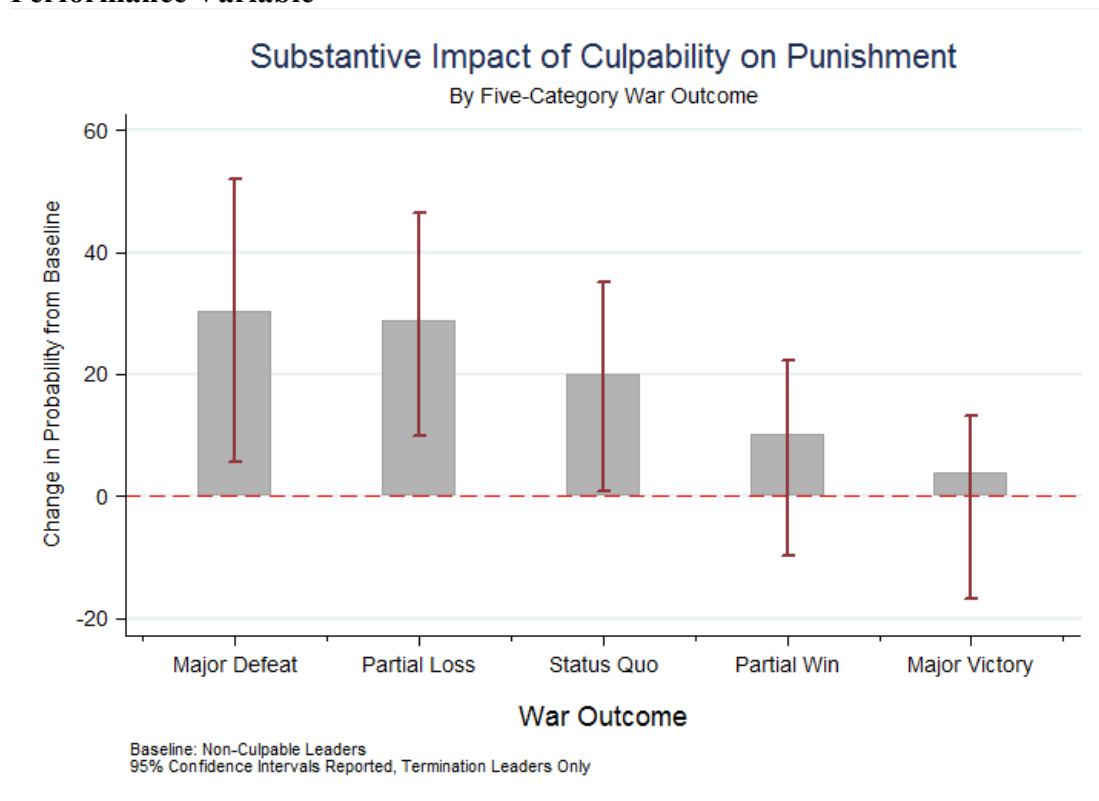
As demonstrated by the first differences presented in Figure 4.10, the results remain robust to this change in specification of culpability. Culpable leaders who perform poorly or maintain the status quo are significantly more likely to be punished than non-culpable leaders who experience the same outcomes. Culpability has no significant influence, on the other hand, for leaders who make progress on the battlefield.

In the second variation on the key independent variables, I use an alternate measure of war performance, based on the war outcome scale discussed in Chapter 3. Specifically, this variable measures war outcome on a 5-point scale from major defeat to major victory. This variable is measured only for leaders in power at war



termination, and the model therefore is run on only termination leaders, with an N of 207 instead of the 447 observations included in all other models. Despite this limitation, the results remain supportive of the hypothesized relationship between culpability and punishment. As demonstrated in Figure 4.11, culpability significantly increases the likelihood of punishment for leaders who suffer either major defeats or partial losses, as well as for those leaders who manage only to maintain the status quo. Culpability has no significant impact, on the other hand, for leaders who are able to achieve favorable outcomes – either partial or major victory – in their respective conflicts. These results, therefore, confirm those using the three-category war performance variable.

**Figure 4.11 Impact of Culpability on Punishment, Five-Category War Performance Variable**



#### 4.4 Accounting for Potential Differences between State and Rebel Leaders

Finally, it is important to confirm that the results are not driven by either rebel or state leaders exclusively. That is, does the relationship between culpability and punishment hold for both state and rebel leaders, or is one group of culpable leaders less susceptible to punishment than the other?

**Figure 4.12 Substantive Impact of Culpability on State vs. Rebel Leader Punishment**

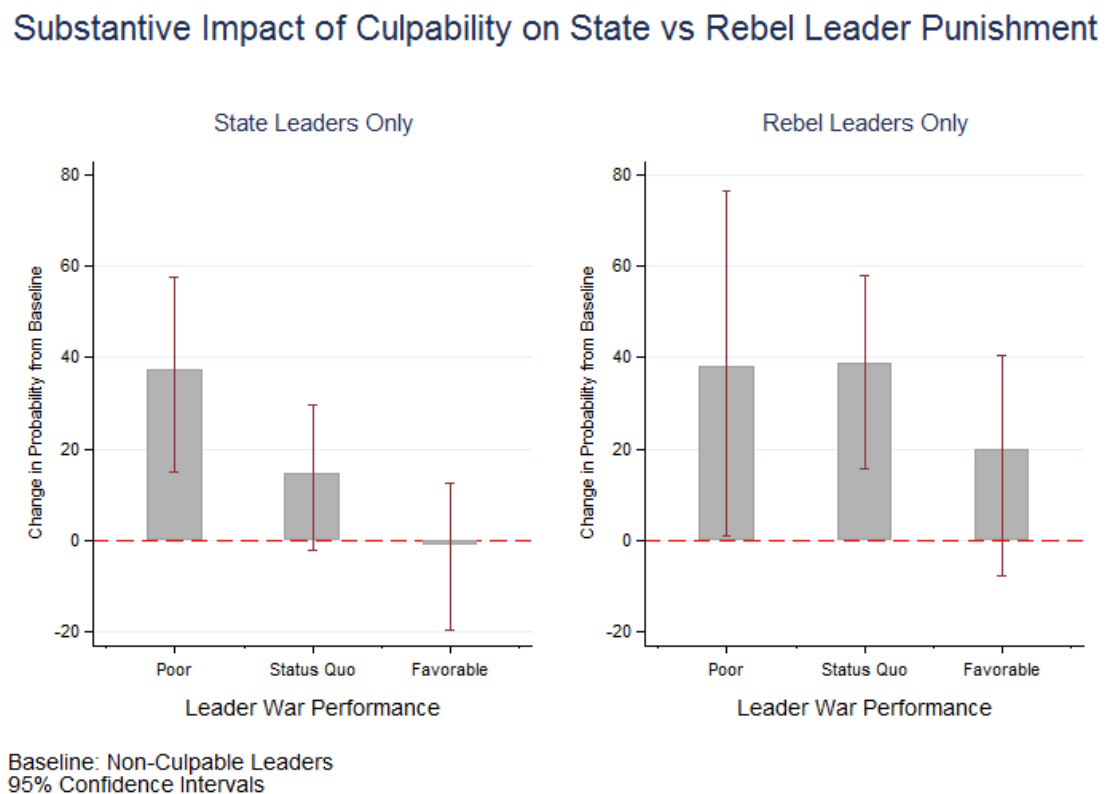


Figure 4.12 provides the substantive results for two separate models, the first run on state leaders only, and the second run on rebel leaders only. As expected, both

culpable state and culpable rebel leaders are significantly more likely to face punishment than their non-culpable counterparts *if* they suffer battlefield losses during their war-time tenure. The change in the predicted probability of punishment for poor-performance leaders is significant in both models. Additionally, the size of the change in the likelihood of punishment is similar for both rebel and state leaders who suffer battlefield losses, with just under a 40 percentage point increase in the likelihood of punishment for both groups when moving from non-culpable to culpable leaders.

The impact of culpability for rebel and state leaders who maintain the battlefield status quo is also largely confirmatory of the expected results, though with some minor exceptions. Culpability has a substantively large and statistically significant impact on the likelihood of punishment for rebel leaders who maintain the status quo. Culpability's impact among state leaders with status quo performances, on the other hand, is less strong; culpability increases the likelihood of leader punishment among this group, but the effect just misses statistical significance at a 0.05 level. This suggests that state leaders who maintain the status quo may be less susceptible to punishment than rebel leaders who perform similarly in war; state leaders may be better insulated from internal removal or attempts at punishment by the opponent under these circumstances, whereas all rebel leaders who fail to make battlefield gains, whether they suffer losses or simply maintain the status quo, are susceptible to internal removal and opponent decapitation attempts. Finally, in line with the main results and H4, culpability has no significant impact on the likelihood of punishment for either rebel or state leaders who make progress on the battlefield.

Overall, these results largely confirm H4 for both rebel and state leaders, while suggesting one potential caveat to that relationship. Specifically, rebel leaders may be more susceptible to punishment for status quo outcomes than state leaders. It is important to note, however, that the split-sample results are based on a relatively low-N, and should therefore be treated as suggestive rather than confirmatory.

## **V. Conclusion**

The results presented in this chapter provide strong support for the theoretical mechanism developed in Chapter 2. Statistical tests using original data on rebel and state leaders, the types and sources of punishment they face as a result of war, and the favorability of their war performances demonstrate that a culpable leader who fails to make progress in war is significantly more likely to be punished than a non-culpable leader who performs similarly poorly in war. In other words, culpability's conditional impact on leader punishment is both significant and substantively large.

These results, furthermore, are robust to a variety of alternative variable and model specifications. Culpability significantly increases the likelihood of both internal punishment and punishment by the opponent, it has a significant effect on both severe punishment and more moderate forms of punishment, and remains a significant predictor of punishment when using alternative measures of culpability

and war performance. Finally, the results are largely consistent for both rebel and state leaders, indicating that culpability's impact is not limited to one type of leader.<sup>54</sup>

These results provide additional evidence in support of the theoretical argument developed in Chapter 2. They confirm that culpable leaders are more often punished than non-culpable leaders for poor war performance. These results thus highlight an important source of variation in the expectations of leaders during civil war, and in so doing, provide support for the argument that culpable leaders will behave differently in civil war than non-culpable leaders. Culpable leaders should, because of the heightened risk of punishment, be more likely to fight longer and experience extreme outcomes than non-culpable leaders, and should also be less willing to make concessions to end a conflict, out of fear that doing so will invite punishment. The next chapter delves further into the mechanisms underlying the theoretical argument through qualitative analysis of the civil war in Angola between 1975 and 2002.

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<sup>54</sup> An additional robustness check is included in Appendix B, in which I test the possibility that culpability's impact on leader punishment is conditional on regime type.

## Chapter 5 : Leader Incentives and the Angolan Civil War

### I. Introduction

The quantitative analysis presented in chapter 3 demonstrated that leader culpability significantly influences civil war termination and outcomes, providing strong statistical support for the hypotheses developed in chapter 2. These results are limited, however, in that they only confirm that culpability is consistently associated with war termination and outcomes, saying nothing about *why* culpable leaders behave differently than non-culpable leaders. Chapter 4 provided additional quantitative evidence as a first step toward addressing this question about the causal relationship between culpability and wartime behavior, showing that culpability also influences the likelihood that leaders face punishment for poor war performance. In this chapter, I supplement the statistical findings from the previous chapter by undertaking a more detailed, qualitative analysis of an individual case in order to determine whether the desire to avoid punishment is, in fact, the mechanism causing culpable leaders to prolong conflicts, gamble for resurrection, and avoid making concessions to their wartime adversaries.

Specifically, this chapter examines the civil war in Angola between government forces and the National Union for the Total Independence of Angola (UNITA) from 1975 to 2002. It focuses, in particular, on factors influencing UNITA's decision-making during four major settlement attempts across two distinct

phases of the conflict. I chose the Angolan case from the 102 civil conflict dyads in the dataset because the change in UNITA's leadership in February 2002 provides a critical node, allowing for a controlled, within-case comparison of the periods before and after the death of UNITA's first leader, Jonas Savimbi. This node thus distinguishes between the period in which a culpable leader headed UNITA and that in which a non-culpable leader, Paulo Lukamba Gato, led the rebel organization. The UNITA case provides an ideal venue for within-case comparison, because major settlement attempts were made both during Savimbi's tenure and after his death. The fact that serious attempts at negotiated settlement occurred during both periods allows me to comparatively analyze the differences between the settlement processes with a culpable versus a non-culpable rebel leader.

This before and after research design, also termed diachronic comparative analysis (Lijphart 1971), maximizes comparability across units by controlling for a variety of factors, both observable and unobservable, that are consistent over time within a single case, in this instance the Angolan conflict. This type of within-case comparison generally offers greater comparability across units than comparison of two or more different cases, thus offering a viable solution to the control problem in comparative case analysis and allowing the researcher to more easily isolate the causal effects of the key independent variable (A. L. George and Bennett 2005; Lijphart 1971). Within-case comparison, however, cannot achieve total comparability, as a single country, group, or leader is not exactly the same over time. That is, more than one variable is likely to change at a time. I therefore supplement the within-case comparison with a detailed tracing of the negotiation/settlement

processes that occurred both before and after Savimbi's death. This process tracing helps identify the plausible causal chain that links leader culpability to threat of punishment to war termination (A. L. George and Bennett 2005, 177).

Practical considerations also led to the selection of UNITA's war with Angola. First, this conflict ended over a decade ago, in 2002. Other cases under consideration for analysis included the conflict in the Philippines between the state and the MILF rebel group. This conflict, however, is ongoing, so does not provide sufficient historical data to draw definite conclusions about the effects of leader culpability on war termination or outcome. Because the Angola civil war ended a decade ago, there has been enough time since the conflict's end to determine that the final agreement reached in 2002 did, in fact, settle the conflict. Second, the UNITA conflict was chosen because it provides an ideal test case for the leader culpability theory relative to important alternative explanations for civil war termination, most notably the economic incentives and commitment problems models of conflict termination.

This chapter proceeds as follows. I begin by providing a brief overview of the origins and early years of the Angolan civil war. I then discuss Savimbi's vulnerability to punishment, specifically examining both the internal and external threats to his tenure and physical safety. The fourth section assesses how those threats to Savimbi's life and leadership influenced his decision-making and the prospect for peace during three major settlement attempts in 1989, 1991, and 1994. The fifth section discusses Savimbi's downfall and the major shift in UNITA strategy and willingness to settle when a non-culpable leader, Gato, came to power. Sections



four and five thus use process tracing and controlled comparison of the periods before and after Jonas Savimbi's death in 2002 to identify culpability's causal impact on the dynamics of negotiation, settlement, and implementation of agreements, demonstrating the importance of leaders' expectations of punishment for the settlement of civil war. Section six addresses potential alternative explanations for the trajectory of the Angolan civil war, and the final section concludes.

## **II. Origins of the Angolan Conflict**

The Angolan civil war grew out of Angola's bloody independence struggle against Portugal, which lasted from 1961 until the fall of the Portuguese regime in 1974. During the conflict with Portugal, three primary armed groups emerged: the Popular Movement for the Liberation of Angola (MPLA), the National Front for the Liberation of Angola (FNLA), and UNITA. Unlike independence movements in neighboring Mozambique, these three groups failed to unify into a united opposition. While each espoused an Angolan nationalist platform, they drew support primarily from three distinct segments of Angolan society, so claims of representative nationalism were often viewed with suspicion. The MPLA's backing came primarily from the Mbundu, and its power was concentrated in the Angolan capital, Luanda. MPLA was largely urban, and composed mainly of mestizo and assimilado: Angolans who were educated and not marginalized in colonial society (Guimaraes 1998). The FNLA, on the other hand, drew its support from the Bakongo ethnic group, and was regionally based in the north, along the border with Congo. Finally, UNITA

developed in 1966 as a representative of the largest ethnic group in Angola, the Ovimbundu (Guimaraes 1998).

A combination of these ethnic differences, political disagreements (MPLA espoused a Soviet-style, single-party state, UNITA a socialist platform with multi-party democracy, and FNLA a capitalist economic/political program), and personal rivalries among the leaders of the three groups ensured that the MPLA, FNLA, and UNITA remained rivals throughout the anti-colonial struggle. At times, these inter-group rivalries even took precedence over the fight against Portugal, leading the three groups to undermine one another to improve their own prospects. In the so-called ‘Timber’ affair, for example, Savimbi allegedly struck a deal with Portuguese forces: in exchange for information on the MPLA, Portugal agreed not to attack UNITA forces in the Eastern zone and released Savimbi’s father from prison (Guimaraes 1998, 82).

Thus, as the colonial era came to a close, the MPLA, FNLA, and UNITA failed to create a united front for the transition to independence. The Alvor Accord of January 15, 1975 set the date for independence, recognized the MPLA, FNLA, and UNITA as “the sole legitimate representatives of the people of Angola”, and established that these three groups along with representatives of the departing colonial power should form a transitional government. However, prospects for a peaceful transition were dashed almost immediately. When Angola became independent on November 11, 1975, it essentially had three governments: the MPLA, with Cuban support, controlled the capital, Luanda, but little else, UNITA exerted authority in Huambo, Angola’s second largest city, and controlled several southern

provinces with South African assistance, and the FNLA ruled in the north, backed by Zairian forces (James 1992; Malaquias 2007). From this backdrop, the Alvor Accord and the transitional government it established were quickly done away with. The MPLA expelled FNLA and UNITA from Luanda, driving the country back into civil war (Malaquias 2010).

The FNLA suffered serious setbacks at the hands of MPLA almost right away, and partially as a result, lost the backing of its external allies, Zaire and the United States. FNLA was defeated by 1976, and the remnants of the group deserted founder and leader Holden Roberto. Roberto was forced into exile and remained there for 15 years, only returning to Angola in 1992 to run in the presidential elections, where he secured only 2 percent of the vote and finished a distant fourth (Agence France-Presse 2007).

This left UNITA as the only significant remaining opponent of the MPLA. Jonas Savimbi, who had started his insurgent career in the FNLA, and had attained the rank of Secretary General of the movement, as well as foreign minister for its government in exile (Guimaraes 1998), is likely to have taken keen notice of Roberto's demise as a lesson for his own political trajectory as, on the heels of the FNLA's defeat in 1976, MPLA forces almost defeated UNITA as well. The US pulled funding for UNITA after news of the CIA's covert support of the rebel organization was leaked to the New York Times in December, 1975 (Guimaraes 1998). The loss of US support left UNITA with only South African backing. The apartheid regime's pariah status worked against UNITA, tipping the balance at the Organization of African Unity (OAU) in favor of recognizing the MPLA regime.

South Africa subsequently negotiated with the MPLA government, and agreed to withdraw forces to within 50 miles of the Namibian border.

With the routing of FNLA forces, the loss of CIA funding, and the withdrawal of South African troops, MPLA fighters and their Cuban supporters were able to take the offensive against UNITA (James 1992). Between February and August 1976, Savimbi's forces were forced to retreat into remote corners of the country (Weigert 2011). Close to defeat, UNITA insurgents dispersed. Savimbi was not dissuaded, however. He regrouped his forces for a four-day conference on May 10, 1976, and issued the Cuanza River Manifesto, which reasserted UNITA's commitment to guerrilla struggle, stating that a protracted insurgency would eventually prevail over the MPLA and its Soviet and Cuban allies (James 1992, 105; Weigert 2011, 64). Thus, faced with imminent defeat, Savimbi remained obstinate. He regrouped and reimagined his forces, committing to a long and difficult guerrilla struggle rather than accepting defeat or seeking a settlement on unfavorable terms. Ultimately, this decision would result in 25 more years of armed conflict in Angola.

### **III. Internal and External Threats to Savimbi**

Jonas Malheiro Savimbi is one of the most influential figures in post-colonial Angolan history. Born to Ovimbundu parents in August, 1934, in Munhango, Moxico Province, Angola, Savimbi went on to study in Portugal in the 1950s. It is likely during his stay in Europe that Savimbi met Holden Roberto, who was already a rising figure in Angola's anti-colonial struggle, having founded the UPA, which later became the FNLA. Roberto reportedly recruited Savimbi to the anti-colonial cause,

and Savimbi decided to leave his studies to begin his career as a member of Roberto's rebel organization. Savimbi became Secretary General of the FNLA before eventually leaving the organization in 1966 over disagreements with Roberto. Upon his departure from FNLA, Savimbi founded UNITA, the organization which he would lead for 36 years.

### **3.1 Internal Challenges to Savimbi's Leadership**

Jonas Savimbi has at times been portrayed as a cult-like figure who ruled UNITA with an iron fist. While Savimbi was undoubtedly a charismatic leader, these depictions belie the true complexities of UNITA's inner workings, as well as the, at times, tenuous nature of Savimbi's position within the organization. Instead, UNITA's history is punctuated by periodic threats, both latent and more immediate, to Savimbi's leadership, beginning as early as the pre-independence period, during the insurgent organization's infancy.<sup>55</sup> As discussed in the previous chapters, threats to a war-time leader can take many forms, from electoral defeat to assassination. This section discusses the sources of internal threats faced by Jonas Savimbi, as well as the forms those threats took, throughout his 36 year tenure as President and Commander in Chief of UNITA.

Over the several decades of its operation in Angola, UNITA developed a relatively complex political organizational structure alongside and overlapping its

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<sup>55</sup> The fact that Savimbi has often been portrayed by international observers as a highly secure rebel leader, despite these threats, serves to highlight the importance of internal threats for the vast majority of rebel leaders who are often less secure in their organizations than Savimbi was.

military structure. At the national level, the party was led by a Central Committee, the top members of which constituted the Political Bureau. These bodies varied in size over the years, but in 1990 consisted of approximately 55 and 20 members, respectively (James 1992, 101). Below these national-level institutions, UNITA was organized into 22 regions led by Political Commisars, who generally served simultaneously as military commander and political head of that region. Below the regional Commisar came the District Committee, the Village Committee, and finally the Cell, made up of just a handful of individuals. Party Congresses were held approximately every four years, beginning in 1966 (Weigert 2011).<sup>56</sup> At these Congresses, all members of the Central Committee faced (re)election, including Savimbi (James 1992). Attendance at Part Congresses varied as the size of the organization fluctuated, from 45 delegates at the first congress in 1966 to a high of 3200 delegates representing all provinces of Angola at the 1991 Party Congress (James 2004). This rather broad representation, and the election process built into each party congress, suggests that Savimbi was, to some extent, dependent upon broad, popular support within the organization to retain his leadership position. That is, this institutional structure allowed for the possibility of electoral removal. In practice, however, Savimbi won re-election at every Party Congress beginning in 1966 (James 2004). Savimbi maintained relatively consistent support among low and mid-level UNITA members (i.e. the majority of voters at Party Congresses), even while his popularity waxed and waned in the higher echelons of the organization.

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<sup>56</sup> In addition to the regular Party Congresses, UNITA held three Extraordinary Congresses, and the Central Committee and Political Bureau held annual conferences to review progress and set policies for the next year.

The greater threats to Savimbi's leadership within the rebel organization stemmed from elite-level UNITA political and military leaders, with whom Savimbi often had serious political and strategy disagreements throughout the course of the conflict. Weigert (2011) notes that Savimbi's power was dependent upon fragile compromises between UNITA militants and moderates, whose "polarized positions frequently had resulted in direct challenges to Savimbi's leadership" (Weigert 2011, 6). These challenges began during the anti-colonial phase of the conflict, and continued, periodically intensifying, throughout the post-colonial civil war.

Further, the challenges emanating from the higher-echelons of UNITA's political/military organization were generally 'irregular' in nature, in that they involved means beyond the electoral mechanisms available for his removal. Specifically, during Savimbi's tenure as leader of UNITA, he faced assassination attempts from within the organization, defections of key members of the organization, and several instances in which splinter factions either arose or appeared imminent. While the threat posed by assassination attempts is quite clear, fractionalization and defections also posed a real danger to Savimbi, as they threatened to not only weaken the organization by dividing it, making it – and by extension Savimbi – more vulnerable to government attack, but also threatened Savimbi's legitimacy among remaining cadres in the rump UNITA organization.

The first attempt on Savimbi's life occurred in the organization's early years, and is likely to have colored Savimbi's subsequent approach to internal monitoring and security. In June 1967, just a year after establishing UNITA, Savimbi was arrested by Zambian officials and forced into exile in Egypt, where he remained until

June 1968. After a year in exile, Savimbi returned to an organization that had fragmented into three semi-autonomous groups and was suffering from serious weapons and supplies shortages, which undermined group morale and led some members of the organization to question his leadership. As a result of these early splits in the organization, Savimbi faced his first internal assassination attempt in September 1968, when three shots were fired at him by unnamed assailants (Weigert 2011, 37). Savimbi survived the attack, but his tenuous position was further weakened by the attempt on his life, and just one month later in November 1968, a top UNITA commander, Samuel Chyala, defected to the FNLA with 150 UNITA insurgents. This defection was followed by another just a few months later, when Tiago Sachilombo defected to the Portuguese in April 1969 (Weigert 2011, 38).

Subsequent alleged assassination plots were thwarted in 1972, 1982, 1986, and 1989. Jose Calundungo, a former FNLA commander who joined UNITA during the anti-colonial struggle, was imprisoned in 1972 for six months on charges of plotting to assassinate the UNITA head (Weigert 2011, 55). In 1982, several high-ranking UNITA members, including founding member Samuel Chiwale, foreign secretary Jorge Sangumbe, and chief of staff Valdemar Chindondo, were accused of attempting to overthrow Savimbi. They were publicly beaten, Chiwale was demoted, and the latter two were executed as a result. A third coup attempt was thwarted in 1986, when UNITA general Geraldo Nunda was arrested after challenging Savimbi's leadership as well as his ties to foreign leaders (Weigert 2011, 101). Finally, Tito Chingunji, UNITA's former Washington representative, was executed in August 1991 after being found guilty in February 1989 of "acts of high treason, including



efforts to oust Savimbi as UNITA's president either by 'defaming him abroad' or by 'attempting to poison him in Jamba'" (Weigert 2011, 108–9). While the true extent of the threat posed by each of these plots is uncertain, it is clear that Savimbi took the threats seriously, responding harshly to specific instigators of coup attempts against him.

These alleged coup attempts were symptomatic of a growing discord within UNITA, which escalated in the late 1980s as the group entered negotiations for the first time, and remained a serious problem for Savimbi throughout the remainder of his tenure. During party congresses as early as 1982, delegates expressed their readiness to negotiate peace with the government. This was reaffirmed at each subsequent party congress until the end of the conflict (James 2004). This growing majority consensus around negotiations and peace, however, was at odds with a strong group of veteran militants within the organization who advocated continued military struggle. These divisions became more noticeable to the outside world beginning in 1988, when younger party members, linked to UNITA foreign secretary 'Tito' Chingungi, pushed for a negotiated settlement while Savimbi advocated continued fighting. Savimbi dealt with this challenge to his leadership harshly, recalling Chingungi to UNITA headquarters and executing him for advocating peace talks on terms not accepted by Savimbi (Weigert 2011, 101).

This event in the late 1980s was followed by increasing dissension as UNITA negotiated the Bicesse and Lusaka accords in the early 1990s. Several high-ranking members of the organization defected throughout this period, sometimes taking low and mid-ranking members with them. In March 1992, in the lead up to elections

established in the Bicesse Accord, two top UNITA ministers, Tony Fernandes and Miguel Puna fled party headquarters in Jamba and announced their formal defection. These key defections were followed by additional desertions by a spat of high-ranking UNITA military commanders, who then joined the ranks of the newly-created FAA. This instability at the top of the organization, along with recent brutal executions of alleged coup plotters, served to further divide UNITA's ranks. Analysts suggest that these divisions played a key role in Savimbi's electoral loss just months later in September 1992 (Weigert 2011).<sup>57</sup>

Savimbi's internal position remained tenuous throughout the remainder of the decade. The failure of the Bicesse Accords and Savimbi's electoral loss seriously undermined the aura of power and control that the rebel leader had carefully cultivated over the past three decades. Deep fissures within UNITA again became apparent leading up to and after the signing of the Lusaka Protocol in 1994, whose terms were not as favorable to the rebel organization as the Bicesse Accords had been. This ultimately manifested, in 1998, with serious splits in the organization. Five UNITA officials led by Jorge Valentim and Eugenio Manuvakola denounced Savimbi and called for an interim replacement President until the next party congress. These officials named their movement UNITA-Renovada (UNITA-R), a faction which ultimately drew relatively little support from the rank and file, but did succeed in instigating other breaks in the rebel organization. By October 1998, it became clear that Savimbi no longer controlled the segment of the organization that had

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<sup>57</sup> The defections of military leaders from UNITA to the FAA also threatened Savimbi because these former UNITA members had intimate knowledge of Savimbi's military strategies and tactics, and within a few months were leading government forces against him.

embraced parliamentary positions provided in the peace agreement. Abel Chivukuvuku, head of UNITA's seated parliamentarians, refused to acknowledge any allegiance to Savimbi, indicating that the organization may have split into three distinct factions (Weigert 2011, 137).

### **3.2 Opposition-Based Challenges to Savimbi's Leadership**

Internal challenges were not the only, nor at times the most imminent, threat to the UNITA leader. From the earliest stages of the Angolan civil war, a major component of the MPLA's strategy involved targeting Savimbi the individual, based upon the expectation that decapitating the organization – i.e. removing its symbolic and operational head – would render it weak, ineffective, and easily defeated. This strategy, importantly, was not private information. As early as November 1978, in a speech given on the third anniversary of Angola's independence, President Neto publicly indicated that the MPLA's strategy for dealing with UNITA was to continue the war with the aim of capturing Savimbi (James 1992, 134).

The MPLA government implemented this strategy in two ways; the first military and the second political. First, FAPLA forces and Angolan police forces made several attempts upon Savimbi's life throughout the course of the war. Second, the MPLA leadership consistently excluded Savimbi from proposed amnesty programs that would allow the rest of the rebel organization to reintegrate into Angolan society without fear of prosecution.

Reported attempts by MPLA forces to capture or kill Savimbi surfaced in the early stages of the conflict. Angola's official news agency, ANGOP, for example,

reported that MPLA forces had been close to capturing Savimbi in late March 1978, and that the UNITA leader had only escaped because South African soldiers were able to reach him before MPLA units did (James 1992, 134). Similar reports arose in 1984 and 1990; in the former case, ANGOP reported that Savimbi had been killed during a raid by FAPLA forces on Jamba, the Free Angola capital established by UNITA. The statement, which was denied by UNITA representatives, turned out to be false. A similar report by ANGOP in 1990 indicated that Savimbi had been wounded in a FAPLA bombing attack on Jamba (James 1992, 134–5). While this report also turned out to be false, it suggests that targeting Savimbi personally remained an important element of MPLA’s military strategy.

Savimbi-centered attacks were not limited to the military forces. In 1992, the so-called ‘Ninja’ element of the police force (i.e. a newly created riot police unit made up of demobilized FAPLA commandos following the Bicesse Accords) was implicated in an alleged MPLA plot to assassinate Savimbi. A scuffle between police and UNITA personnel outside of Savimbi’s Huambo residence in August 1992 reaffirmed fears that the MPLA intended to decapitate the rebel organization prior to elections scheduled for late 1992 (Weigert 2011, 107).

The MPLA even armed their civilian supporters in Luanda, which served to undermine Savimbi’s security. Members of several Angolan political parties voiced concerns to UNAVEM II officials in early 1992 regarding the proliferation of weapons among pro-government civilians, particularly in Luanda, an MPLA stronghold (Anstee 1996, 55; also see Weigert 2011, 107). Despite UN pressure to disarm the civilian population, Luanda largely failed to do so. This remained a

sticking point after the failure of Bicesse and during attempts to implement the Lusaka Protocol, as UNITA officials continually denounced the UN's failure to disarm pro-MPLA civilians and stop persistent violence against UNITA members and supporters (Weigert 2011, 135).

Politically, the MPLA's strategy consistently implied a zero-tolerance stance towards Savimbi. As early as the mid 1980s, government officials proposed an amnesty program for UNITA, including members of the group's leadership, but explicitly excluded the option of amnesty for Savimbi himself (James 1992). This policy was echoed during the Gbadolite negotiations in June, 1989 and ultimately undermined the negotiations. Savimbi's status continued to act as a stumbling block in negotiations throughout the 1990s. Attempts in early 1998 to revive the faltering Lusaka Protocol led the MPLA government to recognize Savimbi's 'special status' as leader of the opposition and to introduce legislation allowing Savimbi to retain a four-hundred-member bodyguard (Weigert 2011, 133), but these concessions were short-lived. By October 1998, the MPLA-dominated National Assembly revoked Savimbi's special status, and instead labeled him a 'war criminal' and issued a warrant for his arrest (El-Khawas and Ndumbe 2007, 111). This about-face, Weigert argues, "proved to be the crossing of a rhetorical Rubicon that eventually led to three more years of brutal hostilities" (Weigert 2011, 10).<sup>58</sup>

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<sup>58</sup> The MPLA's decision to brand Savimbi a war criminal was politically significant, given the international context in 1998. The Rome Statute establishing the ICC had just been finalized in June of that year, the International Criminal Tribunal for Rwanda's (ICTR) scope was expanded in April 1998 via UN Security Council resolution 1165, which created a third trial chamber, and the International Criminal Tribunal for the Former Yugoslavia (ICTY) was well underway in 1998, having

The multiple instances of explicit attempts by the MPLA to assassinate or capture Savimbi, as well as the MPLA's consistent, non-compromising political stance toward Savimbi, provide clear evidence that throughout the conflict, a core component of the MPLA's strategy was based on eliminating Savimbi as a prerequisite for defeating the UNITA threat. Evidence suggests, furthermore, that Savimbi took this threat seriously. He refused to attend the signing of the Lusaka Protocol in 1994, for example, citing concerns for his personal safety, and sent UNITA representative Eugenio Manuvakola in his stead (Weigert 2011, 121). During the tense post-Lusaka years, furthermore, Savimbi continually refused to take up his vice-presidential position or to move his residence to Luanda, again out of fear for his personal well-being.

#### **IV. UNITA Under Savimbi and the Failure of Settlement Attempts**

The core questions explored in this section focus on whether, and how, the internal and opponent-based threats to Savimbi's life and leadership discussed above influenced the course of the civil war in Angola. It examines the role that threats of internal and external punishment had on Savimbi's decision-making during each major settlement attempt during his tenure as rebel leader in the Angolan civil war. This analysis demonstrates that Savimbi's vulnerability to punishment played an important role at key junctures in undermining attempts to both reach agreement (at Gbadolite) and to implement settlements already agreed upon (Bicesse and Lusaka

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commenced trials four years earlier. This politically-charged rhetoric, given the international context at the time, was not likely lost on Savimbi.

agreements). Savimbi's incentives to avoid punishment and maintain his position of power thus played a critical role in extending the Angolan civil war for 27 years.

#### **4.1 The Gbadolite Negotiations: The First Major Opportunity for Settlement**

The Angolan civil war during the 1970s and 1980s was heavily influenced by external dimensions. As the country emerged from colonialism, the MPLA-controlled government became a Soviet Cold War client. Support for the MPLA also came from Cuba, who formed an alliance with the MPLA in 1965 and between 1975 and 1991 sent tens of thousands of Cuban troops to Angola to support the MPLA's forces (E. George 2005).

UNITA, for its part, received significant external backing as well. South African support returned to UNITA after a brief hiatus in the early 1970s, and with it, UNITA was able to expand its guerrilla activity throughout most of the country and develop conventional forces as well. United States' support of UNITA, which had occurred covertly during the 1970s, resumed in the mid-1980s with the initiation of the Reagan Doctrine, designed to 'roll back' communism in the Third World and contain Soviet influence. One of the main components of this policy was US support for anti-communist insurgencies world-wide, including UNITA. A ban on US support for UNITA was lifted in 1985, with almost immediate effects on the conflict: new weapons from the US negated MPLA air-superiority (James 2004).

The late 1980s, however, ushered in a new international context, as the USSR attempted to end Cold War entanglements and draw down its international commitments. Pressures from both the USSR and the US led, in December 1988, to

the signing of the New York Accords, in which Cuba agreed to a phased withdrawal from Angola and South Africa agreed to terminate support for UNITA (James 2004). Faced with the impending withdrawal of international support, and strong international pressure, Savimbi and dos Santos agreed to the first major negotiations since the start of the civil war in 1975. In May 1989, dos Santos presented a peace plan that included national reconciliation, a relatively important concession to UNITA. However, the plan envisaged incorporation of UNITA into the MPLA, UNITA's acceptance of the Angolan constitution calling for a Marxist one-party state, and importantly, exile for Savimbi (Weigert 2011, 92).

A month later in June 1989, a summit was held at Gbadolite in which Savimbi, dos Santos, and 18 African heads of state met.<sup>59</sup> The results of the Gbadolite summit were vague, however. A communiqué issued suggested that the two sides had agreed to form a commission to conduct further negotiations and had agreed upon a general desire for peace and reconciliation, but the meeting produced no consensus on the modalities for peace. Following the Gbadolite summit, MPLA officials claimed that agreement had been reached on the provisions for Savimbi's exile. This assertion created a firestorm immediately. UNITA spokespersons vigorously rejected these statements, asserting that no such agreement had been reached (James 2004, 64–5; Weigert 2011, 93).

Additional meetings made no headway. MPLA and UNITA officials refused to meet in the same room, and indirect talks proved unable to break the negotiating stalemate. Participants at an eight-nation summit in Harare, Zimbabwe in August

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<sup>59</sup> Savimbi and dos Santos never met in the same room; both men refused direct negotiations.



1989 backed the MPLA peace plan, including the provision requiring Savimbi's exile, and Savimbi subsequently refused to attend a second heads of state meeting in Kinshasa the next month.

Savimbi's refusal to accept the terms offered him in these rounds of negotiations spelled the end of this attempt to end the conflict. The diplomatic initiatives at Gbadolite carved out no space for Savimbi in a post-conflict Angola, and thus proved unacceptable to him. Further, vague promises of national reconciliation were not a strong enough foundation upon which Savimbi could sell the negotiations as a success to UNITA members, particularly at a time when internal dissention in the organization was increasing (see section 3.1).

Importantly, party stances adopted at two UNITA Party Congresses held in August 1986 and September 1989 provide an excellent way to gauge to what extent Savimbi's response to the Gbadolite proposals reflected the wishes of the majority of his winning coalition. In 1986, delegates to the Party Congress authorized the leadership to seek peace negotiations and national reconciliation. In 1989, an extraordinary Party Congress reaffirmed this stance; over 3000 delegates voted for direct negotiations with the MPLA, a ceasefire, support for a transitional government, revision of the constitution, and support for free and fair elections.

This internal push for negotiations may have facilitated Savimbi's attendance at the Gbadolite negotiations, but despite internal UNITA support for settlement, the terms offered were unacceptable to Savimbi. He recognized that settlement on the MPLA's proposed terms would be viewed as a policy failure by UNITA members and could thus bolster Savimbi's opponents within the organization, leading to a more

concerted push for his removal. Further, acceptance of the Gbadolite proposals would have essentially guaranteed an end to Savimbi's political power both as head of UNITA and at the national level, as the MPLA's proposals envisaged direct punishment in the form of immediate exile for the UNITA leader. Given the Gbadolite terms, therefore, Savimbi went against his party's stated wishes, acting as an unfaithful agent by rejecting the MPLA's peace proposals and returning to war.

#### **4.2 Bicesse Accords: First Signed Agreement**

While the Gbadolite Declaration failed to produce any genuine steps toward peace, far-reaching changes in the global balance of power would, by 1991, produce another ripe moment for settlement of the Angolan civil war. Between 1989 and 1991, Cuban troops gradually withdrew from Angola and South African intervention was terminated in accordance with the 1988 New York Accords. The collapse of the Soviet Union facilitated the withdrawal of Soviet and US support for the MPLA and UNITA, respectively, as well. In September 1990, the USSR suggested that all parties accept the 'triple zero' proposal, according to which the Soviets and US would stop supplying the MPLA and UNITA, and both combatant groups would agree not to seek weapons from abroad (James 2004). The relatively rapid disentanglement of foreign governments from the Angolan conflict corresponded with diplomatic initiatives by Portuguese, American, and Soviet mediators, beginning in April 1990, to reignite dialogue between dos Santos and Savimbi (Malaquias 2010).

These mediation efforts bore fruit on May 31, 1991, when the MPLA and UNITA signed the Bicesse Accords. The agreement called for an immediate cease-

fire, creation of a national army with equal representation of both combatant groups, and cantonment of existing forces. UNITA also agreed to recognize the Angolan government and dos Santos prior to the holding of multi-party, national elections in September 1992, which UNITA would contest (James 2004; Weigert 2011, 100).

The optimism of early 1991 was dashed within 18 months, however, as the country plunged back into civil war by late 1992. Understanding why the Bicesse Accords collapsed so quickly and spectacularly requires understanding what Savimbi believed would come of the accord, and the vulnerabilities he faced when his expectations were not met. The first years of the 1990s were UNITA's strongest militarily. The rebel group had switched from guerrilla tactics to conventional forces, and had enjoyed many successes against the MPLA, particularly as Cuba ratcheted down its involvement in the conflict. Savimbi entered the Bicesse negotiations, therefore, from a position of strength. For Savimbi, the agreement's provision calling for national elections was an opportunity. He believed he would win the election, and take national-level political power via the ballot box rather than the bullet. Perhaps as a portent of events to come, Savimbi told British television just one week before the September 1992 elections: "If I lose, then the elections were rigged and I will send my men back to the bush to fight again. We will not accept defeat" (quoted in El-Khawas and Ndumbe 2007, 106). UNITA thus entered the elections with optimism and high expectations.

Savimbi's failure to win the 1992 elections, therefore, came as a shock to UNITA and its leader, and ultimately triggered the crumbling of the Bicesse Accords and the return to war. Savimbi lost the presidential vote to dos Santos, 49.6 percent

to 40 percent, but because neither side achieved a majority, a second round of voting was required. Before this could take place, however, Savimbi denounced the early vote tallies as fraudulent and, a day later on October 5, 1992, 11 UNITA generals announced their withdrawal from the newly created Angolan Armed Forces (FAA) (Weigert 2011, 111). On October 7, Savimbi left Luanda for Huambo, a UNITA stronghold. He refused to return to the capital to negotiate terms for the second round of voting.

Savimbi's electoral loss spelled the end of the Bicesse Accords for two reasons. First, because UNITA and Savimbi himself entered the elections with the expectation of victory, defeat was viewed as a major failure which threatened to heighten internal divisions within UNITA and to dislodge Savimbi from his leadership of the organization. Major divisions within UNITA, which first became visible in the late 1980s, intensified as the Bicesse Accords were negotiated and came into force. In an August 1990 interview, Savimbi acknowledged these divisions, stating that "it is when a movement believes that it is going to win that it must be careful. Both friends and enemies form splinter groups. On the eve of victory you find discord" (quoted in Weigert 2011, 102). This statement suggests that Savimbi saw threats to his leadership emanating from many sources within the UNITA organization, and was unable to fully trust even his close advisors or top commanders.

The increase in executions within and defections from the organization suggests that Savimbi did indeed feel vulnerable, which manifested in attempts to violently eliminate potential challengers. In February 1992, for example, Tony da

Costa Fernandes, one of UNITA's co-founders, and Miguel N'Zau Puna, UNITA's deputy leader for nearly 24 years, defected from the organization. Importantly, both were considered potential replacements for Savimbi in the event of his fall from power (James 1992, 135–6). In public statements after their defections, both accused Savimbi of executing prominent UNITA figures (Malaquias 2010, 306).

Thus, Savimbi likely calculated that accepting the electoral loss would directly threaten his tenure. The organization supported the peace process, as evidenced by the seventh UNITA party congress's support for the electoral process and the terms of the Bicesse Accords in March 1991 (James 2004). But the optimism of the early 1990s created high expectations. UNITA members expected to defeat the MPLA at the polls. Savimbi's loss in the presidential election and UNITA's loss in the legislative contest, therefore, represented a major failure for which Savimbi would take the blame. It also threatened to exacerbate already serious tensions within the organization, as UNITA was confronted with its own political limitations and miscalculations. Rather than accept the electoral defeat which would have seriously weakened Savimbi, the rebel leader instead blamed his electoral defeat on fraudulent elections, and adopted a more militant position.

In addition to the internal threats that undermined the Bicesse accords, the MPLA's creation of the 'Ninja' police unit which targeted Savimbi directly, and its strategy of arming the citizenry in Luanda and the resulting 'All Saints Massacre,' served to further convince Savimbi that returning to Luanda to negotiate the terms of second-round voting would critically jeopardize his personal safety.

Savimbi's decision to leave Luanda after the September 1992 election results were announced, and his refusal to return to negotiate terms for run-off elections, reflected a growing perception of his vulnerability to attack by the MPLA and its supporters. Savimbi's residence in Huambo had come under attack from state police forces just one month prior to the elections, in an apparent assassination attempt (Weigert 2011). Further, on October 31, 1992, MPLA soldiers and civilian supporters launched a pogrom termed the 'All Saints Massacre' against Ovimbundu and Bakongo civilians and UNITA members living in Luanda, killing thousands. Among the dead were UNITA Vice President Jeremias Chitunda and Savimbi's nephew, Elias Salupeto Pena, whom Savimbi had sent in his stead to negotiate terms for the run-off election with the MPLA. In the event, both men were pulled from their convoy and shot in the head by MPLA military/police forces (James 2004; Weigert 2011).

The violence against UNITA party members and, in particular, MPLA-sanctioned attacks on high-ranking party officials in Luanda, sent a clear message to Savimbi that a return to Luanda and to negotiations was unsafe. The UNITA leader instead returned to war, and by mid-November 1992 had seized 50 of Angola's 164 municipalities (Weigert 2011, 112). International mediators' attempts to reignite negotiations failed when, in February 1993, Savimbi refused to attend peace talks in Addis Ababa, Ethiopia, claiming repeatedly that his delegation could not travel safely from Angola to Ethiopia without the threat of MPLA attack (James 2004). This perceived threat would persist throughout the remainder of the war.

In sum, Savimbi used the externally-driven peace process in 1991-92 as an attempt to gain national-level political power through the electoral process, rather than through the use of force. When he lost national elections in September 1992, unmet expectations and heightened divisions within UNITA, as well as the increased threat posed by MPLA forces and supporters, led Savimbi to denounce the election results and the peace process more generally. He instead adopted a hard-line approach rhetorically and militarily, launching major offensives around the country by late November and early December 1992.

#### **4.3 Lusaka Protocol: Second Peace Agreement**

Large-scale, conventional fighting characterized the two years following the failure of the Bicesse Accords. UNITA quickly overran much of the country, even briefly threatening government positions in Luanda, in late 1992 and early 1993. This offensive gave way to a slow process of MPLA forces gradually beating back UNITA gains (Malaquias 2010, 309).

Concurrently, international mediators worked hard to formulate a new negotiated settlement to replace the failed Bicesse Accords. Several rounds of negotiations in Lusaka, Zambia, produced the outline of an agreement that retained some key elements of the Bicesse Accords while modifying others. The most important difference perhaps grew out of the Bicesse Accord's failure; while the 1991 agreement had called for national elections to determine the post-war political landscape, the new agreement specified that UNITA would become a junior partner in the government, with several assigned cabinet posts, ambassadorships, and

governorships in three provinces. Savimbi was also offered a vice presidential position, though the powers of this position, and the rules of succession, were not clearly spelled out. The Lusaka accord, therefore, spelled out UNITA and Savimbi's roles in post-war Angola rather than risking leaving these important decisions to elections as had been done in 1991. Militarily, the Lusaka Protocol called for cantonment and disarmament of UNITA forces, the integration of 20,000 UNITA soldiers into a 120,000 strong national army, and the allotment of 5,000 positions in a new police force to former UNITA fighters (James 2004; Weigert 2011, 119–20).

Once again, perceived threats to Savimbi's political power and personal safety from both internal and opponent-based sources undermined implementation of the peace agreement. On the eve of the Lusaka Protocol's official signing in November 1994, deeper fissures than had previously occurred within the UNITA party organization threatened Savimbi's hold on power. Following a government offensive on November 17, Savimbi ordered Eugenio Manuvakola, UNITA's chief negotiator, to leave Lusaka immediately without informing mediators or MPLA representatives. In a blatant display of insubordination, Manuvakola refused Savimbi's order and remained in Lusaka (Weigert 2011, 120–1).

Manuvakola's defiance simultaneously demonstrated two things. First, it highlighted the deep divide within the rebel organization between moderates, in favor of settlement, and hard-liners, in favor of continued conflict. The moderates constituted a majority of voting members of UNITA throughout this period, as party congresses in 1991 and 1995 voted in favor of national reconciliation, negotiations,



ceasefires, and a variety of other peace-facilitating measures (James 2004).<sup>60</sup> Despite the party congresses' obvious desire for peace, however, Savimbi remained committed to the military struggle, even as negotiations proceeded. Second, Manuvakola's defiance demonstrated Savimbi's weakening control over his subordinates. When Manuvakola refused to leave Lusaka and return to Angola, Savimbi was left with little choice but to relent and send the chief negotiator the necessary credentials authorizing him to represent UNITA at the negotiating table. To refuse would have revealed Savimbi's weakness, possibly resulting in an internal coup.

The circumstances under which UNITA and Savimbi agreed to the Lusaka Protocol thus set it up for failure. Savimbi was against the accord from its inception, and had incentives to maintain an intransigent stance given the growing internal discord in the rebel organization and his own increasing vulnerability. In an early 1996 meeting with UN representatives, Savimbi expressed fear that the compromises made in the Lusaka Protocol were too extensive to ensure his political survival, stating: "[I] told them victory would come and now [I] tell them to give up their weapons .... Before I had prestige to protect me, but it is being lost" (quoted in Weigert 2011, 124). This shocking admission suggests that Savimbi believed the terms of settlement negotiated in 1994 were not favorable enough for Savimbi to justify the costs of war to his internal audience, and he was therefore fearful of internal punishment. He therefore had strong incentives to avoid termination on these terms, and to continue the war in hope of a better outcome.

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<sup>60</sup> Manuvakola, a moderate, would later leave the organization, forming a splinter group called UNITA-Renovada.

At the same time, the threat posed by MPLA forces leading up to and after the signing of the Lusaka Protocol served to further undermine Savimbi's commitment to the negotiated settlement. On heightened alert since the failure of the Bicesse Accords, Savimbi did not attend the Lusaka negotiations, nor was he willing to travel to Zambia to sign the agreement in person in November 1994. Savimbi cited concerns for his personal safety as the impetus for his refusal to attend the ceremony; he believed his plane would be shot down by government aircraft should he attempt to travel to Lusaka (James 2004, 90).

Savimbi's concerns for his personal and political safety remained a major stumbling block to implementation of the peace agreement over the next several years. Forward progress on establishing the government of national unity envisaged in the agreement, for example, was delayed multiple times in 1996 and 1997 because UNITA insisted upon clearly determining Savimbi's political role in the new regime before moving forward (El-Khawas and Ndumbe 2007, 109). Savimbi was initially offered the post of vice president, but the actual authorities allotted to this position, and the rules of succession, remained ambiguous, and Savimbi ultimately rejected the post out of fear that this ambiguity would allow the MPLA to effectively sideline him or worse (El-Khawas and Ndumbe 2007; Weigert 2011). Savimbi insisted upon being recognized as the president of the largest opposition party, a role which carried with it a special status, security detail, and would strengthen his political position for the next presidential elections. Until this status was conferred upon him, UNITA refused to send deputies to the legislative assembly or to in any way participate in the national unity government (El-Khawas and Ndumbe 2007, 109). On April 8, 1997,

the National Assembly finally passed legislation conferring upon Savimbi the special status he desired; this legislation gave Savimbi certain privileges and immunities, a salary and a house in Luanda, a large security team, and access to the President for consultations (“Jonas Savimbi Absent Despite Special Status” 1997).

In clear evidence that Savimbi himself had been the major stumbling block to implementation of the Lusaka accord, UNITA deputies accepted their positions in the national assembly and were sworn into office on the very next day. Only two days later, two and a half years after the Lusaka Protocol’s signing, UNITA joined the unity government (El-Khawas and Ndumbe 2007, 109–10). Ominously, however, Savimbi did not attend the swearing in ceremony. He remained, instead, at his stronghold in Bailundo, once again citing fear for his personal safety as the chief reason for his absence (“Jonas Savimbi Absent Despite Special Status” 1997).

Ultimately, the optimism of April 1997 was to be short-lived. Internal turmoil within the UNITA ranks pushed Savimbi toward a more intransigent stance, and Savimbi’s persistent fear of attack by government forces stalled progress toward implementation of the Lusaka accord. UNITA failed to make key demobilization deadlines, and Savimbi repeatedly postponed the handover of key UNITA strongholds including Andulo, Bailundo, Mungo, and Nharea, as well as his planned move to Luanda (Weigert 2011, 134). By August 1998, Savimbi stopped cooperating with Portugal, Russia, and the United States – the countries overseeing the peace process (James 2004). A week later, moderate elements of the organization denounced Savimbi and broke away, forming a splinter group called UNITA-Renovada, headed by former chief UNITA negotiator Eugenio Manuvakola.

Savimbi's unwillingness to implement the Lusaka Protocol's key provisions and his failure to cooperate with moderates within the organization had saved him from a potential coup, but came at the cost of further fracturing of the rebel organization.

Low level attacks escalated from this point, and by December 1998, the MPLA government had launched new offensives against UNITA positions and branded Savimbi a war criminal and international terrorist (James 2004; Malaquias 2010). On July 24, 1999, the government issued an arrest warrant for Savimbi, charging him with armed rebellion, sabotage, and slaughter. At the MPLA's fourth party congress in December 1998, dos Santos reaffirmed the MPLA belief that Savimbi must be targeted in order to achieve peace, stating, "The only way to attain definitive peace today is to isolate Dr Savimbi and his warmongering wing domestically, and internationally, as well as to neutralize him politically and militarily" (quoted in James 2004, 107–8).

The internal and opponent-based threats to Savimbi's life and leadership throughout this period thus served to undermine his ability to commit to the Lusaka Protocol. Internal dissent against Savimbi's rule was at an all-time high by the mid-1990s, and threats emanating from the MPLA reinforced the UNITA leader's insecurity. Savimbi's vulnerability ultimately led him to reject the government's vice presidential offer, to continually hold up demobilization of his forces, and to reject a peace process that his own constituency was in favor of.

## **V. Savimbi's Death and the Memorandum of Understanding: Peace Achieved**

Whereas the previous section detailed the important influence that Savimbi's vulnerability to punishment had on the course of the Angolan civil war and the failure of settlement attempts in 1989, 1991, and 1994, this section demonstrates that threats to the UNITA leadership were much less severe after Savimbi died and was replaced by a non-culpable leader, Paulo Lukamba Gato. Negotiating dynamics and the prospects for and pace of implementation changed dramatically after Savimbi's death in 2002, allowing Angola to finally achieve a lasting settlement to its 30 years of civil war.

### **5.1 Savimbi's Continued Intransigence Despite Dwindling Prospects for Victory**

The final stage of the Angolan civil war lasted from 1998, with the official failure of the Lusaka Protocol, until 2002, when the conflict was finally terminated. Unlike the post-Bicesse period in which a strong UNITA was able to make significant gains against the government, after the breakdown of Lusaka, Savimbi's forces were weakened and on the defensive. MPLA forces gradually gained the upper hand over UNITA by cutting off the rebel group's access to many of the diamond mines which had provided the insurgents' primary source of funding since the end of the cold war. In addition, the government implemented a controversial 'scorched earth' policy, in which they forcibly removed civilian supporters of UNITA from the countryside in order to isolate the rebel group from its support base.

In light of these developments, Savimbi restructured and redeployed his forces in June 2001. Admitting near defeat of his conventional force as he had done nearly

25 years earlier in 1976, Savimbi abandoned conventional tactics. Instead of seeking settlement or negotiations with the MPLA, he maintained his defiant political stance and moved toward a mobile guerrilla strategy against the MPLA (James 2004). At the same time, Savimbi vanished from government radar. He abandoned his headquarters at Bailundo and embraced the benefits of secrecy and mobility that guerrilla tactics provide (Weigert 2011, 153).

This period was thus characterized by increased intransigence on the part of Savimbi, even while his forces gradually lost the strategic advantage and MPLA fighters closed in on him. Savimbi's observed behavior is consistent with the expectations for culpable leaders discussed in the previous chapters; as the likelihood of victory decreased, Savimbi obstinately refused to concede or to countenance a post-conflict Angola in which he did not play a central political role. For Savimbi, the threats to his physical security and political fortunes should he concede were too severe. The strategy that provided him the best hope was to continue to fight, even as the walls closed in around him.

Opponent-based threats to Savimbi were particularly serious as MPLA forces gained the upper hand. Throughout this period, the MPLA's central goal remained the capture or killing of Savimbi, as, after several failed settlement attempts, this was believed to be the key to ending the conflict. Savimbi's success in evading government offensives led the MPLA, in January 2001 and again in December of that year, to float the idea of reviving the Lusaka Protocol as a basis for new negotiations. The MPLA's stance on Savimbi, however, remained unaltered. Angola's defense minister Paihama, for example, stated "no matter what the circumstances, we will

never negotiate with Savimbi” (quoted in Weigert 2011, 159). Dos Santos, similarly, indicated that Savimbi’s options were ‘surrender, capture, or death’ (Weigert 2011, 170). It is possible, therefore, that the proposed revival of negotiations reflected an intentional government strategy of isolating Savimbi, undermining his support within UNITA by once again highlighting his unwillingness to act as a faithful agent and end the costly conflict.

Savimbi, for his part, also remained intransigent. In a telephone interview given in June 2001, Savimbi rejected the idea of declaring a unilateral ceasefire or of unilaterally disarming and demobilizing UNITA troops. He also said he would refuse to serve as one of Angola’s two vice presidents, as the roles were largely ceremonial. In declining this prospective position, Savimbi drew upon the Zimbabwean case, in which ZAPU leader Joshua Nkomo accepted a similar post after suffering setbacks against government forces, and proceeded to be quickly marginalized by Zimbabwean president Robert Mugabe (Weigert 2011, 166). This intransigence in the face of mounting military pressure by government forces suggests that Savimbi was well aware of the fate that awaited him if he willingly gave up the fight.

Throughout late 2001 and early 2002, MPLA forces gradually tightened the net surrounding Savimbi. A breakthrough finally occurred on February 22, 2002, when government forces ambushed Savimbi and his bodyguards in a remote part of Moxico Province, near the Zambian border (Garztecki 2003, 45). The long-time UNITA leader was gunned down, shot 15 times by MPLA forces (Weigert 2011, 172).

## **5.2 Post-Savimbi UNITA and the Memorandum of Understanding**

Savimbi's death marked an important turning point in the Angolan civil war. After his death, and the death from illness of UNITA's vice president Antonio Dembo just days later, UNITA's remaining senior political officials and military commanders rallied around Secretary General Paulo Lukamba Gato. This constituted a remarkable show of unity, given the significant disagreements between UNITA militants and moderates that had plagued the rebel organization over the past decade. In line with contingency plans crafted in 1997 in the case of Savimbi's death, the internal and external wings of the party together created a Management Commission. The Commission was headed by Gato and included 13 other members: 9 generals, Foreign Secretary Alcides Sakala, external representative Isaias Samakuva, and Ernesto Mulato (Weigert 2011, 172).

The Management Commission promptly contacted Angolan government officials to discuss new peace talks and an end to hostilities. On March 13, 2002, UNITA announced a suspension of hostilities, and government forces reciprocated on the same day, ordering the FAA to cease all offensive operations against the rebels. Talks followed, and the government proposed a blanket amnesty for all UNITA soldiers, which was approved unanimously by parliament on April 2, 2002 (James 2004; Weigert 2011, 173).

Negotiations proceeded rapidly, and on April 4, 2002, a Memorandum of Understanding (MOU) was signed, formally ending the civil war. In addition to the amnesty already approved for all UNITA soldiers, the agreement called for the demobilization of UNITA fighters, the incorporation of 40 UNITA soldiers into the



police force and 5000 into the national military, and vocational training for those entering civilian life. A mixed commission consisting of UNITA and MPLA members was established to oversee this demobilization and reintegration process. On the political front, the MOU reaffirmed the terms established at Lusaka in 1994; UNITA would participate in all levels of government through appointment to ministerial and ambassadorial posts, as well as governorships. Interestingly, the MOU was negotiated directly, rather than through international mediators, in contrast to both the Bicesse Accords and the Lusaka Protocol (James 2004, 98).

In a major departure from previous experience in Angola, implementation of the MOU proceeded rapidly and with relative ease. Between April and June, 2002, nearly 80,000 UNITA soldiers entered demobilization camps throughout Angola (James 2004). By August 2, 2002, the UNITA army was formally disbanded, and FAA officers, government officials, and UNITA representatives met in Luanda to officially declare the civil war over (Weigert 2011, 174). By November 2002, the joint military commission established in the MOU to oversee the April ceasefire and implementation of the MOU was disbanded, declaring its work completed.

The rapid negotiation and implementation of the MOU stands in stark contrast to previous experience with the Gbadolite negotiations, the Bicesse Accords, and the Lusaka Protocol. Not only did UNITA's approach to negotiations change drastically almost immediately upon Savimbi's death, but the process and prospects for implementation pre and post-Savimbi also differed significantly. These distinct differences can be attributed to the change in UNITA leadership, from a highly culpable leader facing serious threats to his political power and physical safety, to a

non-culpable leader who faced minimal threat of punishment internally and externally.

Paulo Lukamba Gato joined UNITA in 1975, after the group had begun its fight against the MPLA. He started his insurgent career at the lower levels of the organization, gradually rising through the ranks to General. By the 1990s, Gato had reached the upper levels of UNITA's political/military structure, and was a member of UNITA's delegation to both the Addis Ababa and Lusaka peace negotiations, also serving briefly as UNITA representative to Europe (James 2004, 88). Despite his involvement in multiple peace processes, however, Gato was known as a hardliner within the organization, advocating a militant strategy rather than a moderate one. In 1995, Gato became Secretary General of UNITA, the third highest post in the organization. He would hold this position until the 2002 deaths of Savimbi and Dembo.

Unlike Savimbi, Gato faced little resistance or reason to fear for his physical or political fate internally. Remaining members of the party leadership – those from whom Savimbi had perceived threats against his reign for decades – fell quickly into line behind Gato and his decision to push forward with negotiations and settlement of the conflict, despite the fact that the terms of settlement were less favorable than those offered in either 1992 or 1994. The deep divide between militants and moderates that had threatened Savimbi's power and led to splits in the organization receded when Gato took power, largely because Gato, as a former militant, adopted a moderate position and pushed for peace upon coming to power. As a result, UNITA-R leader Manuvakola stepped down from his post as government-recognized party

leader in July 2002, clearing the way for UNITA proper to transform into a legal political party, and for the two factions to reunite (James 2004).

Additionally, Gato faced little threat of punishment from MPLA forces. The government's strategy throughout the conflict, and particularly in its final stages, was to target Savimbi personally, as his removal was seen as the key to ending the conflict. Gato, despite his history of advocating a militant policy toward the state, thus faced no imminent threat of decapitation by the MPLA once he ascended to the leadership of UNITA. His security in this regard was evidenced by the unanimous April 2<sup>nd</sup> passage of legislation granting amnesty to *all* UNITA members, including high-ranking officials and members of the party leadership, including Gato himself. Thus, lacking the same internal and external punishment-based incentives to continue advocating a hard-line that a culpable leader would face, Gato was willing and able to negotiate peace with the state, and more importantly, to credibly commit to implementing that peace, something his predecessor was never able to do.

## **VI. Alternative Explanations for Conflict Dynamics and Settlement in Angola**

The previous sections have detailed the influence that a leader's incentive to avoid punishment has on the dynamics of negotiation, implementation of agreements, and conflict termination. In this section, I examine possible alternative explanations for the observed trajectory of the Angolan civil war, focusing specifically on leading explanations of war duration and termination from existing literature.

Some existing explanations have little or no bearing on the Angola case, and are therefore discussed only briefly before going into depth on more plausible

alternative explanations. For instance, the veto player model of civil war duration argues that a greater number of combatants with veto power render conflict more difficult to terminate due to difficulty in satisfying all combatants during negotiations. This theory cannot explain the Angolan case, as there were only two warring parties throughout the vast majority of the conflict. Further, only two combatant groups – UNITA and MPLA – were involved in the peace negotiations that took place in 1989, 1991, 1994, and 2002. With no variation on this key variable – the number of combatant groups – the veto player theory cannot explain why negotiations failed in 1989, 1991, and 1994 but succeeded in 2002.

Second, theories arguing that third party interveners influence war duration and outcome are also ill-equipped to explain the Angolan civil war, despite claims that this conflict was an important Cold War proxy conflict though much of the 70s and 80s. Existing studies argue that balanced interventions extend wars (Balch-Lindsay and Enterline 2000), that interveners with independent agendas lengthen conflict (D. E. Cunningham 2010), and that loss of third party support facilitates termination (Fearon and Laitin 2008). The Angolan conflict involved several external actors during its first 15 years. South Africa and the United State supported UNITA, while Cuba and the Soviet Union aided the MPLA with weapons and troops. Given that all external support was withdrawn by the early 1990s, however, the presence of balanced interveners with independent agendas cannot account for why the conflict continued well into the 2000s. Relatedly, the withdrawal of foreign support in the late 1980s/early 1990s should have, according to these theories, led to the termination of the conflict. While the drawdown of external support likely contributed to

negotiations at Gbadolite in 1989 and Bicesse in 1991, successful settlement was not achieved. These theories of third party support, therefore, are insufficient to explain the duration and termination of the Angolan civil war. While they may shed some light on why the conflict lasted through the 1980s, and why the combatants agreed to negotiate in 1989, they cannot explain why those negotiations failed, or why the conflict continued for another 13 years.

The following sections explore additional alternative explanations for the trajectory of the Angolan civil war in more detail.

### **6.1 Economic Model of Conflict**

An economic-based explanation for the duration and termination of Angola's civil war, at first blush, provides a more plausible alternative to the leader culpability/punishment theory discussed above. Angola is a resource rich country, with significant diamond resources in particular. Diamond reserves in Angola are estimated at 180 million carats, and deposits are principally located in the Lunda Norte and Lunda Sul provinces in central and northeastern Angola, areas controlled by UNITA throughout most of the 1990s.

Until the end of the Cold War, diamonds played little role in the Angolan civil war, as external benefactors supplied UNITA and the MPLA with the weapons and funding they needed to fight. After the fall of the Soviet Union and the end of the Cold War caused the relatively rapid withdrawal of foreign support, however, UNITA was forced to find other sources of funding. The illicit sale of diamonds provided the necessary funds; between 1990 and 1999, the bulk of UNITA's resources came from

mining and selling diamonds, providing the rebel organization with between 400 million and 600 million US dollars per year (Malaquias 2007).

However, while resource looting played an important role in the conflict by allowing the combatants to fund their war efforts, the evidence suggests that resource wealth did not lead Savimbi to prefer war over peace, as a rebellion-as-business model would suggest (Collier, Hoeffler, and Söderbom 2004). Peace negotiators anticipated the complications that could have arisen due to the loss of resource wealth, and in an effort to avoid these potential pitfalls, drafted peace accords that would enable rebel leaders to continue to reap the economic benefits of diamond production during peacetime (Ross 2004a). Specifically, the Lusaka Protocol offered Savimbi a position in charge of Angola's natural resources. Furthermore, the termination of conflict promised increased diamond production, as mines abandoned due to conflict would be reopened and new ones developed. Ross (2004a) concludes, based on this evidence, that the failure of the Lusaka accord cannot be explained by a wartime looting model of conflict.

Furthermore, claims that Savimbi grew personally rich off of the sale of conflict diamonds are unsubstantiated. Numerous accusations have been made that Savimbi and other high-ranking UNITA leaders held personal diamond caches in Angola or in sympathetic African capitals and perpetuated the conflict as a way to maintain wealth derived from diamond mining operations. However, no secret accounts to this effect have been found; there is currently no evidence that Savimbi or his family derived significant financial benefit from UNITA's diamond mining operations in eastern Angola (Weigert 2011, 188).

Finally, the economic model of conflict cannot easily explain the last few years of conflict in Angola. By 1999, MPLA forces had managed to retake several diamond mines, and UN sanctions against UNITA hampered the rebel organization's ability to sell its existing diamond stockpiles. This loss of funding, however, did not lead to a change in Savimbi's goals or strategy. Rather, he maintained his intransigent stance toward the state and did not seek a settlement deal despite the dwindling prospects for continued personal economic profit. While this loss of funding certainly contributed to UNITA's military decline, the economic profit model of conflict duration cannot account for Savimbi's behavior over the last three years of conflict.

## **6.2 Commitment Problems, Security Guarantees, and Power-Sharing**

Commitment problems associated with disarming and demobilizing combatants after the 1991 and 1994 settlement agreements provide a second possible alternative explanation for the trajectory of the Angolan conflict. Existing literature on conflict settlement and recurrence notes the often extreme difficulty that civil war combatants have in terminating their conflicts, as the processes of disarmament, demobilization, and reintegration are simultaneously necessary to achieve peace and threatening to combatant groups because they reduce their ability to defend themselves should the opponent renege on the agreement.

At first glance, this seems to be a plausible explanation for the failures of both the 1991 and 1994 peace agreements in Angola. Both the Bicesse and Lusaka accords required the cantonment, demobilization, and disarmament of a large number

of fighters. And in both cases, there were problems and delays with this process. Funding and supply shortages delayed the construction of cantonment sites after the Bicesse Accords were signed, and both sides hesitated to carry through on the full demobilizations called for in the peace agreement. By mid-1992, 85 percent of UNITA soldiers had checked in at assembly sites, but only 4 percent had demobilized. On the government side, only 37 percent of government forces had been cantoned, about half of whom were demobilized (Weigert 2011, 107). UN monitors declared the demobilization process flawed, and warned against the dangers of proceeding with elections despite incomplete demobilization. Similarly, following the Lusaka accord, delays plagued the demobilization process. By February 1996, only 8,200 UNITA soldiers were in cantonment sites, or less than half the number Savimbi originally promised would report by that date (Weigert 2011, 123). While this pace accelerated by the end of 1996, optimism was tempered by reports from defecting UNITA commanders that Savimbi retained a secret army dispersed throughout the country's central and northeastern provinces (Malaquias 2010).

Despite this evidence of problems surrounding the demobilization processes after both the Bicesse and Lusaka agreements, a commitment problem-based explanation is lacking. Most importantly, this theory cannot explain why the 2002 settlement succeeded where the 1991 and 1994 agreements failed. As discussed previously, demobilization proceeded relatively rapidly and largely without incident in 2002, in contrast to 1991 and 1994. The ease of demobilization and disarmament in 2002, relative to 1991 and 1994, and the general success of the settlement agreement becomes even more puzzling once one takes into account the provisions



and design of each settlement agreement. Existing research indicates that third party security guarantees and multiple dimensions of power sharing are necessary to ameliorate commitment problems associated with implementation of peace agreements (Hartzell and Hoddie 2003, 2007; Walter 2002). When these provisions are included in settlement agreements, they are expected to be more likely to successfully terminate a civil war.

The 1991 and 1994 agreements in Angola included both security guarantees from third parties and power-sharing across a range of issues. The Bicesse Accord transformed UNTIA into a legal political party and called for national elections that would integrate UNITA into national politics. It also established a new national military and police force; the proposed national army would be composed of equal numbers of MPLA and UNITA forces, ensuring equitable power-sharing in the security sector. Further, the 1991 accord established a joint commission that included representatives from Portugal, the US, and the USSR to oversee the political process and ceasefire, and deployed a UN verification operation, UNAVEM II, mandated to monitor demobilization and disarmament of forces.<sup>61</sup> The 1994 agreement, similarly, included power-sharing provisions and third party security guarantees, and even included a separation of forces provision that existing research suggests can facilitate settlement (Mattes and Savun 2009). Specifically, the agreement called for (1) the withdrawal of UNITA forces from specific locations to allow for UN monitoring and verification in these areas, (2) proportionate integration of UNITA forces into the

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<sup>61</sup> While this mission was not mandated with the power to enforcement the agreement, Walter (2002) argues that verification should be enough to facilitate credible commitments and deter defection when combatants are relatively evenly matched, as was the case in 1991-92 following the Bicesse Accord's signing.

national army and police force, (3) UNITA participation at all levels of government, including assured participation in the national assembly, ministerial portfolios, governorships, and diplomatic missions, and (4) verification and monitoring of the ceasefire by a UN peacekeeping force (UNAVEM III).

Despite these peace-facilitating provisions, however, both the 1991 and 1994 agreements failed. The 2002 agreement, on the other hand, reaffirmed the political power-sharing provisions established in 1994, but revised the military integration provisions. Rather than 20,000 UNITA troops integrating into the national army and 5,000 into the police force, UNITA was granted only 5,000 slots in the army and 40 law enforcement positions (James 2004). Rather than several thousand UN troops and observers, as had been in the country following the 1994 accord, the UN's presence in Angola in 2002 consisted of only 30 observers, tasked with monitoring troop quartering areas and coordinating humanitarian efforts (Paulo 2004). These provisions, particularly those dealing with military integration and UN monitoring, were substantially weaker and less favorable from a peace-facilitating perspective than those included in the previous two agreements. And yet, in contrast to the expectations of existing theories focused on the amelioration of commitment problems through power sharing and security guarantees, the 2002 agreement succeeded in ending the war.

This calls into question the applicability of these existing explanations to the Angolan case, and suggests that models focused on commitment problems more generally have failed to account for variation in the severity of commitment problems across cases or over time. The peace-facilitating provisions of the Bicesse and

Lusaka agreements were insufficient to solve the Angolan conflict in 1991 and 1994 when commitment problems were particularly severe due to Savimbi's vulnerability to punishment. In 2002, on the other hand, commitment problems were not exacerbated by a culpable leader, and a much less robust settlement agreement succeeded in ending the civil war.

### 6.3 Relative Power

The final alternative explanation for the termination and outcome of the Angolan civil war is based upon the relative power of the combatants in the war. Existing theories in the civil conflict literature make several predictions about the role of relative power in conflict termination. In particular, this body of literature argues that a relative *balance* of power between combatants makes wars more likely to terminate, as military stalemate raises the costs of war and produces uncertainty, making combatants less willing to risk a decisive loss (D. E. Cunningham, Gleditsch, and Salehyan 2009; Walter 2002).<sup>62</sup>

This prediction is not borne out by the evidence from Angola, however. Throughout the 1980s, external support offset any power differential between combatants, allowing both to continue to resist terminating the war. Additionally, throughout the early and mid-1990s, UNITA forces had been built up to the point where they were able to take and hold territory, win conventional battles against government forces, and countenance a military victory against the state. Importantly,

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<sup>62</sup> This goes against predictions of a bargaining approach, however, in which uncertainty is expected to extend conflict rather than facilitate termination (D. E. Cunningham 2010).

this military strength allowed UNITA to achieve military successes upon breaking the Bicesse and Lusaka accords, and indicates that they were unwilling to settle for a negotiated agreement as these models predict. The relative parity between UNITA and the MPLA during the 1980s and much of the 1990s may have played a role in facilitating *negotiations* in 1989, 1991 and 1994, but it cannot explain, based on these existing theories, why those negotiations failed to produce a lasting peace.

Additionally, theories of civil war outcome suggest, straightforwardly, that stronger actors are likely to achieve better outcomes in war. Given that UNITA suffered a slow and steady military decline between 1999 and 2002 and was on the defensive by the time the war ended, this theory could explain why the outcome UNITA achieved in 2002 was less favorable than the proposals of 1991 and 1994, when state and rebel forces were better matched. This cannot explain, however, why UNITA's negotiating strategy and willingness to commit to a settlement agreement changed so drastically when the organization's leadership changed in 2002.

UNITA's prospects were grim in 2001 and early 2002, before Savimbi's death, but the UNITA head remained intransigent, resisting any attempts to renew negotiations. Upon Savimbi's death, however, the organization embraced negotiations, and was able to implement the agreed upon settlement deal. This about-face in the organization's willingness to negotiate and implement a settlement agreement is not easily explained by the relative power model, but can be explained as a result of the culpable leader theory developed in this dissertation.

The leader incentives theory thus provides a more complete explanation for the trajectory of the Angolan civil war, as it can explain the outcome of each major

settlement attempt, as well as why UNITA's strategy changed when the organization's leadership changed from culpable Savimbi to non-culpable Gato.

## **VII. Conclusion**

This chapter has examined the impact of leader culpability and the threat of punishment from internal and opponent-based audiences on the course of the civil war in Angola. Specifically, controlled within-case comparison and process tracing of four distinct settlement attempts during the Angolan conflict were undertaken to isolate the role that the threat of punishment and the leader's incentive to avoid punishment played during each major settlement attempt.

The results of this qualitative analysis demonstrate that both the internal and external threats that culpable leader Jonas Savimbi faced during his tenure as head of UNITA undermined the group's ability to reach agreement in 1989 and to implement settlement agreements in 1992 and 1994. In contrast, non-culpable leader Paulo Lukamba Gato faced significantly less threat of punishment from both internal and opponent-based sources, and as a result was able to reach agreement with the MPLA in 2002 and implement that agreement over the next few months.

The results further demonstrate that common alternative explanations for civil war duration and outcome are less well-equipped to explain the course of the Angolan civil war. Multi-actor models and theories that focus on the role of third parties have little applicability to the Angolan case, while more plausible alternative explanations including economic (i.e. looting) models, commitment problems theories, and

explanations focused on the relative strength of combatants all suffer from important limitations that render them unable to fully account for the conflict's trajectory.

The discussion of the Angolan civil war in this chapter thus provides additional support for the leader incentives model of conflict termination developed in this dissertation. By tracing the process through which a culpable leader's fear of punishment influenced wartime decisions, undermined settlement attempts, and led that leader to act as an unfaithful agent, this discussion bolsters the statistical analysis of the theoretical mechanism from the previous chapter and lends further credence to the theoretical argument developed in chapter 2.

## **Chapter 6 : Conclusion**

### **I. Introduction**

Why do some civil wars end quickly while others drag on for years or even decades? Furthermore, why do some combatants agree to compromise outcomes while others fight to a military end?

Understanding the determinants of civil war duration and outcome is critically important for scholars and policymakers alike, given the high costs of internal conflict. Civil wars are not only more common and more deadly than interstate war, but involve a number of other costs for civilian populations, national and global economies, regional stability, and international actors. Given that civil wars are often long, deadly, destructive, and prone to recur, better understanding the determinants of when and how they end is necessary to help facilitate rapid and successful termination of ongoing conflicts.

Existing explanations for civil war termination and outcome focus on a variety of country and dyad-level factors expected to influence the costs of war, relative bargaining strength, the profitability of conflict, and factors that facilitate credible commitments to peace. These existing theories, however, have difficulty explaining why some conflicts continue despite high costs, low profitability, or robust settlement agreements that include a variety of mechanisms designed to enhance actors' abilities to credibly commit to peace. Further, these existing explanations overlook an

important determinant of combatant wartime behavior by failing to explicitly account for the high-risk nature of civil conflict for the leaders involved. Given that nearly half of all civil war leaders experience some type of punishment as a result of war, leaders' expectations of punishment and their incentives to avoid it are likely to influence their wartime decision-making.

## **II. Theoretical Argument**

To address these critical gaps in our understanding of civil war termination, this dissertation developed a theory of civil war termination that focuses explicitly on the incentives that rebel and state leaders have to avoid punishment in the context of civil conflict. Building upon a basic principal-agent framework and drawing insights from credible commitment theories of civil war, I argued that rebel and state leaders' incentives to retain political power and otherwise avoid punishment are important determinants of combatant wartime behavior.

The central theoretical claim posited in Chapter 2 argued that culpable leaders – those viewed as responsible for their group's participation in the war – face a higher expectation of punishment following unfavorable war outcomes than their non-culpable counterparts. This threat of punishment emanates from both internal and opponent-based audiences, and influences the culpable leader's utility for settlement. Specifically, the culpable leader will have a lower utility for settlement on compromise or losing terms than a non-culpable leader, who can more easily evade responsibility for poor war performance by avoiding association with the original decision to go to war and the war aims established at the start of the conflict. The low



utility for settlement on unfavorable terms generates perverse incentives for the culpable leader to extend losing wars in a gamble for resurrection, through which he hopes to turn the tide, achieve a favorable outcome, and avoid punishment.

A culpable leader's lower utility for termination and incentives to gamble for resurrection are expected to influence wartime behavior, and thus war termination and outcomes, in several ways. First, I hypothesized that culpable leaders will be less likely to terminate a conflict than their non-culpable counterparts, instead choosing to prolong ongoing wars in order to avoid internal and adversary-based punishment. The heightened threat of punishment will also influence war outcomes; culpable leaders will forego compromise settlement opportunities, choosing to continue the fight in the hope of achieving a military victory and avoiding punishment. This military gamble will sometimes pay off, allowing the culpable leader to achieve victory where a non-culpable leader would have settled for less, while at other times, the gamble will fail and culpable leaders will suffer more negative war outcomes than their non-culpable counterparts. Thus, culpable leaders are more likely to experience major victories and total defeats – extreme war outcomes – than non-culpable leaders. Additionally, culpable leaders are less likely to make concessions on key issues at stake in the war than their non-culpable counterparts, who can more easily disassociate themselves with the war aims established at the start of the conflict and thus avoid punishment for conceding on one or more of those central issues. Finally, I hypothesized, in line with the underlying mechanism developed in chapter 2, that culpable leaders are more likely to be punished for unfavorable war performance than

non-culpable leaders, as the latter can more easily avoid attributions of responsibility for poor war outcomes.

### **III. Empirical Findings**

Chapters 3 through 5 test the theoretical argument developed in chapter 2 using both quantitative and qualitative methods. First, chapter 3 tests the hypotheses on war termination and outcome using an original dataset of rebel and state leaders of a global random sample of civil conflicts ongoing between 1980 and 2010. The results are supportive of the hypothesized relationships; as expected, civil conflicts are significantly less likely to terminate when culpable leaders are in power. Additionally, culpable state and rebel leaders are more likely to experience extreme outcomes than non-culpable leaders, and are less likely than non-culpable leaders to make concessions on central war aims at termination. These results are robust to a variety of alternative variable measurements, model specifications, modeling strategies, and tests for potential endogeneity problems.

Chapters 4 and 5 use both quantitative and qualitative evidence to test the underlying causal mechanism in my theoretical argument, that culpable leaders are more likely to be punished for poor war performance than their non-culpable counterparts. Chapter 4 tests this mechanism quantitatively using an original dataset on the fate of every rebel and state leader in my sample, as well as original data on each leader's war performance. Statistical results provide strong support for my theoretical expectations. As expected, culpable leaders who perform poorly in war are significantly more likely to be punished than non-culpable leaders who perform

similarly poorly in war, and these results are robust to a variety of alternative specifications and modeling strategies.

Finally, chapter 5 supplements the quantitative analyses of chapters 3 and 4 with qualitative evidence from the civil war in Angola between 1975 and 2002. Using within-case comparison of the periods before and after UNITA leader Jonas Savimbi's death in 2002, as well as process-tracing of the settlement attempts between UNITA and the Angolan government in 1989, 1991, 1994, and 2002, I examine the role that culpability and the resulting threat of punishment played in extending the Angolan conflict and undermining negotiated settlement agreements. Case evidence from Angola demonstrates that the threat of both internal and opponent-based punishment influenced Savimbi's decision-making at critical junctures in the Angolan civil war, and ultimately undermined efforts to establish a lasting peace. Non-culpable UNITA leader Paulo Lukamba Gato, who came to power after Savimbi's death, faced significantly lower threats of both internal and opponent-based punishment than his predecessor, and as a result, was able to commit to a final settlement agreement in 2002 after 27 years of civil war.

#### **IV. Implications and Contributions**

This project makes several important contributions to existing scholarship on civil war termination, and has implications for policy-makers seeking to facilitate peaceful settlement of ongoing conflicts.

First and foremost, as the first study in the civil war literature to take leaders' incentives into account when examining civil conflict dynamics, this dissertation has

far-reaching implications for scholarly understandings of civil war termination and outcome. The strong quantitative and qualitative support for my theoretical argument demonstrates that existing explanations focused on dyad or country-level factors are valuable but incomplete. This dissertation advances existing literature by recognizing that leaders have personal incentives independent of the interests of the groups they represent, and identifying the conditions under which dyad and country-level factors will be eclipsed by those personal incentives. Given the frequency with which rebel and state leaders involved in civil conflict are punished, incorporating leaders' expectations of punishment into a theoretical explanation for civil conflict behavior fills a major gap that existing research on intrastate war termination had failed to address.

Second, the dissertation's theoretical argument has important implications for existing theories of civil conflict termination, particularly those focused on the costs of conflict and commitment problems. First, scholars who argue that high costs of conflict promote negotiations and peaceful settlement cannot explain why some civil wars continue and some negotiations fail despite extremely high costs of conflict. Taking leader incentives and culpability into account provides a theoretical explanation for this empirical anomaly. The theory developed in this dissertation recognizes that leaders have independent interests and that, at times, culpable leaders have incentives to continue costly conflicts in order to avoid punishment, despite the fact that this goes against the interests of the groups they represent. This theory thus provides a theoretical mechanism to explain why negotiations sometimes fail and conflicts continue, even when war is particularly costly. Specifically, as the costs of

conflict increase, thereby increasing the culpable leader's expectation of punishment, culpable leaders will have incentives to continue and even escalate the war in an attempt to achieve victory as a way to avoid punishment. This suggests that culpable and non-culpable leaders will respond differently to the costs of war, and provides a potential caveat or conditionality in the relationship between the costs of war and war termination that existing theories have failed to account for.

Similarly, existing research on commitment problems as a barrier to civil war settlement cannot explain why some settlements fail despite robust credibility-enhancing provisions, while others succeed with relatively few mechanisms to combat incentives to renege. In other words, these existing theories cannot explain variation in the prevalence or severity of commitment problems either across conflicts or over time within the same war. The leader incentive-based theory developed in this dissertation addresses this limitation by identifying a potential source of variation in the severity of commitment problems. Specifically, I have argued that commitment problems will be particularly severe when culpable leaders are in power, due to the heightened risk of punishment that these leaders face. This provides an explanation for cases that existing theories cannot easily account for, such as the Angolan civil war, where settlements with robust mandates failed when a culpable leader was in power, while an agreement with relatively few mechanisms to generate credible commitments succeeded under a non-culpable rebel leader.

This project also makes important data contributions. First, I developed an original dataset on rebel and state leaders during civil conflicts between 1980 and 2010. This is the first cross-national dataset to identify rebel leaders and to include

information on civil war leaders' time in power, culpability, and post-tenure fates. These new data can be used in a variety of applications in future research on civil conflict. Second, I have developed a novel coding scheme for war outcomes that identifies the favorability of the war outcome for each warring actor, relative to its original war aims. This new measure overcomes major limitations in existing data by eliminating the ambiguity of existing coding schemes that group together all outcomes of a particular type, and by better accounting for how satisfied each actor is with the post-conflict status quo. This new coding scheme not only provides more fine-grained information on the outcome of war, but provides a measure that can better account for the prospects of post-conflict stability.

Finally, this project offers important insights for the policy community. Specifically, the results indicate that mediation strategies aimed at facilitating timely settlement of civil wars should address the personal security concerns influencing leaders' behavior. Settlement proposals cannot be expected to succeed if mediators do not explicitly address the vulnerabilities of culpable leaders, providing them a more favorable post-conflict scenario than they can expect to achieve through continued conflict. This is likely a difficult strategy to implement, however, as conciliatory offers towards culpable leaders are likely to be highly unpalatable to a variety of domestic and international actors, and culpable leaders will be willing to take on high-risk strategies in the hope of achieving victory.

Mediation efforts in the current Syrian civil war, for example, have thus far proven fruitless. Syrian president and culpable leader Bashar al-Assad faces almost certain, severe punishment if he agrees to any settlement deal that affords his political

opponents national-level political power. As a result, mediation efforts by Kofi Annan last spring collapsed, and renewed diplomatic initiatives by UN special envoy to Syria Lakhdar Brahimi last fall failed to produce any progress towards peace. Instead, Assad doubled-down on the war effort, turning to increasingly brutal tactics toward opponents and civilians alike, including bombardments of Aleppo and the suburbs of Damascus, and more recently, the use of chemical weapons against his own citizenry. While Assad's gamble appears to have succeeded, at least in the short term, in shifting momentum toward the Syrian regime, the international community continues to seek a negotiated settlement to the conflict. But world leaders' calls for renewed Syrian peace talks last week at the G-8 are likely to fall on deaf ears. With the exception of Russia, all major international powers envisage a peace deal in which Assad steps down and hands over the reins of power to his political opponents. The international community is thus currently unwilling to consider a settlement that would secure Assad's physical security or provide him security against removal from power. Given the incompatibility of Assad's interests with existing proposals, international mediation efforts are unlikely to succeed, and more robust international intervention may be needed to bring the Syrian civil war to a timely end.

## **V. Conclusion**

In early 1996, faced with a faltering peace process, a splintering rebel organization, and an opponent intent on ending his career as a rebel, UNITA leader Jonas Savimbi told UN representatives in Angola that he feared for his own well-being, stating "Before I had prestige to protect me, but it is being lost" (Weigert 2011,

124). This brief moment of unfiltered honesty provides a glimpse of what, I argue, is in fact a pervasive concern underpinning the strategic decision-making of rebel and state leaders during civil war. Fear of both internal and opponent-based punishment, and the desire to avoid it, influences the wartime decisions of culpable leaders and as a result, their willingness to end a war and to settle for compromise outcomes.

This dissertation has provided extensive evidence that culpable leaders, because of their heightened expectation of punishment, are likely to fight longer, to experience more extreme outcomes, and to avoid making concessions to end a war. As such, it demonstrates the importance of taking leader incentives and characteristics into account when developing a comprehensive explanation for civil war termination and outcome, as I have attempted to do in the preceding pages. Future analyses should continue to unpack the black box of civil war, examining in further depth the role of leaders in a variety of conflict processes.



## **APPENDICES**

## **A Appendix: Measurement of Control Variables**

**Relative Power:** Data on the relative strength of combatants are adapted from Cunningham Gleditsch and Salehyan's (2009) Non-State Actors dataset. These existing data measure a rebel group's strength relative to the state on a five point scale; rebel groups are coded as much weaker, weaker, at parity, stronger, or much stronger than the state. Using this existing measure, I assign each rebel and state actor in the dataset a score from 0 (much weaker) to 4 (much stronger) depending upon its strength relative to its adversary.

In the analysis, two different variations on this scale are used. First, to test hypothesis 1 predicting conflict termination and using a dyadic data structure, I measure the strength disparity between combatants. This variable receives a 0 if the state and rebels are at parity, a 1 if one combatant group is stronger than the other, and a 2 if one combatant is much stronger than the other. To test hypotheses 2 (extreme outcomes) and 3 (concessions), I generate a dummy variable coded 1 for extreme strength imbalances between adversaries. This variable receives a 1 if the relevant actor is either much weaker or much stronger than its opponent, while receiving a 0 if the combatant is weaker, at parity, or stronger than the opponent.

**Third Party Support:** Data on military support is adapted from the UCDP External Support Dataset (Pettersson 2011). Two different variables are included in the analysis. The test of H1 includes a dummy variable, *balanced intervention*, which is

coded 1 if both combatants in the dyad receive foreign military support in a given year. This variable receives a value of 0 for any year in which one or neither combatant receives foreign support. Tests of hypotheses 2 and 3 include a dummy variable coded 1 if the relevant state or rebel leader received external support in the form of foreign troops at any time during his conflict tenure, and zero otherwise.

**Battle-Related Casualties:** To measure battle-related deaths, I use data from the UCDP Battle-Related Deaths Dataset v.5-2012 (Sundberg 2008), which provides yearly information on the total number of battle deaths suffered (both rebel and state) in each warring dyad. The variable used in the conflict termination analysis measures the total number of casualties suffered each year, while the variable used in the outcome equations (H2 and H3) and the punishment equation (H4) measures the total number of fatalities suffered during each leader's tenure.<sup>63</sup> For leaders who either enter or leave power in the middle of the year, that year's total is divided between the leader who exits power and the leader who takes his place according to the number of months each was in power during the year.<sup>64</sup> This variable is logged due to its skewed distribution.

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<sup>63</sup> This includes both rebel and state battle deaths during the leader's tenure. The existing data sources do not distinguish between deaths suffered by the rebels and the state.

<sup>64</sup> By dividing the total yearly death count according to the number of months the leader was in power, this coding rule assumes a constant casualty rate throughout the year. While this may not always be the case, data limitations prevent a more exact distribution of deaths across leaders who take power or exit leadership positions in the middle of the year.

**Democracy:** To measure democracy, I use data from the Polity IV project (Marshall and Jaggers 2010), which measures a state's level of democracy on a scale from -10 to 10. The variable used in the analysis is a dummy variable coded 1 if the state receives a 7 or higher on the polity2 scale, and zero otherwise. This variable is measured yearly for the termination equation, and is coded based upon the polity2 score at the end of each leader's tenure for the war outcome equations.

**Incompatibility (Stakes of War):** The variable used to measure the stakes of war is taken from the UCDP Dyadic Conflict Dataset (Themnér and Wallensteen 2012). It is coded 0 for governmental (i.e. center-seeking) conflicts and 1 for secessionist conflicts.

**ICC Signatory:** The ICC signatory variable included in the war termination analysis is measured with a dummy variable coded 1 in the year a state signs the Rome Statute, and in each subsequent year. The variable receives a zero in all other dyad years. Information used to code this variable is available from the ICC's website.

**Rebel Political Wing:** To measure the presence of a rebel political wing, I use a dummy variable from Cunningham, Gleditsch, and Salehyan's (2009) Non-State Actor dataset. This variable is coded 1 if the rebel group involved in the conflict has a political wing, zero otherwise.

**Mediation:** This variable measures whether third party mediation attempts were made during each leader's tenure. The variable is coded 1 if mediation was present, and 0 if no mediation attempts were made. Data on the occurrence of mediation is adapted from the Civil Wars Mediation Dataset (DeRouen, Bercovitch, and Pospieszna 2011).

**Population:** The state's population is also included as a control in the termination equation. Data on national-level population come from the Penn World Table Version 7.1 (Heston, Summers, and Aten 2012), which provide information on state population between the years 1950 and 2010. The natural log of population is used in the analysis to account for the highly skewed nature of the variable, and is measured yearly.

**GDP Per Capita:** Country-level gross domestic product per capita is also included as a control in the termination equation. These data also come from the Pen World Table Version 7.1 (Heston, Summers, and Aten 2012). The natural log of GDP per capita is included in the empirical analysis, and is measured yearly.

**Leader Conflict Tenure/Duration:** The measure of leader conflict tenure is a count of the number of months the relevant leader presides over the conflict. The natural log of this value is used in the analyses of war outcome, political concessions, and leader punishment.

**Time Controls (War Duration model only):** A counter for the number of years since the start of the war is included in the war termination model, along with the squared and cubed terms of this counter. These time controls are included to account for duration dependence.

**Leader location:** This is an indicator variable used in the punishment equation (H4) to identify where the leader is based. This variable is coded 1 if the leader lives primarily outside of the state in which the conflict is being fought, and zero otherwise. The coding of this variable is based upon original research.

**Term limits:** This dummy variable, included in the punishment equation (H4), is coded 1 if a leader faces term limits, and zero otherwise. It is coded based upon original research.

**Transitional leaders:** This variable, also included in the punishment equation (H4), is a dummy variable coded 1 if a leader is a transitional leader, and zero otherwise. A leader is coded as transitional if he or she comes to power with the explicit mandate/intention to preside over a period of regime change before stepping down for a newly elected leader. This variable is coded based upon original research.

**Table A.1 Descriptive Statistics for Control Variables**

<b>War Termination Model</b>				
<b>Variable</b>	<b>Min</b>	<b>Max</b>	<b>Mean</b>	<b>Mode</b>
Relative Power	2	4	3.33	3
ICC Signatory	0	1	0.21	0
Balanced Interventions	0	1	0.32	0
Casualties (ln)	0	9.397	4.24	--
Democracy	0	1	0.28	0
Population (ln)	5.95	12.34	10.02	--
GDP Per Capita (ln)	5.08	10.24	7.76	--
War Duration	1	48	9.699	--
War Duration Squared	1	2304	167.94	--
War Duration Cubed	1	110592	4000.06	--
<b>Extreme Outcomes and Political Concessions Models</b>				
<b>Variable</b>	<b>Min</b>	<b>Max</b>	<b>Mean</b>	<b>Mode</b>
Strength Imbalance	0	1	0.34	0
Casualties (ln)	0	10.99	5.45	--
Mediation	0	1	0.43	0
External Troop Support	0	1	0.05	0
Incompatibility	0	1	0.39	0
Leader Conf Duration (ln)	0	5.69	2.85	--
Rebel Political Wing	0	1	0.39	0
Democracy	0	1	0.17	0
<b>Leader Punishment Model</b>				
<b>Variable</b>	<b>Min</b>	<b>Max</b>	<b>Mean</b>	<b>Mode</b>
Casualties (ln)	0	10.99	5.81	--
Leader Conf Duration (ln)	0	6.27	3.04	--
Leader Location	0	1	0.07	0
Term Limits	0	1	0.11	0
Transitional Leader	0	1	0.04	0

## **B Appendix: Regime Type and Culpability**

### **I. Culpability and Personalist Regimes**

This appendix addresses the relationship between regime type and leader culpability. Existing research on interstate conflict finds that culpability's influence on interstate war outcomes is conditional on regime type. Specifically, culpable democratic leaders are more vulnerable to removal from power than culpable nondemocratic leaders. Accordingly, culpable democrats are more responsive to the pressures of culpability than culpable non-democrats, being more likely to achieve favorable war outcomes than culpable autocrats because of the increased threat of removal from power (Croco 2011). Given the conditional relationship between culpability and regime type for leaders in interstate conflicts, it may be the case that culpability is similarly conditioned by regime type in the context of civil war.

#### **1.1 Why Culpability's Impact may be Conditional on Regime Type**

The regime-type designation that is particularly relevant in the current context, however, is that between personalist and non-personalist regimes. Weeks (2008) argues that scholars generally underestimate the extent to which nondemocratic leaders can be held accountable by domestic audiences. It is personalist regimes, she argues, that exhibit the classic characteristics generally attributed to autocracies more broadly. Specifically, the crucial distinction between personalist and non-personalist regimes is that the relevant domestic audiences in



non-personalist regimes, even single-party or military governments, can coordinate to sanction or remove a leader, whereas those in personalist regimes cannot.<sup>65</sup>

Personalist leaders can avoid punishment by domestic audiences for two reasons. First, they have monitoring and coercive capacities to punish individuals who oppose them, and second, elites' fates in personalist regimes are closely linked to that of the leader (Weeks 2008). Leaders of personalist regimes control (1) the security organs of the state and (2) political appointments (Geddes 1999, 2003). Control over the security apparatus and appointments allows personalist leaders to fill influential positions in the government and the security apparatus with only trusted associates, and to undercut any potential coordination among elites against his rule. Control of the security apparatus ensures that the personalist leader can arrest, demote, imprison, or execute critics of his rule. Control of political appointments, furthermore, ensures that the leader can use promotions and demotions, as well as rotation of political elites, to prevent potential political challengers from building independent power bases (Geddes 2003; Weeks 2008). Furthermore, elites in personalist regimes will be disincentivized against removing the leader because their own positions of political power are dependent upon the maintenance of the personalist leader. Elites lack independent political bases, are compromised by the regime's corruption, and are often dependent upon state or party offices for their livelihood, lacking independent, private capital holdings (Weeks 2008).

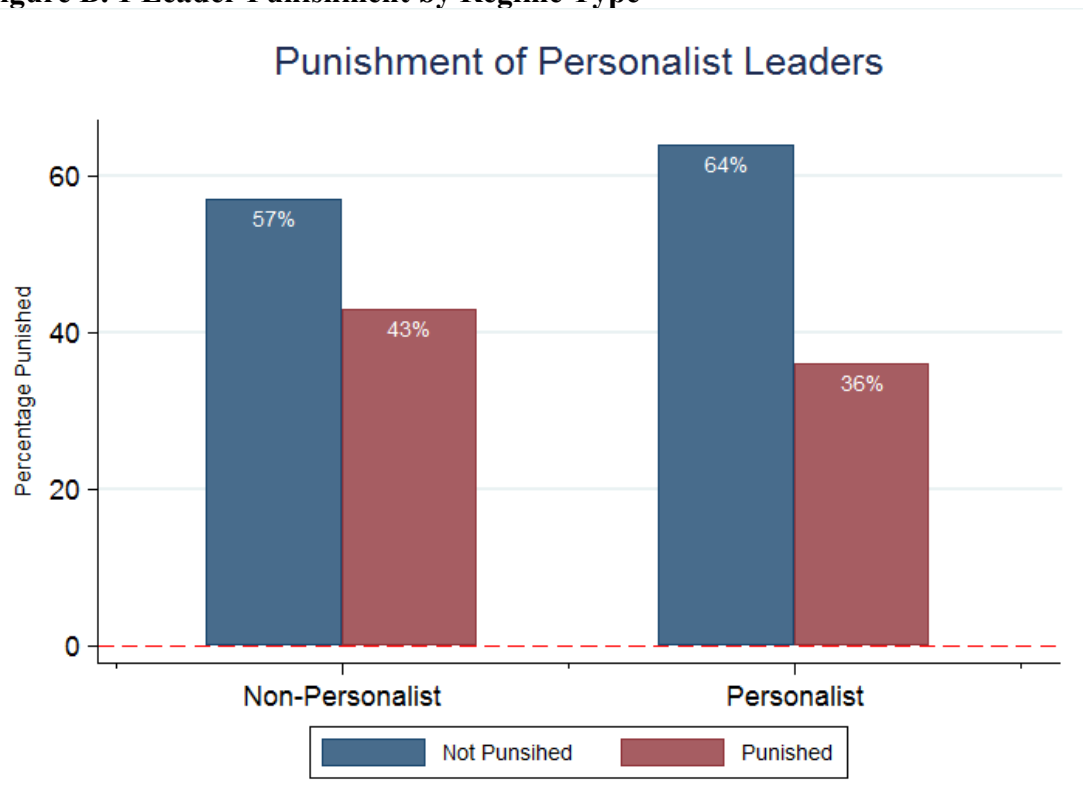
Elites in personalist regimes, therefore, cannot sanction the leader without jeopardizing their own political (or physical) survival. This suggests that personalist

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<sup>65</sup> While Weeks is focused on how leaders generate audience costs to signal resolve, the general point is applicable to this situation as well.

leaders should be insulated against internal punishment, as insider elites face a coordination problem that prevents them from collaborating to remove a personalist leader from office. This trend is generally observable in the data, furthermore. As Figure B.1 demonstrates, 43 percent of non-personalist leaders are punished, while only 36 percent of personalist leaders face punishment. While this seven percentage point difference is relatively small, it does provide initial evidence in support of the idea that personalist leaders are less vulnerable to punishment than non-personalist leaders.

**Figure B. 1 Leader Punishment by Regime Type**



Given that punishing the leader is the key causal mechanism in the culpability argument developed in this dissertation, Weeks's (2008) argument and the trend in Table B.1 suggest that the important regime-type distinction, that which may condition the impact of culpability, is not between democracies and autocracies, but instead between personalist and non-personalist regimes. Further, Weeks's findings have important implications for the relationship between culpability and civil war termination and outcomes. If personalist leaders are insulated against punishment by insider elites, even when they preside over policy failures such as wartime losses, then culpability should have little impact on the likelihood of leader punishment *among personalist leaders*. Culpable personalist leaders, who will be viewed as responsible for wartime losses by winning coalition members, can avoid punishment because regime elites lack the ability and incentive to punish these leaders for their poor wartime performance.

Because personalist, culpable leaders can avoid internal punishment, they should be less responsive to the pressures of culpability when it comes to their wartime decision-making. Because they face no greater threat of punishment than non-culpable leaders, culpable personalist leaders will face no incentive to fight longer or harder in a gamble for resurrection or to avoid concessions-making at war termination. This suggests that, among personalist leaders, culpability should not significantly affect war termination, outcomes, or the likelihood of concessions at termination.

## 1.2 Why Culpability's Impact may *Not* be Conditional on Regime Type

There are other reasons, however, to expect that culpability will affect leader behavior regardless of regime type. That is, even personalist leaders may face incentives to gamble for resurrection and avoid making concessions to an opponent.

This expectation follows from the unique nature of punishment in the context of civil war. Unlike in the interstate war context, leaders during civil war face a threat of punishment not only from within their own winning coalitions, but also by their civil war opponents. The regime characteristics that insulate personalist leaders against internal punishment – control over the security apparatus and political appointments – have no bearing on the ability of opponent elites to punish the adversary's leader. Unlike internal regime elites, opponent elites do not face a coordination problem in attempting to sanction their adversary's personalist leader because their fates are unconnected to that of the personalist adversary leader. Opponent-based punishment, therefore, should still be possible when a personalist leader is in power.

Further, in addition to the incentives opponent elites have to punish a culpable adversary leader, they may have additional incentives to seek punishment when the opposing forces are led by a culpable *personalist* leader. The highly centralized organizational structure of personalist regimes means that the personalist leader exerts a high level of control over decision-making on military and political personnel and promotions, strategy, and tactics. Because decision-making authority is concentrated in the individual in these hierarchically organized regimes, adversary elites may calculate that removal of the leader will cripple the opponent, critically undermining

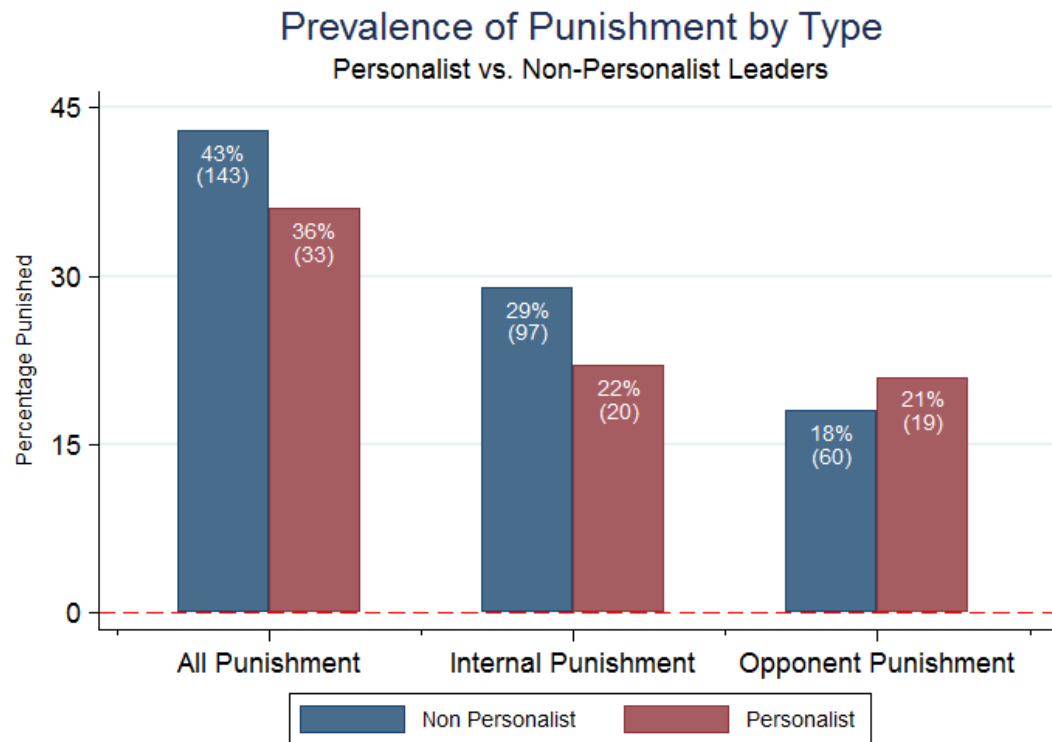
its war effort and increasing their own chances of victory.<sup>66</sup> A personalist organizational structure, therefore, may incentivize opponent elites to seek removal of the culpable personalist leader.

And in fact, when data on leader punishment is broken down by the source of that punishment (i.e. internal or opponent-based), personalist leaders are slightly more likely than non-personalist leaders to face punishment by the opponent. Figure B.2 shows that punishment, regardless of source, is slightly more likely for non-personalists than personalists. This trend continues when looking only at internal punishment: non-personalist leaders are punished internally 29 percent of the time while personalists are punished internally in only 22 percent of cases. As expected, however, this trend reverses when examining only opponent-based punishment. Non-personalist leaders are punished by the opponent in 18 percent of cases while personalist leaders face opponent-based punishment slightly *more* often, 21 percent of the time. These trends, therefore, provide preliminary support for the idea that the impact of punishment is not dependent upon regime type because of the different sources of leader punishment in civil war.

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<sup>66</sup> This justification is commonly used by counterinsurgency strategists; that decapitation of the organization by removing the leader will lead to the military defeat of the group (Johnston 2012; Price 2012). What scholars studying the effects of decapitation strategies fail to account for, however, is the political organization of the group. That is, they fail to recognize that decapitation of the organization is likely to be more effective when a personalist leader controls the organization.

**Figure B. 2 Prevalence of Punishment by Type, Personalist/Non-Personalist**



## II. Theoretical Expectations

The above theoretical discussion suggests that culpable personalist leaders should be insulated against internal punishment, but should face at least as high an expectation of opponent-based punishment as culpable non-personalist leaders. This implies that the overall threat of punishment is diminished only slightly for personalist culpable leaders, relative to non-personalist culpable leaders, and that culpability should still significantly decrease the likelihood of conflict termination and political concessions, while increasing the likelihood of extreme outcomes and leader punishment, even among personalist leaders. The following hypotheses result from this discussion:

**Hypothesis B1:** Culpability will significantly decrease the likelihood of conflict termination when a personalist leader is in power.

**Hypothesis B2:** Among personalist leaders, culpability will significantly increase the likelihood of extreme war outcomes.

**Hypothesis B3:** Among personalist leaders, culpability will significantly decrease the probability of political concessions at termination.

**Hypothesis B4:** Culpable, personalist leaders will be significantly more likely to experience punishment than non-culpable, personalist leaders.

### **III. Research Design and Data**

To test these theoretical expectations, I use the same datasets used for the analyses in chapters 3 and 4, with one notable addition: I code an additional variable which measures each leader's regime type. Specifically, I code each leader in the dataset as either personalist (1) or non-personalist (0) based upon the following coding rules. The key criteria used for coding a leader as personalist include: (1) the leader personally controls the security apparatus and (2) access to political office is dependent upon the leader (Geddes 2003).

Data on state leaders' regime types comes from Geddes, Wright and Frantz's (2012) Global Political Regimes dataset, which codes all autocratic states regime

types between 1946 and 2010. There is no existing cross-national data on rebel groups' regime types. I therefore did original research on each rebel leader in order to classify him or her as personalist or non-personalist leader. I followed the two key coding rules listed above when coding rebel leaders' types, coding the rebel leader as personalist if he met both criteria.

Vellupillai Prabhakaran of the LTTE, for example, is coded as a personalist rebel leader. Sources describe him as enjoying 'absolute power' as supreme leader of the LTTE, which was reflected in the fact that LTTE cadres were required to not only pledge allegiance to the organization, but to Prabhakaran himself. The LTTE leader was both Chairman of the group's Central Committee and Commander in Chief of its military wing, and evidence indicates that he exerted tight control over appointments to both branches of the organization. Further, he punished any challenges to his rule quickly, harshly and violently, thereby maintaining tight control over the organization throughout his decades-long rule.

Coding regime type for rebel leaders is quite difficult in some cases, as there are many rebel groups for which very little is known about their internal political or organizational structures. Due to lack of available information, I was unable to code leader type for 26 rebel leaders. These leaders are consequently excluded from the analyses below. The results, therefore, should be treated as preliminary and suggestive rather than authoritative. Table B.1 provides information on the distribution of regime type for state and rebel leaders.



**Table B. 1 Leader's Regime Type during Civil War**

Regime Type	All Leaders Frequency (%)	State Leaders Frequency (%)	Rebel Leaders Frequency (%)
Non-Personalist	354 (78%)	211 (74%)	143 (86%)
Personalist	99 (22%)	75 (26%)	24 (14%)
Total	453 (100%)	286 (63%)	167 (37%)

### III. Results

To test hypotheses B1 to B4, I reran each of the main analyses using a split-sample. That is, I ran each model separately on the sub-sample of non-personalist leaders and then again on the sub-sample of personalist leaders. This allows me to test whether culpability has an impact on war outcomes and punishment for both personalist and non-personalist leaders.

#### 3.1 War Termination

The results of each analysis are presented below. I begin with the test of hypothesis B1 regarding civil war termination. Model 1 in Table B.2 presents the results of the war termination analysis for the non-personalist leader subsample, and model 2 in Table B.2 presents the results for the analysis run on the subsample of civil conflict dyad years in which at least one personalist leader is in power.<sup>67</sup>

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<sup>67</sup> There are not enough dyad years in which both leaders are personalist to run the analysis on that sub-sample separately.

**Table B. 2 Logit Results for Termination, Personalist/Non-Personalist Samples**

	Neither Leader Personalist	At Least One Personalist Leader
One Culpable Leader	-0.952 <sup>**</sup> (0.481)	-1.041 <sup>*</sup> (0.605)
Both Leaders Culpable	-1.173 <sup>***</sup> (0.420)	-1.553 <sup>***</sup> (0.603)
Strength Disparity	-0.402 (0.374)	-0.106 (0.301)
ICC Signatory	0.838 <sup>*</sup> (0.432)	0.963 <sup>***</sup> (0.330)
Balanced Interventions	0.111 (0.355)	-0.673 <sup>*</sup> (0.370)
Casualties (ln)	-0.112 (0.0846)	-0.0928 (0.0745)
Democracy	-0.204 (0.513)	-1.258 (0.998)
Population(ln)	-0.180 (0.190)	-0.428 <sup>***</sup> (0.166)
GDP Per Capita (ln)	0.116 (0.195)	0.230 (0.226)
War Duration	-0.322 <sup>**</sup> (0.143)	-0.147 (0.192)
War Duration Squared	0.0222 <sup>**</sup> (0.0110)	-0.000233 (0.0201)
War Duration Cubed	-0.000439 <sup>*</sup> (0.000233)	0.000279 (0.000573)
Constant	2.314 (2.232)	2.989 (1.986)
Observations	479	496

Robust standard errors in parentheses, clustered on conflict dyad. \*p<0.10, \*\*p<0.05, \*\*\*p<0.01.

As the results in Table B.2 show, *one leader culpable* and *both leaders culpable* are negative and significant in both models. This indicates, as expected, that even among the sub-set of dyad years in which a personalist leader is in power, culpability significantly decreases the likelihood of conflict termination. That is, having a one or two culpable leaders in power makes civil conflict more difficult to terminate.

### **3.2 War Outcomes**

Table B.3 presents the result of the analysis of extreme war outcomes. Model 1 in Table B.3 presents the analysis using non-personalist leaders only, while Model 2 presents the results when including only personalist leaders in the sample. As these results demonstrate, leader culpability is a positive, significant predictor of extreme war outcomes in both samples. That is, as predicted in hypothesis B2, among personalist leaders (model 2), culpability significantly increases the likelihood that a leader experiences an extreme war outcome. This result indicates, as expected, that culpability's impact on extreme war outcomes is not conditional on regime type; among both personalist and non-personalist leaders, culpability matters.

**Table B. 3 Logit Results for Extreme Outcome, Personalist/Non-Personalist Samples**

	Non-Personalist Leader	Personalist Leader
Culpability	2.634*** (0.586)	3.422* (1.766)
Strength Imbalance	0.455 (0.669)	1.915* (1.006)
Casualties (ln)	0.149 (0.106)	0.455 (0.366)
Mediation	-0.445 (0.514)	-0.447 (0.649)
External Support	-0.465 (0.471)	1.314 (0.800)
Incompatibility	-0.621 (0.591)	-0.958 (0.804)
Leader Conf Duration (ln)	-0.373 (0.281)	-0.830 (0.657)
Rebel Political Wing	-0.695 (0.615)	-1.502** (0.709)
Constant	-0.428 (0.761)	-2.383 (1.829)
Observations	136	53

Robust standard errors in parentheses, clustered on conflict. \*p<0.10, \*\*p<0.05, \*\*\*p<0.01.

### 3.3 Political Concessions

The results for the test of Hypothesis B3 on political concessions are presented in Table B.4. As before, Model 1 presents the results run on the sub-sample of non-personalist leaders, while Model 2 presents the results of the analysis run on the sub-sample of personalist leaders. Hypothesis B3 predicted that among personalist leaders, culpability would significantly decrease the likelihood that the

leader makes concessions to end a civil war.

**Table B. 4 Logit Results for Political Concessions, Personalist/Non-Personalist Samples**

	Non-Personalist Leader	Personalist Leader
Culpability	-2.498*** (0.493)	-1.737 (1.363)
Strength Imbalance	-1.659** (0.652)	-1.951* (1.043)
Casualties (ln)	-0.220** (0.100)	-0.526** (0.261)
Mediation	1.425** (0.588)	2.744*** (1.019)
External Support	0.217 (0.533)	0.111 (0.959)
Incompatibility	-1.535*** (0.535)	-0.580 (1.202)
Leader Conf Duration (ln)	0.720*** (0.191)	0.677** (0.332)
Rebel Political Wing	0.372 (0.614)	1.425 (0.921)
Constant	0.283 (0.788)	0.503 (1.670)
Observations	136	53

Robust standard errors in parentheses, clustered on conflict. \*p<0.10, \*\*p<0.05, \*\*\*p<0.01.

Unlike the results discussed thus far, the results for political concessions are less supportive of the hypothesized relationship. Culpability significantly decreases

the likelihood of political concessions among non-personalist leaders (model 1), but while the effect is negative, as expected, for the personalist leader subsample (model 2), the effect is not significant. This indicates that culpability's effect on the likelihood that a leader makes political concessions to end a conflict may be conditional on regime type. These results are based upon a very small sample (N=53), however, so should be treated with some caution.

### **3.4 Leader Punishment**

Finally, the results for the test of Hypothesis B4 on the likelihood of punishment are presented in Table B.5. Model 1 presents the results for the non-personalist sub-sample, while Model 2 presents the results for the personalist sub-sample. These models include an interaction term between culpability and war performance, so to better understand the statistical significance and substantive impact of culpability, I turn directly to first differences presented in Figure B.3.

**Table B. 5 Logit Results for Leader Punishment, Personalist/Non-Personalist Samples**

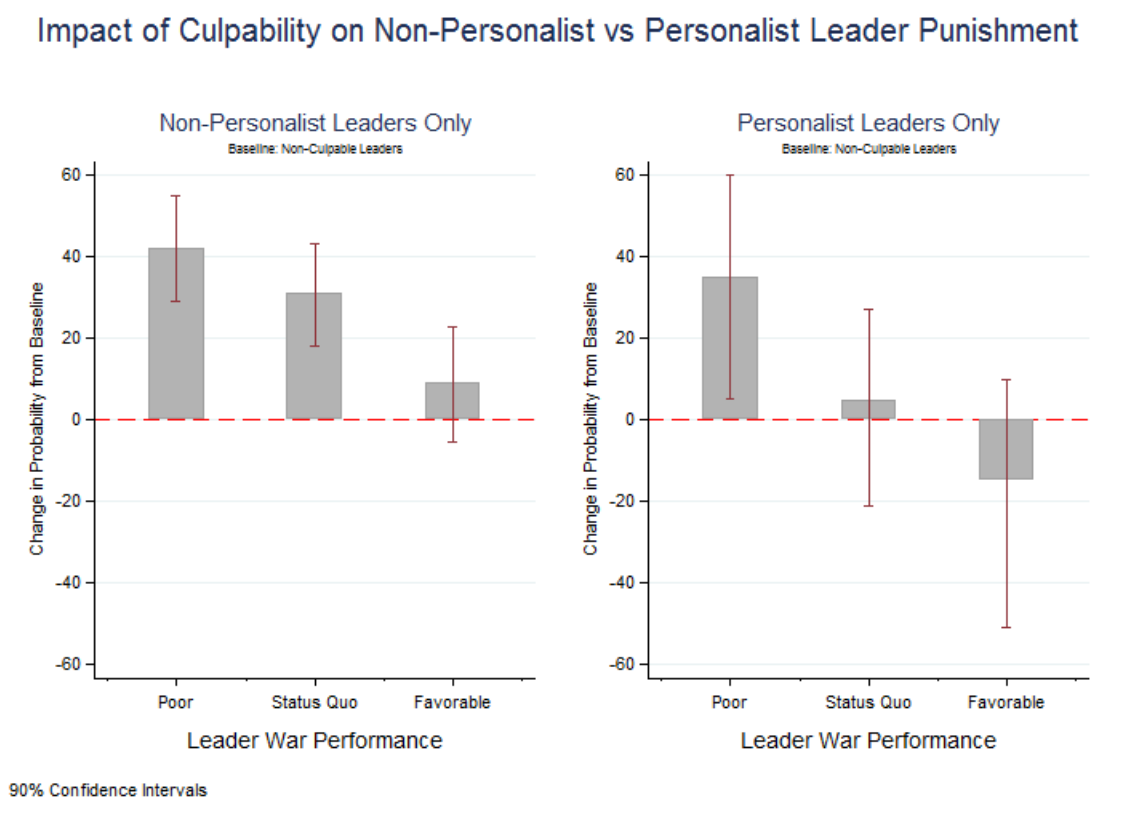
	Non-Personalist Leader	Personalist Leader
Culpability	1.352*** (0.362)	0.332 (0.831)
War Performance	-0.776*** (0.287)	-0.0435 (0.594)
Culpability X War Performance	-0.696* (0.357)	-1.358* (0.735)
Casualties (ln)	0.175** (0.0725)	0.412** (0.174)
Leader Conf Duration (ln)	-0.160 (0.128)	-0.224 (0.250)
Leader Location	-0.506 (0.542)	0.292 (1.415)
Term Limits	-0.314 (0.548)	
Transitional Leader	-2.141*** (0.795)	
Constant	-1.614*** (0.505)	-3.030** (1.247)
Observations	335	92

Robust standard errors in parentheses, clustered on conflict. \*p<0.10, \*\*p<0.05, \*\*\*p<0.01.

The left-hand side of Figure B.3 presents the change in the predicted probability of punishment across different war outcomes when moving from a baseline non-culpable leader to a culpable leader, within the sub-sample of non-personalist leaders. The right-hand side of Figure B.3, on the other hand, presents the same change in the predicted probability of punishment, but among the sub-sample of

personalist leaders instead. As expected, the results for the non-personalist sub-sample reflect those from the original analysis. Culpability significantly increases the likelihood of punishment for leaders who perform poorly and for leaders who only manage to maintain the status quo.

**Figure B. 3 Impact of Culpability on Non-Personalist vs. Personalist Leader Punishment**



Among personalist leaders, the results are slightly different. As predicted in Hypothesis B4, among personalist leaders, culpability significantly increases the likelihood of punishment for those who experience war losses, or perform poorly in



the war. Culpability also increases the likelihood of punishment for personalist leaders who achieve the status quo, but this effect is not significant, unlike in the non-personalist subsample. Finally, culpability actually decreases the likelihood of punishment for personalist leaders who perform favorably in civil war, although this effect is again not significant.

These results suggest that culpability's impact on leader punishment is slightly impacted by regime type. While personalist regime type cannot save a culpable leader who suffers wartime losses, it does appear to mitigate the impact of culpability among leaders who manage to maintain the wartime status quo.

#### **IV. Conclusion**

This appendix has examined the potential conditionality of culpability on regime type. While culpability's impact on war outcomes has been shown to be conditional on regime type during interstate war, I argued that the unique dual sources of punishment that leaders face during civil war mitigate any regime type conditionality in the relationship between culpability and each of the outcome examined in this dissertation during civil war. While leaders of personalist regimes are insulated against internal punishment due to the coordination problem faced by elites in these regimes, personalist regime leaders are just as likely, if not more so, to face punishment at the hands of their wartime opponent. Based upon these theoretical arguments, I hypothesized that culpability would decrease the likelihood of termination and concessions among both non-personalist and personalist leaders,

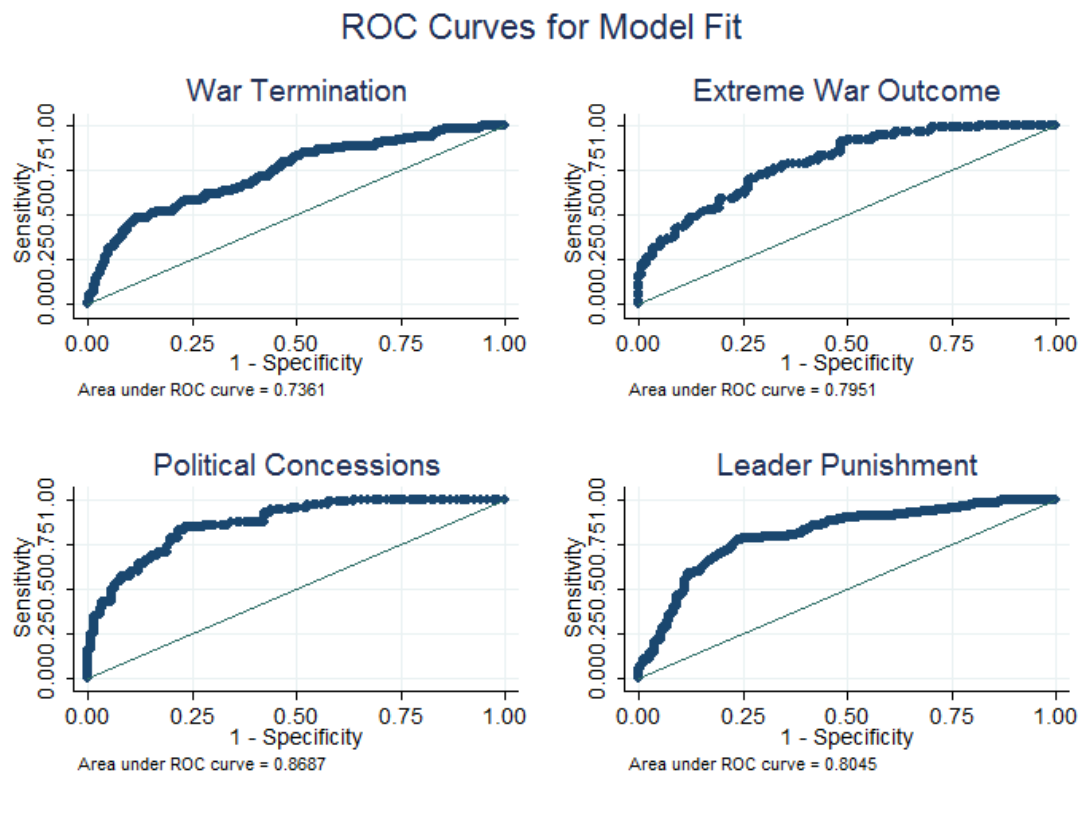
while increasing the likelihood of extreme outcomes and punishment among both non-personalist and personalist leaders.

Overall, the results presented above provide relatively strong support for my expectation that culpability's impact on war termination, war outcome, concessions, and leader punishment is not conditional on regime type. Culpability has a significant impact on termination and extreme war outcomes, even among personalist leaders. Further, it increases the likelihood of punishment among personalist leaders who perform poorly in war. Only culpability's impact on political concessions appears to be conditional upon culpability: personalist, culpable leaders are no less likely to make concessions to terminate a war than personalist, non-culpable leaders.

## C Appendix: Additional Tables and Figures

This appendix includes additional tables and figures excluded from Chapters 3 and 4 due to space considerations. Additionally, it includes Receiver Operating Characteristic (ROC) curves as a model fit diagnostic for each of the four main analysis presented in the main text.

**Figure C. 1 ROC Curves for Model Fit, H1-H4**



The ROC curves presented in Figure C.1 plot the true positive rate against the false positive rate for each of the main analyses. This provides a measure of accuracy for each model, and thus the goodness of fit. The area under a ROC curve can range from 0.5 (very poor fit – the plotted line will follow the 45 degree line) to 1.0 (perfect accuracy – the plotted line will follow the left-hand side and then the top of the graph region). As Figure C.1 demonstrates, the area under the ROC curve for the main models presented in chapters 3 and 4 ranges from 0.736 to 0.869, with the lowest area for the war termination equation and the highest for the political concessions equation. These values indicate that the models range from good to excellent in terms of their level of accuracy.

### **Additional Results Tables from Chapter 3**

The tables below are those not presented in full in Chapter 3 due to space limitations. First, Table C.1 presents the full results table for the main analysis of civil war termination. These are the same results as those in Figure 3.1 from the main text, but Table C.1 includes the time controls and constant, which were omitted from the presentation of coefficient estimates in Figure 3.1.

**Table C. 1 Logit Results for Probability of Civil War Termination**

	B	S.E.	P-Value
One Culpable Leader	-0.746	(0.372)	0.045
Both Leaders Culpable	-1.199	(0.379)	0.002
Strength Disparity	-0.138	(0.208)	0.509
ICC Signatory	0.860	(0.223)	0.000
Balanced Interventions	-0.366	(0.248)	0.139
Casualties (ln)	-0.110	(0.050)	0.027
Democracy	-0.474	(0.371)	0.202
Population(ln)	-0.342	(0.108)	0.002
GDP Per Capita (ln)	0.088	(0.123)	0.472
War Duration	-0.378	(0.101)	0.000
War Duration Squared	0.026	(0.008)	0.001
War Duration Cubed	-0.000	(0.000)	0.004
Constant	3.472	(1.424)	0.015
Observations	1038		

Estimated using logistic regression. Standard errors in parentheses, clustered on conflict dyad.  
P-values in third row.

Table C.2 presents the predicted probabilities, first differences, and percentage changes for the control variables included in the war termination and extreme outcomes models from Chapter 3. The discussion of control variables presented in the main text focused on the political concessions model only, and these tables were excluded from the main text in the interest of brevity.

**Table C. 2 First Differences and Percentage Changes for Control Variables**

<b>I. War Termination Model</b>				
	Predicted Probability of Termination		First Difference	Percentage Change
	Low	High		
Strength Disparity	16.44%	12.77%	-3.67 (-16.21, 5.94)	-22.32%
ICC Signatory	14.34%	28.08%	13.74 (5.48, 24.14)	95.82%
Balanced Interventions	14.34%	10.57%	-3.77 (-9.28, 1.33)	-26.29%
Casualties (ln)	15.70%	12.06%	-3.64 (-7.87, -0.33)	-23.18%
Democracy	14.34%	9.99%	-4.35 (-11.45, 3.25)	-30.33%
Population (ln)	17.69%	11.37%	-6.32 (-11.79, -2.20)	-35.73%
GDP Per Capita (ln)	13.60%	15.18%	1.58 (-2.99, 6.36)	11.62%
<b>I. Extreme Outcomes Model</b>				
	Predicted Probability of Termination		First Difference	Percentage Change
	Low	High		
Strength Imbalance	19.47	34.48	15.00 (1.02, 33.00)	77.04%
Mediation	19.47	9.10	-10.37 (-25.92, -1.29)	-53.26%
External Troops	19.47	31.36	11.89 (-4.03, 33.36)	61.04%
Rebel Political Wing	19.47	9.22	-10.25 (-24.09, -1.56)	-52.65%
Democracy	19.47	8.98	-10.49 (-28.06, 2.73)	-53.87%
Incompatibility	19.47	19.23	-0.24 (-10.94, 13.23)	-1.24%
Ln Casualties	14.96	25.68	10.72 (-0.71, 29.01)	71.64%
Ln Leader Conflict Duration	28.06	12.33	-15.72 (-32.28, -3.96)	-56.04%

Note: To estimate the predicted probability, the covariate of interest is moved from low to high (25<sup>th</sup> to 75<sup>th</sup> percentile for continuous variables) while holding all other covariates at mean or modal values. First differences are computed by subtracting the baseline predicted probability (covariate at low value) from the predicted probability following the change in the covariate of interest. I then divide the discrete change by the baseline probability and multiply by 100 to get the percentage change. Ninety-five percent confidence intervals are presented in parentheses.

Table C.3 corresponds to Figure 3.8 in Chapter 3; it provides full results for the war termination model using a four-category DV.

**Table C. 3 Multinomial Logit Results for Type of Civil War Termination**

	Peace Agreement vs. Ongoing	Peace Agreement vs. Victory	Peace Agreement vs. Other
Both Leaders Culpable	1.097 (0.400) 0.006	2.018 (0.807) 0.012	0.513 (0.614) 0.403
Strength Disparity	0.730 (0.341) 0.033	1.475 (0.539) 0.006	0.680 (0.458) 0.138
ICC Signatory	-1.272 (0.344) 0.000	-0.737 (0.559) 0.187	-0.450 (0.558) 0.420
Balanced Interventions	-0.089 (0.341) 0.795	-0.641 (0.586) 0.274	-0.871 (0.607) 0.151
Casualties (ln)	0.270 (0.066) 0.000	0.505 (0.143) 0.000	0.148 (0.109) 0.174
Democracy	0.614 (0.625) 0.326	0.270 (0.911) 0.767	0.127 (0.885) 0.886
Population(ln)	0.559 (0.139) 0.000	0.318 (0.272) 0.242	0.367 (0.219) 0.094
GDP Per Capita (ln)	-0.035 (0.183) 0.849	-0.319 (0.317) 0.315	0.389 (0.316) 0.219
Constant	-5.893 (1.764) 0.001	-7.246 (2.883) 0.012	-8.443 (2.848) 0.003
Observations	1038		

Standard errors in parentheses, clustered on conflict dyad. P-values in third row. Time controls excluded from output.

Table C.4 presents the full results table that corresponds with Table 3.8 in chapter 3. This presents the results of the multinomial logit model for war outcome using a four-category DV: military loss, negotiated loss, negotiated win, and military victory.

**Table C. 4 Multinomial Logit Results for Civil War Outcome, Negotiated Loss Base Category**

	Military Loss vs. Negotiated Loss	Negotiated Win vs. Negotiated Loss	Military Win vs. Negotiated Loss
Culpability	1.053 (0.633) 0.096	1.237 (0.520) 0.017	1.845 (0.683) 0.007
Strength Imbalance	1.958 (0.782) 0.012	0.090 (0.321) 0.779	2.154 (0.777) 0.006
Casualties (ln)	0.460 (0.161) 0.004	-0.062 (0.067) 0.358	0.574 (0.165) 0.000
Mediation	-3.523 (0.786) 0.000	0.474 (0.264) 0.072	-3.396 (0.797) 0.000
External Troop Support	0.666 (1.312) 0.612	-1.178 (1.631) 0.470	1.368 (0.935) 0.143
Incompatibility	1.685 (0.656) 0.010	0.015 (0.202) 0.940	1.375 (0.659) 0.037
Leader Conflict Duration (ln)	-0.960 (0.261) 0.000	0.188 (0.181) 0.298	-0.942 (0.240) 0.000
Rebel Political Wing	-1.182 (0.683) 0.084	0.235 (0.234) 0.316	-1.382 (0.688) 0.045
Democracy	-0.220 (0.925) 0.812	-0.099 (0.211) 0.638	0.006 (1.033) 0.996
Constant	0.938 (0.961) 0.330	-1.346 (0.725) 0.064	-0.584 (0.923) 0.527
Observations	207		

Standard errors in parentheses, clustered on conflict. P-values in third row. Base Category is Negotiated Loss



Finally, Table C.5 presents the table of coefficient estimates corresponding to the post-estimation results presented in Figure 3.10 in chapter 3. This table provides the results of the analysis of *favorable* war outcomes, using an interaction term between leader culpability and the combatant group's relative strength.

**Table C. 5 Logit Results for Favorable War Outcomes**

	B	S.E.	P-Value
Culpability	0.285	(0.527)	0.588
Relative Strength	-0.251	(0.419)	0.549
Culpability X Relative Strength	0.737	(0.416)	0.076
Casualties (ln)	0.013	(0.038)	0.721
Mediation	0.427	(0.182)	0.019
External Support	0.517	(0.324)	0.111
Loss of Troop Support	-2.290	(0.573)	0.000
Leader Conflict Duration (ln)	0.182	(0.085)	0.033
Constant	-1.489	(0.593)	0.012
Observations	207		

Standard errors in parentheses, clustered on conflict. P-values in third column.

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