

**PEACE AT WHAT PRICE?:  
DOMESTIC POLITICS, SETTLEMENT COSTS AND WAR TERMINATION**

by

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## **Dedication**

To my parents, Ed and Suzanne Croco, and sister, Lacey Croco, for everything.

And to my husband, Ian Hall, for asking.

## **Acknowledgements**

On paper, graduate school sounds like a miserable process: Read till your vision becomes impaired; spend endless hours in libraries and computer labs; come up with an original research topic and then write a book. By yourself.

My experience, however, has been anything but miserable. The years I have spent at Michigan have been some of the most challenging of my life, but they have also been some of the most rewarding. The people I thank here are just some of the many who made this dissertation possible, and who made graduate school a time to be enjoyed instead of something to be suffered through.

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came home from college and declared she was off to graduate school to become—of all things—a political scientist. I have both of them to thank for my love of reading and learning; my mom for the countless phone calls when things were good and bad; and my dad for telling me time and again that getting a Ph.D., like everything else in life, is all about finding the strength to keep putting one foot in front of the other. That single piece of advice got me through several long nights of working on the dissertation. Mom and Dad, you can (finally!) tell all your friends that my “paper” is done.

Last, but certainly not least, on the list of people who made this dissertation possible is my husband, Ian Hall. As in every aspect of our life together, Ian was a true partner throughout the dissertation writing process. Although I often made light of the “Ian Hall Fellowship”, I am very grateful for the break from teaching it permitted. Ian also read several portions of the dissertation and offered much appreciated feedback. Finally, and most importantly, words cannot express how thankful I am for the companionship and love I’ve found in him.

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## Chapter 1

### 1.0 Introduction

In January 2007, as American military forces continued to engage the growing insurgency movement in Iraq, President Bush faced a very different yet equally intractable battle domestically. His approval rating, which reached above 90 percent when the war began in 2003, had been in the 30s for more than a year and showed no signs of improving. The midterm elections of the previous year had also dealt him a serious political blow. For the first time in twelve years a Democratic majority returned to both houses of Congress, armed with what some saw as a clear mandate from the American people to end the war.

The December publication of the report from the Iraq Study Group, a bipartisan effort commissioned by Congress to produce both assessments of the war's progress and recommendations for next steps, also did little to improve the president's standing domestically. Although the report discussed the potential negative consequences of a "precipitate withdrawal," the overall theme of the report—with its gloomy characterization of the "grave and deteriorating" situation in Iraq and repeated calls for drastic changes in strategy—was that President Bush's war was not going well.<sup>1</sup> All of this gave rise to the realization for many Americans that the probability of "victory" in Iraq, however defined, was steadily declining.

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<sup>1</sup> Baker and Hamilton (2006).



Even in this atmosphere of growing dissent, President Bush's commitment to the war remained steadfast. While recognizing public frustration with the lack of progress and taking responsibility for mistakes that had been made, Bush continued to stress that "failure was not an option" and called on Americans to "find our resolve and turn events toward victory."<sup>2</sup>

The debate over the war continued, however, and in the spring of 2007 the Democratic Congress attempted to exercise its "power of the purse" by refusing to approve a supplemental funding bill for the war. This move prompted President Bush to adopt a far less conciliatory tone as he branded the Democratic leadership "irresponsible" and their demands for a timetable for withdrawal "unreasonable."<sup>3</sup> After the bill eventually passed into law in May, President Bush reemphasized his position, stating "when we start drawing down our forces in Iraq, it will be because our military commanders say conditions on the ground are right, not because pollsters say it will be good politics."<sup>4</sup>

Such is the current state of American politics; with a government sharply divided along partisan lines and what the next steps in Iraq should be, the war continues in an atmosphere of domestic political gridlock. In spite of this rather mundane, "business as usual" façade, however, the root cause of the debate should be of interest to political scientists for at least two reasons. First, the fact that a war initiated by a democratic state in 2003 is even an issue in 2008 is something no existing theory of war termination

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<sup>2</sup> "President's Address to the Nation" January 10, 2007; "National Strategy for Victory in Iraq" (February, 2007); "State of the Union Address" January 23, 2007.

<sup>3</sup> "Address to American Legion Post 177" April 10, 2007; "Presidential Address in the East Room" April 16, 2007.

<sup>4</sup> "Presidential Press Conference" July 12, 2007.

would predict—indeed, instances of a democracy prosecuting a protracted war should be exceedingly rare.<sup>5</sup>

Second, the degree to which President Bush is determined to stay in the war *despite* public disapproval represents another major departure from the predictions of extant theory.<sup>6</sup> In a political system driven by a mass electorate, conventional wisdom would predict the leader's preferences would move in sync with the citizens', yet President Bush's position on the war grows increasingly at odds with a larger percentage of the American people with each passing day.

## 1.1 The Puzzle

The combination of these two anomalies—a democracy prosecuting a protracted war it is unlikely to win and a democratic leader so committed to maintaining current policy in the face of widespread public opposition—inspire the puzzle this dissertation will address. Namely, what accounts for the variation we see in war termination behavior? Why do some leaders keep their countries in a war until defeat is forced upon them while others exhibit a willingness to comply with their adversary's demands to bring the war to a close? Why do some leaders bow to popular opinion by ending costly wars while others insist on continuing to fight?

Until now, existing theory has explained the variation in the types of outcomes leaders accept with an expected utility approach—leaders will end wars when the costs of fighting outweigh the potential benefits if victory were eventually achieved.<sup>7</sup> According to this theory, leaders who rush to the negotiating table are those who face a negative

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<sup>5</sup> Reiter and Stam (2002) and Slantchev (2004).

<sup>6</sup> Bennett and Stam (1996 & 1998).

<sup>7</sup> For example, Bueno de Mesquita (1981) and Wittman (1979).

utility for continuing to fight—that is, high costs of fighting and/or low benefits of victory—while leaders who stay in the conflict are those whose expected utility for fighting has yet to enter the red—low costs of fighting and/or high benefits of victory.

However, closer inspection of the historical record (or, for that matter, a contemporary newspaper) quickly demonstrates that the traditional cost/benefit analysis has difficulty explaining several leaders' decisions to fight for ends that do not seem to justify the costs they are willing to pay. To return to the Iraq example, we may be tempted to classify Bush's behavior as stochastic by assuming his intransigence is merely a reflection of an idiosyncratic personality trait, such as a penchant for "cowboy diplomacy" or a simple tendency for stubbornness. The fact of the matter, however, is that Bush's behavior is in no way unique. Instead, President Bush is simply the most recent in a long line of leaders who willingly stayed in costly, low utility wars despite the domestic political hardship that this choice has often entailed.

For instance, in the Korean War Truman refused to agree to the Communists' terms despite a 23% approval rating for the war and over half the American public demanding he drop the bomb and "get it over with."<sup>8</sup> Several British prime ministers exhibited a similar intransigent tendency in conflicts with colonies struggling for independence.<sup>9</sup> But perhaps the most extreme example of political "stubbornness" is the French experience with the extremely unpopular war in Algeria between 1954 and 1962. In the first four years of fighting alone, the war brought down the governments of *six* different prime ministers, all of whom chose losing office over granting Algeria

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<sup>8</sup> McCullough (1993: 872).

<sup>9</sup> Of the twenty-three British wars fought with colonies since the early 1800s, nearly half killed more than three thousand British soldiers and five dragged on for three years or more (source: Correlates of War Sarkees 2000).

independence.<sup>10</sup> These cases clearly suggest that this willingness to fight for limited ends in the face of high costs is a definite and distinct phenomenon and not just random error.

Further examination of current American politics reveals another interesting dynamic that is often overlooked in discussions of war termination: namely, the behavior of the executive's party. Although some may explain Bush's willingness to stay in the war by pointing to his "lame duck" status and the fact that he, essentially, has nothing to lose by continuing to prosecute the war, this explanation fails to account for the behavior of other prominent Republicans. For instance, Senate Minority Leader Mitch McConnell is running for re-election in 2008, yet has remained a staunch supporter of the war. Indeed, he joined President Bush in rejecting the Democratic calls for a withdrawal timetable, vowing that he would never support legislation that amounted to a "surrender date."<sup>11</sup> Similarly, Senator John McCain, the presumptive Republican presidential nominee for the 2008 election, continues to support the war despite growing dissent, stating that "even though politics and *popular opinion* may be pushing us in one direction, to take the easy course, we as elected leaders have a greater responsibility."<sup>12</sup>

If a lack of political vulnerability explains Bush's behavior, we would expect Republicans with vulnerable political positions to try to distance themselves from Bush and align themselves more closely with the growing number of conservative voters who have become frustrated with the war. But this is not the case. Although some Republicans have begun to break ranks and call for a change in policy, the number of members of Congress who continue to stand with Bush *despite* public dissent suggests

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<sup>10</sup> Source: Sarkees (2000) and Goemans, et al. (2006).

<sup>11</sup> "Press Release from Senator McConnell's Office" May 18, 2007.

<sup>12</sup> "Press Release from Senator McCain's Office" July 17, 2007. Emphasis added.

that the reasons behind Bush's commitment to the war may extend to members of his party as well.

## **1.2 The Answer**

The fact that leaders often deviate from the predictions of the existing war termination literature and that other domestic political actors seem to adhere to a similar, puzzling pattern raises many questions for political scientists. Why do some leaders agree to their adversary's demands while others fight to the bitter end? Why do some leaders follow public opinion by ending unpopular wars while others press on in the face of mass dissent? My dissertation will address these questions and more with a comprehensive theory of war termination that allows for a larger and more active role for domestic politics. At the center of this theory are settlement costs, which I define as the political cost (usually a loss of office) that a leader will be forced to pay if he terminates the war before securing a favorable outcome.

The fact that not all leaders will face settlement costs creates critical differences between leaders that should not be overlooked. As I will argue below, as long as scholars continue to think about leaders as undifferentiated representatives of their regime type, instead of individual politicians as their constituents do, existing war termination theories will continue to miss a crucial element: political pressures to stay in the war that stem directly from the leader's *personal* culpability for the conflict. Although current practices within the literature lead scholars to overlook the large amount of variation that exists in the distribution of this settlement cost, later chapters will demonstrate that the role a

leader played in his state's involvement in the conflict is very consequential to explaining war termination.

Briefly stated, my argument is as follows. Leaders whom the public can clearly link to the decision to involve the state in the war will feel a greater onus to secure a favorable outcome for his or her state because they face a higher likelihood of backlash from their supporters<sup>13</sup> in the event of a loss. For the purposes of this dissertation, leaders in this position are classified as “culpable” if they either made the decision to involve the state in the war *or* if they share a political connection with or supported the leader who did.

By contrast, a “non-culpable leader”—a leader who was against the war when it began or who *lacks* a political connection with his or her predecessor(s)—will feel far less pressure to secure a victory, *ceteris paribus*. Although some circumstances exist under which members of this latter group will be as willing to fight as their more culpable counterparts, their political distance from the decision to participate in the war will allow them greater flexibility in the war outcomes members of their winning coalition will accept.

### **1.3 Chapter Outline**

The rest of this chapter proceeds as follows. First, Section 2 presents a slightly longer overview of the theory to provide a better sense of how the interaction of the war with domestic politics matters to the war termination process. Section 3 highlights the major ways in which the theory presented here improves on the existing literature, and how incorporating leader culpability costs will increase our understanding of the war

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<sup>13</sup> For the purposes of this project, “supporters” is synonymous with “selectorate”.

termination process and the internal politics of warring states. Finally, Section 4 provides an overview of the remaining chapters, with a brief discussion of the research methods and results.

## **2.0 A Settlement Costs Theory of War Termination**

The theory presented in this dissertation proposes several new ideas about how individual leaders approach the war termination decision and how citizens view and interact with those leaders. The theory also highlights several dynamics of wartime domestic politics that have hitherto been largely unstudied by war termination scholars: namely, the fact that leaders can change during the war, that such changes are unlikely to be independent of the progress and outcome of war, and that changes in domestic leadership may have the potential to affect the state's behavior in the international arena.<sup>14</sup> This section briefly explains the theoretical framework of the dissertation to help structure the discussion of the interaction between the war and the domestic politics of the combatants.

As I argue in the next chapter, the key to understanding why some leaders are so reluctant to end wars when others would settle lies in the role the leader personally played in the country's participation in the war. If a leader can be clearly linked to the decision to enter the war, members of the selectorate will hold him to the original war aims and assurances of victory, and will think unfavorably of the leader if he does not deliver on these promises. This leads to a heightened probability of being removed from office for culpable leaders if the state fares poorly in the war, which provides an

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<sup>14</sup> While there are notable exceptions to this claim (see, in particular, Downs and Roche 1994 and Smith 1998), no existing study examines the empirical implications of a change in leadership.

additional incentive for them to stay in the war. I argue that this incentive is largely *independent* of the costs of fighting or the issue at stake in the war. Even if victory is uncertain, costs are high and the issue at stake in the conflict is relatively small in value, culpable leaders will always have this additional incentive to stay in the war and “gamble for resurrection” to avoid political punishment at home.<sup>15</sup>

Leaders acquire this burden of culpability in the eyes of the selectorate in one of three ways: being a first leader, sharing a political affiliation with the first leader, or supporting the war since initiation. The first scenario, which accounts for the majority of culpable leaders, applies to leaders who are in office when the war begins. Importantly, these first leaders *all* will be considered culpable for the purposes of this dissertation, regardless of whether their state’s entry to the war was the result of an active choice on the part of the leader as an initiator or third-party joiner, or an instance of failed deterrence in which the state was attacked.<sup>16</sup>

Leaders can also be culpable if they come to power during the war (i.e., are a “new leader” in this study’s terms). New leaders can inherit their predecessor’s culpability in one of two ways. First, citizens will likely transfer culpability (and the associated pressures) for the war to the new leader if the leader is from the same party, junta, cabinet or (in the case of a monarchy) family of the first leader. Although these political affiliations vary in terms of the closeness they represent from country to country, it is reasonable to say that citizens are more likely to see new leaders drawn from these groups as more connected than a new leader drawn from outside these groups.

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<sup>15</sup> Downs and Rocke (1994). I expand on the differences between my theory and that of Downs and Rocke in Chapter 2.

<sup>16</sup> I relax this assumption in Chapter 3. This assumption stems from an argument put forward by Bueno de Mesquita and Siverson (1995), wherein citizens of targeted states become angry at their leaders for failing to prevent the attack from occurring in the first place.



Leaders can also inherit culpability based on *actions* they took before coming to power. If a new leader was publicly supportive of the decision to go to war and remained a firm supporter of the war before taking office, he will likely take on the first leader's burden to secure a favorable outcome. The new leader's previous position on the conflict binds him to the first leader's decision and its consequences, good or bad.

Not all leaders who come to power during war, however, share a political connection with their predecessor. Although the political institutions of many countries often contain mechanisms that encourage such connections, such as a pre-established line of succession through family ties or a direct political relationship (e.g., a vice president), the accession of someone from outside the current leader's circle of power during times of war is not unheard of. A leader in this position has a substantial advantage over her more culpable counterparts since she can credibly distance herself from the previous leader and the decision to go to war.<sup>17</sup> As Chapter 2 will discuss in greater detail, this distance renders a wider range of war outcomes "acceptable" in the eyes of the public, and removes any incentive for the new leader to continue fighting.

### **3.0. Implications for Existing Practices and Future Research**

Before elaborating on the theory further, it is instructive to identify why the existing literature continues to overlook the culpability of individual leaders. Doing so will highlight the many ways in which the theory presented here advances our collective knowledge of war termination. The theory makes two core assertions. The first is that is that the leader's culpability will trigger a divergence in preferences between him and his citizens in costly wars, and the second is that culpability creates important differences

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<sup>17</sup> In the interest of clarity, non-culpable leaders will always be referred to as "she".

between leaders, even of the same regime type or from the same state. I argue that three specific practices within the existing literatures lead scholars to overlook these two new insights: the tendency to focus on warring states instead of warring leaders; the large amount of theoretical emphasis placed on differences in the political structures between the various regime types; and restricting discussions of leader-specific costs to crisis situations. I will now review each of them in turn, while taking care to point out how changes suggested by the new theory will lead to a more nuanced understanding of the war termination process.

The first problematic practice is scholars' tendency to focus on the warring *state* as the unit of observation instead of the warring leader. In many ways this tendency should not surprise us. Politicians and historians alike often frame wars as battles between two societies, not policies attached to particular leaders. Similarly, the war aims and potential consequences of the conflict are often discussed as though they apply to the leader and citizens of the state collectively—for example, “America’s mission in Iraq” or “Germany’s loss in World War II.”

This approach is understandable, but it is inaccurate. Wars are in fact the policy choices of individual leaders, and the consequences of these choices affect leaders and citizens asymmetrically. Leaders will face the political *costs* associated with a war, while citizens will face the physical *costs*.<sup>18</sup> This distinction is crucial since it underscores the fact that the utility functions of leaders and citizens are comprised of different terms. Leaders will receive all of the potential political fallout that would accompany a loss (settlement costs), but unless they are personally leading the forces into combat, they will

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<sup>18</sup> These costs, of course, are not unrelated; citizens impose political costs on leaders *because* of the physical costs they are made to bear. The implications of fact that settlement costs are both created and imposed by citizens will be discussed in greater detail in Section 3.0 of this chapter.

*not* suffer directly for losses sustained on the battlefield. Instead, these costs will be borne entirely by the citizen population.

The costs are also lopsided in terms of when they affect the actors; settlement costs can only be paid at the end of the war and only then if the leader fails to accomplish his objectives. The physical costs, by contrast, are paid throughout the war, regardless of the eventual outcome. These twin asymmetries create a divergence in preferences between the leader and his citizen.<sup>19</sup> If a non-culpable leader comes to power, settlement costs will cease to be an issue and the preferences of the two will realign.

Using the state as the unit of analysis also draws scholars' focus away from important dynamics that take place inside the warring state, such as leadership changes. This trend is reinforced by the second troublesome practice of placing the majority of the theoretical emphasis on the differences between regime types. Unlike the practice described above, the logic behind this one is not incorrect: The domestic political structures that define autocracies, anocracies and democracies create important differences between states, and scholars would be remiss to ignore them. The emphasis on these differences, however, has robbed scholars of the incentive to look for variation *within* leaders of the same regime type since it focuses on *static* things, like institutions.

In democracies, for instance, the presence of the electoral check will certainly create incentives for democratic leaders that will set them apart for leaders of non-democracies, but the set of democratic leaders is not homogenous in terms of the incentives they face. Instead, culpability costs will produce important differences among them. These differences can even arise in a single state in the same war. Because

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<sup>19</sup> I discuss the limited circumstances under which citizens will share the preferences of a culpable leader in Chapter 2.

culpability costs can change with leaders over the course of the war, it is not safe to pool leaders based on regime type and assume they will behave the same way. As Chapters 2 and 3 will demonstrate in more detail, the differences between culpable and non-culpable leaders of the same regime type are significant and stark.

The final current practice that leads scholars astray is the tendency to limit discussions of leader-specific, political costs to crisis situations. The notion of audience costs—that is, that a leader creates the possibility of political punishment by publicly setting goals he may not achieve—is a sound one. Indeed, the logic behind audience costs informs culpability costs to a large degree since it speaks to a settlement cost that only applies to the leader. The problem here is one of execution. The current practice falters because it only discusses how the audience costs affect the leader (and the citizens' perceptions of that leader) over the course of the immediate crisis, which will typically last for days or, at the most, weeks. The standard argument implicitly and *incorrectly* assumes that leaders will not suffer audience costs once they make the transition from crisis to war. Doing so not only discourages scholars from thinking about how the leader's public promises will *continue* to affect him as the crisis progresses to a war but also leads them to overlook the important differences that can arise between the culpable leaders who first made the promises and the non-culpable leaders who may succeed them over the course of the war.

#### **4.0 Overview of the Remaining Chapters**

The remainder of this dissertation proceeds as follows. Chapter 2 will elaborate on the theoretical framework described above by providing a more detailed discussion of

the circumstances that produce culpable leaders, the expected distribution of culpable leaders across warring states, and how the effect of culpability varies across regimes. I will also explore new insights provided by theory in more detail. Finally, I will present several testable hypotheses derived from the causal propositions suggested by the theory. The first set of hypotheses describe the differences between the outcomes the two types of leaders are likely to settle for while the second set will test for differences in the likelihood of punishment across the two different leader types while holding outcome constant.

Chapter 3 evaluates the hypotheses presented in Chapter 2 using an original dataset and an innovative research design. The dataset consists of every leader who participated in an interstate war between 1816 and 1991, including 79 wars, which produce 257 warring states and 352 wartime leaders. The dataset also includes several new independent variables such as a state's war aims, the strength of the political connections between successive leaders and a measure of how the state was faring in the war at any given time. Finally, this chapter will present findings that strongly support the theory presented in Chapter 2; non-culpable leaders are more likely to preside over poor outcomes and are less likely to be punished for these outcomes than culpable leaders.

The research design used in Chapter 3 is well suited for testing hypotheses about wartime leaders, but can only provide information about how citizens assess these leaders in the aggregate (e.g., whether the electorate punished the leader because of the war). To evaluate the causal chain regarding how citizens transfer accountability for a conflict from one leader to the next more directly, I conducted a survey experiment on a

nationally representative sample of American citizens.<sup>20</sup> Respondents were asked to evaluate a U.S. Senator based on a statement from him calling for an immediate withdrawal of American forces from Iraq. The name of the senator remained the same across all treatment groups while the senator's partisan affiliation and earlier position on the war varied from group to group.

A survey experiment is well suited to this project for two reasons. First, and most obviously, the survey provides greater insight about how individual citizens form opinions of their leaders by allowing me to compare reactions to two types of responsibility: associational and technical. If the causal story presented in Chapter 2 is correct, respondents should be most willing to assign responsibility to the experimental Senator if they think he is a Republican who always supported the war and least willing to assign responsibility to him if they think he is a Democrat who never supported military action.

In addition to this basic test, the survey experiment will allow me to assess the relative strength of the two cues in determining a citizen's assessment of a leader, since the design presents respondents with leaders who represent every possible combination of party membership and earlier support for the war. This aspect of the design is especially appealing since all four possible leader types are unlikely to appear in a dataset that is based on an observational design.

Chapter 5 concludes the dissertation by reviewing the main theoretical arguments and the findings in preceding three chapters. I also discuss new avenues for exploration in

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<sup>20</sup> This survey was funded by a grant from the National Science Foundation (Grant Number 094964) and administered by Time-sharing Experiments for the Social Sciences (TESS).

the broad, yet understudied, areas of wartime politics and citizen assessment of leader accountability.

## **Chapter 2**

### **1.0 Introduction**

In the previous chapter I argued that the key to understanding the variation that exists in leaders' war termination behavior lies in settlement costs, specifically, a leader's culpability. If a leader believes that the act of settling before victory is achieved will result in him losing office, he has no incentive to terminate the war—in fact, he has every incentive to continue prosecuting it, even if the costs of fighting are high. This chapter illustrates the consequences of introducing an individual leader's role in the conflict into theories of war termination.

First, Section 2 will discuss another type of settlement cost—the adversary's war aims--that most closely mirrors the definition used in existing approaches to the leader culpability costs I raise here. Although the primary theoretical contribution of this dissertation centers on leader culpability costs, discussing how the adversary's war aims inform the termination decision is also instructive. It underscores the need to recognize the variety of sources from which settlement costs can spring, demonstrates the benefits of incorporating the leadership culpability settlement cost that can only affect leaders, and illustrates how this settlement cost can vary over the course of the war and arise regardless of the issue at stake.

Section 3 will describe the leader culpability cost in greater detail. Topics will include what types of leaders the cost will apply to; why citizens have incentives to create



this cost; the mixture of culpable and non-culpable leaders we should expect to find within the set of history's warring leaders and how the pressures generated by this cost will vary with a leader's regime type.

Section 4 will discuss the underlying assumptions of the leader culpability theory and discuss two new insights offered by the it, with an eye towards how they account for more of the observed variation in leader behavior than other existing approaches. Specifically I explain when a leader's preferences will diverge from his citizens', and why some leaders are willing to gamble for resurrection while others are not. Section 5 will present several testable hypotheses while Section 6 will conclude.

## **2.0 War Aims of the Adversary**

Framing the war aims of the adversary as a first type of settlement cost closely mirrors the Expected Utility-based approach that underpins many existing war termination theories. According to this logic, if states are fighting over a "salient" issue, they stand to gain a large benefit and, therefore, have much to lose if the adversary wins. In this context, the cost of settling is inherent in the failure to secure the benefit that would have come with victory.<sup>21</sup> In some cases the state is fighting to obtain a benefit it does not currently have (e.g., territory), while in other wars the state may be trying to prevent a negative outcome from occurring (e.g., a loss of sovereignty). Either way, the "net" expected value of winning is positive since the state either receives a gain or avoids a loss.

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<sup>21</sup> Bueno de Mesquita (1981), Bennett and Stam (1996 & 1998), Wittman (1979), Morrow (1985) and Stam (1996).

If a state's opponent is set on annexing territory, enacting a regime change, or occupying the entire country, settling before the pre-war status quo is re-established will have negative consequences for the leader and the citizens alike. This settlement cost, in other words, affects both actors symmetrically. Although the costs for the leader are obvious in wars where a loss of power would come at the hand of the adversary (e.g., wars that involve a regime change or a total loss of sovereignty), a leader should also be concerned about the prospect of losing territory. Depending on the size and location of the territory, surrendering it to an enemy could entail the loss of natural resources, a strategically important position (e.g., Syria's de facto loss of the Golan Heights) or citizens (and the resources they provide the state in the form of labor, defense and taxes).<sup>22</sup>

Citizens should also be concerned for their own welfare when the adversary has high war aims. Wars involving a loss of territory will force the citizens who live in the disputed area to be absorbed into the adversary's jurisdiction, under circumstances where they are unlikely to receive a warm reception. Wars of occupation and regime change have also been costly for civilian populations beyond the initial brutality of the conflict itself as they often usher in leaders who view their new citizenry as a target for exploitation rather than a group worthy of protection.<sup>23</sup>

Because wars against an aggressive adversary entail settlement costs for leader and citizen, we should expect both actors to exhibit higher tolerances for the costs of

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<sup>22</sup> Although some may question my emphasis on the value a leader assigns to maintaining the size of his population, scholars have often used a country's population as a rough approximation of its current and potential future power. (See, for example, (Sarkees 2000) and Organski and Kugler (1980)).

<sup>23</sup> Some wars, of course, remove despots and replace them with leaders whose interests are more in line with those of the public. These wars of "liberation", however, are the exception to the rule and are likely to be regarded as a violation of sovereignty by a large portion of the population.

fighting in these wars than in wars where the adversary is less demanding. The leader may have additional reasons to stay in the war (i.e., culpability costs) but in wars involving an aggressive adversary citizens should share his reluctance to surrender.<sup>24</sup> Evidence of this increased willingness to bear large costs of fighting in the face of high settlement costs appears in recent empirical work and case studies. In work on civilian and military casualties, for instance, scholars have found that having an adversary with expansionist war aims is a strong and consistent predictor of the human toll a state will pay in any given war.<sup>25</sup>

A similar story exists for states fighting to avoid accepting an unconditional surrender, where the terms place the vanquished at the mercy of the victor, and often result in regime change or total occupation. In World War Two, for example both Japan and Germany refused to agree to the Allies' terms and withstood incredible costs until they were absolutely certain defeat was imminent.<sup>26</sup> This idea has led some scholars to conclude high war aims may be counter-productive for the aggressor because the value the target places on the status quo (or preventing the adversary from achieving his war aims) will make him difficult to coerce.<sup>27</sup>

In short, although existing war termination scholarship has traditionally cast the issue at stake in the war as a benefit, in practice aggressive war aims on the part of the adversary function as a settlement cost. Creating these costs for the opponent by setting

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<sup>24</sup> Pape (1996).

<sup>25</sup> Valentino, Huth and Croco (2006).

<sup>26</sup> Slantchev (2003a), Pape (1996: Chapter 8) and Klingberg (1966).

<sup>27</sup> Werner (1996), Filson and Werner (2002), Bueno de Mesquita, et al. (2003), Pape (1996) and Pillar (1983). Savvy aggressors will recognize the potential for this and may choose to manipulate their opponent by making scaling back their demands to increase his utility for settling. Werner (1998) and Coser (1961). Strategies like this are reminiscent of Schelling's (1966) discussion of "salami tactics" where an adversary achieves a larger goal by getting its opponent to agree to a long series of small concessions.

high war aims provides sufficient motivation for the opponent to stay in the war since the value of preventing the consequences of a loss outweighs the costs of fighting.

### 3.0 Leader Culpability Costs

Leadership culpability costs, as suggested in Chapter 1, factor into the war termination decision in a very different way. Three important differences exist between the leader culpability settlement cost and the settlement costs associated with an aggressive adversary. First and most importantly, they are asymmetrical in how they affect the two relevant actors—culpability costs *only* affect the leader. Second, this settlement cost arises *regardless* of the issue at stake in the war. Finally, leader culpability costs can change during the war (from being present to being absent) as a direct result of the political dynamics within the state. All of these characteristics result from the fact that the citizens (or, more specifically, the selectorate) *create* this settlement cost for leaders.<sup>28</sup> The anticipation of domestic punishment for backing down in a war he involved the state in motivates the leader to stay in the conflict until a victory can be secured.

With this type of settlement cost, the *act* of settling (and making the outcome of the war certain in the eyes of the public) generates the cost. The individual settlements leaders agree to may vary in their consequences for the state since wars are waged for a variety of reasons, but citizens will *always* want to punish a culpable leader if he fails to secure a victory, regardless of the agreement's substantive content. Citizens will, in other words, be as upset with a culpable leader who set out to defend a piece of territory and then *loses* it as with a culpable leader who sets out to conquer and then fails to *gain* one

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<sup>28</sup> The rationality of this behavior on the part of citizens will be discussed in Section 3.2.

from the neighboring state. They are not necessarily upset about the failed goals themselves—they are more upset that the leader failed to achieve the goals he set.<sup>29</sup> This is why this type of settlement cost only affects the leader—indeed, it can hardly affect the citizens since they inflict the cost—and why culpability costs will affect the leader’s utility function for the war no matter what the war is being fought over. I will discuss the implications of these two aspects (the omnipresence of this settlement cost for culpable leaders and the asymmetrical way in which culpability cost affects leaders and citizens) in greater detail in Section 4.

The third aspect—that culpability costs can change over the course of the war—highlights why the domestic political landscape during the war is essential. Because culpability costs are tied to specific leaders, knowing when the leadership changes (if any) took place, and the circumstances surrounding the changes, is critical if we want to know whether culpability costs affected a particular leader. The following four subsections describe in greater detail the circumstances under which domestic audiences will hold their leaders culpable; why we should expect rational citizens to follow through on this settlement cost by punishing culpable leaders; why we should expect to see a mix of culpable and non-culpable leaders in the set of international conflicts; and how the effect of culpability varies across regime types.

### **3.1 Determining Culpability**

Citizens should be willing to assign culpability to three types of leaders. The first type is comprised of leaders who were in charge of the state when the war began (i.e.,

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<sup>29</sup> Of course, citizens could be upset because of the nature of the specific failure *in addition* to being angry with the leader for failing. The point is that the presence of this settlement cost is not dependent on the particulars of the war.

“first leaders”). “First leader”, importantly, is not synonymous with “initiator”. Instead, this group includes all initiators, leaders who join conflicts already in progress and leaders whose states were attacked. The defining criterion is that the leader presided over the state’s transition from peace to war. Although initiators and third-party joiners could, arguably, deserve more culpability since participation in the conflict resulted from an active choice, some scholars have argued that targeted leaders will acquire a similar burden. According to this logic, citizens will hold targeted leaders responsible for the war not because they chose to involve the state in it but because they failed to deter the attack from occurring.<sup>30</sup> This assertion that citizens will hold all first leaders equally culpable is an open empirical question, and is included in the hypotheses below.

The second type of leader whom citizens should hold culpable is a leader who comes to power during the war *and* who is from the same ruling group as the first leader. The term “ruling group” is purposely vague so that it can apply to any regime type. In democracies, for instance, culpability will transfer from one leader to the next if they are from the same political party or, in the case of a parliamentary system, the same cabinet.<sup>31</sup> Citizens should be willing to transfer culpability along political lines because of the close associations they imply between political actors.<sup>32</sup> Moreover, once the original culpable leader leaves office and is no longer available for punishment, his fellow party members

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<sup>30</sup> Bueno de Mesquita and Siverson (1995).

<sup>31</sup> In practice, members of a prime minister’s cabinet are often also members of the prime minister’s party, but coalition governments are a different case. In cases like this, the proximity of the cabinet member to the executive in terms of decision-making, and the prime minister’s dependence on cabinet members’ support, links these political actors in the minds of citizens. This link overrides any political distance (and flexibility regarding the course of the war) that might have otherwise accrued to the cabinet member based on partisan differences. There is, of course, room for variation; members of parties that are clearly in the cabinet as the result of a coalition bargain (i.e., cabinet members drawn from “king maker” parties such as Shas in Israel or the Free Democratic Party in Germany) may be able to distance themselves more from the leader than, say, a member of the leader’s party who is not in the cabinet. Such ideas are difficult to test in a large-N setting, however,

<sup>32</sup> This notion is especially applicable in countries with governments marked by strong party discipline (e.g., the United Kingdom.)

are the most easily recognizable substitutes.<sup>33</sup> This leads to a high probability that the pressures associated with the culpability costs will span two (or more) administrations or governments if the chief executive's party does not change.<sup>34</sup> Although these successive leaders do not have *direct* responsibility for the war, the clear ties that exist between the new leaders and their predecessors will lead their domestic audiences to transfer the blame with only minimal discounting.

In autocratic states a similar transfer of culpability occurs if the new leader comes from the former leader's inner circle or winning coalition. Examples of this might include a colonel succeeding a general in a military junta, a prince succeeding his father in a monarchy, or leaders drawn from a one-party system (e.g., the Communist Party in the Soviet Union or China.) This type of political system, where power is highly centralized, facilitates a transfer of power from the culpable first leader to a culpable successor from the winning coalition since the masses are denied any input in the selection of the new leader.

Finally, the third type of leader that citizens will hold culpable is a new leader who publicly supported the decision to involve the state in the conflict when it began.<sup>35</sup> In practice, most leaders from the same ruling group as the first leader will also fall into this

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<sup>33</sup> Cotton (1986) provides several examples of this phenomenon from his study of American elections where war was a central issue. During several American wars, legislators who were in the president's party suffered in the midterm elections if the war was unpopular with the electorate.

<sup>34</sup> This point is key: even though the first leader "pays" the settlement cost by losing office, his successor can still inherit the incentives associated with culpability if the war has not ended.

<sup>35</sup> Politicians in this category vying for the executive position during the war may try and shift their stance to one of opposition if the war is going badly, but this maneuver is unlikely to succeed. In a political system with even minimal competition, long-time opponents of the war will be quick to highlight this policy inconsistency in an effort to discredit the candidate.

category, but politicians outside the ruling group can support the decision as well.<sup>36</sup>

Indeed, as some scholars have argued, we should expect this third type of culpable, new leader to be highly prevalent in the legislatures of democratic systems.<sup>37</sup>

Given the incentives (and power) held by the opposition to punish the leader for a bad decision, democratic executives should be very selective in the wars in which they choose to participate. We should only expect these leaders to choose wars they believe their state has a high likelihood of winning and/or that the opposition would be foolish (be it for practical or political reasons) not to support. This selection pools all of the members of legislature based on their position on the war, and makes partisan differences less salient. A similar effect occurs when the democratic state is attacked. In cases like this, opposing retaliation would be politically risky for the opposition, regardless of the state's chances of eventual victory.<sup>38</sup>

Not all leaders who come to power during wars, however, will inherit the culpability of their predecessors. Wars often bring leaders to power who are from a different ruling group than the first leader and/or who were against the war when it first began. The absence of political ties to the previous leader allows the new leader to distance herself from the decision that brought the state into the conflict and, consequently, presents her with more options regarding the future course of the war. She can, for example, continue fighting if the adversary is intent on occupying the country, *or* she can suggest a plan for withdrawal if no other settlement costs exist.

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<sup>36</sup> In practice, citizens may hold leaders drawn from this third group less culpable than members of the executive's party who supported the war because of the lack of the partisan connection. I test for this potential rank ordering in Chapter 4.

<sup>37</sup> Schultz (2001).

<sup>38</sup> Given the high plausibility of either of these wartime scenarios, we should not expect the pool of culpable leaders to be homogenous in terms of political affiliation. I return to this idea in Section 3.3.



### 3.2 The Rationality of Leader Punishment

The rationale behind citizens' willingness to punish the leader appears to effectively create a settlement cost for leaders that compels the leader to work directly *against* the citizens' own interests by staying in a costly war. Although such behavior would seem to demand irrationality on the part of the citizens, this need not be the case. At least two plausible reasons exist for why citizens will want to punish culpable leaders who lose wars, and spare non-culpable leaders who do the same: incompetence and concerns for the state's international reputation.

Culpable leaders are more susceptible than non-culpable leaders to the brand of "incompetent" because citizens can be easily link them to the decisions that led to wars that turned out poorly. The first place a leader's incompetence can manifest itself in the eyes of citizens is in the selection of the conflict. If a leader chooses to involve the state in a war in which the state is clearly outmatched and/or which goes poorly very quickly, opponents of the war could easily make a case that the leader lacks competence in the realm of foreign policy and that the leader's incompetence risks the state's security. Citizens will have incentives to punish this leader not only to prevent him from making the same mistake again but also to deter future leaders from behaving in a similar manner. Punishing non-culpable leaders for this reason is practically impossible since, by definition, they cannot have been responsible for the war's onset.

The second way a culpable leader can reveal his incompetence (again, in the eyes of citizens) is in the way he prosecutes the war. If a leader selects into a war the citizens initially view as a "good bet," and goes on to eventually lose (or simply fail to win),

citizens will assume the leader squandered their chances of victory.<sup>39</sup> Even if the war went sour because of something completely beyond the leader's control (e.g., the loss of a major ally due to upheaval within the ally's domestic political arena) citizens would probably not have access to this sort of micro-level knowledge, even in a society with a free and open press.<sup>40</sup>

Even if informational limitations were not at issue, citizens do not have any incentive to be forgiving of leaders. Even if citizens recognize that the problem likely originated as a result of a command issued by a general in the field, and not the leader in the capital, the leader of the state presents a larger and, more importantly, a more symbolic and *reachable* target for their ire and their desire to bring the war "under new management." A non-culpable leader, predictably, will have an easier time dodging accusations of poor war management. The political distance between the new leader and her predecessors means she can credibly distance themselves from the war's initial phases and/or any earlier tactical decisions made by the previous leader(s) that may have led to the state's poor fortunes on the battlefield.

The second reason citizens will be more likely to punish culpable leaders than non-culpable ones has to do with how the leader is perceived by the international audience. As others have argued, citizens will have incentive to change their national leader when they feel his international reputation has been damaged in some way.<sup>41</sup>

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<sup>39</sup> Critically, the fact that the war "seemed like a good idea at the time" should not encourage the citizens to be more magnanimous in their treatment of the leader if the war ends poorly. Indeed, it should inspire more outrage. If a state had, say, a 95% chance of winning a war at the outset and loses, citizens will likely assume that a loss in the face of such overwhelmingly favorable odds must have resulted from the leader's severe mismanagement of the war. On the general topic of the willingness of citizens to interpret poor conflicts outcomes as a sign of leader incompetence see Gelpi and Greico (2001) and Wolford (2007).

<sup>40</sup> Indeed, Achen and Bartels (2002) demonstrate that citizens are more than willing to punish their leaders for "acts of God", such as floods and shark attacks.

<sup>41</sup> Guisinger and Smith (2002) and McGillivray and Smith (2000).

Although these discussions are typically couched in the context of international cooperation, where leaders damage their reputations by renegeing on an agreement, the concept can be applied to conflict situations as well.<sup>42</sup> In the case of a war, a leader can damage his reputation by backing down from his earlier war aims and/or suffering a humiliating defeat. Either action will call the leader's resolve into question in the eyes of other national leaders, perhaps inviting more challenges in the future. Citizens, therefore, should want to remove this type of leader from office and replace him with someone who does not have a marred record with the international audience.

This preference favors non-culpable leaders because of their ability to disassociate themselves both from the decision to go to war in the first place and from the previous leaders.<sup>43</sup> Bringing a new, non-culpable leader to power is the most effective way the citizens can rebuild their state's international image. The new leader may eventually prove to be a poor choice, and she may damage her own reputation at a later date, but in the current time period, a leader without a record is far better than a leader with a tarnished one.

### **3.3 Mixture of Leaders Types**

The circumstances under which new leaders might come to power affects the distribution of culpability across the set of warring leaders. Because wartime leadership changes often occur as a result of a state's battlefield misfortunes, one might expect the majority of new leaders to be non-culpable. That is, apart from the occasional leader who

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<sup>42</sup> For example, see Tomz (2007).

<sup>43</sup> To test the claims that, 1) Citizens care about how their leader is perceived by the international audience and 2) that non-culpable leaders are seen as more preferable replacements, I have designed a second survey experiment that will be administered on a national sample in May 2008. The existing literature assumes the first claim is true and my theory implies the second one is valid, but neither claim has been evaluated yet.

is forced to leave office for structural reasons, such as term limits, any new leader who comes to power during the war will have been selected to replace a leader who managing the conflict poorly. This notion, however, is only partially true. As the next chapter will illustrate in more detail, non-culpable leaders do come to power under such circumstances, but a number of other scenarios are capable of bringing new culpable leaders to power as well, even when the war is going badly. If, for instance, the first leader dies naturally or is assassinated, the pre-determined successor will, more likely than not, share a political connection with the previous leader and the culpability costs will pass on.<sup>44</sup>

Apart from these morbid circumstances, other members of the first leader's government will also have incentives to elevate a new leader who will share the first leader's culpability costs. In parliamentary systems, for instance, the cabinet may force the prime minister to resign in the face of public dissent, and then replace him with a member of the same party. Sacrificing the first leader in this manner allows the party to avoid calling an election that they would likely lose. It also allows them to maintain control of the executive while appeasing the public by removing the cause of their displeasure.<sup>45</sup>

Finally, in the case of democratic systems, the leader's management of the conflict (or his party's management) need not always be the central issue in a citizen's mind, even during wartime elections. As the next chapter illustrates, leaders careers are

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<sup>44</sup> When John Curtin of Australia died in July of 1945, for instance, Deputy Prime Minister Frank Forde, who was also a member of the Labor party, succeeded him and served as Acting Prime Minister until new elections could be held. (Elections that, coincidentally, elevated another Labor Party member to the chief executive position: Benjamin Chifley.) Source: Author's dataset.

<sup>45</sup> The British experience in the Crimean War provides an example of a "Cabinet Coup". After a string of defeats in the East, Prime Minister Aberdeen resigned in disgrace. Palmerston, who was Home Secretary at the time and who was a strong supporter of the war when it began, replaced him. Source: Author's dataset.

often cut short by domestic issues, wholly unrelated to the war (e.g., budget concerns or political scandal), which create an opening for a member of the same party to take over.<sup>46</sup> Given the multitude of circumstances under which leaders can change, we should not expect any strong patterns among new leaders based on culpability.<sup>47</sup>

### **3.4 Culpability in Different Political Systems**

We should, however, expect patterns to emerge in terms of how culpability costs affect leaders from different regime types. The degree to which the culpability costs will factor into a leader's war termination calculus should vary based on the vulnerability of his or her position, which largely varies with a state's political structures. The presence of the culpability cost provides the domestic audience with the willingness to punish the leader, while the leader's vulnerability provides the opportunity. I will now discuss how a leader's vulnerability varies across (and sometimes within) autocratic, anocratic and democratic regimes.

#### *3.4.1 Autocratic Leaders*

Of all leader types, autocratic leaders such as despots, dictators or absolute monarchs, are the least vulnerable to the pressures associated with culpability costs. The often near-absolute hold on power and the availability of strong repressive mechanisms make domestic dissent largely irrelevant for this leader type. The small winning coalitions that define autocratic systems mean that leaders need only bribe a handful of

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<sup>46</sup>The government of Japan's Terauchi, for instance, fell in 1918 over a rice shortage in Tokyo. Source: Author's dataset.

<sup>47</sup> Given this, the concern that any relationship between leader culpability and war outcome or between leader culpability and leader punishment are based on epiphenomenal processes should be minimal. I explore this notion more in the next chapter.

key supporters to maintain their hold on office.<sup>48</sup> Members of the winning coalition, consequently, have very little incentive to remove the leader because doing so would end their own streams of private benefits. Autocratic leaders, therefore, have the ability to weather unfavorable outcomes, provided that losing the war does not imperial their ability to satisfy their winning coalition.<sup>49</sup>

Room for variation exists within this leader type, however. Among autocrats, we should expect leaders who have held power for a long time prior to the war to be less susceptible to culpability costs than leaders with shorter pre-war tenures.<sup>50</sup> Newer autocratic leaders will still have small winning coalitions, but, in contrast to their more established counterparts, they will have had less time to solidify their hold on power by rewarding coalition members with private benefits; the potential for revolt from within still exists. This vulnerability makes these leaders less likely to settle for defeats than more secure autocrats since losing may weaken their ability to secure support from their members of their winning coalition and may jeopardize their future tenure.<sup>51</sup>

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<sup>48</sup> Bueno de Mesquia, et al. (2003).

<sup>49</sup> Some scholars have put a fair amount of theoretical weight on the fact that removal from office for autocratic (and anocratic) leaders is often followed by a secondary punishment (e.g., death, imprisonment, exile, etc). (Goemans 2000). This had led scholars to assume that these leaders will have greater incentives than democratic leaders not to lose a war because the worst thing that can happen to a democratic leader is to lose office. It is important not to make too much of this diversity. The form domestic punishment takes in democratic polities may be mild *relative* to the types of audience costs that exist in autocratic states, but we should not expect democratic leaders to take much solace in this fact. Democratic leaders may be trying to avoid losing office while autocratic leaders may be trying to avoid the firing squad, but both leaders are responding to the *maximum punishment* their relevant domestic audience is capable of delivering. Overlooking this fact artificially inflates the size of a non-democratic leader's settlement costs. It is also important to consider the probability that the leader will actually receive the punishment. An autocrat's punishment may be many times worse than a democrat's (say, -10 as opposed to -1), but the probability that he will receive that punishment is likely very small (say .02 compared to .9). Once this probability is accounted for, the relative size of the punishment matters less.

Expected Value of the Punishment = Size of the Punishment \* Probability of Receiving the Punishment

Expected Value of the Autocrat's Punishment = -10 \* 0.02 = -0.2

Expected Value of the Democrat's Punishment = -1 \* 0.9 = -0.9

<sup>50</sup> Bueno de Mesquita and Siverson (1995).

<sup>51</sup> I evaluate whether any differences exist in terms of pre-war leader tenure in the next chapter.

### 3.4.2 Anocratic Leaders

Among the set of non-democratic leaders, leaders of anocracies are the most unstable since their regimes straddle the political spectrum between the autocratic and democratic extremes; they enjoy neither the legitimacy of democratic leaders nor the repressive capabilities of autocratic ones.<sup>52</sup> Instead, anocratic leaders operate in a political system where they hold a fair amount of power, but the influence of the opposition persists.

The combination of a viable opposition and a lack of repressive mechanisms guarantees that the leader will never have the near total control over his political fate. This means that unfortunate outcomes in international conflict can have substantial domestic repercussions since the opposition can capitalize on the government's mistakes. The lack of an effective repressive apparatus means the anocratic leader will have a difficult time defending himself from those who wish to punish him. This means the probability the anocratic leader will be punished is quite high, especially when compared to autocratic leaders. Therefore, *among non-democratic rulers*, these leaders should be the most vulnerable to culpability costs and, as a result, should be very resistant to settling without a win.<sup>53</sup>

### 3.4.3 Democratic Leaders

Democratic leaders are the most vulnerable of all leader types largely because democratic institutions intentionally permit easy sanctioning of the leader. Variation in domestic institutions, however, creates differentiation between even members of this

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<sup>52</sup> See Goemans (2000) for more on the repressive advantage of autocratic leaders.

<sup>53</sup>Ibid.

group. In parliamentary systems, for instance, elections can (essentially) come at any time, while presidential democracies usually change their leaders according to a fixed election schedule. Given that election timing in parliamentary systems is often endogenous to both the leader's recent actions and the resultant public disapproval, we should expect prime ministers to be especially sensitive to settlement costs since citizens can wield the electoral check fairly easily.<sup>54</sup> Presidents, on the other hand, often have a multi-year respite between elections, which means culpability costs are of more immediate concern in the months preceding a scheduled election.<sup>55</sup>

We should not, however, expect presidents to be significantly more cavalier than prime ministers in the types of settlements they willing accept. Even though removing the leader in the short term may be difficult, other domestic political actors are still capable of making a leader “pay” the settlement costs by purposely blocking any legislation the executive proposes, and essentially halting his domestic agenda. This threat is especially credible in presidential systems where the executive's party does not necessarily control a legislative majority. Because of this, the leader will be (at least somewhat) at the mercy of the opposition, which has the greatest incentives to punish him if the war is going poorly.<sup>56</sup>

Although a president may be in a position where his personal political career is relatively immune from the damaging effects of settlement costs—say, a Mexican president who has served the maximum six year term—the long-term consequences of

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<sup>54</sup> Smith (2003).

<sup>55</sup> Gaubatz (1991). Unfortunately, testing for differences between these two types of democratic systems is impossible given the preponderance of parliamentary democracies in the set of warring states and the general scarcity of presidential systems.

<sup>56</sup> Huth and Allee (2002: 82) make a similar point in the context of territorial disputes where they argue that, “the risks of a military setback or the failure to achieve diplomatic gains through the use of coercive pressure are generally higher in the face of strong [domestic] political opposition.”



the culpability costs he generated give members of his party incentives to keep him from conceding to the adversary. The leader's fellow party members have an interest in preventing the current leader from sullyng the party's reputation by agreeing to unfavorable terms because of the aforementioned inability of members of the leader's party (who may run for office in the future) to distance themselves credibly from the previous leader's policies (and failures).<sup>57</sup> Therefore, although we might expect leaders of parliamentary democracies to be more vulnerable than leaders of presidential democracies, any differences are muted by the incentives culpability costs create for *other* domestic political actors.

In general, democratic leaders will be most sensitive to the effects of culpability costs, and we should see evidence of this in the settlements they are willing to accept and their treatment in the event of a loss. Anocratic leaders are less vulnerable to the effects of culpability and autocratic leaders even less so. I present hypotheses to evaluate the validity of this rank ordering below.

#### **4.0 New Insights from the Theory**

To review, the two types of settlement costs I have discussed thus far are those related to the war aims of the adversary and those related a leader's personal culpability for the conflict. Scholars have studied something similar to the former type of cost in

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<sup>57</sup> This notion has its origins in the economic literature on "overlapping generations" (OLG) where scholars study the effect of having members with short and long time horizons within the same organization. (Alesina and Spear 1988 and Cremer 1986). Recently, political scientists have posited that political parties also have an OLG quality to them, which leads to policy bargains between junior and senior members who often have different personal preferences. (McKelvey and Riezman 1992, Stokes 1999 and Simmons 2008). In the case of international conflict, the original culpable leader is the "senior member", whose time horizon is rapidly shrinking. The junior members are all of the members of the leader's political party who are still in active leadership positions. Their continued involvement in politics (and presumed desire to remain involve) extends their time horizon considerably. The junior members can enforce the bargain by refusing to grant the senior member support on other policies apart from the war.

work on the issue at stake in a war. The role of a leader's culpability in the war termination decision, however, is the primary theoretical contribution of this dissertation. In this section I focus on two issues the new theory allows us to address more accurately: identifying the circumstances under which leader and citizen preferences will diverge, with a special emphasis on the implications for theories about democracies at war; and why only some leaders will have incentives to escalate their war aims in an attempt to gamble for resurrection.<sup>58</sup>

#### **4.1 Diverging Preferences**

Understanding when and why the preferences of leaders and citizens will differ over whether a particular war should continue requires examination of how the various costs of war affect the two types of actors. As Section 3 noted, culpability costs are asymmetrical in that they only provide motivation for *leaders* to stay in wars. By contrast, having an adversary with expansive war aims creates a symmetrical settlement cost that will deter both leaders *and* citizens from settling before achieving victory. The presence of settlement costs provides one explanation for why an actor will want to avoid termination.

Predicting when a divergence will occur (i.e., when one actor has motivation to stay in the war while the other has a motive to leave) however, requires acknowledgement of the role of a more traditional cost of war: casualties. Casualties are similar to culpability costs in that they affect leaders and citizens asymmetrically. In this case, citizens bear the cost entirely. A leader should be concerned about mounting

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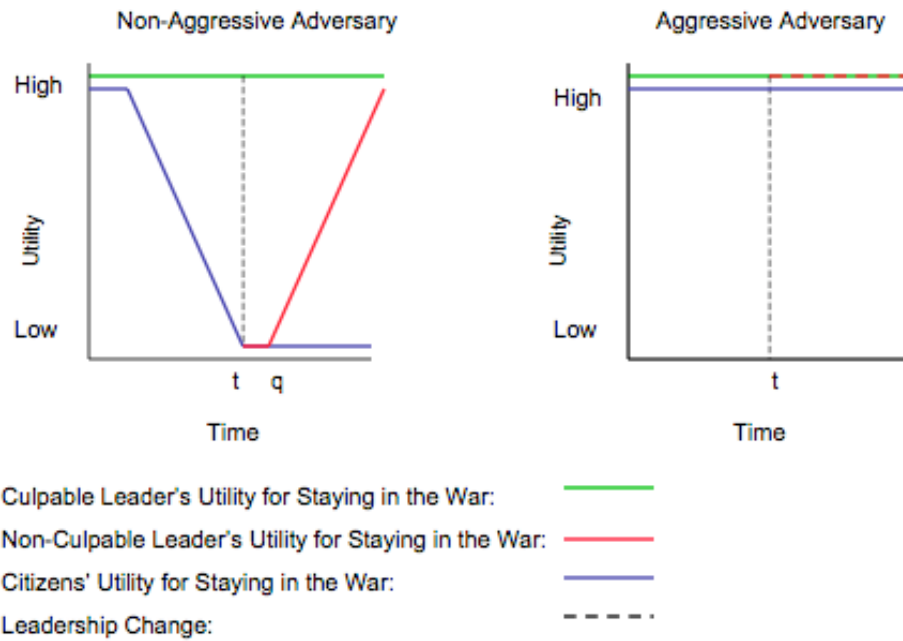
<sup>58</sup> Downs and Roche (1994).

casualties since they are likely a sign the war is going poorly, but we should not expect them to figure into his utility function directly.

Casualties differ from culpability costs, however, in terms of *when* and *why* actors incur them. Leaders only pay culpability costs at the end of the war (or, in the case of a leader removed during the war, at the end of their time in office), and only then if they fail to secure a victory. A culpable leader will thus always have a reason to stay in the war until victory is achieved, *regardless* of the adversary's war aims or the costs to the citizens. The pressures created by this culpability settlement cost only disappear when a non-culpable leader is brought to power, and only then if the adversary does not have expansive war aims.

Citizens, on the other hand, must suffer casualties throughout the war, even if the state eventually wins the conflict. Consequently, a citizen's utility function for staying in the war should decrease monotonically over time. High war aims on the part of the adversary may work to counter this, but in the absence of this kind of settlement cost a citizen's utility for continuing to fight should become negative long before a culpable leader's does. The combination of these characteristics means a divergence will occur when a culpable leader is prosecuting a costly war against an adversary with non-expansive war aims. Figure 2.1 demonstrates this graphically.

**Figure 2.1 Preferences of Leaders and Citizens**



The diagram on the left depicts a war in which the adversary does not have high war aims, while the diagram on the right represents a war in which the enemy is intent on conquering territory, replacing the leader or occupying the state entirely.<sup>59</sup> In both figures, the y-axis shows the actor’s utility and the x-axis indicates time since the war’s inception. The green and red lines represent a culpable and non-culpable leader’s utilities for continuing to prosecute the war, respectively, while the blue lines represent the citizens’ utility for continuing to fight in the two types of wars. The dashed vertical line represents the point at which a leadership change occurred (at time  $t$ ). If the leadership change elevates a culpable leader to power, the new leader’s utility function will be identical to his predecessors—hence the unbroken green line. If, however, the leadership change

<sup>59</sup> For the purposes of this example, I assume the casualty rates are equal in the two panels. Although wars involving aggressive adversaries will likely be more costly on average, citizens may have a lower threshold for casualties in wars where their state’s territorial integrity or continued sovereignty are not at risk.

brings a non-culpable leader to power, the new leader's utility function will follow the path indicated by the red lines.

Figure 2.1 illustrates at least three important points. First, the utility functions in the diagram on the left present the divergence in preferences of a culpable leader and his citizens clearly. Although citizens may initially support the war, their utility for continuing to fight begins to fall as time passes and casualties grow.<sup>60</sup> The culpable leader's utility for staying in the war, however, remains consistently high, regardless of casualties. For the purposes of this example, the leadership change coincides with the citizens' utility function's nadir. Although this suggests that the public dissent *caused* the leader's removal, leadership changes do not necessarily have to adhere to this mechanism. As discussed in Section 3.4.3, structural causes (for example, term limits) and fixed election schedules may present citizens with opportunity to remove the leader earlier.

Second, as a comparison of both panels of the diagram demonstrates, the non-culpable leader's preference regarding whether to stay in the war corresponds with the citizens' when she takes office and in the immediate period thereafter. The period between times  $t$  and  $q$  represents the window during which a non-culpable leader will try to end the war. After point  $q$ , the leader's utility for staying in the war begins to increase as her persisted participation in the war causes her to accrue culpability costs of her own. The longer the non-culpable leader stays in the war after coming to office, the harder it

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<sup>60</sup> (Gartner and Segura (1998) and Mueller (1973.)) Although the line in the graph depicts a linear decrease, this need not be the case. A citizen's utility for staying in the war could follow a step function pattern, as she reacts to major setbacks or spikes in the casualty rate. The takeaway point is that her overall utility should follow a downward trend as casualties mount. Recent work (Feaver and Gelpi 2004) suggests that citizens respond to the probability of victory instead of casualties, and that they will be willing to stay in a conflict so long as the perceived likelihood of victory remains high. The figure presented here, of course, is too simplistic to capture a factor that could, potentially, rise and fall over the course of the war, so I assume the probability of victory is uncertain for citizens.

will be for her to disassociate herself from it, and therefore her own culpability costs will make ending the war without a victory harder for her.<sup>61</sup>

Third and finally, the comparison of the two panels illustrates how the settlement costs generated by an adversary with high war aims can augment or offset other costs. In wars where the adversary has expansive war aims, citizen and leaders will always have reason to want to stay in the war, and the change in leadership (even to a non-culpable leader) is completely inconsequential.

An examination of the two actors' utility functions across the panels demonstrates this. First, the citizens' utility functions differ greatly across the two conditions. In the panel on the left, citizens grow weary of the war quickly since there is nothing to counter the costs they are paying in the form of casualties. In the panel on the right, however, the threat of losing territory or being completely overrun provides a countervailing pressure that motivates citizens to bear the costs and continue fighting.

An equally striking difference emerges when we compare the utility functions of the non-culpable leaders across the two conditions. As discussed earlier, a non-culpable leader's utility for continuing the war will track with her citizens', especially in the period immediately following her ascent to office. Figure 2.1 demonstrates that this prediction will hold *regardless* of the nature of the adversary's war aims. As above, in the case of a non-aggressive adversary, the non-culpable leader should be willing to break with the previous leader's policy and appease citizens by ending the war. In the case of a war involving an aggressive adversary, however, the non-culpable leader's utility

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<sup>61</sup> Although it is unclear as to whether this function should be linear, it should certainly become increasingly positive. I will discuss reasons why a non-culpable leader might not take advantage of this window in Chapter 5.

function will continue to mirror the citizens' but will *also* trace the same pattern the culpable leader would have followed had he remained in office. This highlights the power of the adversary war aims settlement cost to trump a lack of personal culpability a leader may feel for a conflict.

Current approaches to war termination overlook this divergence because of the tendency to focus on the warring *state* as the theoretical unit of observation instead of the individual warring leader(s). This hinders theoretical development in two ways. First, the focus on the state effectively merges the utility functions of the leader and his citizens. This unified utility function forces scholars to assume that the costs of war affect the two actors equally. For example, scholars commonly assume that mounting casualties should lower a *leader's* utility for continuing to fight and affect his bargaining position with the adversary. The assumption, however, ignores the fact that leaders and citizens are largely concerned with different costs. The utility functions only move in sync--and, therefore, the existing approach is *only correct*--when the state faces an aggressive adversary. Focusing on the warring state also traps scholars into overlooking the importance of both the pressures that affect individual leaders and how these pressures might change as the war progresses and new leaders come to power.

Second, Figure 2.1 also offers new insights about the dynamics between a democratic leader and his citizens in times of war. The conventional wisdom of the existing literature posits that a "declining advantage of democracy" exists: As wars get longer and, presumably, more costly in terms of casualties, democratic leaders will be more willing to accept unfavorable outcomes to end the war quickly.<sup>62</sup> This prediction should only be accurate, however, if a *non-culpable* leader has been elevated to power. A

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<sup>62</sup>Bennett and Stam (1998) and Slantchev (2004).

culpable democratic executive will want to stay in the conflict, regardless of the length of time already spent at war. The presence of the viable opposition in democratic states compounds this incentive for the culpable leader. Since conceding to the adversary in a costly war will give his opponents ample ammunition, and provide them with evidence of his poor war management skills, the leader has every reason to continue fighting, even as victory grows increasingly unlikely.<sup>63</sup>

Although empirical support exists for the “declining advantage of democracy” notion, in retrospect the findings are more likely evidence of a “declining advantage of culpable leaders”.<sup>64</sup> As wars get longer and become more costly to citizens, a culpable, democratic leader’s hold on power becomes increasingly tenuous. At the same time, the probability of a non-culpable leader being installed in his place, often with a specific mandate to end the war, increases.

Under the current approach, in which the warring state is the unit of observation, democratic leaders appear to be more willing to settle, but the conventional wisdom omits a crucial step of the causal mechanism: the switch from a culpable to a non-culpable leader.<sup>65</sup> Public dissent has the power to end a war for a democratic state, but only if it is strong enough first to remove the culpable leader from power and then to replace him with a non-culpable one. When a non-culpable leader assumes power, the leader-specific settlement costs vanish, bringing the new leader’s utility for continuing the war more in

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<sup>63</sup>As Robert Pape explains, “even when the costs of further resistance clearly outweigh any attainable benefits, governments tend to hold out longer than society wants because there are domestic costs to admitting defeat...Appeasing the enemy allows domestic political rivals to charge the ruling regime with incompetence, betrayal or both.” (1996: 32).

<sup>64</sup>I discuss why existing work has overlooked this idea and, with further investigation, actually lends supports this new interpretation in Chapter 3.

<sup>65</sup>Existing empirical practices reinforce this omission. Focusing on the warring state obscures any wartime leadership changes that may have taken place inside the state. See Bueno de Mesquita et. al (2003) for an exception.



line with the public's. The new leader will be more responsive to public opinion, and will end the war with a higher probability than her predecessor.

Recognizing that democratic leaders will respond to dissent differently based on their personal culpability also sheds light on the variation we see in how leaders try to *shape* public opinion. Scholars have long acknowledged the powerful effect leaders can have on how citizens interpret major events, and they have noted that this power is especially pronounced for chief executive.<sup>66</sup> In highlighting the executive's ability to harness the power of the bully pulpit, however, the existing approach begs the question of why some democratic leaders choose not to use it.<sup>67</sup> The theory presented here offers a simple answer: Culpable leaders will spend political capital willingly and freely in an effort to marshal support for an increasingly unpopular war, while non-culpable leaders will choose to save their capital for future political battles.

A similar pattern emerges when we examine the variation in leaders' responses to sunk costs. Although scholars have posited that higher casualties will compel a leader to stay in the war out of a desire to avenge the fallen, the fact that not all leaders do so poses a quandary for the sunk costs explanation.<sup>68</sup> The culpability argument offers an alternate causal story, albeit a much more self-serving one from the point of view of the leader. Leaders will use sunk costs arguments to motivate the public to stay in the war when culpability costs are a personal concern for them, and they will downplay the need for retribution when their utility of settling is greater than the utility for fighting. Culpable

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<sup>66</sup> Zaller (1992) and Feaver and Gelpi (2004).

<sup>67</sup> Feaver and Gelpi (2004: 135), for instance, make note of the fact that President Bill Clinton choose not to mobilize the public in 1993 to support the UN/US mission in Somalia, but do not offer an explanation for why he did so. The culpability theory offers an answer: Because the mission to take down Aideed and restore order to Mogadishu was initiated by the previous president, George H. W. Bush, Clinton was able to distance himself from the mission and draw down American troops. This allowed him to preserve his political capital for future endeavors instead of spending it in the first year of his administration.

<sup>68</sup> Ikle (1995) and Mitchell and Nicholson (1983).

leaders, in other words, are more likely to assert that the fallen “will not have died in vain” while non-culpable leaders will be quick to frame the deaths as costs or burdens that no more families should be forced to endure.

The insights from the preceding discussion underscore just some of the problems inherent in focusing on leaders instead of states. Concentrating on the warring state draws scholars’ attention to the effects of the state’s domestic institutions, and, consequently, places the theoretical emphasis on explaining the variation between leaders of different regime types. Although this line of inquiry has highlighted the effects of the general differences between autocratic, anocratic and democratic regimes, the preceding discussion, and Figure 2.1 in particular, illustrate the dangers of pooling leaders on regime type and assuming the (static) domestic political structure will affect all leaders from that state uniformly.

#### **4.2 War Aims Revision**

The second insight that results from the incorporation of culpability costs relates to when and *why* leaders might choose to revise their war aims. Other scholars have recognized that leaders may have incentives to raise their war aims, but they have not addressed why the incentive to “gamble for resurrection” does not seem to affect all leaders.<sup>69</sup> Incorporating culpability costs provides an answer. Only culpable leaders will have reason to raise their war aims in a risky all-or-nothing push, and these pressures to increase the state’s war aims will be inherited by the next leader if he is also culpable.

The inability to explain why only some leaders will choose to expand their war aims stems from scholars’ tendency to limit discussions of the political consequences of a

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<sup>69</sup> Downs and Roche (1994).

leader backing down to crisis situations.<sup>70</sup> Theories of audience costs often use specific threats or ultimatums that are thought to capture the public's short-term interest.<sup>71</sup> The typical example includes a leader making a threat; the public then observes the opponent's immediate response and then the leader's reaction, often over a very short time period (i.e., days or weeks). The primary function of audience costs in the existing security literature is to provide a causal mechanism for "lock in" to a crisis, and debates often center on how certain types of leaders might gain an edge on their adversary in this regard.<sup>72</sup>

Once both states reach lock in, however, and the question becomes one of war continuation instead of crisis escalation, audience costs disappear from the theoretical discussion.<sup>73</sup> The arguments presented here about culpability costs suggest this omission is premature. A leader's culpability certainly plays a role in crisis scenarios, but we should not assume that culpability ceases to matter to citizens as the war progresses. Indeed, as Figure 2.1 suggested, the leader's culpability may well be more important in the war than in the crisis, particularly once the casualties begin to mount and citizen support for the war wanes. Recognizing that a leader's culpability continues to matter as the war progresses allows for more refined predictions about when and *why* certain leaders will revise their war aims up and when they will choose to lower them. Culpable

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<sup>70</sup> This tendency has the additional side effect of leading scholars to overlook the fact that the incentive to gamble could pass from one leader to the next. Although scholars have recognized that wartime leadership changes can happen and that leadership changes in general can have powerful effects on a state's interactions with other countries, the possibility (and consequences) of a connection between successive leaders has not been appreciated.

<sup>71</sup> Fearon (1994), Schutz (2001a) and Tomz (2007).

<sup>72</sup> Eyerman and Hart (1996) and Fearon (1997).

<sup>73</sup> Schutlz (2001b), for instance, only has audience costs enter the model in the crisis stage; once both states move to war, war costs (which are purposely distinct from audience costs) become a concern. This type of argument focuses on the "signaling" power that audience costs play during crises and downplays the idea that the "lock in" aspect can continue to be an issue for leaders as the war progresses.

leaders will augment their war aims not only to “increase the variance” of possible war outcomes, but also to make the war seem more vital to their citizens.<sup>74</sup>

Appreciating the powerful effects of culpability costs also presents a challenge to the idea that leaders can easily adjust their war aims to reflect the probability of victory (i.e., leaders will raise them if they think they are going to win, and lower them if they think they are going to lose).<sup>75</sup> Although culpable leaders may long for the ability to revise the war aims downward to facilitate exit in a war that is going poorly, the settlement costs caused by their culpability remain constant and high, regardless of how the state is faring in the war. This demonstrates how culpability costs play a role beyond the crisis phase—Culpable leaders will have a hard time reducing their war aims because of the public backlash that will follow.

Non-culpable leaders will not share this dilemma. The political distance between them and the original decision to go to war affords them a much greater degree of flexibility regarding what to do with the war aims established by the earlier leader. In general, non-culpable leaders should take advantage of this and revise the war aims downward. Indeed, apart from wars that involve an adversary with very high war aims, non-culpable leaders should have very little incentive to stay in the conflict at all; the glory of any victory would have to be shared with the culpable leader who was in charge when the war began, and this sharing is not politically plausible in competitive democratic systems. Even if the non-culpable leader was not elected specifically to end

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<sup>74</sup> Goemans (2000). Such efforts may be in vain, but we should not be surprised if leaders try to make the war seem more important than it actually is. Citizens may be more willing to bear casualties for longer if they think the cause is worthwhile or that “salient” issues are at stake.

<sup>75</sup> Ibid.

the war, we should still expect her to end the war as soon as it is feasible to avoid expending any more of the state's resources that she could use for other purposes.

## **5.0 Hypotheses**

The final step in the theory-building process is specifying a set of hypotheses that will allow us to test the theory's central claims. Each of the following hypotheses falls into one of two categories. The first set of hypotheses will test whether leaders respond to the incentives associated with culpability costs in ways that the theory would predict. If the ideas presented in Sections 3 and 4 are correct, we should expect culpable leaders to behave very differently from leaders who are relatively free of this settlement cost; the former should be very reluctant to agree to anything less than very favorable terms while the latter should be more willing to make concessions to facilitate the termination process.<sup>76</sup> The second set of hypotheses tests claims about the probability of leader punishment. If the ideas presented in Sections 3 and 4 are correct, we should see variation in how the domestic audience treats the different types of leaders in the event of an unfavorable settlement.

### **5.1 Hypotheses on War Outcomes**

Culpability costs will compel a leader to avoid terminating a war until a favorable outcome is secured. The threat of domestic punishment inspires culpable leaders to “gamble for resurrection” and stay in a conflict, even if doing so means having their citizens bear more costs. This suggests the following hypothesis:

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<sup>76</sup>The fact that many first leaders will enjoy the advantages that come with being the initiator should not be overlooked. I control for this in the statistical models presented in the next chapter.

**Hypothesis 1:** Culpable leaders should be more likely to secure favorable war outcomes than non-culpable leaders.

As Section 3.4 suggested, we should also expect the interaction of a state's regime type and a leader's culpability to matter. If a state's domestic political structures facilitates the selectorate's removal of the leader, the leader should be more responsive to the pressures generated by the settlement cost and, therefore, more likely to hold out for victory. In practice this means that democratic leaders should be most sensitive to the pressures inherent in culpability. This suggests the following hypothesis.

**Hypothesis 2:** Among culpable leaders, democratic leaders should be more likely to secure favorable outcomes than autocratic leaders.

## 5.2 Hypotheses on Leader Punishment

As previous discussions demonstrated, citizens have a multitude of reasons for wanting to punish culpable leaders who lose, and they lack incentives to punish non-culpable ones. Recognizing the domestic audience's willingness to punish the culpable leader, however, only addresses part of the story.

Because we can only observe instances where the leader is punished, we need to specify hypotheses with the audience's willingness *and* opportunity in mind. Citizens of an autocratic state, for instance, may be furious with their leader, but remain relatively powerless in terms of sanctioning power. Their willingness to punish their leader may be quite high, but their opportunity to do so is minimal. The fact that the domestic audience must have the *ability* to punish the leader means that not all leaders will face the same likelihood of punishment. As Section 3.4 demonstrated, we should expect substantial

variation in terms of vulnerability across leaders from different regimes. The combination of regime type and responsibility suggest the following hypotheses:

**Hypothesis 3:** Culpable leaders who lose are more likely to be removed from office because of the war than non-culpable leaders who lose.

**Hypothesis 4:** Among culpable leaders who lose, democratic leaders should be most likely to be removed from office because of the war.

As discussed in Section 3.1, the possibility for variation in terms of the probability of punishment among first leaders also exists. Although some leaders actively choose to participate in the war (either by initiating or joining), leaders whose states were attacked may possibly face a smaller probability of punishment if they eventually lose. Although these leaders technically had a choice regarding their participation in the conflict (i.e., resistance vs. immediate surrender), their citizens may be more understanding of the leader's unfortunate position should the war end poorly. This suggests the following hypothesis.

**Hypothesis 5:** Among first leaders who lose, leaders who voluntarily made the decision to participate in the war should be more likely to be removed because of the war than leaders of targeted states.

## **6.0 Conclusion**

The goal of this chapter was to develop a richer understanding of how culpability costs work in practice. Significant progress was made on several fronts. First, after a brief discussion in Section 2 of how the adversary's war aims function as a settlement cost, Section 3 covered a new settlement cost—leader culpability—in greater detail. This discussion revealed several interesting aspects of this settlement cost, including (but not

limited to) its asymmetrical effects on leaders and citizens, and how their salience to leaders should vary with regime type.

Section 4 built on Section 3 by fleshing out two new insights suggested by the inclusion of the leader culpability cost. The first insight suggested that we should not expect a leader's preference for staying in the war to match his citizens in all circumstances, even in democracies. Instead, the utility functions of these two actors should only coincide if the leader is non-culpable. The second insight centered on when and why only some leaders will have incentives to gamble for resurrection. If the theory presented here is correct, only culpable leaders will have incentives to elevate their war aims; non-culpable leaders, by contrast, will lack this incentive and should take steps to facilitate exit from the war soon after coming to office.



## Chapter 3

### 1.0 Introduction

The preceding chapters presented a theory of war termination that incorporated a new element into a leader's decision-making calculus: his culpability for involving the state in the conflict. The purpose of this chapter is to present and execute a research design capable of testing the hypotheses regarding war outcomes and leader punishment suggested by the theory in Chapter 2. If the theory is correct, a leader's culpability should have a strong effect on the types of settlements he is willing to accept and the probability that the domestic audience will punish him in the event of a loss.

In short, culpable leaders will fight longer and harder to secure favorable outcomes than their non-culpable counterparts since they face a higher likelihood of domestic punishment should they fail to achieve their objectives. This heightened probability of punishment stems from the ease with which the domestic audience can link the culpable leader to the decision to involve the state in the war. A non-culpable leader, by contrast, faces a much smaller probability of domestic punishment, regardless of the ultimate terms of settlement she achieves, because of the credible distance she can put between herself and the state's participation in the conflict.

Crucially, these differences between the leader types should hold regardless of the citizens' attitude regarding the war. Culpable leaders should continue fighting even amidst high costs and public dissent because the leader's *personal* settlement costs

remain high irrespective of how the state is faring in the war. Non-culpable leaders, on the other hand, should be far more receptive to public opinion because their relative lack of personal settlement costs affords them more freedom of action in terms of the war outcomes they can accept.

To illustrate the theory's explanatory power, I first describe the dataset and the coding of the dependent and independent variables. Next, I present the results of tests that demonstrate the strong effect of leader culpability on war outcomes. I then turn to hypotheses regarding the effect of a leader's culpability on his likelihood of losing office as a result of the war, where I find strong support in favor of the theory's predictions. The final section summarizes the implications of the findings for the theory and remaining empirical questions that will be addressed with different methods in subsequent chapters.

## **2.0 Research Design**

### **2.1 Hypotheses**

In the previous chapter, I identified five testable hypotheses regarding the effect of a leader's culpability on the war outcomes the leader would be willing to accept and his probability of being punished by the domestic audience in the event of a loss.

**Hypothesis 1:** Culpable leaders should be more likely to secure favorable war outcomes than non-culpable leaders.

**Hypothesis 2:** Among culpable leaders, democratic leaders should be more likely to secure favorable outcomes than autocratic leaders.

**Hypothesis 3:** Culpable leaders who lose are more likely to be removed from office because of the war than non-culpable leaders who lose.

**Hypothesis 4:** Among culpable leaders who lose, democratic leaders should be most likely to be removed from office because of the war.

**Hypothesis 5:** Among first leaders who lose, leaders who voluntarily made the decision to participate in the war should be more likely to be removed because of the war than targets.

The remainder of this section will discuss the design of the dataset, how the variables were operationalized and how they will be employed in models of war outcome and leader punishment to test the theory in the most accurate way possible.

## 2.2 The Dataset

The dataset used for the analyses in this chapter was built specifically for this dissertation, but it has roots in datasets commonly used in the general security literature. The population of the 257 warring states included in the analysis is taken from the Correlates of War Project's list of interstate war participants.<sup>77</sup> To determine the population of leaders who were in charge of these states during the conflicts, I relied largely on two existing datasets of national leaders.<sup>78</sup> Leaders were included in the new dataset if they were in charge of the state at any point during the war.<sup>79</sup> The resulting dataset includes one observation per leader per war, for a total of 352 observations. The difference between the number of warring states and the number of warring leaders is a result of leadership changes that take place in roughly 20% of the wars. These changes make multiple observations for a single state in the same war possible. For example, there

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<sup>77</sup> Sarkees (2000). One minor change was made: the Austro-Sardinian War (1848-9) was broken into two wars since it consisted of two distinct conflicts separated by several months of peace.

<sup>78</sup> "The Logic of Political Survival Datasource" Bueno de Mesquita et al. and "Archigos: A Database on Leaders 1875-2004" Goemans et al. (2006). Discrepancies in the start and end dates of leader tenure were corrected based on information in Lentz (1994) and Allen (1977).

<sup>79</sup> In states where two executive positions exist, a head of state and a head of government, the leader who possessed the authority to determine policy regarding the state's participation in the war was included in the dataset. More often than not, the head of government holds these powers, but, as is the case in some autocracies where the head of government is essentially a puppet, it is not unheard of for the head of state to possess this authority.

are two observations for the United States' participation in the Korean War, one for Truman and one for Eisenhower. As later sections will make clear, including *all* leaders who participated in wars in the analysis is absolutely critical to test the theory in the most appropriate way possible and to avoid sample bias.

Testing the hypotheses presented in Chapter 2 required coding two dependent variables, war outcome and leader punishment. Although these variables are common within the security literature, the codings used here differ from the standard approach in important ways. I also coded two novel independent variables: a leader's culpability for the conflict and a measure of the state's progress during the war. I will now briefly discuss each of the variables used in the analysis and the rationale behind the coding rules.

## **2.3 Dependent Variables**

### *War Outcome*

Measuring the war's outcome required devising a coding method that would allow me to utilize every warring state in the dataset as a separate observation.<sup>80</sup> Although this may seem like a straightforward need for any research design related to questions of war outcome, the traditional "win, lose, or draw" coding scheme prevents researchers from including all warring states in their analyses simultaneously. Scholars must drop all but one state from each war from their analyses because including all of the

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<sup>80</sup> Importantly, this need is unrelated to the decision to use leaders as the unit of analysis instead of states. As I discuss below, models that have a dependent variable of war outcome only utilize one observation per war (the leader who presided over the final settlement) so having multiple leaders in the larger dataset is not a problem. Instead, as I discuss above, the problem at issue here stems from the statistical dependence between the outcomes of the wars the leaders are fighting.

states would introduce perfect statistical dependence in the dataset in terms of the dependent variable.<sup>81</sup> Table 3.1 illustrates this point using two hypothetical bilateral wars.

**Table 3.1: Statistical Dependence in War Outcomes**

Observation	War	Participant	Opponent	Outcome
1	A	Sweden	Norway	Win
2	A	Norway	Sweden	Lose
3	B	Sudan	Chad	Draw
4	B	Chad	Sudan	Draw

In War A, Sweden loses to Norway; War B between Chad and Sudan ends in a draw. Because each state's outcome is perfectly predicted by the outcome of its opponent—after looking at the outcomes for Sweden and Sudan we do not have to look at the outcomes for Norway and Chad to know the former lost and the latter tied—including all four of these states in an analysis of a model of war outcome would violate the assumption of independence between observations. Consequently, Norway and Sudan (or Sweden and Chad) must be dropped from the analysis.

Including one state from each war is suitable for some research questions, but is unsatisfactory for the purposes of this dissertation because would require me to drop all but 73 of the observations.<sup>82</sup> To create a war outcome dependent variable that avoids perfect prediction and the loss of several observations, I designed a coding scheme that focuses on the degree to which a leader achieved the state's original war aims instead of how the he fared relative to his opponent. This resulted in the following six-point scale (see Table 3.2.)

<sup>81</sup> See, for example, Bennett and Stam (1998) and Stam (1996: 77-8).

<sup>82</sup> This number represents the total number of wars (as opposed to war participants) in the dataset.

**Table 3.2: War Outcome Coding Scheme**

Value	War Outcome	Frequency (Percent)
2	Total Victory	101 (39%)
1	Partial Victory	18 (7%)
0	Pre-war Status Quo	32 (13%)
-1	Partial Loss	61 (24%)
-2	Major Loss	11 (4%)
-3	Loss of Sovereignty/Occupied	34 (13%)

This scale has two primary advantages. First, and perhaps most importantly, the coding scheme provides a more accurate measure of a leader's *achievement*.<sup>83</sup> Under the new scheme, a leader who annexes a large piece of his adversary's territory he set out to conquer is as successful as a leader who gains nothing but whose only goal was to maintain the pre-war status quo. Although the traditional coding method would likely treat the former as a win and the second as a draw, in reality the domestic audiences of both states should be pleased with their leader because he succeeded in fulfilling his objective. Because the theory is interested in the reaction of the domestic audience, recoding the variable in a manner that captures the basis on which citizens assess their leader is critical to testing the theory accurately.

From a purely empirical perspective, in addition to offering a more nuanced conceptualization of the different ways in which wars can end, a coding scheme based on the degree to which the leader achieved his state's war aims takes a large step towards ameliorating the problem of perfect prediction. Although certain combinations of outcomes are more likely to arise than others (e.g., if a leader finds his state totally occupied at the end of the war, his adversary has likely achieved all of her goals) the

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<sup>83</sup> The outcome of the war, of course, can only provide a measure of achievement for the leader who presides over the war's termination. I will address the issue of measuring the achievements of leaders who leave during the war in subsequent sections.

perfect correlation between opponents is eliminated since a win for one leader does not necessarily imply a loss for his or her opponent.<sup>84</sup>

This scheme was applied in the following way. I assigned leaders a 2 if they succeeded in securing all of their war aims without sustaining a loss of any kind.<sup>85</sup> Moving down the scale, leaders received a 1 if they were unsuccessful in securing the full set of war aims but did not suffer a loss. These partial victories are relatively rare (see Table 3.2), but it is important to distinguish them from the total victories since domestic opponents could draw attention to the war aims the leader failed to achieve. I assigned leaders a 0 if they did not achieve any of their war aims but also managed to avoid any losses. This category is most comparable to the traditional notion of a “tie” but, importantly, it is coded irrespective of how the leader’s opponent fared in the war.<sup>86</sup>

Partial losses are coded as a -1 and represent outcomes in which a state gains nothing and is forced to make minor concessions to the adversary. Minor concessions can range from a policy change to reparations to a small loss of territory, either from the state’s homeland or colonial territory. By contrast, major concessions, which I code as -2, always involve the loss of a substantial amount of homeland territory. Finally, the worst possible outcome for a leader, represented by a -3, is to be overthrown and have his state annexed or otherwise occupied by the adversary. A dichotomous version of this variable

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<sup>84</sup> Indeed, within the dataset, leaders that preside over total victories do so over opponents who suffer a wide range of “losses”, from a return to the pre-war status quo to total annexation.

<sup>85</sup> “Loss” here is meant in the general sense (i.e., having territory annexed by the adversary, being coerced into changing a policy, etc). All states, by virtue of participating in the war, will, of course, suffer casualties.

<sup>86</sup> Although this coding may seem to re-introduce the statistical dependence problem since if State A wins nothing but also loses nothing, the same must also be true of State B, this overlooks the possibility of a leader fighting to maintain the pre-war status quo. In this example, if State B’s leader’s war aims were purely defensive, he would be coded as securing a total victory. Multilateral wars also reduce concerns about perfect prediction since it possible for two allies to obtain different degrees of success against the same adversary.

is used in models of leader punishment; it takes on a value of 1 for total, major and partial losses and a 0 otherwise.<sup>87</sup>

### *Domestic Punishment*

Although the concept of leader punishment is frequently discussed in the war termination literature, I chose to operationalize this variable in a different way. To illustrate why this change was necessary, it is helpful to examine the three ways in which scholars traditionally approach this concept. The first two approaches focus on the effect of a war's outcome on a leader's longevity by measuring the length of a leader's tenure in office starting either from the day the war begins (Method 1) or the day the war ends (Method 2).<sup>88</sup> These methods both rely on hazard models to obtain their statistical estimates and expect that leaders who perform poorly in international conflicts will have shorter post-war tenures than leaders who do well. The third approach (Method 3) parallels the latter two but utilizes a dichotomous dependent variable instead to highlight instances of leader removal within a set time frame, usually within one year after the war ends.<sup>89</sup>

Although these methods appear to approach the notion of domestic punishment in a reasonable and intuitive way, they are not particularly well suited for this dissertation because they are only designed to incorporate one leader per war. Method 1 focuses on the leader who was in charge when the war began, while Methods 2 and 3 center on the leader who was in charge when the war ended. This difference would be irrelevant if the

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<sup>87</sup> As a robustness check, I counted status quo outcomes as losses instead of wins. My results remained consistent.

<sup>88</sup> For example, Bueno de Mesquita and Siverson (1995) and Goemans and Chiozza (2004).

<sup>89</sup> For example, Goemans (2001), Bueno de Mesquita, et al, (2003) and Bueno de Mesquita, Siverson and Woller (1992).



leader who started the war were always the same as the leader who ended it, but this is not always the case. As Table 3.3 demonstrates, although a fair number of leaders who start wars remain in charge for the duration of the conflict, some states witness high leadership volatility during times of war.

**Table 3.3: Wartime Leadership Volatility**

Position of Leader Who Ended the War	Frequency (Percent)
First	206 (80%)
Second	30 (12%)
Third	9 (4%)
Fourth	7 (3%)
Fifth	1 (<1%)
Sixth	2 (1%)
Seventh	1 (<1%)
Ninth	1 (<1%)

Recognizing that an intra-war leadership change can bring a leader to power who faces a very different array of settlement costs than her predecessor reveals the sample bias inherent in any method that only allows for one leader per war. Sample bias arises because leaders who take office during wars are often systematically different from leaders who were in charge at the beginning of the war with respect to culpability and their (consequent) probability of being punished. This creates problems for the analysis, regardless of which leader the researcher chooses to study.

For example, if the sample only includes leaders who were in charge when the war began, testing hypotheses regarding culpability becomes impossible since all of the leaders, by definition, are culpable. The independent variable of interest becomes a constant and its effect cannot be estimated. Examining the leaders who are in charge when the war ends introduces a different problem, but it is one that is equally likely to confound attempts at statistical inference. Under this approach, the dataset contains none

of the leaders who were removed from power during the war and includes all the leaders who were put into office to replace them. This exclusion is consequential because it systematically eliminates from the sample all of the culpable leaders who were removed during the war because of poor performance. This research design also “over samples” non-culpable leaders who may have been brought to office with a specific mandate to end the war, many of whom will be non-culpable.<sup>90</sup> The non-random nature of the sample in terms of leader type is extremely problematic for a study interested in the effects of culpability because it means several observations that align precisely with the core prediction of the theory have been excluded from the analysis.

The dataset presented here avoids problems related to sample bias by including all warring leaders. This is a major improvement both theoretically and methodologically since, as the previous chapter argued, all leaders who participate in wars are equally relevant to the study of culpability costs, regardless of which portion of the conflict they oversee. The dependent variable representing domestic punishment was coded as a 1 if the leader was removed from power *because of the war* and a 0 otherwise. To code this variable I relied on a variety of general collections of leader biographies and other reference volumes.<sup>91</sup> Leaders qualify for a coding of 1 if the majority of sources agreed their removal was a clear and direct result of the war. Although this first group is

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<sup>90</sup> This is not to say that culpable leaders never come to power during wars—indeed, as the next section demonstrates, this is not uncommon. Non-culpable leaders, however, are still more likely to appear in a sample that only includes leaders who end wars because, unlike their more culpable counterparts, they will try to terminate their wars quickly to avoid domestic punishment, even if this means settling for a net loss. Culpable leaders, on the other hand, do not want the war to end until the state’s war aims have been accomplished. Because culpable leaders who come to power during wars face an identical set of settlement costs as their culpable predecessors, they are equally likely to continue fighting (and “select” themselves out of the sample of leaders who end wars) in a (vain) attempt to gamble for resurrection.

<sup>91</sup> While the complete list of reference materials cannot be listed here, several key volumes served as excellent general references on vast majority of the leaders: Lentz (1994), Goemans et al. (2006), Encyclopedia Britannica (2007), Eccleshall and Walker (1998) and Bell et al. (1990).

overwhelmingly comprised of leaders who are removed through political means, the two incidences of leaders committing suicide during a war are also included since their untimely deaths were linked to their state's poor performance in the war.<sup>92</sup>

Leaders received a coding of 0 if the majority of sources agreed they left office for reasons other than the war, which includes a wide variety of scenarios. First, leaders can leave for institutional reasons that are wholly separate from the war, such as serving the maximum number of terms in office or fulfilling their position as an interim, regent or other provisional leader.<sup>93</sup> In these scenarios, the leadership change would have taken place even if the war had not occurred. Five other scenarios exist that also arise independently of the war but are more stochastic in nature. These include natural death, leaving for another high-ranking political position, sickness, retirement and assassination.

While the first of the five is self-explanatory, the latter four are not as straightforward. Instances of leaders leaving the executive office to take another key position in government are rare, but careful investigation of the circumstances surrounding their transition from one post to another demonstrated that they were not cases of leader punishment.<sup>94</sup> Cases in which the leader retired or resigned for health reasons had to be coded very carefully since both of these reasons are often given to provide cover for a leader who wants to escape political problems. A coding of retirement required evidence that the timing of the retirement was predetermined or that the leader

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<sup>92</sup> Both of these leaders, coincidentally, are from World War Two: Adolph Hitler of Germany (d. April 30, 1945) and Alexandros Korizis of Greece (d. April 18, 1941).

<sup>93</sup> Sir John McEwen, for instance, served as acting prime minister of Australia for three weeks following the presumed drowning of Prime Minister Harold E. Holt while Australia was involved in the Vietnam War. He stepped down after Sir John Gorton was chosen as the replacement leader for the Liberal party. (Lentz, 1994, v2: 52).

<sup>94</sup> Both instances are from France: Charles Alexandre Dupuy, who left the office of prime minister to become President of the Chamber during the Franco-Thai War in 1893 and Etienne-Alexandre Millerand, who left the prime ministership in 1920 during the Franco-Turkish War to assume the presidency when the current president resigned for health reasons. (Bell, Johnson and Morris 1990: 132 & 291).

truly left at his or her own choosing.<sup>95</sup> Departures due to illness required that leader leaders were incapacitated or otherwise bed-ridden.<sup>96</sup>

Instances of assassination are interesting because although they appear to be prime examples of leader punishment in its most extreme form, a lethal shot from a lone anarchist does not qualify as a political sanction from the domestic audience and thus receives a coding of 0 for this variable.<sup>97</sup> Even if the trigger is pulled because the assassin was angered by how the leader handled an international conflict, the fact that the removal was not a result of a regular political process and/or did not involve any members of government disqualifies it from being included as a case of leader punishment.<sup>98</sup> Finally, several others types of cases are coded as zeroes: leaders who lost power because their state was absorbed into a new political entity (e.g., the small European states that eventually were united under German control), leaders who remained in power for several years after the last war in the dataset ended (the first Gulf War, which ended in

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<sup>95</sup> For instance, the decision of Australia's Robert Menzies' to leave office in 1966 during the Vietnam War was widely regarded as a completely voluntary decision: "When he retired from parliament (the last prime minister to do so while still secure in office), he dominated public life. His supremacy was such that contemporaries argued only over the merits of his success, and these arguments still linger in the estimates of biographers and historians." (Davison, et. al., 1998 426).

<sup>96</sup> For example, Ahmad Hassan al-Bakr, who ruled Iraq from 1968 to 1979, suffered a heart attack in 1976 that forced him to delegate most governmental matters to his second in command, Saddam Hussein. He formally resigned in 1979 and died in 1982. (Lentz (1994: 410) and Encyclopedia Britannica Online (2007)).

<sup>97</sup> I classify four cases as "questionable assassinations", where the killing is either ordered or carried out by someone from within the leader's ruling circle who may either have had personal designs on the executive position or more general political motives. Yahya Ibn Muhammad Ibn Hamid Al-Din, for instance, ruled the Yemen Arab Republic from 1911 until 1948 when he was killed by a guard in a palace revolt (Goemans et. al, 2006: 269) and Encyclopedia Britannica Online (2007).) None of these killings, however, were in response to the targeted leader's lack of success in an international conflict.

<sup>98</sup> For example, Liaquat Ali Khan (who was the prime minister of Pakistan from 1947 until his death in 1951) was killed by a fanatic who was upset with Khan's handling of relations with India. (Lentz, 1994: 610).

1991), leaders who were removed or who resigned over domestic matters, or leaders who were overthrown by a foreign power during a war.<sup>99</sup>

Coding leaders based on *why* they were removed from office rather than *when* has three distinct advantages in terms of this project. First, and foremost, it provides the most accurate test of the theory possible. The theory's main contribution centers on why domestic audiences remove some leaders for poor war outcomes while allowing others to remain in office. It offers no predictions as to when, precisely, the punishment will be carried out.

Second, from an empirical standpoint, this approach avoids the risk of conflation inherent in a coding scheme that identifies punished leaders based on whether they were removed from office within a set period of time (e.g., within one year after the war's conclusion). Although most leader punishments occur fairly quickly, assuming that all leaders who leave shortly after the war are doing so because of their performance in the conflict would be erroneous. As this section has demonstrated, leaders leave power for many reasons, so we cannot assume that their departure is always because the domestic audience is unhappy with how their state fared in the most recent war.<sup>100</sup>

The third advantage also has to do with time, but deals more with issues related to the political processes of different regimes. Several confounding factors may impede

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<sup>99</sup> With regards to the latter two categories, there are several examples of leaders who are punished by the domestic audience shortly after the war for reasons unrelated to the conflict. Elpidio Quirino of the Philippines, for instance, was voted out of 156 days after the end of the Korean War because "his administration was tainted by widespread graft and corruption." (Encyclopedia Britannica Online, 2007). Although one could argue that poor performance in a war could rob the leader of political capital and make him more vulnerable to attacks from domestic opponents, identifying cases of this sort are beyond the scope of this dissertation. Instances where the leader is removed by a foreign power are dropped from the final analysis since members of the domestic audience are denied the opportunity to punish the leader themselves.

<sup>100</sup> Indeed, a full 27% of leaders who were *not* punished because of the war's outcome leave office within a year of the war ending.

research designs that examine the length of a leader's post-war tenure instead of the reason the leader was removed. For example, autocratic leaders will generally have longer tenures in office than democratic leaders since, by the nature of their near-absolute hold on domestic authority, they are harder to remove from power once they have solidified their winning coalition's loyalty.<sup>101</sup> This has the potential to inflate the effect of a state's regime type, even though other factors could be equally important.<sup>102</sup>

Although controlling for the regime type of the state addresses this concern somewhat, other problems exist that make this type of analysis unattractive. For instance, in some states, leaders can only be removed at pre-determined times (e.g., the fixed election cycle that exists in many presidential democracies). The fact that leaders are only "at risk" for a small fraction of their time in office clashes with the idea of what a hazard function is meant to capture.<sup>103</sup> These different domestic structures may also allow leaders to stay in office even if the public is displeased with them since citizens may have to wait years for an opportunity to punish the leader. Focusing on whether the leader was removed from office because of the war circumvents these problems because time does not play a role in the coding decision.

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<sup>101</sup> Bueno de Mesquita et al. (2003).

<sup>102</sup> To illustrate this point, imagine two hypothetical leaders who both end wars in 1920 with partial victories: one is the leader of a presidential democracy and the other is a ruler of an absolute monarchy. The democratic ruler serves his maximum term and leaves office in 1926 while the autocratic leader rules until he dies in 1952. Both leaders enjoyed the maximum time in office allowed by their political systems as neither was punished, but the autocratic leader will look far more immune to the effect of the war's outcome. Censoring the democratic leader's observation does not remedy this problem. If this observation is censored the hazard model will not treat his departure as a failure, but it will incorporate the (far shorter) length of the democratic leader's post-war tenure into the analysis. Allison (1995) and Cleves et al. (2004).

<sup>103</sup> These models are designed for studies in which the units under observation are exposed to risk *continuously* over the course of the study. For example, in a study of the effect of a new drug on the lifespan of a group of patients, the patients are always at risk of dying. The exact point at which they die can vary from patient to patient, but there are no periods in which dying is not possible.

## 2.4 Independent Variables

### *Leader Culpability*

The key independent variable for this study is a leader's culpability for his state's involvement in the conflict. Leaders are considered culpable if they meet one of two criteria: (1) they were in charge of the state when the war began (a "first leader"), or (2) they share a political connection with a culpable predecessor.<sup>104</sup> For the purposes of this chapter's analyses, a political connection exists if the new leader is from the same political party as the culpable leader, or was a member of the culpable leader's cabinet, military junta, close circle of advisors or, in the case of monarchies, the culpable leader's family.<sup>105</sup> Non-culpable leaders, by contrast, are leaders who come to power during wars who lack such connections with the earlier, culpable leader.<sup>106</sup>

This variable was coded using leader biographies and reference volumes that detail the composition of governments throughout history.<sup>107</sup> Of the 352 leaders in the dataset, 95 (27%) are new leaders who come to power at some point during the war. Of these 95 new leaders, 54 (56%) are non-culpable. Finally, out of the 257 warring states in the dataset, non-culpable leaders are in charge of the end of their state's participation 26 times (10% of the total).<sup>108</sup>

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<sup>104</sup> This predecessor is usually a first leader, but some wars can generate several consecutive, culpable leaders beyond the leader who was in charge when the war began. Such is the case of Japan in World War Two, where most of the leaders were connected to the military in some fashion.

<sup>105</sup> Victor Emmanuel II of Sardinia (later Italy), for instance, took over for his father, Charles Albert, in the second phase of the Austro-Sardinian War in 1849.

<sup>106</sup> I address the question of whether earlier public support matters in the next chapter.

<sup>107</sup> Lentz (1994) and Allen (1977) were especially helpful in this regard.

<sup>108</sup> This number is small but predictably so since, as the theory chapter explained, we should expect most first leaders to be strategic in their war participation and select into those wars they think will bring quick and easy victories. Although this selection effect should be more powerful among democratic first leaders, it is reasonable to presume that no leader willingly selects into a war with the idea that things will go so poorly as to result in his premature removal. There should not, in other words, be very many opportunities for new leaders to come to power provided everything goes according to plan for the first leader.

### *Regime Type*

This variable was coded using the Polity IV dataset, which contains information on the nature of a state's domestic institutions.<sup>109</sup> Since I am interested in how the state's domestic institutions affect how the leader is treated when he leaves office, the information was drawn from the last year the leader was in power while the war was in progress. So, for example, in the two cases that comprise the participation of the United States in the Vietnam War, the Polity information for the observation representing Lyndon B. Johnson is taken from 1969 while the information for Richard Nixon comes from 1973. Following standard practice, I subtracted the "democracy" and "autocracy" scores for the state for each observation and created a scale in which -10 represents the most autocratic states and 10 represents the states that are most democratic. To create the regime type categories used in final analysis, I trichotomized the scale at the following points: states with a score of -10 to -5 were considered autocracies, -4 through 5 were considered mixed regimes and states that scored between 6 and 10 were considered democracies.

### *Voluntary Participation in the War*

As discussed in the previous chapter, the question of whether all first leaders should be considered equally culpable remains an open one. Although some have argued that leaders whose states are attacked should be blamed by the domestic audience for failing to deter the adversary from striking, it is also reasonable to think that leaders who willingly chose to involve their states in the conflict would face a higher probability of domestic backlash should the conflict end poorly for the state. To test this idea I created a

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<sup>109</sup> Marshall and Jaggers (2000).



variable that takes on a value of 1 if the leader either initiated or voluntarily joined the war and a 0 if the state was attacked. For the purposes of this project, leaders who claim to be striking preemptively are still considered initiators. This variable was coded using secondary sources that describe the initial military clashes that mark the beginning of the war.<sup>110</sup>

### *Battlefield Advantage*

As the discussion of the domestic punishment variable made clear, all leaders who participate in wars must be included in the analysis to address this dissertation's question accurately. Because some leaders are removed before the war ends, a measure of how the state was faring at the end of the leader's tenure during the war is required to determine if the state's fortunes on the battlefield played a role in the leader's departure from office. We cannot safely assume that the war's outcome can be extrapolated back in time and applied to leaders who leave before the war ends since the state's battlefield advantage can often shift, for better or worse, as the war progresses.

In light of this, I constructed a variable that captures how the state was progressing in the war on a monthly basis. This variable took a value of 1 if the state had suffered a major setback during the month in question; setbacks include a significant loss of territory gained earlier in the war or a failure to prevent a major offensive by the adversary. This variable only highlights negative events that would be consequential enough to be considered major checks in progress. Fluctuation in the position of the battle lines by small amounts is certainly important over the entire length of the war, but

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<sup>110</sup> Although many sources were used, Clodfelter (1992), Phillips and Axelrod (2004) and Brecher and Wilkenfeld (1997) served as excellent general sources.

the purpose of this variable is to capture major failures during the war. The analysis uses this variable for observations in which the leader leaves before the war ends, and the value of the war outcome variable for all cases in which the leader was in charge when the war ended.<sup>111</sup>

## 2.5 Control Variables

### *Adversary's War Aims*

The adversary's war aims are included as a control for two reasons. First, it is important to account for this variable in models that aim to explain war outcomes. Leaders set their state's war aims strategically and are only likely to select high war aims if they feel they are likely to achieve them. Leaders facing adversaries with high war aims, therefore, are more likely to suffer larger losses than states facing less ambitious opponents since high war aims are effectively a signal of the opponent's determination to succeed.<sup>112</sup>

The second reason the adversary's war aims need to be controlled for in the analyses is because of their relationship with the probability the targeted leader will be punished by the domestic audience. Because the adversary's war aims proxy for how much the targeted country's citizens are likely to suffer if the war is lost, citizens of the

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<sup>111</sup> For example, there are two observations representing Britain's participation in the Crimean War: "Aberdeen 1855" and "Palmerston 1856". The first observation is assigned a 0 for this variable because in the month Aberdeen left office Britain was not making any gains but was also not suffering any major setbacks. The second observation is coded with the war's overall outcome since Palmerston presided over the war's conclusion.

<sup>112</sup> As a robustness check, I included a variable measuring the state's own war aims, which I coded using a scheme very similar to the one presented in Table 3.4. This variable performed very poorly, only achieving standard levels of statistical significance when the voluntary participation variable was removed from Model 1. This result is not surprising; taken together, the voluntary participation variable and the variable representing the adversary's war aims absorb most of the variation in war outcomes a the state's own war aims might explain.

targeted state should be more upset with a leader who is unable to prevent an aggressive adversary's victory than they would be with a leader who capitulates to an opponent with minimal demands. To control for this effect, I created a variable that is based on the following scale.

**Table 3.4: Adversary's War Aims Coding Scheme**

Coding	Adversary's War Aim	Frequency (Percent)
1	Pre-war Status Quo	59 (23%)
2	Policy Change	23 (9%)
3	A Portion of an Ally's Homeland Territory	67 (26%)
4	A Portion of the Targeted State's Homeland Territory	74 (29%)
5	Regime Change of the Targeted State	13 (5%)
6	Total Conquest of the Targeted State	21 (8%)

Adversaries are considered as having "high" war aims if they received a coding of 4 or higher on this scale.

### *Casualties*

This variable captures the number of military fatalities suffered by the state and serves as the second proxy for the national costs citizens bear during conflicts. As with the adversary's war aims variable, casualties must be controlled for because of the strong effect they have on the probability that the leader will be punished.<sup>113</sup> This variable was coded from several sources; when possible high and low estimates were collected and averaged.<sup>114</sup> In states that witnessed at least one wartime leadership transition, each leader's share of the casualties was weighted to reflect his personal share of the war's duration (e.g., if a leader was in charge for 75% of the war, he was allotted 75% of the

<sup>113</sup> Goemans (2001) and Gartner and Segura (1998).

<sup>114</sup> The primary sources for information on casualties were Clodfelter (1992), Valentino, et al. (2006) and Phillips and Axelrod (2004). If information could not be found in other sources, I relied on the figures provided by the Correlates of War Project.

casualties). This method is admittedly crude, but is the best alternative in the absence of reliable casualty data in smaller temporal units than the entire war (i.e., monthly or even annual figures). The final, weighted variable was logged to account for a skewed distribution.

### *Pre-war Tenure*

This variable counts the number of days from the day the leader took office to the day the war began.<sup>115</sup> This variable is included as a control to account for the effect of pre-war tenure on post-war office security. Leaders with longer pre-war tenures are generally thought to have a more solidified hold on power and, consequently, should stand a better chance of retaining office in the event of a policy failure (e.g., losing a war).<sup>116</sup> As such, it is a necessary control for models of leader punishment. The final version of this variable is logged to account for a skewed distribution.

### *Relative Capabilities*

This variable captures how strong an individual state is relative to its opponent(s). This is a necessary control for models concerning war outcome since a state with a significant advantage over its adversary is likely to achieve a better outcome than a states that is outmatched in terms of relative capabilities. This variable is calculated as the ratio of the state's capabilities over the sum of capabilities on the battlefield.<sup>117</sup> Data on

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<sup>115</sup> The value of this variable is zero for all leaders who come to power during the war.

<sup>116</sup> Bueno de Mesquita and Siverson (1995). Because the effect of pre-war tenure is thought to vary across the different regime types, I include an interaction term to account for the different intercepts associated with democratic and autocratic states as a robustness check. My results remained consistent.

<sup>117</sup> The precise equation used is:  $(\text{State A's Capabilities}) / (\text{State A's Capabilities} + \text{Capabilities of State A's Opponent(s)})$ . For this measure I only examine the effect of State A's individual capabilities. I created an additional measure that assumed all of the capabilities of State A's coalition partners (in instances of

national capabilities are drawn from EUGene; the composite capability (CINC) score was used in the final calculations.<sup>118</sup>

### 3.0 War Outcomes

In the previous chapter I argued that a positive relationship should exist between a leader’s culpability and the probability of victory (Hypothesis 1). I test this hypothesis using an ordered logistic regression in which the dependent variable is war outcome; higher numbers on this scale are associated with more favorable outcomes (see Table 3.2). Because I am interested in the war’s ultimate outcome, I only included the final observation for a state in a given war.<sup>119</sup> The results of this analysis are presented in Table 3.5.<sup>120</sup>

**Table 3.5: War Outcomes and Culpability**

Model 1			
Variable	Coefficient	S.E.	P-value
Culpable Leader	<b>0.85</b>	<b>0.38</b>	<b>0.012</b>
Your Capabilities	1.89	0.52	0.000
Adversary’s War Aims	-1.41	0.37	0.000
Voluntary Participant	0.21	0.33	0.263
Democratic State	0.70	0.28	0.015
Log Likelihood	-361.11		
Probability > $\chi^2$	0.00		
N	257		

multilateral wars) can be utilized in State A’s war effort [(Capabilities of State A’s Coalition)/(Capabilities of State A’s Coalition Capabilities of State A’s Opponent(s)]. Interchanging this variable with this one used in the final analysis did not affect the results.

<sup>118</sup> Bennett and Stam (2000).

<sup>119</sup> For example, the observations representing the United States’ participation in the Korean and Vietnam Wars for this equation are “Eisenhower, 1953” and “Nixon, 1973”, respectively. The full dataset includes an additional observation for each of these wars, “Truman, 1953” and “Johnson, 1969”, but because these leaders were not in charge when the final settlements were brokered, their observations are not used in the estimation of models of war outcomes. Observations in Models 1-4 are clustered by war.

<sup>120</sup> Unless indicated otherwise, all tests are one-sided.

The positive and significant coefficient for the culpability variable in Model 1 provides strong support for Hypothesis 1. Culpable leaders are more likely to achieve favorable outcomes than their less culpable counterparts, even in the presence of many of the literature's standard predictors of victory. The marginal effects of moving from a non-culpable leader to a culpable leader for each of the war outcomes (presented in Table A.1 of Appendix A) confirm this result. Non-culpable leaders are always more likely to preside over unfavorable outcomes than culpable leaders, while culpable leaders are almost twice as likely to preside over total victories than non-culpable leaders (47% vs. 28%).<sup>121</sup>

These findings lend credence to the idea that culpable leaders do not achieve their victories solely by choosing wars wisely or targeting weak opponents. The domestic political pressures associated with the culpability for the war also play a role because they motivate the leader to stay in the war, regardless of what this decision may mean in terms of the costs his citizens will be forced to pay.

The difference in terms of the outcomes culpable and non-culpable leaders are willing to accept also supports the alternate causal story presented in the previous chapter for the oft-cited “declining advantage of democracy” result, in which democratic leaders appear more likely to accept unfavorable outcomes as wars become more costly.<sup>122</sup> The extant explanation for the finding assumes that as democratic electorates grow weary with the mounting costs of war, their leaders will be more likely to settle for unfavorable outcomes to end the war and remain in the public's good graces.

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<sup>121</sup> Table A.1 in Appendix A presents the probabilities of the different leaders achieving each type of outcome, the difference between these probabilities and the 95% confidence interval surrounding the size of the change. In this case, the confidence interval remains on one side of zero for two outcomes: partial losses and total victories. This finding is not surprising given the frequency of these outcomes.

<sup>122</sup> Bennett and Stam (1998).

The findings in Table 3.5, however, suggest that this finding may be driven by the behavior of non-culpable leaders. Culpable leaders should be determined in their pursuit of victory, even at the cost of their own citizens' lives, while non-culpable leaders should be more willing to stop short of victory and settle for less favorable outcomes. The existing literature's explanation assumes the leader's commitment to the conflict wanes as public dissent grows, but, as I argued in the theory chapter, this characterization only applies to non-culpable leaders. The "declining advantage of democracy" result is more a reflection of the political processes of democratic states that make it easier to bring leaders who will feel less pressure to continue fighting (i.e., non-culpable leaders) to power during wars.<sup>123</sup>

Interacting democracy with culpability in a single model of war outcomes examines the notion that democratic leaders should be particularly responsive to the settlement costs associated with culpability (Hypothesis 2).

**Table 3.6: Democracy, Culpability and War Outcomes**

Variable	Model 2		
	(All Final Leaders)		
	Coefficient	S.E.	P-value
Culpable Leader	0.52	0.39	0.094
Your Capabilities	1.90	0.52	0.000
Adversary's War Aims	-1.43	0.36	0.000
Voluntary Participant	0.19	0.32	0.270
Democratic Leader	-0.01	0.55	0.494
Democratic, Culpable Leader	0.88	0.73	0.115
Log Likelihood	-360.4		
Probability > $\chi^2$	0.00		
N	257		

<sup>123</sup> An examination of the original finding reinforces this argument: of the six democratic leaders who accept draws or losses, three are non-culpable. I arrived at this conclusion after examining the dataset used in Bennett and Stam (1998). The leaders who accept the two draws and a loss are: Eisenhower (US, Korean War), Nixon (US, Vietnam War) and Brissot (France, Sino-French War). Two Indian prime ministers accept draws with Pakistan (Nehru and Shastri) and George I of Greece accepts a loss to Turkey, but all three of these leaders were the only leaders who rule their state during the conflict, and consequently are culpable.

Model 2 is identical to Model 1 (above) in terms of the sample on which it is estimated, but it includes an interaction term (“Democratic, Culpable Leader”). If culpability’s effect on a leader’s probability of settling for favorable outcomes differs across regime types, this interaction should be significant, but instead it falls just outside the traditional bounds of significance. The marginal effect being a culpable, democratic leader is positive, but the effect is not statistically distinguishable from the marginal effect of being a culpable, autocratic leader, which is also positive.<sup>124</sup> This suggests that although the effect of culpability may be stronger in a substantive sense for democratic leaders, non-democratic leaders are not immune from their pressures.

Models 3 and 4 in Table 3.7, which are estimates of the same model on two samples split by regime type, corroborate this notion.

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<sup>124</sup> This can be illustrated by comparing the marginal effects of culpable, autocratic and culpable, democratic leaders. The marginal effect of being a culpable, autocratic leader is simply the coefficient on “Culpable Leader” (CL) in Model 2 since this is the effect of culpability when the other component of the interaction term (democracy) is set at zero. As Table 3.6 demonstrates, the effect of this variable is indeed positive, but just barely qualifies for standard levels of significance. The marginal effect of being a *democratic* culpable leader is calculated by adding the coefficient on the culpable leader (CL) variable to the coefficient on the interaction term (CLD) itself:  $0.52 + 0.88 = 1.4$ . Both effects are positive (and the effect of culpability appears to be larger for democratic leaders), but as the insignificant interaction term suggests, there is not a significant difference between them.



**Table 3.7: Regime Type, Culpability and War Outcomes**

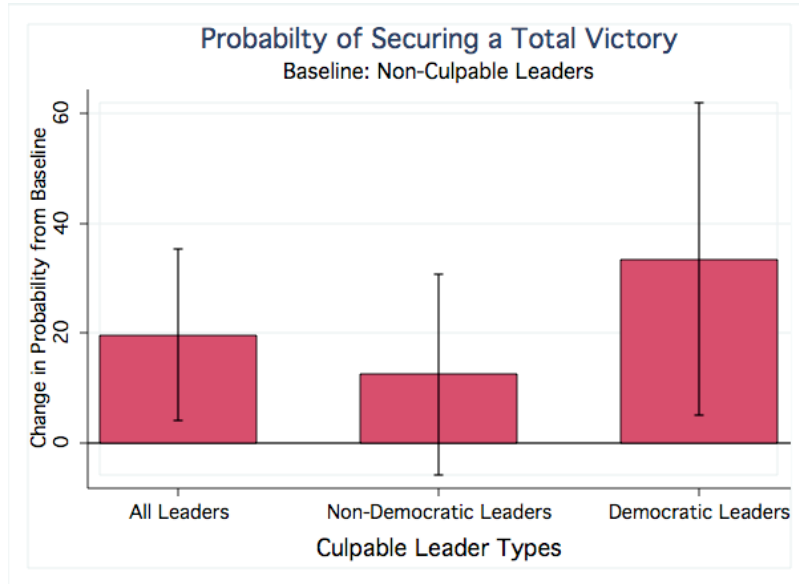
Variable	Model 3 (Just Democratic Leaders)			Model 4 (Just Autocratic Leaders)		
	Coefficient	S.E.	P-value	Coefficient	S.E.	P-value
Culpable Leader	<b>1.50</b>	<b>0.60</b>	<b>0.006</b>	<b>0.49</b>	<b>0.394</b>	<b>0.105</b>
Your Capabilities	1.69	1.09	0.062	2.04	0.60	0.000
Adversary's War Aims	-0.81	0.57	0.078	-1.53	0.45	0.000
Voluntary Participant	0.86	0.91	0.173	0.07	0.37	0.427
Log Likelihood	-69.82			-285.90		
Probability > $\chi^2$	0.004			0.000		
N	58			199		

The effect of culpability among democratic leaders is larger than the same effect among autocratic leaders, and while the first effect is highly significant the second just falls outside conventional levels of significance in a one-tailed test.<sup>125</sup>

These effects are also apparent in Figure 3.1, where the bars depict the *change* in the probability of securing a total victory moving from a non-culpable to culpable leader in all states, in non-democracies and in non-democracies. The black lines are the 95% confidence around the size of the change. The fact that the confidence interval for the effect of culpability on democratic leaders does not cross zero (with the line in the non-democracy subset does) increases our confidence in the notion that this effect is somewhat more pronounced in democracies.

<sup>125</sup> Table A.2 of Appendix A illustrates this result in a different way: democratic culpable leaders are twice as likely to preside over total victories than their non-culpable counterparts (68% vs. 34%) and less than half as likely to settle for any type of loss. The finding regarding total victory is not surprising given the frequency of this particular outcome for democratic states. (Reiter and Stam, 2002.) Culpable, autocratic leaders are also less likely to settle for losses and more likely to achieve total victories than non-culpable autocrats, but the magnitude of the difference between these two types of autocratic leaders is never significant.

**Figure 3.1**



Some may be concerned that the relationship between leader changes and war outcomes could be epiphenomenal: that is, non-culpable leaders only come to power when the war is going poorly and therefore have the poor outcomes forced upon them instead of actively *choosing* to settle for lower terms. As the previous chapter discussed, such concerns should be minimal. Even if all non-culpable leaders came to power specifically to end the war, the behavior of the culpable leaders still runs counter to the predictions of the extant literature. The distinction, therefore, is still very much a meaningful one.

As it happens, there is little evidence of an epiphenomenal relationship. I arrived at this conclusion by estimating a model where the dependent variable was a whether a new leader was culpable or not and the independent variables were how the state was faring in the month before the new leader came to power and whether his immediate predecessor left because of the war (see Table 3.8).

**Table 3.8: Tests for a Possible Epiphenomenal Relationship**

Variable	Model 5			Model 6		
	Coefficient	S.E.	P-value	Coefficient	S.E.	P-value
Reason for Predecessor's Removal	0.546	0.43	0.199	-	-	-
Recent Battlefield Performance	-	-	-	-0.295	0.291	0.310
Log Likelihood	-64.12			-64.43		
Probability > $\chi^2$	0.198			0.306		
N	95			95		

**Table 3.8 Tests for a Possible Epiphenomenal Relationship (continued)**

Variable	Model 7		
	Coefficient	S.E.	P-value
Reason for Predecessor's Removal	0.442	0.47	0.343
Recent Battlefield Performance	-0.170	0.32	0.593
Log Likelihood	-63.98		
Probability > $\chi^2$	0.378		
N	95		

If the relationship were epiphenomenal, both of the independent variables should serve as strong predictors of the new leader's culpability: non-culpable leaders should come to power when the state is faring poorly and/or follow leaders who were removed because of the war. Neither variable demonstrates a significant relationship with the new leader's culpability in either a bivariate or multivariate specification. With these encouraging results, I proceed to the hypotheses related to the effect of leader culpability on the probability of domestic punishment.

#### 4.0 Leader Punishment

In the previous chapter I argued that a leader's culpability would have a positive relationship with his likelihood of being punished by the domestic audience in the event of a loss (Hypothesis 3). To test this hypothesis, I use a logistic regression model to

estimate the effect of the independent variables on the dichotomous dependent variable of leader punishment. I utilize two indicator variables for this initial test to highlight the effect of the difference between culpable and non-culpable losers.<sup>126</sup> Table 3.9 presents the results of the test of Hypothesis 3.

**Table 3.9: Culpability and Domestic Punishment**

	Model 8		
Variable	Coefficient	S.E.	P-value
Culpable Loser	<b>1.68</b>	<b>0.38</b>	<b>0.000</b>
Non-Culpable Loser	0.25	0.64	0.347
Adversary's War Aims	0.51	0.37	0.083
Casualties (Logged)	0.33	0.07	0.000
Pre-war Tenure (Logged)	-0.18	0.05	0.000
Constant	-4.70	0.70	0.000
Log Likelihood	-110.15		
Probability > $\chi^2$	0.00		
N	328		

This model specification dictates that the coefficients on these variables must be interpreted with respect to the excluded (or baseline) category, which is comprised of the winners in the dataset. These results align with the theory's predictions precisely: culpability has a strong, positive effect on whether a leader will be removed because of poor performance in a war. Culpable leaders are more likely to be removed than winners, while non-culpable, losing leaders are statistically indistinguishable from winners in terms of their likelihood of domestic punishment.<sup>127</sup> Comparing the marginal effect of moving from the baseline category (winners) to each of these loser types confirms this

<sup>126</sup> Because this set of models examines all leaders who participate in wars and not just the leaders who end them, the measure of the state's progress in the war at the time of the leader's removal was used for leaders who left during the war while the war's outcome was applied to leaders who oversaw the war's termination. (See footnote 111 for an example). For Models 8-10, observations are clustered by state and war (e.g., the Truman and Eisenhower observations for the Korean War share the same cluster number).

<sup>127</sup> A Wald Test between the coefficients for culpable losers and non-culpable losers confirms this finding and rejects the null hypothesis that the two coefficients are equal (one-sided p-value is 0.0156).

finding.<sup>128</sup> Culpable, losing leaders are more than four times as likely as winners to be removed from office because of the war (16% vs. 3.5%) while the difference in the probability of removal because of the war between non-culpable losers and winners is less than 1%.<sup>129</sup>

Hypothesis 4 explores this relationship in greater detail by testing for differences between culpable losers of different regime types. As the theory chapter discussed, democratic, culpable leaders should be more likely to be removed from power because of the war than their autocratic counterparts because of the political mechanisms that facilitate leadership change in democratic states. I test this hypothesis with two different model specifications: Model 9 combines leaders of autocracies and mixed regimes into a “non-democratic” category, while Model 10 examines the three regime types separately. I present these results in Table 3.10.

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<sup>128</sup> See Table A.3 in Appendix A.

<sup>129</sup> The confidence interval (presented in Table A.3 of Appendix A) surrounding these changes lends further support. The confidence interval surrounding the effect of moving from a winner to a culpable, losing leader is completely to the right of zero, indicating that culpability has a positive and significant effect on the probability of punishment for this leader type. By contrast, the confidence interval surrounding the effect of moving from a winner to a non-culpable loser includes zero, suggesting the possibility of no effect. (See Figure 3.2).

**Table 3.10: Culpability, Regime Type and Domestic Punishment**

Variable	Model 9			Model 10		
	Coefficient	S.E.	P-value	Coefficient	S.E.	P-value
Culpable, Non-Democratic Losers	<b>1.61</b>	<b>0.38</b>	<b>0.000</b>	-	-	-
Non-Culpable, Non-Democratic Losers	0.25	0.77	0.372	-	-	-
Culpable, Autocratic Losers	-	-	-	<b>1.19</b>	<b>0.47</b>	<b>0.005</b>
Culpable, Mixed Regime Losers	-	-	-	<b>2.08</b>	<b>0.48</b>	<b>0.000</b>
Non-Culpable, Mixed Regime Losers	-	-	-	0.53	0.81	0.255
Culpable, Democratic Losers	2.76	0.23	0.001	2.77	0.89	0.001
Non-Culpable, Democratic Losers	0.28	1.01	0.391	0.36	1.03	0.363
Adversary's War Aims	0.49	0.38	0.093	0.36	0.39	0.174
Casualties (Logged)	0.33	0.07	0.000	0.33	0.07	0.000
Pre-war Tenure (Logged)	-0.19	0.05	0.000	-0.17	0.05	0.000
Constant	-4.77	0.71	0.000	-4.73	0.72	0.000
Log Likelihood	-109.44			-107.97		
Probability > $\chi^2$	0.00			0.00		
N	328			328		

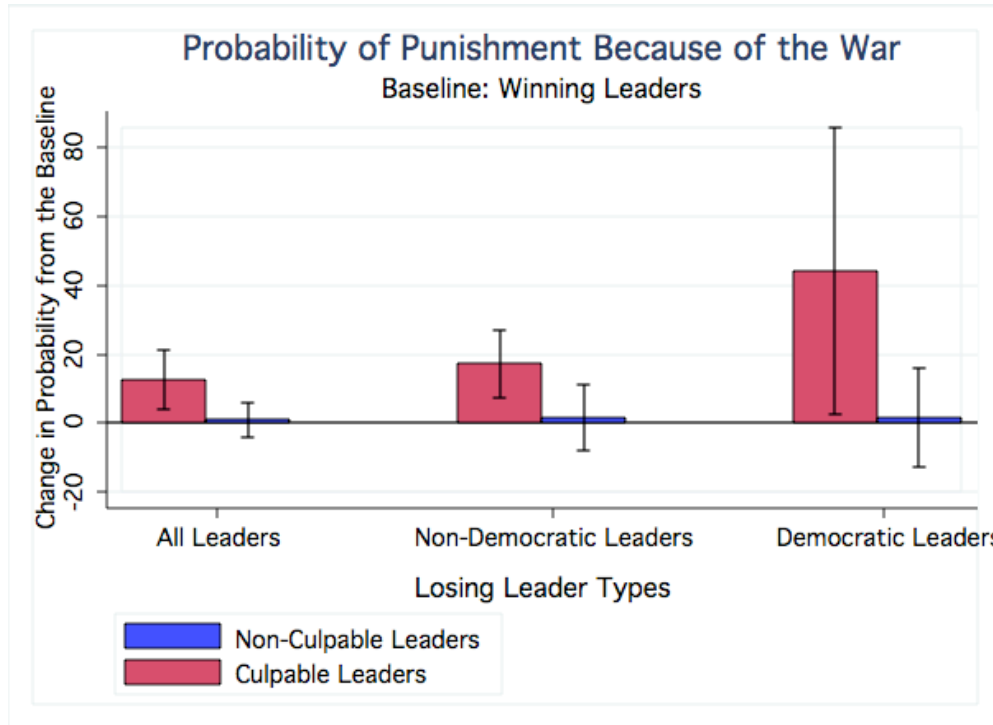
Once again, culpability exhibits a strong effect on the likelihood a losing leader will be punished, regardless of his or her regime type. In both models culpable, losing leaders are always significantly more likely than winners to lose power because of the war, while non-culpable, losing leaders are not different from winners in terms of the likelihood of punishment.<sup>130</sup> The large difference in terms of coefficient magnitude between the culpable, losing leader types and non-culpable, losing leader types lends further support to this conclusion.

Figure 3.2 illustrates these points graphically. The red bars represent the size of the change that results from a move from a winning leader to a *culpable, losing* leader while the blue bars capture the size of the change that results from a move from a winning leader to a *non-culpable, losing* leader. As above, I present the results for all

<sup>130</sup> The “non-culpable, autocratic loser” category is excluded from Model 10 because it perfectly predicts the dependent variable; none of the leaders in this category are ever punished because of the war, which is precisely in line with the theory’s predictions.

leaders, non-democratic leaders and democratic leaders; the black lines represent the 95% confidence interval around the size of the effect.

**Figure 3.2**



The results could hardly be more striking. As the significance values from Tables 3.9 and 3.10 predicted, the confidence intervals surrounding the effects for culpable, losing leaders *never* cross zero, no matter how the leaders are grouped, indicating that these effects are indeed positive. By contrast, the confidence intervals surrounding the effects for non-culpable, losing leaders *always* include zero, suggesting that there is no difference, in a statistical sense, between the likelihood of punishment for non-culpable losing leaders and winners.

Evaluating Hypothesis 4 directly also requires testing whether the coefficients for the different types of culpable, losing leaders are statistically different from one another. In Models 9 and 10, the coefficients for the democratic, culpable losers are larger than the

coefficients for culpable, non-democratic losers, but we cannot be certain of a difference without additional analysis.<sup>131</sup> Table 3.11 presents the results of several tests of equality between the coefficient for democratic, culpable losers and those of other culpable losers.<sup>132</sup> A significant result indicates that the null hypothesis of equality should be rejected.

**Table 3.11: Tests of Coefficient Equality Between Losing Leader Types**

Coefficient Comparisons			P-value (one-sided)
Model 9	Democratic, Culpable	Non-Democratic, Culpable	0.098
Model 10	“	Autocratic, Culpable	0.041
Model 10	“	Mixed Regime, Culpable	0.000

The evidence from Table 3.11 provides strong support for Hypothesis 4. Although the first test is only marginally significant in a one-tailed test, the comparisons with the coefficients from Model 10 unequivocally reject the null assumption of equality. Comparing the magnitude of the changes between autocratic and democratic culpable losers (Tables A.4A and A.4B in Appendix A) is also instructive. Although losing, culpable leaders are always more likely to lose power than their non-culpable counterparts across regime types, the magnitude of this change for democratic, culpable

<sup>131</sup> The marginal effects presented in Tables A.4A and A.4B in Appendix A reflect this as well. The effect of moving from winners to the non-culpable leader types is always very small and the confidence interval always includes zero. Moreover, the effect of moving from a winner to a culpable democratic leader is almost three times as large as the effect of moving from a winner to a culpable autocratic leader, as the larger (and highly significant) coefficient on democratic, culpable leaders in Model 9 implies. The 95% confidence interval includes zero for the democratic effect, but this is likely a result of the fact that the adversary’s war aims are held at “low” in the estimation of these marginal effects. This is the modal value of the variable for this sample, but holding it at zero may artificially deflate the likelihood of punishment. This is a common problem when calculating the marginal effects for models that contain dummy variables. When the adversary’s war aims are held at “high”, the marginal effects reflect the predictions of Model 9 precisely. The confidence intervals surrounding the size of the effect of moving from winners to non-culpable leader types always includes zero while the effect of moving from winners to culpable leaders on punishment is always positive and significant, regardless of regime type. Additionally, the difference in the size of the change created by culpable autocratic leaders versus culpable democratic leaders becomes more striking. I present these results in Table A.4B.

<sup>132</sup> The first row is based on the estimates from Model 9 and the latter two are based on Model 10.



leaders is more than *twice* that of culpable, autocratic losing leaders.<sup>133</sup> Democratic, culpable leaders, therefore, should be more concerned about failing to secure a favorable outcome than non-democratic, culpable leaders since they face a higher likelihood of punishment. This finding lends support to the notion that of all leaders who participate in wars, culpable democratic leaders should have the strongest incentives to gamble for resurrection.<sup>134</sup> Non-democratic, culpable leaders will also feel this pressure, but it will be most pronounced in democratic states.

Having established that culpable leaders of any regime type are always more likely to be punished than non-culpable leaders, I now turn to Hypothesis 5, which explores a key potential difference among culpable leaders. As discussed in Chapter 2, although some have argued that the pressures of culpability should apply equally to all first leaders, regardless of whether the leader chose to join the war or was attacked<sup>135</sup>, it is reasonable to posit that culpable leaders whose state was the target of an attack or invasion will face a lower probability of domestic punishment should they lose. Although the domestic audience could fault them for failing to deter the adversary, the fact that war was forced upon the state should also play a role in the audience's assessment of the leader's wartime performance.

To evaluate this hypothesis, I estimated a logistic regression model on first leaders who either lost the war or who were removed when the war was going poorly. I use this subset of the data to ascertain whether leaders who chose to participate in the war and eventually lost face a greater risk of punishment than leaders who eventually lost the

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<sup>133</sup> This is true regardless of the value of the adversary's war aims variable.

<sup>134</sup> Downs and Roche (1994).

<sup>135</sup> Bueno de Mesquita, Siverson and Woller (1992).

war but who had little choice in fighting. Model 11 in Table 3.12 presents the results of this test.

**Table 3.12: Voluntary Participation and Culpability Costs**

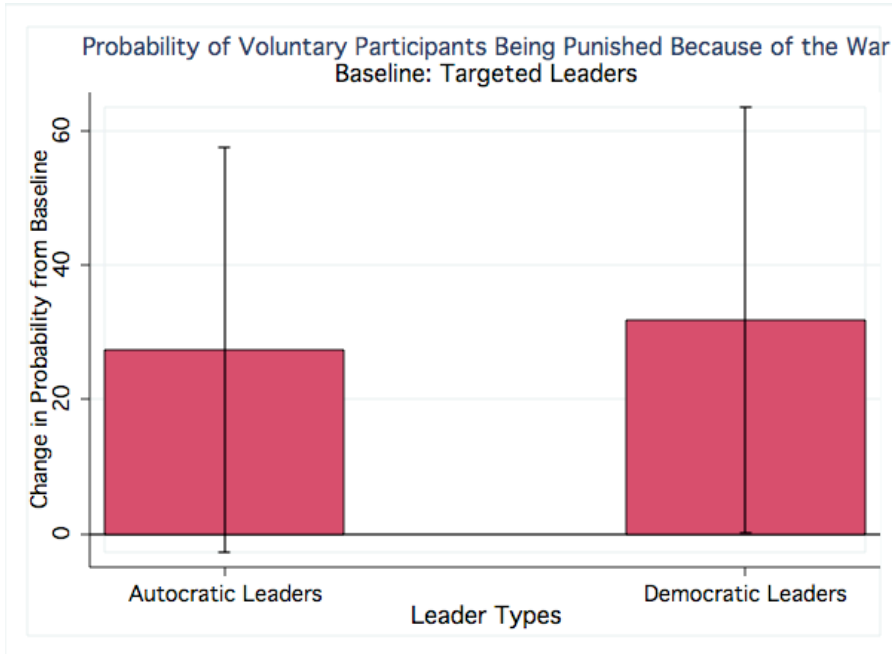
Variable	Model 11 (First Leaders)			Model 12 (All Culpable Leaders)		
	Coefficient	S.E.	P-value	Coefficient	S.E.	P-value
Voluntary Participant	<b>1.32</b>	<b>0.72</b>	<b>0.033</b>	<b>0.94</b>	<b>0.59</b>	<b>0.056</b>
Democratic Leader	0.46	1.21	0.351	1.08	1.06	0.153
Adversary's War Aims	1.83	0.88	0.018	1.61	0.71	0.011
Casualties (Logged)	0.16	0.11	0.078	0.27	0.12	0.010
Pre-war Tenure	-0.31	0.16	0.027	-0.19	0.07	0.004
Constant	-2.27	1.57	0.074	-3.85	1.23	0.001
Log Likelihood	-35.72			-43.87		
Probability > $\chi^2$	0.015			0.00		
N	80			97		

As expected, of first leaders who lose, initiators and joiners are significantly more likely to be punished than leaders who were targeted. Figure 3.3 demonstrates this graphically.<sup>136</sup> The marginal effects of moving from targets (the baseline) to voluntary participants (as shown by the red bars) is very large and positive, although this effect is, not surprisingly, more pronounced among democratic leaders. The size of the effect is similar for autocratic leaders, but the confidence interval surrounding the left bar includes zero.<sup>137</sup>

<sup>136</sup> The effect is nearly identical when all leaders are considered. See Table A.5 in Appendix A.

<sup>137</sup> The magnitude of the *change* is higher for autocracies, but this is likely due to the relatively high baseline probability of leader removal in democratic states. See Table A.5 in Appendix A.

**Figure 3.3**



Comparing this finding to the same finding in Model 12 offers further insight as to the varying degrees of culpability. Model 12 includes all culpable, losing leaders, even if they were not in charge when the war began. Interestingly, when culpable leaders who come to power during the war and eventually lose are introduced into the sample, the effect of choosing to join the war remains positive and significant. This suggests that the extra burden of culpability that comes with *personally* choosing to involve the state in the war transfers to the leader's culpable successor. Taken together, these results highlight important distinctions among culpable leaders; as a group, they are more likely to be punished than non-culpable leaders, but leaders who can be directly linked to the decision to begin fighting will face the highest likelihood of punishment.

## 5.0 Implications and Remaining Questions

The analyses presented in this chapter provided strong support for the core arguments of the theory. Culpable leaders are more likely to preside over better outcomes, even while controlling for other factors, such as initiation and relative strength, which might account for victory. This analysis also found broad support for the argument that culpable leaders are motivated to achieve such outcomes because of a greater likelihood of domestic punishment should they lose. Culpable losers are always more likely to be punished than non-culpable losers, and this effect is especially prominent in democratic states. In addition to this variation across regime types, this chapter also provided evidence in support of the idea that culpable leaders who initiate wars are more likely to be punished than culpable leaders who are attacked or culpable leaders who inherit a war from a predecessor who was the original aggressor.

These results are very encouraging, but the question of what factors determine a leader's culpability in the eyes of individual citizens has yet to be addressed. This chapter operationalized culpability by examining political connections between leaders. As Chapter 2 made clear, however, culpability costs can also stem from a leader's position on the war when it first began. If a leader who comes to power during the war has publicly supported the conflict in the past, she will have difficult time disassociating herself from a decision that has likely become unpopular with the public. I explore the relative strength of these two culpability cues (political connections with the culpable leader and previous support for the conflict) in Chapter 4 by analyzing a survey experiment designed with this specific question in mind.

## Chapter 4

### 1.0 Introduction

The previous chapter addressed hypotheses regarding the relationship between a leader's culpability for a given conflict, the type of outcome the leader settled for, and a losing leader's probability of being removed from office. Empirical evaluation of these hypotheses demonstrated clear causal links between the three. First, even after controlling for a range of other predictors of war outcomes, including those related to strategic selection (i.e., voluntary participation), culpable leaders are significantly more likely to preside over favorable outcomes than non-culpable leaders. Second, culpability also plays a role in determining a leader's post-war (or, frequently, wartime) fate. Culpable leaders are very likely to lose office when they fail to secure a victory, while non-culpable leaders are significantly less likely to lose power, regardless of the conflict's outcome.

These clear differences between culpable and non-culpable leaders lend credence to the advantages of the theoretical innovation suggested in Chapter 2: to understand fully the war termination process scholars must recognize that new leaders who come to power during wars will often face dissimilar incentives for continuing the war than their predecessors did. In Chapter 3, a simple coding rule served as a proxy for this change in incentives. New leaders who shared a political connection with the first leader (i.e., being from the same party in democratic states or the same ruling group in autocracies) were

assumed to inherit their predecessor's culpability and the associated incentives to continue fighting until a favorable outcome could be secured. New leaders who lacked this political connection, on the other hand, were thought to have a clean (or at least cleaner) slate domestically and, consequently, an increased incentive to terminate the war.

Although this approach represents a good first cut, the aggregated nature of the analysis (looking at the behaviors and fates of warring leaders) meant I could only *infer* how an individual citizen would react to a culpable leader. This limitation is substantial since the reasons *why* citizens might be more prone to punishing certain leaders play a crucial role in the causal mechanism specified in Chapter 2.

This chapter problematizes Chapter 3's assumption about differentiating new leaders based on their political affiliation to their predecessor. Do citizens transfer culpability for their state's involvement in a conflict along political lines when a new leader comes to power? Or does culpability always pass from one leader to the next, regardless of political connections? To determine when and why a new leader will inherit culpability for a conflict, and thus test the appropriateness of Chapter 3's assumption, I designed a survey experiment in which respondents assessed the responsibility of a hypothetical senator for American involvement in Iraq.<sup>138</sup>

The experiment focuses on two types of responsibility: a leader's *technical* responsibility (i.e., did he vote for the war?) and his *associational* responsibility (i.e., is he from the same party as the leader who presided over the beginning of the war?). If the causal mechanisms specified in Chapter 3 are correct, leaders who are responsible by

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<sup>138</sup> The shift in terminology from "culpable" to "responsible" is intentional. To avoid introducing bias into the survey as to whether the decision to invade Iraq was a wise one, "culpable" was not used in the treatment wording because of its negative connotation.

association for the conflict should be held more responsible than leaders who do not share a political affiliation with the war's first leader. The technical responsibility cue should also matter in the minds of citizens. As Chapter 2 argued, leaders who publicly supported a war will also be held culpable, even if they are from a different ruling group than the first leader.

An experimental design is ideal for the purposes of this chapter because, as later sections will make clear, it allows all characteristics of the (fictional) senator presented in the treatment to remain constant except for the key manipulations of interest: his party and whether he supported the war in 2002. This aspect of the experiment makes three sets of insights possible. First, and most importantly, it sheds light on how *individual citizens* respond to leaders who express various combinations of the two types of responsibility. This addresses the gap in the theory's causal chain that Chapter 3 could not, and also contributes significantly to our understanding about how citizens think about the accountability of individual leaders in a wartime setting.

Second it allows me to assess the validity of the coding rule used in Chapter 3 by examining the effect of the associational responsibility cue, first in isolation and then in terms of how it compares in terms of magnitude to the technical responsibility cue. Second, since the design incorporates all four possible combinations of the two cues, I can also explore other characteristics of the cues, namely whether there is an additive effect in certain combinations of the cues as well as any interactive effect between the two treatments and important characteristics of the respondent.

This chapter proceeds in four sections. Section 2 highlights several hypotheses suggested by Chapters 2 and 3 and briefly reviews how existing scholarship informs the

present study. Section 3 describes the experimental design and its advantages over other survey methods in more detail. Section 4 presents the empirical findings from the survey experiment, including the power of the associational responsibility cue, the existence of a strong additive effect when both types of responsibility are cued, and the stability of these effects across different subgroups of respondents. Section 5 concludes with a brief discussion of implications of the findings for the larger project.

## **2.0 Hypotheses Regarding Responsibility**

In the interest of clarity, the hypotheses will be presented in three groups; each group is designed to address a different aspect of how the cues work. The first set examines how each type works individually. That is, regardless of how the fictional senator voted, do respondents assign more or less responsibility to him if they know he is a Republican? (Or, similarly, holding party constant do they assign more responsibility to the senator if they know he voted for the war?) This set also includes hypotheses that allow me to ascertain the relative magnitude of the size of each of these individual treatment effects.

The second set of hypotheses builds on the first by examining how respondents respond to certain *combinations* of the cues (e.g., the reaction of a Republican who voted for the war compared to a Republican who voted against it). These hypotheses allow me to discern whether an *additive* effect exists when both responsibility cues are present as opposed to a single cue. Finally, the third set briefly explores whether respondents react differently to the cues based on their own partisan leanings by interacting the different treatments with the respondent's party identification. Although the second and third sets



of hypotheses are not as crucial to address the validity of the coding rule from Chapter 3 as the first, they do have the potential to shed light on how citizens assess a leader's responsibility more generally.

## **2.1 Types of Responsibility**

The purpose of this section is to determine the general effect of presenting a respondent with cues that highlight a leader's associational responsibility or technical responsibility. These hypotheses represent the most basic and important test of the theory about how citizens assign responsibility to leaders and how that responsibility might be passed on to the leader's successor. If the theory presented in Chapter 2 (and the coding rule presented in Chapter 3) is valid, the associational cue should have a strong, positive effect on a citizen's evaluation of a leader's responsibility. Although the justification surrounding the rule is less concerned with the effect of cuing a respondent to a leader's technical responsibility, it is important to determine its effect (both individually and relative to the associational responsibility cue) given the comparisons of the combinations of cues explored in the next section.

A leader is technically responsible for a political action if she either made the decision to carry out the action herself (i.e., was a first leader) or if she publicly supported a decision made by another leader. The survey experiment presented here ascribes technical responsibility to the senators who voted for the war in 2002, and the hypotheses are phrased to reflect this.<sup>139</sup> Although the war, like most foreign policy decisions, has come to be largely associated with the Executive (President Bush),

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<sup>139</sup> A similar rule is used for the hypotheses regarding the experimental senator's associational responsibility (i.e., hypotheses are phrased in terms of Republicans and Democrats instead of senators with or without associational responsibility).

obtaining Congressional approval was not a trivial matter. The legislature's agreement to authorize the war (and the funds needed to prosecute it) was necessary in both a legal and political sense.<sup>140</sup> An affirmative vote on the Iraq War Authorization Bill (JR114 October 2002), therefore, represents the highest degree of technical responsibility an individual senator could have in this situation. Consequently, this should lead respondents to attribute more responsibility to the senator when presented with this cue, *regardless* of the experimental senator's political party.

**H1:** Respondents who believe the statement is from a *senator with technical responsibility* will assign more responsibility to the senator than respondents who believe the statement is from a *senator without technical responsibility*.

The second type of responsibility stems from a leader's political affiliation. Because the survey experiment occurs in a democracy, the senator's political party determines whether this cue is present. If the logic presented in Chapter 2 is correct, citizens should hold Republican senators more responsible for US involvement in Iraq, by virtue of sharing a partisan label with President Bush, the war's initiator.<sup>141</sup>

**H2:** Respondents who believe the statement is from a *senator with associational responsibility* will assign more responsibility to the senator than respondents who believe the statement is from a *senator without associational responsibility*.

Knowing how the two cues work individually is an important first step, but testing the relative size of the effects generated by each of the cues is also informative if we wish

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<sup>140</sup> From a legal standpoint, the War Powers Act of 1973 limits the Executive's ability to prosecute international conflicts for long periods of time without Congressional authorization. From a political perspective, as some scholars have suggested (Schultz 2001a), gaining broad legislative approval increased the war's legitimacy in the eyes of the public.

<sup>141</sup> Because President Bush was also the leader who initiated the war in addition to being the war's first leader, I cannot, with this survey, determine if the same effects would hold for a new leader who shared a political affiliation with *any* first leader, as the coding rule in Chapter 3 implies.

to gauge the strength of the associational cue. Typically, the cues should be highly correlated with one another (i.e., a Republican senator should be more likely to support the war while a Democratic senator should oppose it). In democratic political systems with a relative lack of party discipline like the United States, however, the two will not always synchronize perfectly. Because divergent combinations of technical and associational responsibility (e.g., a Democrat who supported the war) are always possible and, on some issues, even probable, the relative strength of the two remains an open empirical question. In theory, one of three possible relationships could exist between the cues; party could dominate vote, vote could dominate party, or the cues could trigger similar responses.

Scholars of American politics have consistently found evidence that citizens place a great deal of weight on a given leader's political party membership.<sup>142</sup> Citizens appear to use the leader's partisan affiliation as a simple heuristic to predict the leader's position on an issue in the absence of specific information about the of the leader's *actual* position. Although the need for the heuristic is less pressing here since the treatment provides the respondent with information about how the experimental senator voted, the possibility exists that the associational responsibility cue will still outweigh the technical responsibility cue.

**H3a:** Respondents who believe the statement is from a *senator with associational responsibility* will assign more responsibility to the senator than respondents who believe the statement is from a *senator with technical responsibility*.

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<sup>142</sup> Erikson (1988), Cotton (1986), Cohen (2003), Goren (2002), Kam (2005), Malhotra and Kuo (2007). Skitka and Robideau (1997: 975), for instance, found that “the majority of [their experimental] subjects voted as a function of party label, even when candidates’ positions on the issues were not very consistent with their own.”

Although the strength of associational cues has received broad empirical support, it is important not to overlook the leader's *technical* responsibility. The international relations literature, for instance, often assumes citizens pay close attention to a leader's specific decisions and actions.<sup>143</sup> If this description of reality is accurate, citizens treat leaders who voted for the war differently from leaders who voted against it. Although many have noted Americans' traditional apathy towards matters of foreign policy<sup>144</sup>, empirical evidence suggests that citizens are capable of moving beyond the simple partisan heuristic when evaluating leaders.<sup>145</sup> If citizens make judgments in this even slightly more sophisticated manner, they will weigh the technical responsibility cue more heavily.

**H3b:** Respondents who believe the statement is from a *senator with technical responsibility* will assign more responsibility to the senator than respondents who believe the statement is from a *senator with associational responsibility*.

Finally, since citizens can blame multiple leaders for a given decision, both cues could potentially trigger similar reactions from respondents for *different reasons*, with neither cue dominating. In other words, if faced with a group of senators with different combinations of party membership and positions on the war, a respondent may be willing

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<sup>143</sup> The audience costs literature provides the clearest example of this; scholars commonly assume citizens pay close attention to individual threats and declarations. (Baum 2004), Schultz (2001a and 2001b), Fearon (1997).

<sup>144</sup> Aldrich et al. (1989).

<sup>145</sup> Hibbing and Alford (1981), for instance, find that voters are only more likely to punish *incumbents* of the president's party in midterm elections instead of all member of the president's who are running for office. This finding suggests that citizens do not employ the partisan heuristic cue blindly. Although the punishment may be somewhat arbitrary since not all incumbent members of the president's party may have voted the same way, citizens are at least willing to condition their punishment based on whether the legislator *could* have voted that way. If the partisan cue completely dominated, we would expect incumbents and non-incumbents to have an equal likelihood of punishment.

to blame one leader because of his party and another leader because of his vote.<sup>146</sup> This suggests the following hypothesis.

**H3c:** Respondents who believe the statement is from a *senator with associational responsibility* will assign equal amounts of responsibility to the senator as respondents who believe the statement is from a *senator with technical responsibility*. The direction of both effects should be positive and their magnitudes should be similar.

The hypotheses presented so far can be summarized in the following table:

**Table 4.1: Summary of Hypotheses 1-3c**

Hypothesis	Expectation
H1	VT coefficient significant and positive.
H2	RT coefficient significant and positive.
H3a	RT>VT in terms of magnitude and significance.
H3b	VT>RT in terms of magnitude and significance.
H3c	RT=VT in terms of magnitude and significance.

VT= “Voted for War” treatment, RT= “Member of Republican Party” treatment.

## 2.2 Combining the Cues

The second set of hypotheses examines whether certain *combinations* of cues affect respondents’ assessments of leaders in different ways. This suggests the possibility of an interesting additive effect among the cues. If, for instance, we have reason to suspect that cuing a respondent to a leader’s technical *or* associational responsibility might lead the respondent to hold the leader more responsible, assigning both cues simultaneously may increase a respondent’s assessment of the senator’s responsibility even further.

Some may find this notion implausible since, in a practical sense, any senator who voted for the war is equally responsible for it as any other who voted for it, regardless of

<sup>146</sup> Although respondents only evaluate one type of leader, the random assignment of treatment groups allows me to generalize about how a given respondent would have reacted to the other leader types.

party membership. We should not discount the possible additional effect of the associational cue without further investigation, though. Respondents who are presented with a Republican senator may, for instance, infer from the partisan label that the senator was a more enthusiastic supporter than a Democrat who voted the same way, and may have worked harder to make sure the bill passed.

More generally, because both cues suggest a dichotomy in the mind of the respondent (e.g., a senator can either be a Republican *or* Democrat, voted either for the war *or* against it), we should not be surprised if cuing the respondent to one type leads her to compare the senator to the two, hypothetical senators with opposite characteristics. If, for example, a respondent were asked to assess the responsibility of a Republican senator who voted against the war, the relevant comparison types would be a Democratic senator who also voted against it and a Republican senator who voted for it. If we assume that an additive process is at work, we might expect this respondent to assign a middling degree of responsibility to the experimental senator she was presented with since he is less responsible than the Republican who voted for it yet more responsible (by his partisan affiliation) than the Democrat who voted against it.

In the present survey experiment, the two cues create four possible combinations, which produce three different additive values (see Table 4.2). A 1 indicates the presence of the cue while a 0 signifies its absence.

**Table 4.2: Cue Combinations**

Treatment	Associational Responsibility	Technical Responsibility	Additive Cue Total
1. Republican, voted for the war	1	1	2
2. Republican, voted against the war	1	0	1
3. Democrat, voted for the war	0	1	1
4. Democrat, voted against the war	0	0	0

As Table 4.2 illustrates, if the additive logic is correct, the Republican who voted for the war should be attributed the highest level of responsibility while the Democrat who voted against the war should receive the least.<sup>147</sup> The remaining two senators represent the divergent cue combinations since both voted counter to their party's general position. They each only have one cue assigned to them and should, therefore, elicit similar reactions from respondents. The relative ranking of the different cue combinations' additive totals presented in Table 2 suggest several hypotheses. The first group speaks to the relationship between the treatment group with the highest presumed effect—the Republican who voted for the war—to the other treatments.

**H4:** Respondents who believe the statement is from a *Republican senator who voted for the war* will assign more responsibility to the senator than respondents who believe the statement is from a *Democratic senator who voted against the war*.

**H5:** Respondents who believe the statement is from a *Republican senator who voted for the war* will assign more responsibility to the senator than respondents who believe the statement is from a *Republican senator who voted against the war*.

**H6:** Respondents who believe the statement is from a *Republican senator who voted for the war* will assign more responsibility to the senator than respondents who believe the statement is from a *Democratic senator who voted for the war*.

The second set of hypotheses parallels the logic suggested by Table 2 and targets the relationships between the treatments with one or fewer additive cue totals.

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<sup>147</sup> Table 4.2 illustrates why these hypotheses are additive and not interactive. The difference between the Republican who voted for the war and the Republican who did not ( $2-1=1$ ) is the same as the difference between the Democrat who voted for and the Democrat who voted against it ( $1-0=1$ .) The addition of the technical responsibility cue, in other words, increases the overall responsibility of senators *by the same amount*, regardless of party. (A parallel story could be told for the associational cue.) Because we do not expect the presence of the technical responsibility cue to affect senators of the two parties *differently*, the hypotheses are not interactive. Appendix B illustrates this point graphically.

**H7:** Respondents who believe the statement is from a *Republican senator who voted against the war* will assign equal amounts of responsibility to the senator as respondents who believe the statement is from a *Democratic senator who voted for the war*.

**H8:** Respondents who believe the statement is from a *Republican senator who voted against the war* will assign more responsibility to the senator than respondents who believe the statement is from a *Democratic senator who voted against the war*.

**H9:** Respondents who believe the statement is from a *Democratic senator who voted for the war* will assign more amounts of responsibility to the senator as respondents who believe the statement is from a *Democratic senator who voted against the war*.

### **2.3 Respondent Interactive Hypotheses**

The third set of hypotheses explores whether respondents react differently to the various leader types depending on their own characteristics, such as personal party membership. The American political literature contains a great deal of evidence that citizens are *less* likely to blame leaders of their *own* political party (and, by implication, *more* likely to blame leaders of the *opposing* political stripe) for poor policy outcomes.<sup>148</sup> This implies an interactive hypothesis since we would expect the treatments to elicit *different* responses from respondents based on the *respondent's* partisan affiliation. For example, we would expect a Democratic respondent to evaluate a Democratic senator who voted for the war more favorably than a Republican respondent would (and vice versa if a Republican senator who voted for the war was the treatment of interest.) The

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<sup>148</sup> Malhotra and Kuo (2007). Cohen (2003: 811), for instance, found that cueing experimental subjects to the percent of Republicans or Democrats in Congress that supported a proposed welfare program “overrode policy content” in determining whether a citizen would support a policy. Subjects would overwhelmingly support a policy if a majority of members of Congress of their preferred party were in favor of it.



effect of the associational cue, in other words, is contingent on the *respondent's* party ID. Hypotheses 10a and 10b test this causal logic.<sup>149</sup>

**H10a:** *Republican* respondents will assign more responsibility to a *Democratic* senator who voted for the war than to a *Republican* senator who voted for the war.<sup>150</sup>

**H10b:** *Democratic* respondents will assign more responsibility to a *Republican* senator who voted for the war than to a *Democratic* senator who voted for the war.

### 3.0 Testing the Hypotheses

To test these hypotheses, I designed and embedded an experiment in a nationally representative survey. A professional survey research firm, Knowledge Networks, administered the experiment, using a randomly selected subset of a pre-assembled panel of respondents.<sup>151</sup> More than 800 people responded over the course of June and July 2007. Respondents were asked to evaluate the degree to which a fictional senator, John Harris, was responsible for American involvement in Iraq. This section describes the experimental procedure in more detail to illustrate how it will enable tests of hypotheses about how the associational and technical responsibility cues affect respondents' assessments of the different leaders.

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<sup>149</sup> Although the hypotheses here focus on how respondents react to leaders who voted for the war, the models that actually test these hypotheses will test for any differences across all leader types.

<sup>150</sup> Because a majority of Republicans were strong supporters of the war when it began, this tendency to blame the other party may be stronger for Democrats for this particular conflict. Of all respondents, Republicans have the highest probability of being among the (dwindling) group that still supports the decision to invade Iraq. Because of this, Republicans, on average, may be less actively seeking a party to blame since, for them, the war was (and is) a good idea. Democrats, on the other hand, (again, on average) should be more eager to punish a leader for the war, especially if the leader is from the Republican Party. Even though the vast majority of Democratic members of Congress voted for the war when it began, Democrats may be more willing to forgive their fellow partisans.

<sup>151</sup> Respondents are recruited using a random digit dialing (RDD) process to ensure the panel is as representative as possible.

### 3.1 Experimental Design and Procedure

The experiment employs a fully crossed, 2x2 design, resulting in four treatment groups that represent the full range of cue combinations discussed above (see Figure 4.1.)<sup>152</sup>

**Figure 4.1: Experimental Design**<sup>153</sup>

Partisan Affiliation	Previous Position on the War	
	Voted Yes	Voted No
<b>Republican</b>	<b>Treatment Group RYV:</b> John Harris is a Republican who voted for the war in 2002.	<b>Treatment Group RNV:</b> John Harris is a Republican who voted against the war in 2002.
<b>Democrat</b>	<b>Treatment Group DYV:</b> John Harris is a Democrat who voted for the war in 2002.	<b>Treatment Group DNV:</b> John Harris is a Democrat who voted against the war in 2002.

First, respondents were randomly assigned to one of the four treatment groups.<sup>154</sup>

This critical step allows for the comparison of the treatment effects across groups.

Because the respondents were assigned in a random way, we can be confident that any

<sup>152</sup> This design is similar to a survey experiment carried out by Kuklinski & Hurley (1994). In their study, all respondents were read an identical statement about affirmative action but the race and political ideology of the speaker varied across the different conditions.

<sup>153</sup> Treatment group abbreviations (e.g., “RYV”) will be used for the remainder of the chapter.

<sup>154</sup> In the initial survey, a control group was also included. Respondents assigned to this fifth group received no information about the senator’s partisan affiliation or how he voted on the war. Although this group is technically correct in terms of what a control group should do, further reflection led me to drop this group from the analysis since respondents in this group were forced to make an assessment of Senator Harris with *no* information (as opposed to neutral information) and, consequently, cannot easily be compared to respondents who were given information about Harris’ party and voting record. Respondents in the control group are most likely giving a completely random response while respondents in the other treatment conditions have the relevant information they would expect when asked to evaluate a leader. In practice, respondents assigned to the control group reacted to their fictional senator in very similar ways to respondents assigned to treatment groups with divergent cue combinations, RNV and DYV (see Appendix C). Again, while this could be evidence of the divergent cues triggering similar degrees of confusion as a cue with no information about the leader, such comparisons should be interpreted with caution. More generally, the results of tests that included the control group mirrored the results of tests where the control was excluded (e.g., the RYV treatment effect was higher than the control while the DNV treatment effect was lower).

significant differences between the groups are the result of the treatments and not some other factor.<sup>155</sup>

Next, respondents were asked a short series of multiple choice questions regarding their position the Iraq war, their own partisan leanings and their feelings on whether force should be used to solve international problems.<sup>156</sup> These respondent characteristics will serve as important robustness checks or interactions in the statistical analysis below.<sup>157</sup> Asking respondents for this information in the pre-treatment phase avoids bias; asking in the post-treatment phase (i.e., after they had read the statement from Senator Harris regarding the war) introduced the risk of biasing their response in light of the information they were given about the Senator and the war.

After the pre-treatment questions, respondents read a statement from a senator about the war in Iraq. The text for Treatment Group RNV follows; italicized portions of the text varied across the groups accordingly:

“Senator John Harris, a *Republican* who is a senior member of the Foreign Relations Committee and who *voted against* the war when it began in 2003, has just released the following statement: “Despite the fine efforts of our armed forces, careful analysis of the current situation in Iraq has led me to conclude that our continued presence is unlikely to achieve the goals set in 2003 and will likely make an already bad situation even worse. Given this assessment, it is my opinion that all US forces should be withdrawn from Iraq over the next three months.”<sup>158</sup>

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<sup>155</sup> Statistical tests confirm that the randomization was successful. Please see Appendix D.

<sup>156</sup> This final question was included to gauge whether the respondent tended towards the “hawkish” or “dovish” end of the spectrum. All pre-test questions regarding a respondent’s party ID, and the degree to which they agreed with the war were based on question wording from the American National Election Study. Appendix E gives the precise question wording.

<sup>157</sup> The survey firm administering the experiment—Knowledge Networks—also provided several variables with basic demographic information (e.g., age, gender) about each respondent.

<sup>158</sup> The actual wording of the treatment speaks to when the ground invasion began (March 2003) a date respondents are more likely to identify as “the beginning of the war”, even though the actual vote that granted President Bush the freedom of action to use military force took place in October of the previous year. The 2002 vote, therefore, is technically a vote for or against the war that began in 2003. Although this particular choice is somewhat awkward (especially since so many Congressmen have come under fire for their vote), there is no reason to suspect respondents would have reacted to the treatments differently if 2002 was used instead. 2003 was used, again, to coincide with the far more public invasion date and

Several aspects of the precise wording of the treatment are worth discussing in greater detail. First, a senator is the focus of investigation because of the Senate's influential role in foreign policy and because many leaders who eventually occupy the key executive position begin their careers as legislators at the national level.<sup>159</sup> Second, the experimental senator was given the additional position on the Foreign Relations Committee both to lend credence to the idea that he was making an informed decision at the time and also to imply that he has considerable influence (insofar as a senator can) over foreign policy.

Holding *everything* about the treatments constant except for John Harris' party and position on the Iraq War ensures the manipulations of these characteristics are the only source of systematic variation across the treatment groups. Holding the senator's name constant, obviously, necessitated the use of a fictional name. Currently (and at the time of the vote in 2002), nobody by the name of John Harris serves in either chamber of Congress.

Although an actual senator's name could have been used for each of the four treatment groups<sup>160</sup>, using a real person's name carries a high potential of undermining the effect, if any, of the experimental design. If, for instance, Senator Hillary Clinton (D-NY), was used in Treatment Group DYV, respondents may assess her based on the

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because a number of bills related to the funding of the war, which were also more in the public eye at the time, were passed soon after the war began. The second part of the statement, which suggests withdrawing American forces over the next three months, was included to test other hypotheses that not directly relevant to the issue at hand.

<sup>159</sup> A member of the House of Representatives, of course, is also a legislator who plays a role in foreign policy, but the fact that, historically speaking and in the American context, more presidents have come from the Senate makes a senator a more credible "next leader" than a representative.

<sup>160</sup> Although the vast majority of the Senate voted in favor of the war (77 to 23), 21 Democrats dissented along with one Republican—Senator Lincoln Chafee of Rhode Island. One Independent also dissented: Senator Jim Jeffords of Vermont.

manipulations of interest the current study, but they also may evaluate her based on issues wholly unrelated to the matter at hand (e.g., her stances on national health care or teaching evolution in schools). Because the survey does not allow me to ask the respondent *why* they are making particular choices, variation in wording across the different treatment groups had to be minimized.

After reading the treatment, respondents were asked the following question: “On a scale of 1 to 5, with 1 meaning “not at all responsible” and 5 meaning “very responsible” where would you place Senator Harris in terms of responsibility for American involvement in Iraq as a whole?”<sup>161</sup> Once this brief post-treatment phase was complete, the respondent was thanked for her time and debriefed of the fact that the entire statement she had just read and Senator Harris were both entirely fictitious.

### **3.2 Advantages of an Experimental Design**

Before proceeding to the results section, it is important to understand the advantages of an experimental survey to other, more traditional survey approaches for answering the hypotheses presented here. Any survey that relies on a standard observational design will either be unable to test the full range of hypotheses or will introduce serious concerns regarding internal validity. For example, conducting a panel survey before and after the presidential election in 2008 would not allow a researcher to determine the effect (if any) of the new leader’s earlier position on the war or partisan affiliation since the new leader will have either been for *or* against the war since it began and either a Democrat *or* Republican in 2002. Because this design only allows us to

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<sup>161</sup> To make the interpretation of the results more clear (and to bring things more in line with conventional practices), the responsibility assessments have been rescaled on a scale of 0-1. (See, for example, Brader (2006).

observe how citizens would respond to one type of new leader (say, a Democratic leader who supported the war in 2002), we cannot know with certainty which process is at work because we lack the necessary comparison groups.

Moving to a purely hypothetical situation in which respondents are asked about how they would feel about a given leader's actions at a future date (e.g., "How would you feel if a Republican president who voted for the war chose to withdraw all US forces from Iraq in March 2009?") would allow for the observation of responses to different types of leaders, but it would also put the study on questionable grounds in terms of validity. Asking respondents about how they would feel about a hypothetical next president requires them to project what their opinion would be in a time period several years in the future with almost no contextual information. The experiment design here alleviates these problems since it allows for variation on the earlier position and partisan affiliation of the new leader while keeping the question in the current time period.

#### **4.0 Results**

The hypotheses presented in Section 2 were designed to determine three things: the effect of a given treatment, how these treatment effects compare to one another once combined and, finally, how the treatment effect varies (if at all) within a subset of respondents. I will now discuss these three sets of findings, elaborating, where appropriate, on model specification.

#### 4.1 Treatment Effects

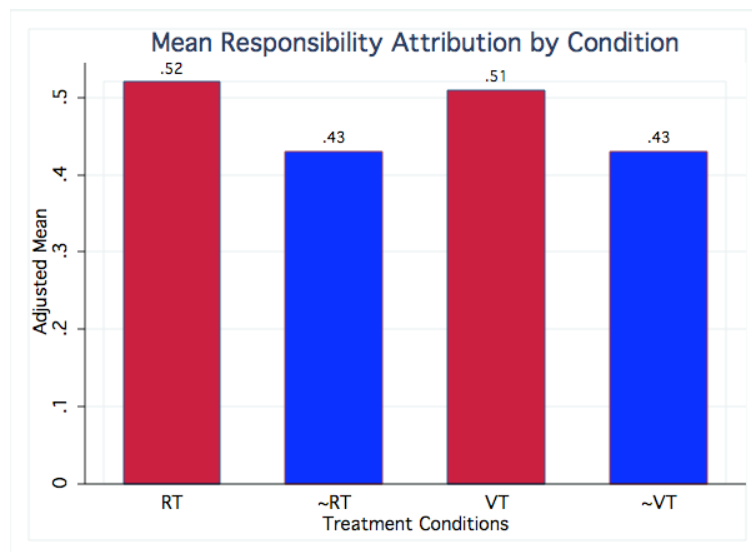
The first set of hypotheses (H1 and H2) posited that respondents who were cued with a senator who exhibited either associational *or* technical responsibility would assign more responsibility to that senator than respondents who were presented with a senator who lacked these cues. To determine the effect of cuing each type of responsibility separately, I pool pairs of treatment groups, first collapsing by row and then by column (see Figure 4.1). To ascertain the effect of associational responsibility, I compare the mean attribution of responsibility of all respondents who were presented with a Republican senator (i.e., treatment groups RYV (Republican, Voted Yes) and RNV (Republican, Voted No) to the mean attribution of all respondents who were presented with a Democratic senator (treatment groups DYV (Democrat, Voted Yes) and DNV (Democrat, Voted No)).

For the purposes of the figures below, the first group (where senators are pooled based on party) will be referred to as RT (Republican Senator Treatment) while the second will be ~RT (Democratic Senator Treatment). A similar process determined the effect of technical responsibility: respondents who were shown a senator who voted for the war (treatment groups RYV and DYV) were compared with respondents who were cued with a senator who lacked technical responsibility (RNV and DNV). The first pooled group in this second set will be referred to as VT (Senator Who Voted Yes Treatment) while the second will be ~VT (Senator Who Voted No Treatment.)

Comparing the means of these pooled groups provides strong support for Hypotheses 1 and 2. First, when comparing the assessments of the Senator from respondents cued with associational responsibility (RT) to those who were not (~RT), a

sizeable and significant ( $p < 0.00$ ) difference of eight points exists. A similar effect emerges comparing respondents who were cued to technical responsibility (VT) to those respondents who were cued the opposite way ( $\sim$ VT). The responsibility assessments of respondents who were presented with an experimental senator who voted for the war were, on average, 9 percent points higher than respondents who were told the experimental senator did not vote for the war.<sup>162</sup> This difference is also highly significant ( $p < 0.00$ ). These comparisons are captured in Figure 4.2.

**Figure 4.2**



Means have been rescaled to range between 0 and 1.

<sup>162</sup> Again, means were rescaled from 0-1 for ease of interpretation. The actual means from the 1-5 scale presented in the question are as follows: RT: 3.07,  $\sim$ RT: 2.72, VT: 3.06, and  $\sim$ VT: 2.71. Although these means may appear relatively low, this deflation is likely a result of the treatment wording. Respondents were asked to evaluate the responsibility of the Senator and were given no constraints as to which other leader(s) the Senator might be compared to. Without such limitations, respondents likely compared the Senator to President Bush (who is easily the most identifiable person associated with the decision to go to war) and downgraded the Senator's responsibility to a middle-range value, reserving the higher values for President Bush. This design element has been changed in the second version of the survey, which is set to go into the field in the Spring of 2008. In the new version, respondents will be asked to assess the Senator's responsibility relative to other members of Congress.



These effects are mirrored in Table 4.3, which present the estimates from a regression of the two treatments on the degree of responsibility attributed to Senator Harris. Both treatment dummies (Republican and Voted for War) are positive and highly significant, suggesting the presence of true effect.<sup>163</sup> As expected, the strength of these results holds, even in the presence of several controls that would, presumably, affect a respondent's assessment.<sup>164</sup>

**Table 4.3: Treatment Effects**<sup>165</sup>

	Model 1			Model 2		
	Coefficient	S.E.	P-value	Coefficient	S.E.	P-value
<i>Treatment</i>						
Republican	0.335	0.084	0.000	0.300	0.079	0.000
Voted for War	0.333	0.084	0.000	0.322	0.079	0.000
<i>Respondent Variables</i>						
Republican	-	-	-	-0.456	0.089	0.000
Hawk	-	-	-	-0.383	0.096	0.000
Disagree with War	-	-	-	0.294	0.091	0.001
Male	-	-	-	-0.151	0.080	0.059
Age	-	-	-	-0.004	0.002	0.126
Attention to War	-	-	-	0.114	0.131	0.382
Constant	2.559	0.072	0.000	2.893	0.171	0.000
N	810			788		

The inclusion of the controls in Model 2 also reveals several interesting things about how different types of *respondents* behave, holding the effects of the treatments constant. For instance, Republican respondents assign less responsibility, on average, than Democratic respondents just as respondents who are more hawkish assign less responsibility than respondents who identify as doves. Respondents who disagreed with

<sup>163</sup> In results not shown here, the interaction between “Republican” and “Voted for the War” was insignificant, confirming the absence of an interactive effect.

<sup>164</sup> The models presented were estimated using OLS; the results hold under an ordered probit specification. This is true for all models presented in this chapter. Strictly speaking, thanks to the randomization of the treatment assignments, controls are not necessary. The randomization of the treatment ensures that the differences between groups are a result of the treatments, and not any characteristic of a particular group of respondents. Their main purpose here is to serve a robustness check.

<sup>165</sup> T-tests for all treatment variables are one-tailed for all models; all other variables are always two-tailed.

the decision to invade Iraq were more likely to attribute more responsibility than respondents who agreed with or who were neutral on the decision.

Taken together, none of these results is particularly surprising, especially since the three variables are highly correlated: Republicans are both less likely to disagree with the war and more likely to be hawks.<sup>166</sup> If the word “responsible” is read with a negative connotation, and interpreted as “to blame” or “culpable,” then it makes sense that Republicans and hawks would be less likely to ascribe responsibility to Senator Harris while people who disagree with the war, eager to point a finger, would be likely to assign more.

Of the standard demographic controls, males are slightly less likely to assign responsibility to the senator, but this effect is only marginally significant; age appears to have no significant relationship with the dependent variable. Finally, turning to the war attention variable, the complete lack of any statistical relationship is somewhat puzzling, though could perhaps be a result of pooling the responses to the different senator types as attentive people may be more likely to ascribe blame along technical rather than associational lines of responsibility.<sup>167</sup>

Moving on to Hypotheses 3a-c, testing whether one cue dominates over the other can be accomplished with an F-test. The similarity of the size of the effects alone suggests the strength of the cues two are roughly equivalent and a test of the equality of the treatment coefficients demonstrates that the null (“Republican Treatment” = “Voted

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<sup>166</sup> The pair-wise correlation coefficients are all highly significant (Hawk/Republican: 0.2775,  $p < 0.00$ ; Hawk/Disagree with War: -0.335,  $p < 0.00$ ; Republican/Disagree with War: -0.4020,  $p < 0.00$ .)

<sup>167</sup> Interactions between the treatment groups and the respondent’s self-identified level of attention to the war were never significant.

for War Treatment”) cannot be rejected ( $p < 0.848$ ).<sup>168</sup> This supports Hypothesis 3c, which posited that the cues would produce an equal, positive effect in respondents’ assessment of Senator Harris’ responsibility for American involvement in Iraq. In substantive terms, this result means that associational responsibility is *as important* as technical responsibility in the eyes of respondents in assigning blame to leaders.

## 4.2 Comparing the Cues

The finding that the two types of responsibility trigger similar reactions among respondents, however interesting, is only the first step. Establishing the existence of the general treatment effect demonstrates the importance of the larger experiment, but since the treatment groups were pooled for Models 1 and 2, we cannot determine how citizens would respond to certain *combinations* of the two cues. Instead, we only know that the *mean* response of respondents who are cued with technically responsible senators is higher than respondents who are cued with technically non-responsible senators, and similarly with associational responsibility for those cued with Republicans when compared to those cued with Democrats. To determine whether senators who voted for the war are associated with different degrees of responsibility based on their party, (or that Republicans are assigned more responsibility if they voted for the war than Democrats who did the same), requires examining the treatment effects of the original four treatment groups (RYV, RNV, DYV, DNV).

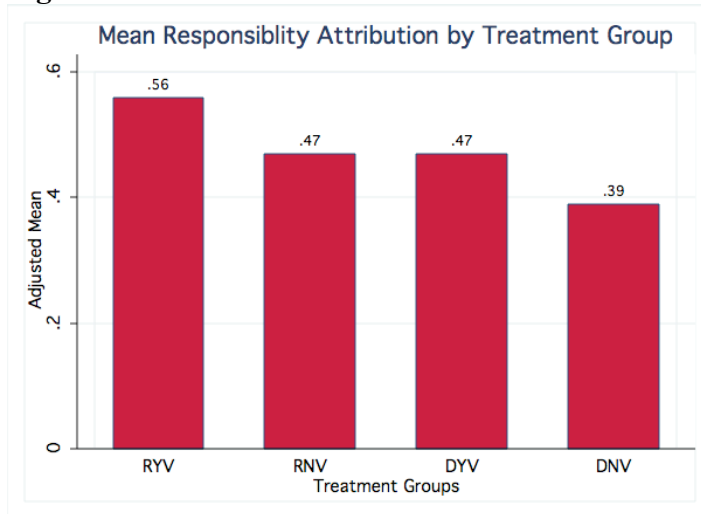
As before, I present the treatment effects graphically (see Figure 4.3) and as coefficient estimates (see Table 4.4). Figure 4.3 presents the (rescaled) mean for each treatment group while Table 4.4 displays the coefficient estimates for the different

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<sup>168</sup> The F-test derives from Model 2. When estimated after Model 1, the same result holds ( $p < 0.990$ ).

treatment groups, where the baseline or excluded category is the “Democrat, Voted No” condition.

**Figure 4.3**



Means have been rescaled to range between 0 and 1.<sup>169</sup>

**Table 4.4: Effects of Cue Combinations**

	Model 3			Model 4		
	Coefficient	S.E.	P-value	Coefficient	S.E.	P-value
<b>Treatment</b>						
Republican, Yes Vote	0.669	0.117	0.000	0.623	0.111	0.000
Republican, No Vote	0.326	0.121	0.003	0.268	0.114	0.009
Democrat, Yes Vote	0.324	0.121	0.002	0.292	0.109	0.004
<b>Respondent Variables</b>						
Republican	-	-	-	-0.457	0.089	0.000
Hawk	-	-	-	-0.386	0.097	0.000
Disagree with War	-	-	-	0.292	0.091	0.001
Male	-	-	-	-0.151	0.080	0.060
Age	-	-	-	-0.004	0.003	0.126
Attention to War	-	-	-	0.114	0.131	0.384
Constant	2.563	0.082	0.000	2.910	0.176	0.000
N	810			788		

<sup>169</sup> The non-rescaled means are as follows: RYV = 3.232, RNV= 2.889, DYV= 2.888 and DNV= 2.563.

Figure 4.3 and Table 4.4, as expected, tell very similar stories about the relative strength of the different combinations of the two responsibility cues. Given the large number of hypotheses, I will review them in the following groups (see Table 4.5).

**Table 4.5: Hypothesis Groups**

Group	Hypothesis	Expectation
Dual-cue Dominance	Hypothesis 4	$R_{YV} > D_{NV}$
	Hypothesis 5	$R_{YV} > R_{NV}$
	Hypothesis 6	$R_{YV} > D_{YV}$
Single-cue Equivalence	Hypothesis 7	$R_{NV} = D_{YV}$
Single-cue vs. No-cue	Hypothesis 8	$R_{NV} > D_{NV}$
	Hypothesis 9	$D_{YV} > D_{NV}$

#### 4.2.1: Dual-cue Dominance

First, Hypotheses 4 through 6, which posited that the RYV treatment would produce the largest effect, each found strong support. Beginning with Hypothesis 4, a simple examination of means (either in Figure 3 or Model 3) demonstrates that the RYV treatment elicits a much higher degree of responsibility attribution (17 percentage points) for Senator Harris than the DNV treatment.<sup>170</sup> In many ways, this result is not at all surprising; recalling Table 4.1, the former treatment invokes both responsibility cues while the second raises none.

What is more intriguing is that Hypotheses 5 and 6, which predicted that the RYV treatment would produce the strongest effect when compared to other treatment groups that cue at least one type of responsibility (RNV for associational and DYV for technical), *also* find strong empirical support. A simple difference of means test demonstrates that the 9-point difference between the RYV treatment and the other two is

<sup>170</sup> The p-values on the “Republican, Yes Vote” treatment in Models 3 and 4 confirm the significant differences between this treatment and the excluded treatment (“Democrat, No Vote”). The difference of means test confirm this ( $p < 0.000$ ).

highly significant. F-tests of joint coefficient equality confirm this finding, regardless of model specification.<sup>171</sup>

Taken together these findings point to a strong additive effect among the cues: respondents appear to assign *different* degrees of responsibility to senators who both voted for the war based *purely* on their partisan affiliations. Indeed, looking at the coefficients on the treatment variables in Model 3 (and, to a slightly lesser extent, Model 4) the effect of being presented with a senator with two responsibility cues (RYV) is almost exactly the sum of the two single-cue treatments (RNV and DYV). While the difference between the RYV treatment and the RNV treatment is somewhat less surprising given the total lack of a technical responsibility cue for the latter group, the RYV and DYV groups differ *only* in terms of associational responsibility. As with the evidence of support for Hypothesis 3c, this finding suggests that the effect of cueing a respondent to a senator's political affiliation should not be underestimated.

#### 4.2.2: Single-cue Equivalence

The power of this cue is equally evident in other comparisons. Hypothesis 7, for instance, posited that the two treatment conditions that only cue one type of responsibility each (either associational or technical responsibility) should produce treatment effects of roughly equivalent sizes. Testing this hypothesis is accomplished with a simple test of whether the coefficients on RNV and DYV are equal. Once again, their equivalence is readily apparent when depicted graphically (Figure 4.3), and the null hypothesis that the

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<sup>171</sup> In Model 3, RYV vs. RNV ( $p < .005$ ) and RYV vs. DYV ( $p < .003$ ). In Model 4, RYV vs. RNV ( $p < .002$ ) and RYV vs. DYV ( $p < .003$ ).

two coefficients are equal cannot be rejected ( $p < 0.995$  in Model 3 and  $p < 0.830$  in Model 4).

The substantive significance of this finding should not be overlooked. That respondents are willing to assign equal amounts of responsibility to a senator whom they were *explicitly told did not vote for the war* and a senator who clearly voted *for* it should be of interest to scholars of political behavior. Although one would expect respondents to “do the best they can with the information they have”<sup>172</sup> and assume the Republican senator voted for the war in the *absence* of a cue as to how he voted, the voting cue is not only present in this case but runs directly counter to the expectation the heuristic would have predicted.

The comparison of the RNV and DNV groups (Hypothesis 8) follows a similar pattern. In this case, the treatments differ in terms of the associational cue—the former has it while the later does not; technical responsibility is absent in both cases. Once again, as Table 4.1 and Hypothesis 8 predicted, the treatment group with the larger number of cues will always produce a stronger treatment effect, even when the level of *technical* responsibility is identical across the two groups (in this case, zero). The positive and significant coefficient on the RNV variables in Models 3 and 4 (and the significant 8-point difference in the rescaled means of the RNV and DNV treatment groups), therefore, lends even more support to the strength of the associational responsibility cue.

#### 4.2.3: *Single-cue vs. No-cue*

The final comparison in this set of hypotheses examines a pair of treatments in which neither senator has associational responsibility, but only one has technical

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<sup>172</sup> Malhotra and Kuo (2007:12).

responsibility (DYV). As expected, the positive and significant coefficient on the DYV variable in Models 3 and 4 suggests a clear difference exists between this treatment effect and that of the excluded group (DNV). Finally, as Figure 4.3 illustrates, the eight-point difference is significant in substantive terms as well, much like the difference between the RNV and DNV treatment groups.

#### *4.2.4: Comparing the Cues Summary*

A comparison of the different combinations of the two responsibility cues confirms the expectations of Tables 4.2 and 4.4 and Hypotheses 4 through 9. The relative ranking of the different treatment effects, as suggested by the hypotheses, is captured by the following inequality.

#### **Expression 4.1:**

$$\text{DNV} < \text{RNV} = \text{DYV} < \text{RYV}$$

First, respondents who are cued with a senator who exhibits at least one type of responsibility will have significantly higher assessments of that senator's role in the Iraq War than respondents who are cued with a senator who has none (Hypotheses 6, 8 and 9). The effect of the DNV treatment, in other words, is dominated by the effect of every other treatment group.

Second, respondents in the two treatment groups that both focus on senators with only one type of responsibility (RNV and DYV) are likely to generate equal assessments of responsibility for the senator. The strong support for this hypothesis is striking since, from a technical responsibility standpoint, the RNV experimental senator is wholly *non*-responsible. Respondents, therefore, are basing their elevated assessment of his



responsibility as compared to the other non-technically responsible senator (DNV) *solely* on the senator's partisan affiliation. If the partisan cue was not important, we would expect the RNV senator to be assessed similarly to the DNV senator and less than the DYV senator, yet this is not the case.

Finally, and perhaps most interestingly, the treatment effect of the RNV group dominates the effect of all other treatments. This suggests the presence of an additive effect since the treatment effect of the group with two cues was always larger than the treatment effect in groups with only one responsibility cue (RNV and DYV). The presence of this additive effect is especially interesting in the DYV/RNV comparison, as it suggests citizens are willing to assign different levels of responsibility to senators who are *both* technically responsible. This result, together with the finding from Hypothesis 4 regarding the RNV>DNV relationship, offer strong support for the idea that partisan connections act as a major conduit for political responsibility in the minds of citizens.

#### **4.3 Respondent Interactive Hypotheses**

The final set of hypotheses examines whether the effect of the different treatments varies across subsets of respondents. In light of the strong effect of the *senator's* partisan affiliation, the primary interactive effect of interest is between the treatments and the *respondent's* self-identified partisan identification.<sup>173</sup> As Hypotheses 10a and 10b suggested, we should expect respondents to assign more responsibility to Senator Harris when the respondent does not share a partisan affiliation with the experimental senator

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<sup>173</sup> Interactions of the treatment group with the respondent's position on the war and hawkish tendencies were also performed. The interactions were never significant.

presented to them. The effect of the treatment on the respondent’s assessment of the Senator, in other words, may depend on the respondent’s own political association.

The presence of an interactive effect can be determined by multiplying each of the treatments by the dummy variable indicating whether the respondent is a Republican or Democrat.<sup>174</sup> As above, Model 5 presents the basic model while Model 6 is estimated with the standard set of controls (see Table 4.6)

**Table 4.6: Interactive Models**

	Model 5			Model 6		
	Coefficient	S.E.	P-Value	Coefficient	S.E.	P-value
<i>Treatments</i>						
Republican, Yes Vote	0.745	0.15	0.000	0.762	0.147	0.000
Republican, No Vote	0.369	0.155	0.018	0.347	0.152	0.011
Democrat, Yes Vote	0.441	0.148	0.003	0.467	0.146	0.000
<i>Respondent Variables</i>						
Republican	-0.513	0.159	0.001	-0.229	0.163	0.161
RYV * Republican	-0.226	0.228	0.323	-0.316	0.224	0.158
RNV * Republican	-0.153	0.236	0.517	-0.165	0.231	0.475
DYV * Republican	-0.312	0.225	0.167	-0.402	0.222	0.070
Hawk	-	-	-	-0.392	0.097	0.000
Disagree with War	-	-	-	0.305	0.091	0.001
Male	-	-	-	-0.15	0.08	0.061
Age	-	-	-	-0.004	0.003	0.138
Attention to War	-	-	-	0.117	0.131	0.374
Constant	2.783	0.108	0.000	2.793	0.189	0.000
N	810			788		

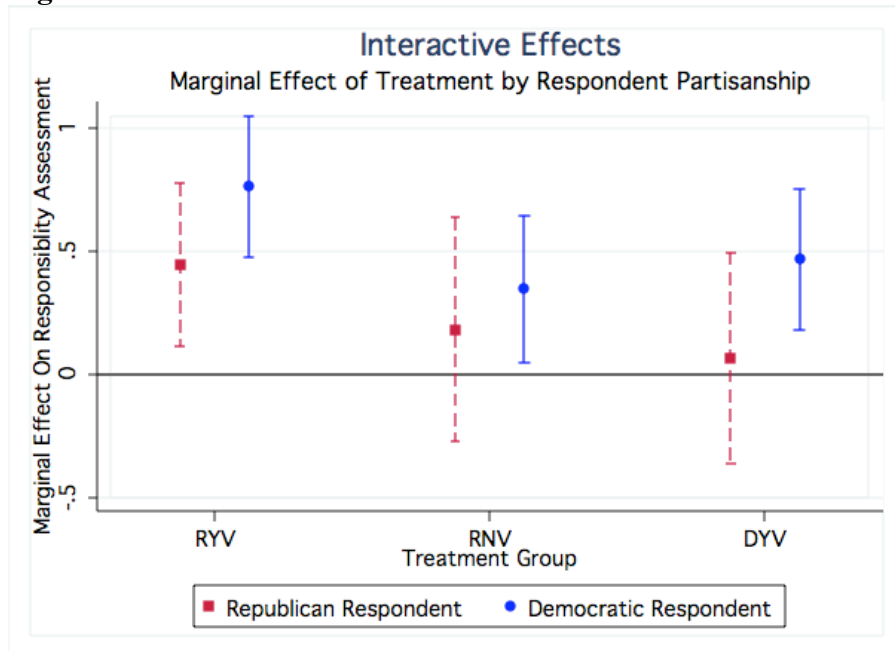
As the significance tests on the interaction terms indicate, a strong interactive effect is not evident. Joint significance tests of the set of interaction terms confirm this (p< 0.561 for Model 5 and p<0.286 for Model 6). Substantively, this suggests that although the treatments still have strong effects on respondents’ evaluations of the

<sup>174</sup> Existing work suggests that all lower order terms, including tacit interactions, need to be included in interactive models (Braumoeller (2004) and Brambor et al. (2005)). Tacit interactions arise when two variables are interacted with the same modifying variable (i.e., A\*C and B \* C). In this case, the tacit interaction would be between A and B. Excluding the A\*B term risks biasing the other coefficients since it constrains the effect of A\*B to 0. This problem may concern researchers in some contexts, but is less of a concern here since the treatment categories are mutually exclusive—respondents can only be in one treatment group. Although tacit interactions *technically* exist in this model (e.g., RYV \*RNY) the fact that at least one of these terms *must* be zero while the other is 1 means it is safe to peg this term’s effect to zero.

experimental senators in general, there is no evidence of systematic differences in these effects between respondents of different political stripes. The effect of the treatment, in other words, is not contingent upon the respondent’s own political identification.

Despite this, Models 5 and 6 can still offer several new insights about how the effects of the treatments vary across respondents of different parties since the interaction terms allow for the isolation of the effect of one portion of the interaction term (and its significance) while holding the other component constant at a set value. Figure 4.4 illustrates this concept. In the Figure, the three treatment group variables included in Models 5 and 6 (RYV, RNV and DYV) are all held (separately) at 1 while the respondent partisan affiliation variable is changed from 1 to 0 (or from Republican to Democrat).<sup>175</sup>

**Figure 4.4**



<sup>175</sup> Graphs of interactions between dichotomous variables (say X and Z) usually only include two lines; one line when X equals 1 and Z equals 0, and another for when X equals 1 and Z equals 1. Figure 4.4 necessitates six lines since there are three different and mutually exclusive dichotomous treatment variables that need be interacted with “Republican respondent”.

Figure 4.4 depicts the marginal effect of the treatments by a respondent's partisan affiliation. As in Models 3 and 4, the effect of any treatment group must be interpreted relative to the excluded treatment, DNV. With this in mind, the first pair of estimated marginal effects demonstrates that both Republican and Democratic respondents assign more responsibility to Senator Harris when they are in the RNV group than when they are in the DNV group. A significant difference exists between the effects of the *treatments* since neither marginal effect's 95% confidence interval includes zero; the lack of an *interactive* effect is evidenced by the fact that the two confidence intervals overlap substantially. The fact the respondents behave in similar ways across party lines is not too surprising considering these two treatment groups represent the extreme combinations of the two responsibility types.

The results become more complex as we move to the second and third set of lines. In both cases, as before, the overlapping confidence intervals suggest the absence of a strong interactive effect. What is interesting, however, is that while Democratic respondents assigned to groups RNV and DYV are willing to attribute (significantly) more responsibility to Senator Harris than Democrats assigned to group DNV, a similar effect does *not* exist among Republican respondents. If Republican respondents saw differences between the RNV senator, the DYV senator and the DNV senator the 95% percent confidence intervals on the RNV and DYV marginal effects would not include zero, yet each does and the marginal effect itself is quite small.

These differences in how respondents of the two parties react to the treatments, however, should be interpreted with caution. The aforementioned F-tests and the fact that all of the confidence intervals in Figure 4.4 overlap suggest that there is little support for

Hypotheses 10a and 10b. Although the important treatment effects across groups in general (as evidenced by Hypotheses 1-9) still hold, no strong evidence supports a claim that the effect of the treatment varies with the *respondent's* political affiliation. Instead, the effects of associational responsibility cues and technical responsibility cues appear to operate independently of the respondent's own political leanings.

## **5.0 Implications**

The analyses presented in this chapter provide strong support for Chapter 3's assumption that citizens see leaders who share a political affiliation with a conflict-initiating leader as more responsible than leaders who lack this affiliation. Although the experimental Democratic senators who voted for the war were seen as more responsible than Democratic senators who voted against it, Republican senators were held more responsible than dissenting Democrats, *regardless* of how they voted. The fact that respondents were willing to assign significantly more responsibility to the Republican who voted against the war based *solely* on his associational responsibility demonstrates the power of the political affiliation cue.

That respondents were also willing to assign more responsibility to a Republican who voted for the war than a Democrat who also voted for the war is equally important in this regard. In this comparison, both senators were equally technically responsible for the war (insofar as a senator could be), yet respondents saw the Republican as having a higher degree of responsibility. Both of these findings lend support to the coding rule employed in the previous chapter. Although technical responsibility certainly matters, the additional bump in responsibility provided by the associational responsibility cue

should not be overlooked or underestimated by scholars of international relations or domestic politics alike.

## Chapter 5

### 1.0 Introduction

March 20, 2008, marked the fifth anniversary of the American invasion of Iraq and the start of a sixth year with more than 125,000 troops deployed in the region. With nearly 4,000 American fatalities over 66% of the public opposing the war and nearly 4,000 American casualties, much of the media coverage in the days leading up to the anniversary centered on the costs of the war.<sup>176</sup> An op-ed piece in *The Washington Post* by economists Linda Blimes and Joseph Stiglitz threw the financial burden of the war into particularly sharp relief. Claiming the war “was not only the second longest in American history (after Vietnam) [but] also the second most costly, surpassed only by World War II,” the authors projected the war would eventually cost the American people a staggering 3 trillion dollars.<sup>177</sup>

Amidst such criticism, George W. Bush delivered a speech at The Pentagon to defend the war that had dominated his agenda for more than half of his time in office. The speech largely focused on the gains made since the 2007 troop surge and the necessity of a continued American military presence, but the president addressed the costs as well. Although Bush began by dismissing recent estimates of the monetary cost as “exaggerated,” he conceded that “no one would argue that this war has not come at a

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<sup>176</sup>“Five Years Later, Bush Says Iraq War Must Go On.” CNN.com. The poll was conducted over the course of three days (March 14-16) in 2008.

<sup>177</sup> Blimes and Stiglitz (2008.)

high cost in lives and treasure.”<sup>178</sup> The President continued, however, with a grim prediction of the consequences of succumbing to these costs—an emboldened terrorist movement with “an even greater determination to dominate the region and harm America” — and emphasized the need to press on. Bush concluded his speech by acknowledging the men and women who had “given their lives to the War on Terror,” and encouraged the public to, “honor them by making sure their sacrifice was not in vain.”<sup>179</sup>

As noted in Chapter 1, the conventional wisdom within the security literature cannot account for Bush’s behavior. Nothing about Bush’s speech would strike a contemporary observer as particularly puzzling or out of the ordinary. That a democratically elected president would deliver a speech to motivate the public to stay in an enormously (and unexpectedly) costly and increasingly unpopular war *five years* after he had initiated, however, it presents a strong challenge to the standard scholarly expectations. Instead of heeding the public’s opposition to the war and ending the war when costs surpassed some threshold, Bush has gone to great lengths to underscore the need to stay, and has made every attempt to connect it to the broader fight against terrorism.

Existing theory also falters for explaining the behavior of politicians other than the chief executive. As presidential hopefuls from both parties jockey for support in anticipation of the November election, an intriguing division has emerged. Although candidates need broader popular support beyond members of the party base to secure the requisite number of Electoral College votes, it seems that only the Democratic

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<sup>178</sup> “President Bush Discusses Global War on Terror” (March 19, 2008.)

<sup>179</sup> Ibid.



candidates, Senators Hillary Clinton and Barak Obama, appear to be trying actively to capitalize a recent poll that suggests that 61% of Americans think the next president should remove most troops from Iraq “within a few months of taking office.”<sup>180</sup>

While the Democratic senators spar over which of them is better suited to lead the country out of the war<sup>181</sup>, the presumed Republican nominee, Senator John McCain, has taken a different tack, asserting that, “[I]t’s not a matter of how long we’re in Iraq, it’s whether we succeed or not.”<sup>182</sup> Despite growing dissatisfaction with the Iraq war, even among Republicans, McCain’s commitment to the war has shown no signs of waning—a strategy few political scientists would predict for a man intent on capturing the nation’s highest office in less than five months.

The behaviors of President Bush and Senator McCain exemplify the two primary questions at the center of this dissertation. Bush’s commitment to the war speaks to the question of why some leaders, especially leaders of democracies, stay in wars than have become unexpectedly costly (or unpopular) when the chances of victory are slim. Likewise, Senator’s McCain’s steadfast support for a continued American presence in Iraq highlights the need for a better understanding of how war informs the actions of leaders beyond the executive.

These questions (and others) grew from a larger, more general question: Why do wars end when they do? In this dissertation I argued that answering this larger question required an understanding of how settlement costs factored into a leader’s war termination calculus. With this goal in mind, this project developed a theory about the

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<sup>180</sup> “Poll: 71 Percent Think Iraq Spending Hurts Economy.” CNN.com. The poll was conducted over the course of three days (March 14-16) in 2008.

<sup>181</sup> “Clinton Says She Is the Only Candidate Who Can End the War.” CNN.com

<sup>182</sup> “McCain Defends ‘100 Years in Iraq’ Statement” CNN.com.

role of a particular type of settlement cost—leader culpability—in international conflict. Chapter 2 specified the conditions under which leaders would feel the pressures associated with culpability and how these pressures might pass from one leader to the next in the same war. Following this, Chapters 3 and 4 explored two sets of observable implications suggested by the theory; the former focused on the effects of leader culpability on war outcome and leader punishment, while the latter explored how individual citizens assign culpability to politicians in and outside the executive office.

The rest of this chapter proceeds in three sections. Section 2 summarizes the goals and findings of the previous chapters. Following this, Section 3 reviews some of the major insights suggested by these findings. Finally, Section 4 concludes by identifying remaining unanswered questions and several next steps for theories of war termination.

## **2.0 Goals of the Study and Findings**

In Chapter 2, I explored how a leader's personal culpability for a war can function as a settlement cost. To illustrate this settlement cost, I compared it to another factor that scholars commonly assume influences the war termination decision: the adversary's war aims. Although both of these factors certainly figure into the choice of continuing to fight or capitulating the enemy, Chapter 2 identified three ways in which the two differ in the minds of leaders.

The first difference is the manner in which the two types of settlement costs affect leaders and citizens. An adversary with high war aims will deter leaders *and* citizens from ending the war before their enemy has been defeated; doing otherwise would almost certainly guarantee an unpleasant post-war status quo. Leader culpability costs are

different in that they affect leaders and citizens *asymmetrically*; they only provide *leaders* with an incentive to stay in the war. As I discussed in Chapter 2 (and will elaborate below), this characteristic creates a significant divergence in the preferences of leaders and citizens even in democratic states and challenges a great deal of the conventional scholarly wisdom.

The second difference in the two types of settlement costs centers on when they arise. The war aims of the adversary will only create an incentive to stay in the war if the aims are expansive<sup>183</sup> Leader culpability costs, on the other hand, will be present in every war, though not necessarily for every leader, regardless the adversary's intentions. The independence of leader culpability costs from the issue at stake compounds the aforementioned preference divergence problem since it creates situations where the leader will want to continue fighting while the citizens will want the war to end.

The third and final difference between the two types of settlement costs is how they change over the course of a war. Although an adversary's war aims will fluctuate slightly, they will rarely change from purely defensive to completely offensive, or vice versa. Leader culpability costs, by contrast, will vary in very predictable ways with changes in leadership, and identifying the means by which they change was the second major goal of Chapter 2.

I identified three ways in which leaders can become culpable in the eyes of their domestic audiences. The first way, which includes the bulk of the culpable leaders, is for a leader to be in charge of the state when the war begins. Although the possibility that leaders of states that were attacked would be granted some leeway by their citizens

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<sup>183</sup> For example, the adversary is intent on conquering a large piece of territory, overthrowing the current regime, or taking over the country completely.

remains an open empirical question, I argued that, generally speaking, first leaders should feel the pressures associated with culpability.

The second and third paths to culpability apply to leaders who take power *during* the conflict. New leaders can inherit the settlement cost from their predecessor if they are from the same ruling group as the first leader, or if they publicly supported the war since its initiation. These two conditions allow citizens to link the new leader to the original decision to go to war, even in minimal-information environments like autocracies, and they will blame him for any resulting hardship. After establishing the conditions for culpability, I turned to a discussion of the nature of wartime leadership changes, the distribution of culpability costs we should observe across the set of warring leaders, and how the leader's regime type mitigates or amplifies the pressures associated with culpability settlement costs.

## **2.1 Empirical Findings—Part 1**

Chapter 2 also presented two sets of hypotheses about the theory's observable implications. The first set analyzed how a leader's culpability (or lack thereof) should affect his or her willingness to accept certain types of settlements. The second set examined how culpability affected the probability a leader would be punished in the event of a loss.

Chapter 3 tested these hypotheses using a research design that offered three distinct improvements over existing techniques. First, I presented a new coding of war outcome that allowed all states (and their leaders) to be included in the analysis simultaneously. Previous research designs required scholars to drop all but one state (or,

in case of multilateral wars, all but one coalition) to avoid problems of statistical dependence. My design solved this problem by coding the war outcome based on the degree to which a leader accomplished the state's original war aims instead of on how the state fared relative to the adversary.

Second, my design used warring *leaders* as the unit of observation instead of warring states. This innovation constitutes a major improvement over current designs and, as the preceding chapters demonstrated, has substantial ramifications for how we interpret existing empirical work. Using warring states as the unit of observation (in both a theoretical and empirical sense) overlooks crucial differences between leaders and lulls scholars into thinking that looking at one leader per war is a safe practice. In actuality, as I discussed in Chapter 3, this is not the case; no matter which leader a researcher chooses to focus on (the first or the last), sample bias will corrupt the findings.

The third improvement centered on how I coded leader punishment. Current work on this topic determines post-war punishment based on whether a leader was removed from power within a certain period of time after the war ended. Doing so, however, implicitly assumes that all leaders removed in this time period were punished *because of the war*. This assumption is unfounded. As Chapter 3 demonstrates, leaders leave power for a number of reasons, some of which are entirely unrelated to their state's fortunes on the battlefield. In light of this, I only coded a leader as "punished" if he or she was removed as a direct result of the war. This new scheme not only allows for a more nuanced representation of political reality but also highlights the remarkable variation that exists in the circumstances surrounding wartime transfers of power.

After assembling a new dataset designed with these improvements in mind, I subjected the hypotheses to a rigorous empirical analysis. I found broad support for all five of my hypotheses. First, with regard to war outcomes, culpable leaders are significantly more likely to preside over favorable outcomes than their non-culpable counterparts. This effect is especially pronounced in the comparison between democratic and autocratic leaders. Although some may suspect this result stems from the fact that *all* of the initiating leaders (and the advantages inherent in being a member of this group) are coded as culpable leaders, the effect of leader culpability remained strong even after controlling for whether a leader actively chose to involve his state in the conflict.

The hypotheses regarding leader punishment met with equal success. Culpable leaders who lost were significantly more likely to be punished than non-culpable leaders who presided over similar outcomes. Indeed, the probability of a non-culpable, losing leader being punished, regardless of regime type, was indistinguishable from that of a winning leader! Moreover, as with earlier hypotheses, this effect was larger for democratic leaders than autocratic ones, a non-surprising finding given the relative ease with which democratic citizens can remove their leaders.

Finally, I also found support for the notion that domestic audiences do not hold targeted leaders to the same standard as leaders who initiated wars or leaders who involved their state in an ongoing conflict. Targeted, losing leaders are significantly less likely to be punished than leaders who chose to involve their state in a war that they eventually lost. Taken together, these findings point to the powerful effect of a leader's personal culpability on the types of outcomes she will accept and her likelihood of

punishment if she fails to secure a settlement that approximates victory. I will explore the implications of these finding further in Section 3.

## **2.2. Empirical Findings—Part 2**

Chapter 4 presented the second set of empirical results, about how individual citizens assign responsibility to different types of leaders. I designed the survey experiment employed in this chapter with an eye to gain insight about how citizens might pass responsibility for a conflict from one leader to the next. By using a fictitious U.S. senator (John Harris) I was able to isolate the effects of cues intended to capture two types of responsibility: technical responsibility for senators who voted for the war, and associational responsibility for senators who share partisan ties with the war's first leader (i.e., Republicans.) Respondents were randomly assigned to one of four treatment groups, asked to read a statement from the Senator and then asked to rate the Senator's responsibility on a scale of 1-5.

The goal of this chapter was to ascertain whether citizens would assign more responsibility to senators when the associational responsibility cue was present. The experiment also allowed me to determine the effect of the technical responsibility cue and how combinations of the two cues might affect the respondent's assessment. The ability to evaluate the cues in isolation and in combination was necessary for two reasons. First, based on the theory presented in Chapter 2, the associational cue *alone* should have an effect on how citizens think about leaders. The second condition for culpable leaders only required that the new leader be from the first leader's ruling group; earlier support for the

war from this leader was not a necessary condition for the culpability to pass on. The experimental design permits the cleanest possible test of this idea.

Second, exploring how the cues work in combination allowed me to test hypotheses about a possible additive effect. That is, a senator associated with both cues would be held more responsible than a senator associated with one cue, who in turn would be held more responsible than senators associated with neither type of responsibility. Although the theory does not suggest the additive relationship explicitly, investigating the possibility allows for a more informed understanding of how leader accountability works (at least in the context of the American presidential system.)

The results strongly confirmed the importance of the associational cue: Respondents assigned significantly more responsibility to the Republican senators than Democratic ones. Evidence of a strong additive effect was also present. Respondents assigned the Republican senator who voted for the war the most responsibility while the Democratic senator who voted against it was assigned the least. Meanwhile, respondents assigned the two senators representing the “divergent cue” combination middling and nearly *identical* degrees of responsibility.

A final set of tests, which explored the possibility of any interactive effects between a respondent’s political leanings and her assessment of the senator did not find strong support. The differences that exist between Republican and Democratic senators were never significant. Overall, however, these results confirmed the expectations established in Chapter 2 and lent credence to the coding rule employed in Chapter 3.

These findings add to our knowledge about how and *why* citizens hold certain leaders accountable. Although the literature has always assumed that citizens will want to



punish leaders associated with poor conflict outcomes, this dissertation examined how the blame for the war might pass from one leader to the next. Which leaders does the public consider “associated”? Answering this question represents a major theoretical and empirical advance. Other scholars have explored how blame for policy outcomes might spread from one leader to other members of his party in the same time period, but not how blame might be *inherited* when the war spans the governments of two different leaders.

### **3.0 New Insights**

The findings from this dissertation offer at least four new insights for studies of war termination. The first, which has implications for all types of countries, is the importance of appreciating that the domestic political scene of a warring state is a dynamic environment. For too long scholars have black-boxed the internal politics by stopping at the warring state as the unit of analysis. Although some have begun to recognize the importance of looking one level deeper at leaders, even these scholars still focus on only one leader per war.<sup>184</sup> Theories associated with these types of analyses limit themselves unnecessarily since they cannot include important leader-specific characteristics, such as culpability; their research design forces them to assume all leaders (at least within the same regime type) will face the same set of incentives.

The second insight, which stems largely from the first, is that we cannot expect a leader’s preferences to always align with his citizens’, even in democracies. Instead, these two actors will have different utility functions with very few terms in common.

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<sup>184</sup> For examples of the former see Goemans, et al. (2006). For examples of the former see Bueno de Mesquita and Siverson (1995), Goemans (2001) and Goemans and Chiozza (2004).

The two utility functions will only move in sync if the leader is non-culpable; otherwise, they will diverge. This insight challenges the conventional wisdom, which commonly overlooks *leader-specific* settlement costs, like culpability. This oversight leads scholars to link (at least implicitly) the utility functions of leaders and citizens and to assume the actors will respond to costs in similar ways. A classic example of this is the wartime bargaining literature where military and civilian casualties are thought to affect a *leader's* willingness (or utility) to continue fighting even though neither cost factors enter the leader's utility function directly.

As I discussed in Chapter 2, the physical costs of prosecuting the war should only affect a leader's willingness to fight under two conditions. The first is if the losses are so massive that they affect the military's ability to continue resisting the enemy. In cases where the "power to hurt"<sup>185</sup> is lost, even the most culpable leader will consider surrender. The second, more common condition is if the leader is not culpable for the state's involvement in the conflict. In cases like this, the leader has no personal reason to stay in the conflict, and every incentive to please her citizens. The absence of a culpability counter-pressure brings her preferences in line with the public's and encourages her to terminate the war.<sup>186</sup>

This pattern should hold even outside democratic systems, although a slightly different causal mechanism is at work. Even though a non-culpable autocratic leader is concerned with a much smaller winning coalition (whose members are likely insulated from the physical costs of war) there is little reason for her to spend resources on a

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<sup>185</sup> Slantchev (2003a.)

<sup>186</sup> This notion is supported by the evidence presented in Chapter 3, but is not tested explicitly. I will explore this in future research with case studies, including careful analyses of the public statements issued by non-culpable leaders regarding when and why the war should end.

conflict she will likely lose. Her motivation for ending the conflict, then, stems not from a desire to appease the masses but rather from a desire to preserve the largest possible pool of resources for the agenda of her new regime.

The theory presented here also offers a new perspective on why and in which direction leaders will choose to revise their war aims. Scholars often assume leaders expand or lower their war aims based on the probability of victory, which they update over the course of the war in response to losses and gains on the battlefield. The leader culpability theory suggests this is not the case. Instead, leaders will revise their war aims in predictable ways based on their own personal role in the conflict.<sup>187</sup> Culpable leaders will have very little reason to scale their war aims back. Even though doing so may make ending the war easier from bargaining standpoint, the public will still want to punish the leader for engaging the state in a war it could not win. The leader, therefore, stands to gain nothing by lowering the war aims since his probability of punishment will not decrease.

Culpable leaders will, however, have incentives to revise their war aims *up* since doing so may increase the salience of the conflict in the eyes of his citizens and encourage them to stay in the war (and to allow the leader to stay in power). These upward revisions may coincide with an increased probability of victory, but they can also be triggered by losses. As the state's prospects in the war become more desperate, the

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<sup>187</sup> As with the previous insight, although the evidence presented here supports this notion, more explicit investigation of this idea is better left to case work since quantifying the change in war aims during wars on a large scale would be difficult.

culpable leader will (perhaps in vain) try to gamble for resurrection in an attempt to reduce his probability of punishment.<sup>188</sup>

Non-culpable leaders, importantly, will not share this incentive. Instead, these leaders will only want to revise the war aims down, regardless of how the state is faring on the battlefield or the eventual probability of victory.<sup>189</sup> These leaders stand to gain very little by staying in the conflict and their likelihood of being punished for backing down is minimal. Consequently, they will try to end the war as soon as possible upon coming to power.

The fourth insight centers on the relationship between a democratic leader and his citizens. The existing literature (reflecting the aforementioned tendency to meld the utility functions of leaders and citizens) often assumes that a “declining advantage of democracy” exists, where democratic leaders will grow more willing to end wars as they get longer and more costly for their citizens. The presumed causal mechanism behind this is the electoral check: a democratic leader will end a costly war to appease his public in return for the public allowing him to stay in office.

The theory presented here suggests that this causal story has been applied to democratic leaders too broadly. All democratic leaders will be *interested* in public opinion, but we should only expect non-culpable leaders to *respond* to it. Culpable, democratic leaders, by contrast, will often go directly *against* the wishes of their citizens

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<sup>188</sup> Culpable leaders are also likely to engage in risky military strategies (e.g., massive influxes of troops, bold strikes into enemy territory, etc.) for similar reasons.

<sup>189</sup> In theory, a non-culpable leader may choose to stay in the war if the state has an overwhelming chance of victory, but this scenario is highly unlikely to occur in reality. If a state has a high probability of vanquishing its opponent, the win is likely to come quickly, and under the tenure of a culpable leader. Non-culpable leaders, on the other hand, will often (though not always) come to power when the state has either been in the war or when the state is faring poorly.

and stay in a war longer, even when losses are high.<sup>190</sup> Existing theory, therefore, is only half right. The electoral check does not motivate a leader to bow to the public's wishes; instead it allows citizens to replace the leader refuses to do so with a leader who will. This insight suggests that anti-war protests are likely to fall on deaf ears when a culpable leader is in power. Protests are only likely to be effective when they either coincide with a leadership change that elevates a non-culpable leader (who will be more receptive to their demands) *or* if they are strong enough to bring such a leadership change about.

#### **4.0 Next Steps**

This dissertation offered several new contributions, but still has potential for further development. This is especially true in the realm of case studies where more in-depth knowledge of particular cases can allow for more refined testing of some hypotheses. I briefly focus on two such opportunities here.

The first centers on the reaction of an actor who has not featured prominently in the dissertation thus far: the leader of the adversary. Although earlier sections discussed the importance of the adversary's war aims, the adversary leader's reaction to the culpability (or lack thereof) of the opposing leader is an intriguing question for future research. Wars will always start out with two culpable leaders but, as this dissertation demonstrated, war is often an engine of domestic political change that can bring new leaders into office. Such changes would presumably be of great interest to leaders of other combatants. How does the adversary's leader respond when a non-culpable leader takes over the chief executive position in the other state? Does she capitalize on the

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<sup>190</sup> Although instances of this should be rare since the *anticipation* of public dissent provides democratic leaders with strong incentives to avoid selecting into wars that will end in a loss they will be held culpable for, democratic leaders do not always choose wars wisely.

opportunity to end the war quickly by offering reduced terms she knows her opponent is likely to accept? Or does she drive for a harsher settlement since she knows her opponent will likely lack the political will to resist? The answer likely depends on how well equipped the adversary is—in both political and military terms—to continue fighting.

In the Korean War, for instance, the Communists appeared to be keenly aware of the change from Truman to Eisenhower in the United States. Indeed Eisenhower had not even been in office for a full year before the North Koreans and Chinese produced a new settlement for his consideration. Although the terms were very similar to those offered to Truman a few months earlier, Eisenhower accepted them and came home to a warm hero's reception.

Is this an example of the Communists capitalizing on Eisenhower's lack of culpability? Truman had withdrawn from the 1952 presidential election before the nominating process had even begun, but we can safely assume he knew that Democratic candidate would face an uphill battle because of his handling of the conflict. Eisenhower won the election handily over Adlai Stevenson, and control of the White House (and, more importantly for the Communists, control of US foreign policy) passed from a culpable Democrat to a non-culpable Republican. Was the timing of the new terms intentional to provide Eisenhower with terms he could accept more easily than Truman could have? Case work on this war, and others like it, may uncover interesting insights about how leaders respond to leadership changes that bring non-culpable adversaries to power.

The second potential avenue of exploration centers on other types of settlement costs that exist. The two settlement costs discussed in this dissertation—adversary war

aims and leader culpability costs—are certainly important, but they are surely not the only factors that might inspire a leader to stay in a war. Leaders might also be deterred from settling without a win if the adversary is long-term rival or if the conflict is part of an ongoing, larger struggle. The war in Vietnam from the American point of view exemplifies this type of concern. Although Nixon ran on a platform of “Peace With Honor” in 1968 and was otherwise inculpable for entangling US forces in Southeast Asia, large numbers of US troops remained involved in the conflict until 1973.

Why did Nixon stay in so long? One answer might be the Cold War. Even though Americans and Soviets never engaged each other in combat directly, several “proxy conflicts” erupted in the four decades following World War II that many observers saw as part of a larger battle between the Communist East and the Capitalist West. Vietnam was one such conflict as was Korea. In conflicts like these, I would argue, *all* leaders, regardless of their personal culpability for the conflict would feel pressure to secure favorable terms since the conflict was seen as part of a larger whole. For American citizens, the loss of South Vietnam was not important in and of itself—what was important was the allowing the larger threat, the Soviet Union, to gain the geopolitical advantage and to spread its ideology. Because of this, any American president who served during the Cold War would have felt strong pressures to avoid looking “soft on Communism” even if it meant staying in a costly war he did not start.<sup>191</sup>

A similar story could also be told for a non-culpable leader fighting a state that has historically been a frequent adversary (e.g., the long-standing rivalry between India and Pakistan based on numerous disagreements over territory.) In these cases, the lack of

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<sup>191</sup> This notion is often referred to as the “Domino Theory” wherein the loss of one country to Communism would trigger a chain reaction within the region, bringing more states under the Soviet influence.

culpability for the leader is less relevant. Because of the high likelihood of fighting the same state at some point in the near future, citizens will be concerned about *any* leader's reputation for resolve, even if he was not culpable for starting the conflict.

I hope to explore these ideas and more in future research with case studies to gain a better understanding of the intricacies behind the broader causal mechanisms I have established here.



## **APPENDICES**

## Appendix A

This Appendix presents the marginal effects from the equations presented in Chapter 3. These tables were calculated using the SPost Stata ado file, which allowed me to move the independent variable of interest while holding the other variables at their medians (in the case of continuous variables) or modes (in the case of dichotomous variables) within the sample of interest to approximate a “representative” case.<sup>192</sup>

**Table A1:**  
Marginal Effects for Table 3.5: War Outcomes and Culpability  
Moving from Non-Culpable to Culpable Leaders

War Outcome	Non-Culpable Leaders	Culpable Leaders	Change in Probability of Outcome	95% Confidence Interval for Change in Probability
-3	0.1286	0.0591	-0.0695	-0.1479 to 0.0088
-2	0.0515	0.0264	-0.0251	-0.0514 to 0.0012
-1	0.3224	0.2151	-0.1073	-0.2118 to -0.0029
0	0.1492	0.1427	-0.0065	-0.0440 to 0.0309
1	0.0728	0.0848	0.0120	-0.0140 to 0.0381
2	0.2755	0.4720	0.1965	0.0398 to 0.3532

These values were calculated using the coefficients from Model 1.

**Table A2a:**  
Marginal Effects for Table 3.6: Democracy, Culpability and War Outcome  
Moving from Non-Culpable Democratic Leader to Culpable Democratic Leader

War Outcome	Non-Culpable Leaders	Culpable Leaders	Change in Probability of Outcome	95% Confidence Interval for Change in Probability
-3	0.0979	0.0262	-0.0717	-0.1576 to 0.0141
-2	0.0410	0.0122	-0.0288	-0.0620 to 0.0045
-1	0.2871	0.1168	-0.1703	-0.3522 to 0.0115
0	0.1539	0.0994	-0.0544	-0.1089 to 0.0000
1	0.0805	0.0702	-0.0102	-0.0501 to 0.0296
2	0.3397	0.6752	0.3355	0.0498 to 0.6213

These values were calculated using the coefficients from Model 2.

<sup>192</sup> Long and Freese (2005).

**Table A2b:**

Marginal Effects for Table 3.6: Democracy, Culpability and War Outcome  
 Moving from Non-Culpable Autocratic Leader to Culpable Autocratic Leader

War Outcome	Non-Culpable Leaders	Culpable Leaders	Change in Probability of Outcome	95% Confidence Interval for Change in Probability
-3	0.0972	0.0601	-0.0371	-0.1013 to 0.0272
-2	0.0407	0.0267	-0.0140	-0.0367 to 0.0087
-1	0.2861	0.2175	-0.0686	-0.1732 to 0.0360
0	0.1539	0.1443	-0.0096	-0.0333 to 0.0141
1	0.0806	0.0854	0.0048	-0.0090 to 0.0186
2	0.3415	0.4660	0.1245	-0.0586 to 0.3076

These values were calculated using the coefficients from Model 2.

**Table A3:**

Marginal Effects for Table 73.9 Culpability and Domestic Punishment  
 Moving from Winners to Different Types of Losers<sup>193</sup>

Leader Type	Baseline Pr(Punish)	New Pr(Punishment)	Change	Magnitude of Change	95% Confidence Interval for Change in Probability
Non-Culpable Loser	0.0350	0.0446	0.0095	0.27	-0.0427 to 0.0618
Culpable Loser	0.0350	0.1632	0.1282	3.66	0.0420 to 0.2144

These values were calculated using the coefficients from Model 8.

<sup>193</sup> Holding the adversary's war aims at "high" instead of "low" does not affect the significance of these changes.

**Table A4a:**

Marginal Effects for Table 3.10: Culpability, Regime Type and Domestic Punishment  
Moving from Winners to Different Types of Losers (Adversary War Aims are Low)

Leader Type	Baseline Probability of Punishment	New Probability of Punishment	Change	Magnitude of Change	95% Confidence Interval for Change in Probability
Non-Culpable, Autocratic Leader	0.0350	0.0444	0.0094	0.27	-0.0535 to 0.0724
Culpable, Autocratic Leader	0.0350	0.1536	0.1186	3.39	0.0370 to 0.2001
Non-Culpable, Democratic Leader	0.0377	0.0491	0.0115	0.31	-0.0795 to 0.1025
Culpable, Democratic Leader	0.0377	0.3824	0.3447	9.14	-0.0709 to 0.7603

**Table A4b:**

Marginal Effects for Table 3.10: Culpability, Regime Type and Domestic Punishment  
Moving from Winners to Different Types of Losers (Adversary War Aims are High)

Leader Type	Baseline Probability of Punishment	New Probability of Punishment	Change	Magnitude of Change	95% Confidence Interval for Change in Probability
Non-Culpable, Autocratic Leader	0.0564	0.0712	0.0148	0.26	-0.0815 to 0.1110
Culpable, Autocratic Leader	0.0564	0.2301	0.1737	3.08	0.0760 to 0.2715
Non-Culpable, Democratic Leader	0.0606	0.0785	0.0179	0.30	-0.1256 to 0.1614
Culpable, Democratic Leader	0.0606	0.5049	0.4444	7.33	0.0274 to 0.8614

Values for Tables A4a and A4b were both calculated using coefficients from Model 9.

**Table A5:**

Marginal Effects for Table 12: Voluntary Participation and Culpability Costs  
Moving from First Leaders Who Are Attacked (Targets) to First Leaders Who Chose to  
Fight (Initiators)

Leader Type	Probability of Punishment for Targets	Probability of Punishment for Initiators	Change	Magnitude of Change	95% Confidence Interval for Change in Probability
Autocratic Leaders	0.1842	0.4583	0.2741	1.49	-0.0282 to 0.5764
Democratic Leaders	0.3415	0.6603	0.3188	0.93	0.0022 to 0.6353

These values were calculated using the coefficients from Model 11.

## Appendix B

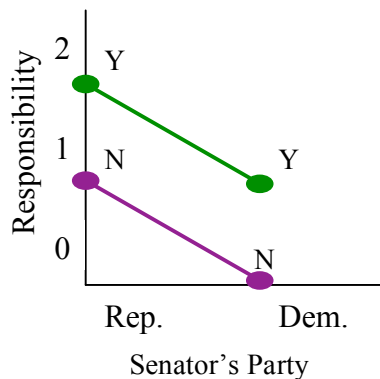
The following two figures illustrate the additive nature of the second set of hypotheses (and the interactive nature of the third) in Chapter 4. First, it is helpful to return to the cue combinations.

**Table B.1: Cue Combinations**

Treatment	Associational Responsibility	Technical Responsibility	Additive Cue Total
1. Republican, voted for the war	1	1	2
2. Republican, voted against the war	1	0	1
3. Democrat, voted for the war	0	1	1
4. Democrat, voted against the war	0	0	0

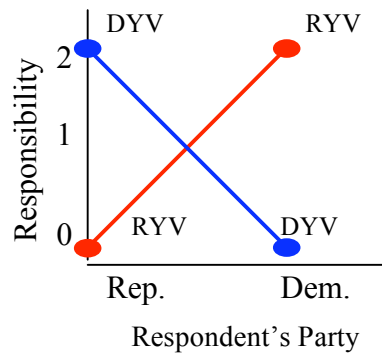
Figure B.1 plots the additive cue totals based on the Senator's party; the green and purple lines connect the points of Senators who voted the same way (yes and no, respectively). As the additive hypotheses would predict, the lines are parallel; the technical responsibility cue raises the overall responsibility assessment of both Senators by equal amounts, regardless of their party membership.

**Figure B.1 Additive Effect**



An interactive relationship, which applies to the hypotheses in the third set of Chapter 4, would produce a very different plot pattern. In Figure A4.1.2, I plot the responsibility assigned to two Senators who both voted for the war, based on the *respondent's* party membership. The blue line represents a Democratic senator who voted for the war and the red line represents a Republican who voted the same way.<sup>194</sup>

**Figure B.2 Interactive Effect**



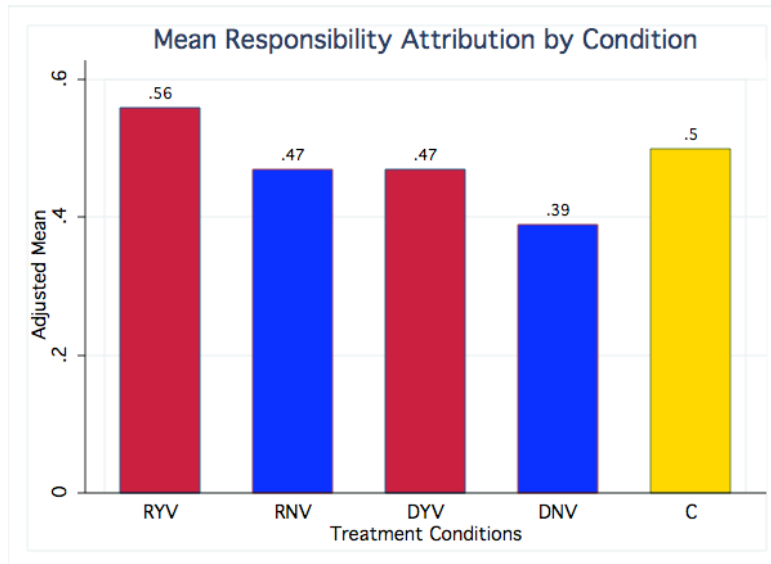
In this figure the lines are not parallel because the treatments affect Democratic and Republican respondents differently. Let us assume, for the moment, that respondents interpret “responsible” using a negative connotation and equate it with “blame”. If this is the case, Republican respondents, in an effort to punish Democratic leaders while protecting Republican ones, will assign more responsibility to the Democratic leader and less to the Republican. The same pattern emerges in the case of a Democratic respondent.

<sup>194</sup> I selected these two treatments for illustrative purposes only. The treatments involving Senators who voted against it would produce an analogous graph.

## Appendix C

Figure C.1 and Table C.1 are identical to Figure 4.3 and Table 4.4 in Chapter 4 except the control group is now included. The control treatment is identical to the other treatments except for the exclusion of any information about the Senator's party membership or previous vote on the Iraq War. In Table C.1, the control treatment is the excluded category.

**Figure C.1: Mean Responsibility Attribution by Condition With Control**



**Table C.1: Effects of Cue Combinations**

<i>Treatments</i>	Model 1			Model 2		
	Coefficient	S.E.	P-value	Coefficient	S.E.	P-value
RYV	0.216	0.122	0.039	0.167	0.114	0.073
RNV	-0.127	0.125	0.155	-0.184	0.118	0.059
DYV	-0.128	0.120	0.143	-0.160	0.113	0.079
DNV	-0.453	0.121	0.000	-0.457	0.113	0.000
<i>Respondent Variables</i>						
Republican				-0.427	0.081	0.000
Hawk				-0.343	0.087	0.000
Disagree With Iraq War				0.345	0.082	0.000
Male				-0.122	0.072	0.089
Age				-0.005	0.002	0.021
Attention Paid to the War				0.143	0.114	0.211
Constant	3.017	0.089	0.000	3.343	0.162	0.000
	N= 987			N= 965		



As discussed briefly in Chapter 4, respondents in the control group behave as expected. In both models, respondents assigned to the control treatment assign significantly less responsibility to their senator than respondents in the RYV condition, and significantly more than respondents in the DNV condition. There is no significant difference between the treatments with divergent cues (RNV and DYV) and the control group in Model 1, while in Model 2 the divergent cue treatments trigger slightly smaller assessments of responsibility than the control condition.

Although this treatment constitutes a true control since it does not cue a respondent to either of the two types of responsibility, it is difficult to interpret any results that include the control since the two treatment conditions do not have a “neutral” category. A Senator is either a Republican or Democrat and either voted for the war or voted against it. Although “Independent” and “Abstained” may seem to be suitably neutral for use in a control treatment, in practice both carry non-neutral connotations. This necessitates the use of the current control treatment wording.

Overall, the results are not surprising, but given that respondents in the control treatment are provided with *no* information on two central characteristics of the Senator that respondents in the other treatments are given, we should not read too much into any comparisons between the groups. This is especially true for comparisons between the RNV, DYV and control groups. Members of all three treatment groups may be confused about how to respond, but they will be confused for different reasons. Members of the former two treatment groups, for instance, may be confused since the Senator’s vote clashes with their expectation of his position based on his party membership, while members of the latter group may struggle with the question because they have no

information on which to base their answer. This raises the possibility that respondents in the RNV and DYV groups are assigning a middling level of responsibility since the two cues “cancel” one another out, while respondents in the control group are choosing the middle category since it represents the “best guess” in the lack of any other information.

## Appendix D

**Table D.1: Multinomial Logistic Regression Predicting Treatment Group Assignment**

Treatment: RYV	Coefficient	S.E.	P-value
Republican	-0.17	0.22	0.46
Hawk	-0.12	0.24	0.62
Disagrees with Iraq War	0.14	0.23	0.54
Male	0.23	0.20	0.25
Age	0.01	0.01	0.34
Attention to War	0.02	0.34	0.96
Constant	-0.38	0.42	0.37
Treatment: RNY			
Republican	-0.25	0.23	0.28
Hawk	-0.33	0.25	0.18
Disagrees with Iraq War	-0.18	0.24	0.45
Male	0.11	0.21	0.61
Age	0.00	0.01	0.81
Attention to War	0.07	0.34	0.84
Constant	-0.04	0.42	0.93
Treatment: DYV			
Republican	-0.27	0.22	0.22
Hawk	-0.39	0.24	0.10
Disagrees with Iraq War	-0.25	0.23	0.28
Male	0.24	0.20	0.24
Age	0.00	0.01	0.60
Attention to War	-0.08	0.32	0.80
Constant	0.16	0.40	0.69
Excluded Category: DNV			
N=799			
Log Likelihood: -1100.47			
LR Statistic (~): 11.58			
Prob. > $\chi^2$ : 0.8683			

Following the example set by Tomz (2007), I use major respondent characteristics as predictors of treatment group assignment. If the randomization worked, none of the respondent characteristics should be significant predictors of the respondent's group (e.g., hawkish respondents should not be more likely to be assigned to Treatment RNV than Treatment DNV). None of the coefficients' p-values approach .05, so we can be confident that the randomization process was successful and that treatment effects are the result of the treatments themselves and not a result of the attributes of the respondents in

the groups. The likelihood ratio test confirms this. If all of the coefficients were 0, we would observe the Chi-square test statistic with a probability of 0.8683. Consequently, we cannot reject the null hypothesis that all of the coefficients are 0.

## Appendix E

### Treatment Wording

Treatment RYV: “Senator John Harris, a Republican who is a senior member of the Foreign Relations Committee and who voted for the war when it began in 2003, has just released the following statement: “Despite the fine efforts of our armed forces, careful analysis of the current situation in Iraq has led me to conclude that our continued presence is unlikely to achieve the goals set in 2003 and will likely make an already bad situation even worse. Given this assessment, it is my opinion that all US forces should be withdrawn from Iraq over the next three months.”

Treatment RNV: “Senator John Harris, a Republican who is a senior member of the Foreign Relations Committee and who voted against the war when it began in 2003, has just released the following statement: “Despite the fine efforts of our armed forces, careful analysis of the current situation in Iraq has led me to conclude that our continued presence is unlikely to achieve the goals set in 2003 and will likely make an already bad situation even worse. Given this assessment, it is my opinion that all US forces should be withdrawn from Iraq over the next three months.”

Treatment DYV: “Senator John Harris, a Democrat who is a senior member of the Foreign Relations Committee and who voted for the war when it began in 2003, has just released the following statement: “Despite the fine efforts of our armed forces, careful analysis of the current situation in Iraq has led me to conclude that our continued presence is unlikely to achieve the goals set in 2003 and will likely make an already bad situation even worse. Given this assessment, it is my opinion that all US forces should be withdrawn from Iraq over the next three months.”

Treatment DNV: “Senator John Harris, a Democrat who is a senior member of the Foreign Relations Committee and who voted against the war when it began in 2003, has just released the following statement: “Despite the fine efforts of our armed forces, careful analysis of the current situation in Iraq has led me to conclude that our continued presence is unlikely to achieve the goals set in 2003 and will likely make an already bad situation even worse. Given this assessment, it is my opinion that all US forces should be withdrawn from Iraq over the next three months.”

Control Treatment: “Senator John Harris, a senior member of the Foreign Relations Committee, has just released the following statement: “Despite the fine efforts of our armed forces, careful analysis of the current situation in Iraq has led me to conclude that our continued presence is unlikely to achieve the goals set in 2003 and will likely make an already bad situation even worse. Given this assessment, it is my opinion that all US forces should be withdrawn from Iraq over the next three months.”

Question Wording<sup>195</sup>

1. Based on the proposal you just read, how do you feel about the way Senator Harris is handling the current war in Iraq?
  - A. Strongly approve
  - B. Approve
  - C. Neither approve nor disapprove
  - D. Disapprove
  - E. Strongly disapprove
  - F. Don't know
  
2. Think about Senator Harris and the proposal you just read. In your opinion, does the phrase 'He provides strong leadership' describe Senator Harris extremely well, quite well, not too well, or not well at all?
  - A. Extremely well
  - B. Quite well
  - C. Not too well
  - D. Not well at all
  - E. Don't know
  
3. On a scale of 1 to 5, with 1 meaning "not at all responsible" and 5 meaning "very responsible" where would you place Senator Harris in terms of responsibility for American involvement in Iraq as a whole?
  - A. 1
  - B. 2
  - C. 3
  - D. 4
  - E. 5
  - F. Don't know
  
4. How much attention have you been paying to what is going on in Iraq?
  - A. A good deal
  - B. Some
  - C. Not much
  - D. Don't know
  - E. Refused
  
5. Do you approve or disapprove of the decision to use military force against Iraq?
  - A. Strongly approve
  - B. Approve
  - C. Neither approve nor disapprove

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<sup>195</sup> Question wording for all questions (except for dependent variable questions 1 and 3 and background question 2) is based on questions taken from the American National Election Study. Background question 1 is based on a similar question asked in the 1970s regarding American involvement in Vietnam.

- D. Disapprove
- E. Strongly disapprove

6. Generally speaking, do you think of yourself as a Republican, a Democrat, an Independent, or what?

- A. Republican
- B. Democrat
- C. Independent
- D. Other
- E. Don't know

7.

[If Respondent considers self a Republican]

Would you call yourself a strong Republican or a not very strong Republican?

- A. Strong Republican
- B. Not very strong Republican

[If Respondent considers self a Democrat]

Would you call yourself a strong Democrat or not a very strong Democrat?

- A. Strong Democrat
- B. Not very strong Democrat

[If Respondent's party preference is Independent, Other or Don't Know]

Do you consider yourself closer to the Republican Party or the Democratic Party?

- A. Closer to Republican Party
- B. Closer to Democratic Party

8. Some people believe the United States should solve international problems by using diplomacy and other forms of international pressure and use military force only if absolutely necessary. Suppose we put such people at 1 on this scale. Others believe diplomacy and pressure often fail and the US must be ready to use military force. Suppose we put them at number 7. And of course others fall in positions in-between, at points 2, 3, 4, 5, and 6.

Where would you place yourself on this scale, or haven't you thought much about this?

- A. 1
- B. 2
- C. 3
- D. 4
- E. 5
- F. 6
- G. 7
- H. Don't know, haven't thought much about it
- I. Refused

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