IN IT TO WIN IT: DOMESTIC POLITICS AND LONG TERM RESOLVE
IN INTERNATIONAL CRISES

A Dissertation in
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by
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Abstract

Extant theories of the effect of domestic politics on international conflict hold that democracies are transparent and very selective in choosing when to go to war. If these theories are correct, it is difficult to explain why wars involving democracies occur at all as well as why democracies sometimes find themselves fighting prolonged, politically divisive wars. To explain these puzzles, I argue that we need to move past static conceptions of accountability (which is assumed to be constant within and across democracies) and resolve (which is often seen simply as the willingness to fight or not). I develop two game-theoretic models illustrating how domestic political processes within the state (such as party competition, the popularity of the leader, and the electoral cycle) can produce variation in the degree to which leaders expect to be held accountable for foreign policy outcomes, and the implications such variation has for the level of resolve the leader will exhibit in international crises. I derive several novel predictions regarding the duration of interstate conflict, the timing of low-level disputes, the outcomes of conflicts, and the domestic political consequences of international outcomes. Statistical analyses provide at least qualified support for nearly all of the theoretical expectations. Several illustrative cases for each theoretical model are discussed. I conclude that scholars of international relations would do well to view accountability as the outcome of dynamic political processes taking place within the state, rather than characteristics of institutional arrangements.
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Chapter 1

Introduction

“And the question in my mind is how many additional American casualties is Saddam worth? And the answer is not very damned many. So I think we got it right, both when we decided to expel him from Kuwait, but also when the president made the decision that we’d achieved our objectives and we were not going to go get bogged down in the problems of trying to take over and govern Iraq.” (SPI, 9/29/2004)

Early in 1991, a year before President George H.W. Bush would face reelection, the United States waged war with Iraq. Early in 2003, a year before a different President Bush would face reelection, the United States again went to war with Iraq. Yet these two wars played out quite differently, particularly in that George H.W. Bush ignored calls from inside his administration to invade Iraq. In August of 1992, then Secretary of Defense Dick Cheney defended this decision (see above quote). Yet as Vice President of the current administration, Dick Cheney appears to have taken a different view. In a March 2008 interview, Cheney was reminded that roughly two-thirds of Americans do not believe the war is worth fighting. “So?” Cheney asked. His interviewer replied, “So? You don’t care what the American people think?” “No,” Cheney said, going on to say that “fluctuations in opinion polls” should not alter government policy (NYT, 3/19/2008).

How did these two presidents arrive at different conclusions about invading Iraq? Why
have the very same actors who praised the restraint of the former defended the decision of the latter? The answer lies, at least in part, in the differing domestic political situations facing them, particularly with regard to the behavior of the Democrats in Congress.

In recent years, scholars of international relations have increasingly emphasized the importance of domestic politics in explaining international outcomes. The current wave of interest in domestic politics stems from the well-known finding that democratic states are less likely to fight wars with each other than are other pairings of states (Russett & Oneal 2001). One explanation for this pattern is that leaders of democratic states, having greater reason to fear removal from office following policy failure, are more careful in choosing which wars they deem worth fighting (Reiter & Stam 2002, Bueno de Mesquita, Morrow, Siverson & Smith 1999, Bueno de Mesquita, Smith, Siverson & Morrow 2003, Schultz 2001). There appears to be evidence that democracies do behave this way, as democratic states also win the wars that they fight more often (Reiter & Stam 2002), and their wars often do not last as long as wars initiated by non-democracies (Bennett & Stam 1996). However, there are considerable problems with this argument. In light of other important findings in the literature, the standard argument about how democratic institutions induce different behavior in regards to international conflict fails to offer a satisfactory explanation for two empirical patterns. In short, if we accept existing explanations of the democratic peace, we are left with two puzzles. First, why do we observe wars involving democracies at all? While advocates of the democratic peace have only focused on a dyadic relationship (peace amongst pairs of democracies), the logic used to explain this pattern leaves little room for wars involving democracies, regardless of the other participant’s regime type. Second, why do democracies sometimes find themselves fighting unpopular prolonged conflicts with little hope for victory in the short term? Such behavior, typified by the blatant disregard for public opinion apparent in Vice President Cheney’s recent remarks, is inconsistent with the high level of responsiveness to public demands assumed so widely in the literature. Were such
behaviors unique to this administration, perhaps we could retain our confidence in extant theories while simply taking care to note that any general pattern may have the occasional counterexample. But U.S. experiences in the wars in Korea and Vietnam suggest that it is not uncommon for American presidents to “stay the course” in the face of opposition, even despite little prospect for the sort of quick and cheap victory democracies are allegedly so good at producing. In fact, in the modern era, the quick victory without any loss of popularity or significant legislative opposition observed in the 1991 Persian Gulf War appears to be more atypical than the tragic characteristics of the 2003 Iraq War. Nor is stubbornly pressing on with a unsuccessful and divisive war a uniquely American pattern, as demonstrated by Israel’s experiences in the 1969 War of Attrition.

In order to address these two puzzles, we must unpack the concepts of democracy and war. Arguments that focus on regime type to explain why certain pairs of states are more or less likely to fight wars with one another than are other pairs of states often fail to explicitly identify what it is about one regime type as opposed to another that produces the behavior to be explained. In other words, if we are to claim to explain international outcomes through focusing on domestic politics, we must focus our attention on political goings-on within the state rather than making assumptions about fixed, unchanging characteristics supposed to inhere in broad, simplistic categories of regime types. The latter approach is incapable of speaking to such puzzles as the one with which this chapter opened - the United States’ regime type did not change from 1991 to 2003. As I will discuss in the next chapter, a wealth of evidence suggests that accountability may not be a fixed feature of democratic systems, but may be better viewed as an outcome, one that may (or may not) occur as a result of strategic behavior by actors within the system. If the degree to which democratic leaders are held accountable for their actions varies over time and across states, as it appears to, then scholars seeking to identify domestic sources of international outcomes will benefit from focusing directly on the presence or absence of such factors within the state rather than
treated accountability as a constant.

It is equally important to recognize that wars are not static events. Though scholars of international relations have long been citing Clausewitz’ famed observation that war is the continuation of politics by other means (Clausewitz 1976), only recently have scholars begun to take seriously the notion that war is a dynamic political process (Smith 1998, Wagner 2000, Slantchev 2003a, Slantchev 2003b, Powell 2004a, Powell 2004b, Slantchev & Levontoglu 2007). Statistical tests of the relationship between regime type and war have primarily focused on whether wars involving two democracies occur or not (Russett & Oneal 2001) or whether democracies win or not (Reiter & Stam 2002). Formal models seeking to explain the relationship between domestic politics and conflict, while having made important contributions, have treated war as a game-ending move, assuming away any political decision-making once fighting begins (Schultz 2001, Bueno de Mesquita et al. 2003). In the next chapter, I will explore in detail the shortcomings of such approaches. In short, the problem is that treating war as an instantaneous event forces us to conceive of resolve in the limited sense of the willingness to fight or not.

As I hope to demonstrate in this dissertation, contrasting short-term resolve (whether one is willing to use force in pursuit of one’s objectives) with long-term resolve (the costs one is willing to incur in pursuit of one’s objectives) produces several important insights. I develop two theoretical arguments linking domestic political factors to both short-term and long-term resolve. I contend that developing a theory that can not only address the two theoretical puzzles raised by the extant literature but also yield new insights requires us to treat both accountability and resolve as dynamic factors that are capable of changing over time within a given state, even during an ongoing war.

I do not present such a theory here. This project is but a first step towards that goal. I believe the evidence presented here makes a compelling case that changes in the domestic political arena can effect the long-term resolve of democratic states. Without establishing
such a link, I would have done little to advance the literature beyond the two limitations identified above. Having demonstrated the usefulness of focusing on dynamic conceptions of accountability and resolve, I have paved the way for future work to use such concepts as the building blocks of a more complete theory of domestic politics and international conflict.

1.1 Organization of the Dissertation

The dissertation is organized as follows. In Chapter 2, I address in greater detail the claims briefly presented here regarding accountability and resolve as dynamics that can vary over time within a single state even while its regime type and whether it is at war or not remain constant. In the process, I will address the current state of the literature regarding three important questions. The first concerns the determinants of incumbent punishment (reward) for bad (good) policy outcomes. While the literature has identified many factors, I will focus in particular on the role of partisan competition. Second, I will turn to the literature on domestic determinants of international conflict behavior. I will pay particular attention to those works that have emphasized accountability. The third area of research I will discuss is the bargaining model of war. The majority of work in this area serves to refine the tradition of realism, exposing logical flaws and deriving new propositions from the core assumptions of that paradigm. However, I will argue that in order to offer a complete and logically consistent account of the relationship between domestic politics and international conflict, one must address the important insights developed by the bargaining model of war. This requires attention to the importance of uncertainty and beliefs about resolve. I conclude that international outcomes should be responsive to partisan competition between the government and the opposition as well as overall level of vulnerability of the incumbent, as these factors directly bear upon the accountability of the leader, and thus their resolve in international crises.
In Chapter 3, I develop a series of game-theoretic models to illustrate the importance of partisan competition in determining the long-term resolve of a democratic state. I first present a simple model of war-fighting that assumes away domestic political interaction. Here, two states, acting solely in their national interest, choose whether or not to engage in a potentially infinite number of battles. I derive from this framework baseline expectations about the relationship between the costs of fighting relative to the importance of the issue at stake and previous battle outcomes on the willingness of rational unitary actors to continue fighting. I then substitute a government and opposition for one of the states while retaining the unitary actor assumption for the second state. I then examine the effect of partisan competition between the government and opposition parties on the government’s value for continuing to fight. I briefly characterize the optimal strategies for the opposition, but place greater emphasis on the implications of opposition to continued fighting. I make note of the extent to which the actors’ decision-making depends upon the history of the conflict itself. The main implications of this analysis are that, compared to a world where domestic political competition either doesn’t exist or has no bearing upon international outcomes, the government is more likely to continue fighting a war if the opposition party advocates continuing; if the opposition had previously supported the war but switches to opposing its continuation, the government is less likely to continue fighting; but if the government does continue fighting after the opposition withdraws its support, the effect of the politicization of the war is to discourage the government from quitting though their actual utility for continuing may be quite low. In addition to these general implications, several time-dependent results emerge. The probability that the opposition would be successful in changing the government’s preference for continuing the war by withdrawing its support is strictly increasing the longer a war persists; however, the level of costs the government is willing to bear to continue fighting the war is strictly decreasing the longer the war persists. Taken together, these last two results suggest that an opposition party with no ideological concerns and no
coordination problems will delay withdrawing its support until such a time as doing so will force the government to quit, and the government will always quit immediately when the opposition withdraws its support. This suggests that the explanation for why we observe wars where the government continues fighting after the opposition withdraws its support must either be that the opposition party has ideological preferences or fell victim to coordination failures encouraging members of the opposition to compete over who was the first to oppose the war.

In Chapter 4, I subject the theoretical implications derived from the game-theoretic analysis of the previous chapter to empirical testing. Using an original data set measuring opposition positions over time for all interstate wars involving at least one democracy in the postwar era (1945 – 1997), I examine the effect of changes in the opposition’s position on the duration of interstate war. I also examine the effect of war performance on the duration of opposition support and the relationships between opposition position and war outcome. The empirical results generally support the expectations of the game-theoretic analysis.

In Chapter 5, I illustrate the importance of differentiating between short-term and long-term resolve by discussing a scenario in which the former will be extremely high while at the same time, the latter is expected to be extremely low. Here I address the case of diversionary conflict. I develop a bargaining model to illustrate that the most vulnerable leaders, who are therefore most sensitive to changes in public opinion and might in some ways be considered highly accountable, have a perverse incentive to produce low-level crises with every intention of backing down. This incentive to provoke targets and incite international crises for domestic political gain is driven by the tendency for the public to rally around the leader in times of crisis. I demonstrate that factors previously argued to safeguard against such opportunistic behavior are insufficient to prevent it. In order to address these claims, I build an important element into the game absent in many previous game-theoretic treatments of diversion. By including explicit bargaining between the leader and the target state, I am able to one of
the most common critiques of diversion: that strategic targets will not allow themselves to become the victims of such aggression. I demonstrate that there is little prospect that the targets of such aggression will strategically avoid it, because to do so they would be required to make considerable concessions, while resisting will bring relatively minor costs as the leader is not expected to be resolved and will likely back down. I address the criticism that the public would not reward such behavior in several ways. First, I find that the conditions I associate with diversionary incentives do not increase the probability that the US initiates a dispute, while they do increase the probability that the US is involved in a dispute initiated by another state. This suggests that a vulnerable leader provoking a crisis for domestic gain will make extreme threats in private, hoping to provoke publicly observed aggression on behalf of the target state. The public is unable to determine whether the leader has produced the aggression of the target towards their state through non-serious bargaining or whether the leader acted in good faith when dealing with an unexpectedly resolute foreign leader. Further, such disputes, while occurring during election years, do not occur immediately before election day, but are instead disproportionately likely to occur six months or more prior to a presidential election. The implications of this analysis are that high levels of accountability can produce socially undesirable outcomes, further highlighting the value of distinguishing between factors that indicate a willingness to enter into conflicts versus the willingness to bear costs once involved in a conflict.

In Chapter 6, I subject the implications derived from the bargaining game of the previous chapter to empirical testing. Using data on the conflict behavior of the United States from 1949–2000, I demonstrate strong support for each of the model’s predictions. In presidential election years, as presidential approval approaches 50%, the United States becomes more likely to be targeted in an international dispute. The United States is more likely to back down during these disputes than disputes that begin under other circumstances. Consistent with the theoretical argument, these disputes are especially unlikely to be fatal. They are
more likely to occur early in the election year rather as "October surprises". I also analyze changes in presidential approval for every quarter in the time period and demonstrate that if a dispute occurs under the conditions associated with diversionary incentives, the net gain (in comparison to no dispute) is approximately 3 percentage points, consistent with the expectation that the public will be somewhat suspicious of such behavior but not so much so that the president fails to benefit. Finally, I discuss two cases selected from the data to illustrate the logic. The discussion suggests the events unfolded in a manner consistent with the model and highlights patterns not captured by the theoretical model, suggesting avenues for future research.

In Chapter 7, I discuss the contributions of the project to the study of domestic impacts on international conflict. I return to the two puzzles identified in this chapter and discuss how the findings here help explain the different decisions of President George H.W. Bush and President George W. Bush with regards to invading Iraq. I point out shortcomings of the current work and plans for extending the project in the future.
Chapter 2

Accountability, Democracy and War

While some explanations of the democratic peace focus on a uniquely democratic culture that places greater value on norms of conflict resolution (see Doyle 1986 and Maoz and Russett 1993), as contrasted with explanations focusing on how democratic institutions structure the incentives of egoistic actors, most recent work suggests there is either little difference between the approaches (Bueno de Mesquita et al. 1999) or that, to the extent that the two offer different expectations, the structural or institutional account outperforms the normative or cultural approach (Huth & Allee 2002). In light of the atrocities committed by democracies against their own people (Davenport & Armstrong 2004), against citizens of their imperial holdings (Henderson 2002), and even against citizens of other democracies, provided the actions are performed covertly and likely to escape notice (James & Mitchell 1995), the normative explanation appears quite problematic. Increasingly, scholars have begun to attribute the difference in behavior between democracies and non-democracies to differences in accountability induced by the institutional framework within which decisions are made rather than different underlying preferences of those making the decisions.

However, few scholars of international relations have sought to identify a mechanism by which leaders might be held accountable, instead simply assuming that democratic leaders
are accountable to their publics while other types of leaders are not. Recent work on variation within autocracies calls the latter into question (Weeks 2008). Other studies suggest the former is equally problematic. Those few scholars who have sought to explicitly link the behaviors of leaders in the international arena to domestic outcomes have often found that the relationship between democracy and accountability is highly conditional on the domestic political circumstances.

In this chapter, I will develop a theoretical explanation of democratic accountability, both in general as well as in regard to international outcomes. While the existing literature has identified many other important factors, including (but not limited to), the media (Brunetto & Weder 2003, Slantchev 2006, Choi & James 2007, Berry & Howell 2007), electoral systems (Samuels 2004, Chang 2005, Chang & Golden 2006), access to information (Colaresi 2007) acts of God (Achen & Bartles N.d.), and election campaigns (Kam 2006), there is considerable evidence that one of the most important mechanisms for holding governments accountable is party competition. Where incumbents face little threat of replacement by the opposition, for whatever reason, they will have little incentive to anticipate public response to their decisions. Thus I will focus in this chapter, and throughout the dissertation, on competition among political parties as a mechanism for holding the government accountable for its actions in office.

Of course, these factors are not entirely independent. When the news media air footage of an opposition legislator criticizing the government, thus causing the public to reevaluate their perception of their value for retaining the incumbent, both freedom of the media and legitimacy of open political competition have facilitated the accountability of the government. Nonetheless, I choose to focus on political competition and not the media, electoral rules, access to information, or campaign effects for several reasons. First, most of these factors do not vary much over time any more than regime type does, and thus are no better positioned to explain such puzzles as why two U.S. Presidents made different decisions about invading
Iraq. The exception is campaign effects, where incumbents are argued to be less likely to be held accountable for their policies when they run more effective campaigns, are more likeable, or in some other way enjoy a valence (non-policy) advantage over their challengers (Baum 2005, Meirowitz 2007). The problem with focusing on campaign effects in developing a theory about variation in accountability to explain the behavior of democratic leaders in the international arena is that the identity of the challenger, their appeal to voters for non-policy reasons, and the quality of the campaign they will run are not likely to be known to leaders at the time they are making their decisions. By focusing on the contemporaneous behavior of the opposition party as a determinant of the government’s accountability, I hope to strike a balance between those factors which do not vary frequently enough to explain variation in the behavior of a single state and those factors which vary so much that they are unlikely to weigh heavily in the decision-making calculus of the government.

After developing the link between party competition and accountability, I will address the role of accountability in explaining the variation in international conflict behavior across regime types. Previous work identifies important implications of accountability for conflict behavior. I hope to demonstrate in subsequent chapters that some important effects of accountability have been overlooked. Nonetheless, the primary shortcoming of the previous studies of domestic politics and international conflict lies not in what effects they assume follow from accountability, but in the tendency of these studies to view accountability as a constant within democracies rather than focusing on variation in degrees of accountability within democratic states. Though we certainly have much yet to learn about the effect of accountability on leader’s behavior, I am not claiming that previous arguments about the conflict behavior of democracies are fundamentally incorrect so much as that they should only hold under certain conditions. Depending upon the domestic political context, democratic governments might indeed exercise great restraint regarding the use of force. But other times, we should expect very much the opposite. It all depends upon what the government
expects the electoral consequences of using force (or not) to be.

If the extent to which democratic governments expect to be held accountable for their policies varied across democracies but not over time within any given democracy (or only over very long periods of time) then there would be little need to challenge the practice of treating war as though it were an instantaneous event. Recognition of this variation would certainly be useful in comparing the behaviors of different democracies, and democratic peace theory might be revised, such that our expectation would simply be that two democracies with vigorous competition among parties are less likely to fight wars with one another, exempting democracies with dominant parties. This would not be an unimportant revision to this prominent theory, particularly given the role of democratic peace theory in current American foreign policy. But the change would be one of explanatory domain, leaving the underlying logic intact. On the other hand, if the extent to which democratic governments expect to be held accountable for their policy choices can instead change rapidly within a given country, even within the context of an ongoing international event, then our existing theories require serious revision.

As I hope to demonstrate, there is indeed reason to expect significant variation over short periods of time in the likelihood that the government’s continued rule will depend upon their foreign policy decisions. I will focus on two causes of such variation to illustrate the importance of more micro-level theory building, while hoping that others will explore the possibility of still other mechanisms. Abrupt changes in the level of support for government policies from the opposition party and short-term fluctuations in the popularity of the government can alter beliefs about the extent to which the government’s prospects for retaining office are dependent upon foreign policy outcomes. Given that the government’s expected probability of being held accountable for their policy choices can change very quickly, it is important to treat international conflict as a dynamic process as well, rather than an instantaneous event.

The bargaining model of war, though often dismissive of domestic politics, lends critical
insights in this regard. The outgrowth of an attempt to identify logical inconsistencies within neorealism (Fearon 1995), the bargaining model of war emphasizes the ever-present alternative of a mutually beneficial bargain, forcing us to explain why costly fighting is preferable if we are to explain the occurrence of war. Factors giving rise to a conflict of interest are not necessarily determinants of war, according to his approach. Rather, war results from the inability to arrive at a bargain immediately due to either to the uncertainty created by informational asymmetries (preventing states from knowing what the terms of such a bargain should be) or due to commitment problems (whereby states cannot credibly promise to uphold the conditions of such a bargain). I will summarize the implications of this burgeoning literature, with particular attention to the role of uncertainty about resolve. While extensive literatures have explored various means through states that were willing to fight could persuade their opponents of this (Fearon 1997, Zagare & Kilgour 2000, Slantchev 2006), the bargaining approach emphasizes the importance of beliefs about the level of costs a state is willing to suffer in pursuit of its objectives, even when its willingness to suffer at least some costs of fighting is not in question (Powell 1999, Wagner 2000, Slantchev 2003a, Slantchev 2003b). Throughout the dissertation, I will build upon this literature, arguing that changes in the domestic political environment have implications for the resolve of a democratic state involved in an international conflict.

This chapter seeks to establish three important points. First, that accountability is not a static feature that democracies possess and other regime types lack. Rather, accountability for government policy decisions varies from issue to issue over time within any given state. Democratic governments very likely are more often more accountable for their policy performance than are leaders of other regimes (Bueno de Mesquita et al. 2003), but the extent to which a government fears jeopardizing its prospects for retaining office contingent upon the outcome of any given policy choice depends upon the political context, particularly the behavior of the opposition party and the current popularity of the government. Second,
variations in accountability explain variation in choices regarding international conflict. Allowing for the possibility of changes in accountability over time requires a revision of previous arguments relating domestic politics to conflict behavior. Finally, such revisions must not only treat accountability as a dynamic concept, but must also treat war as a process rather than an instantaneous event. I will develop each claim in turn.

2.1 Government Accountability

The goal of democratic governance is for public decisions to reflect the will of the people. The social choice literature highlights problems with direct democracy by unanimous rule (which greatly empowers elites to preserve the status quo) (Rae 1975, Mueller 2003) or majority rule (due to preference cycling and the relatively narrow conditions under which it is meaningful to speak of the will of the people as a coherent concept) (Arrow 1951, McKelvey 1976, Mueller 2003). Even if direct democracy were feasible, there are thus reasons to question its desirability. Rather, modern democratic states rely upon the notion of representative democracy. The people endow political elites with the power to make decisions for them, enabling those with expertise, judgment, and information to serve their interests. Of course, the political elites endowed with such authority may not have the best interest of the public at heart. For Madison, the core idea of representative democracy is to pit ambition against ambition so that office-holders will expose the choices of rivals which do not serve the public interest, in order to advance their own private interests. In other words, representative democracy is the quintessential principal-agent problem and the solution to the problem is to rely on electoral accountability to constrain the decisions of agents. (Ferejohn 1986).

Ferejohn (1986) develops a principal-agent model of purely retrospective voting, where voters disregard policy platforms, instead choosing to reelect incumbents who deliver good policy outcomes and replacing incumbents do not. He demonstrates that if implementing
the principal’s most preferred policy is costly to the agent and good outcomes occur with some probability even when inferior policies are implemented, there are conditions no voting rule is sufficient to motivate the agent to implement the principal’s most preferred policy. However, depending upon the government’s value for holding office, its valuation of future outcomes, the cost of implementing the desired policy, and so forth, there are conditions such that simple retrospective voting does ensure that the agent implements the principal’s most desired policy. In other words, even if voters disregard all campaign promises and simply vote to retain incumbents following good policy outcomes while replacing incumbents following bad outcomes, they will often ensure that the government will be motivated to produce good policy where otherwise they would not.

However, such voting rules go astray if the public fails to distinguish between undesirable outcomes that are attributable to the policy choices of government and those that result from exogenous forces, such as nature. Achen and Bartles (draft) demonstrate that voters blame incumbents for acts of God, such as droughts and shark attacks. If implementing the policies most desired by the public is costly to the government, either because it prevents them from implementing competing policies that they find preferable or because they consume resources the government would prefer to reserve for private consumption, as assumed by Ferejohn, the key to motivated governments to absorb this cost is the belief that they are more likely to retain office if they choose to do so than if they do not. However, if governments believe that the voters sense of whether the government has performed well or poorly is largely determined by events beyond the control of the government, such as droughts and shark attacks, the motivation to implement policies that promote the public interest over the government’s private interest is weakened.

Therefore, in order for retrospective voting to play any role in promoting government accountability, there must be some reason for the government to believe their probability of retaining office is at least weakly a function of their policy choices. The greater the
ability of the public to discern the relationship between government policy choices and observed outcomes, the more useful retrospective voting will be as a mechanism of ensuring accountability. This highlights the importance of information.

To be sure, there are many factors important in ensuring the public is armed with sufficient information to properly evaluate government performance. The role of the media should not be understated, though I will focus my attention elsewhere. However, even a diffuse range of media outlets and expansive education system are not sufficient to ensure that citizens acquire sophisticated understanding of the workings of government, let alone policy-relevant facts of the issues of the day. One study of American public opinion characterized public information as being distributed in a diamond shape (Delli Carpini & Keeter 1996). That is, very few citizens are at the bottom, with next to no politically-relevant knowledge at all. However, relatively few citizens are at the top. The bulk of the public falls somewhere in the middle, knowing such basics as which party is more conservative, which controls the House of Representatives, but not much else. While there are important dynamics over time in the level of knowledge, and the impact of changes in the dominant form and style of media have profound implications (see (Baum 2002) on the unexpected impacts of soft news), the overall conclusion of the literature is that aggregate public opinion appears to respond to events in much the way we would expect, but the average citizen possesses strikingly little politically relevant information (Page & Shapiro 1992). One way of explaining this dilemma is to assume that the vast number of uninformed citizens hold incorrect views that are not biased in any particular direction, such that errors in one direction are canceled out by errors in the other, allowing the sophisticated knowledge of the few citizens at the top of the information pyramid to emerge.

Another way of accounting for the surprising stability and responsiveness of public opinion to events is the focus on shortcuts (Popkin 1991, Lupia 1994, Lupia & McCubbins 1998). Shortcuts are observable indicators that help citizens deduce which position on a complicated
issue is most consistent with their underlying preferences. Shortcuts can derive from many sources. Popkin (1991) discusses events on the campaign trail that reveal information about candidates, such as the infamous failure of Ford to shuck a tamale, and thus explains the rationality of voting on such seemingly irrelevant criteria as a candidate’s personality. One important type of shortcut is the position taking of political elites. Lupia (1994) talks about the positions of interest groups and the effect they had on determining voter choice on highly technical ballot initiatives. Several scholars have applied this approach to the behavior of the political parties (Zaller 1992, Berinsky 2007).

2.1.1 Party Competition

Drawing heavily upon political psychology, Zaller (1992) develops a link between the positions of elites and opinion formation. He assumes that individuals have an underlying preference on the traditional left-right political spectrum, which is a function of genetics, socio-economic background, socialization, etc. However, absent sufficient information and political sophistication, individuals will not understand terms such as liberal and conservative, let alone be able to discern where on this spectrum a given policy proposal falls or where their own ideal point would be. He also assumes that people prefer to avoid cognitive dissonance that arises as a result of holding conflicting arguments, opinions, or bits of information in one’s head. Finally, people do not hold fixed attitudes so much as constructing “preference” statements on the fly, drawing semi-randomly from all the pieces of relevant information in their minds, with extra emphasis given to those that are more salient. The implications of these premises are that the most uninformed individuals internalize all political information to which they are exposed uncritically, although they are not likely to be exposed to a great deal of information. When asked a question, their response is unpredictable, as they hold bits of information that will bias them towards a liberal position
as well as bits of information that will bias them towards a conservative position. This is why the least informed citizens simultaneously appear the most moderate and the most contradictory. Citizens with enough political sophistication to understand the basics of the left-right spectrum will resist bits of information inconsistent with their underlying preferences, and so will provide answers that are more consistent around some true underlying political position. However, they will lack the information to determine what position on any given issue, especially highly technical ones, most accords with their underlying preference structure. Thus, they will take cues from elites whom they have frequently agreed with in the past. In this manner, a relatively uninformed but more-or-less politically aware conservative will not collect all relevant facts before deciding whether or not they support the government’s position on wiretapping. Rather, they will deduce that because the right-wing talk radio personalities with whom they have frequently agreed in the past support the position, this signals that the government’s wiretapping program is consistent with their underlying preferences, and they choose to support it. Only the small fraction of the public that is most politically sophisticated will form opinions more or less in isolation of elites, possessing the capability to evaluate on their own whether the details of the governments wire-tapping deviate sufficiently from their own preferred balance between security and liberty.

Berinsky (2007) argues that the public takes cues from both sides of the political spectrum. In this sense, even when one side of the political spectrum is silent on an issue, party competition facilitates efficient opinion formation (in the sense that the public adopts positions that approximate the ones they would adopt if they collected all relevant information and properly considered each argument before selecting a position, while avoiding the costs of this process). He argues that a conservative does not need to hear what right-wing pundits or politicians have to say about an issue to know what their position should be so long as they know what left-wing pundits and politicians have to say, because more often than not, their position will be the opposite. Absent explicit signals of agreement from the elites whom
they sympathize with, most citizens will default to adopting the view directly opposite to the one espoused by elites with whom they do not sympathize. He demonstrates this pattern in public opinions regarding the war in Iraq. Half of the respondents of a survey conducted were asked to identify the number of US fatalities in Iraq to date as best they could and were then asked whether they supported the war. The other half of respondents were randomly chosen to receive a prompt, whereby they were told the true number of US fatalities to date after they were asked to guess, and only after being corrected (in the case that they were off by a considerable margin, as approximately sixty percent of respondents were) were they asked whether they supported the war. There was no significant difference in support for the war between the two halves of the sample, providing strong evidence that individuals not only can form opinions absent relevant information, but moreover, even when presented with relevant information that is potentially at odds with their perception, they do not take this information into account. Berinsky demonstrates that the political orientation and party affiliation of the respondents predicts the position on the war far better than their perceived amount of US fatalities. He further demonstrates that the same pattern was observed regarding support for entry into World War II, but only while the Republican Party opposed entry. For a brief period in 1940 when Wendell Wilkie was the party’s nominee for president and adopted a position far closer to FDR’s than his party had previously, aggregate support for entry into the war increased, especially among self-identified Republicans. After Wilkie lost and the isolationist wing of the party, headed by Taft, regained control of the party, public opinion again polarized, until the Republican Party converged on a pro-war position in the months before the attacks on Pearl Harbor. His analysis of American public opinion during World War II and the current war in Iraq strongly suggests that the positions taken by the political parties explain variation in public support for war, as Zaller (1992) had argued for the Vietnam War. Arena (2008) demonstrates that this partisan foundation of opinion polarization has profound implications for accountability. Analyzing all post-World War II
elections in continuously democratic states that fought an interstate war (US, UK, Israel and India), he argues that war outcomes only play a role in determining election outcomes when the opposition party(ies) opposed the war while it was ongoing. When the opposition supported the war, either explicitly or implicitly, the outcome of the war exerts almost no effect on the incumbent party’s probability of retaining office in subsequent elections. When they opposed the war, good (bad) outcomes become an electoral asset (liability). That is, opposition to the government’s policy does not systematically undermine the government nor alienate voters by calling the opposition’s patriotism into questions. Rather, opposition merely acts as an alarm bell, calling public attention to a critical issue that otherwise would not weight heavily in the public’s mind when assessing the value of the government (Arena 2008).

The implication of this line of research is that the public delegates power to the government with little restrictions, granting them wide discretion. Strong partisans will either support or oppose the government uncritically, but strong partisans are not enough to assemble a winning coalition. The critical moderate voters wish to know whether the government has implemented policies in line with their preferences, but is unwilling to watch the government’s decision-making closely enough to make a determination for themselves whether the government has done so. It is costly to monitor the government’s decisions, collecting information about each decision and potential alternatives, and such costs are prohibitive. However, to ensure some control over their agent, the moderate portion of the public trusts that, through self-interest, the opposition party will be motivated to collect such information and inform the public when the government’s decisions are not in the public’s best interest. When the opposition party supports a government policy, the public does not update its belief about the government’s value as an agent. However, when the opposition party opposes the government’s policies, the public pays attention and awaits the outcome of the policy, knowing that this will reveal valuable information about the government’s quality as
an agent. This strongly suggests that it is inappropriate that categorically assume that governments anticipate being held accountable for their foreign policy decisions in democracies. Accountability arises only as a function of the strategic behavior of the opposition party, as their support for (or opposition to) the government’s policies acts as the mechanism through which the portion of the public whose decision is not predetermined by the strength of their partisanship determines whether or not to consider the outcome of the policy in assessing the government’s performance.

Note, however, that this literature assumes a truly competitive party system. As I will discuss in the next section, if one party is expected to enjoy a natural advantage among the electorate, there is little incentive to provide policies in line with public preferences when in office. Existing literature has explored the implications of one-party dominance within advanced democracies and the negative impacts on governance. Pempel(1990) provides a general discussion of one-party democracies. Scheiner (2005) addresses the causes and consequences of LDP dominance in Japan. At the sub-national level, are recent study argues that the 1965 Voting Rights Act, which effectively marked the end of Democratic dominance in the South in the US, increased party competition and this increased party competition is estimated to have triggered no less than a 20% increase long-run per capita income in the average affected state (Besley, Persson & Sturm N.d.).

Less scholarly attention has been devoted to exploring the implications of temporary advantages accruing to parties that might induce short term variation in accountability. In essence, there are reasons to expect that within a democratic state that overall is characterized by close competition between two or more parties, if the incumbent party is overwhelming popular, it may temporarily behave as do dominant parties in one-party democracies. It is this possibility that I now turn my attention to.
2.1.2 Prior Policy Performance

Let us return to Ferejohn’s (1986) principal-agent model of electoral accountability in light of this discussion of the importance of party competition in shaping retrospective voting. Ferejohn also considered the case where there are two parties, each of which is either competent or not (where the competence of each party is independent of the other party’s competence such that both may be competent, neither, or just one). Here, competent parties are assumed to have a higher probability of producing good policy outcomes, regardless of effort. Ferejohn demonstrates that the expected probability that the party out-of-government will produce good outcomes determines the threshold at which the public decides the incumbent’s policy record is sufficiently poor to warrant replacement. These simple results highlight the implications of ignoring party competition in developing arguments about accountability.

Consider now the possibility that policy outcomes are a function not only of the party’s level of competence but also their effort. Parties known to be more competent will have an obvious incentive to invest less effort in producing good policy, knowing they are likely to retain office anyway. In the absence of strong party competition, the ability of retrospective voting to constrain agents and ensure the implementation of preferred policies breaks down. Ferejohn’s model illustrates the importance of party competition in restricting the government’s belief that they can count on being returned to office regardless of their policy choices. The less likely the public is to be willing to place the opposition in office, the less incentive the government has to implement the public’s preferred policies. Ferejohn models the public’s belief that the government is competent as a function of all prior policy outcomes while they were in office, while the belief that the opposition is competent remains fixed at the prior belief, as the public has not observed any events that could alter their initial belief. This suggests that the incentive for governments to implement the policies most preferred by the public is a function of the success of their past policies.
One of the most important aspects of policy performance (besides prevention of shark attacks) in the eyes of most voters is the state of the economy. A vast literature emphasizes the role of economic voting (Hibbs 1982, Powell & Whitten 1993, Mueller 2003) and performance of the economy in explaining approval (Erikson, MacKuen & Stimson 2002). If voters respond to the state of the economy, there is an obvious incentive for leaders to manipulate the economy for electoral gain, producing a political or partisan business cycle (partisan if the direction the public would like to see the government steer the economy is a function of political orientation, as it appears to be). Some scholars have argued that rational expectations should prevent the occurrence of a political or partisan business cycle (Schultz 1995, Mueller 2003). However, many scholars have demonstrated that there are indeed systematic differences in the performance of the economy by party and across the electoral cycle (Hibbs 1982, Alesina & Rosenthal 1995, Mueller 2003). Specifically, parties of the right rely upon the vote of higher income voters, who find inflation to be a great threat to the depreciation of their accumulated wealth but consider fluctuations in unemployment to be extremely unlikely to threaten their economic situation. Parties of the left rely upon the vote of lower income voters, who are often hit hard by even relatively small increases in unemployment but may or may not concern themselves much with inflation, as decreases in the value of the dollar also tend to lead to higher wages and may or may not have any impact at all on their purchasing power, while losing a job most certainly will effect their ability to maintain the same lifestyle. Upon surveying sixteen studies testing the partisan business cycle thesis, Mueller (2003) finds the literature evenly divided between evidence in favor of and against a partisan model.

A potential flaw in previous studies is that they fail to account for the possibility that governments who anticipate easy victories due to the accumulated effect of previous successful policies have little incentive to implement economic policies in line with the demands of their core constituents heading into an election. That is, if the public assesses the govern-
ment’s performance on the basis of the performance of the economy, it does not necessarily follow that the government will always seek to manipulate the economy immediately prior to elections, as strong economic performance to date will assure the government that retrospective voters already prefer to return them to office rather than replace them with the opposition, and will have no incentive to further move policy closer to the ideal point of their supporters.

I have reevaluated the partisan business cycle predictions in light of this claim. Using quarterly data from the US from 1949 – 2000 on presidential approval, unemployment and inflation (Burbach N.d.), I test the claim that presidents are more likely to implement the policies preferred by their core supporters (lowering inflation in the case of Republicans, lowering unemployment in the case of Democrats) in election years if their approval is within 5 percentage points of 50% than if it is outside this vulnerable range.

I employ binary measures rather than continuous measures since the theoretical argument here does not suggest linear effects. While I expect that a Republican president facing a close election has a greater incentive to cut inflation and motivate the base to turn in higher numbers than a Republican president who expects to an easy victory, I do not expect that the latter would have any incentive to increase inflation. Therefore, as I only hypothesize a greater probability of a drop in inflation under certain conditions, with no theoretical expectation about the trend in inflation under other conditions, my dependent variable is a binary measure of whether the quarterly change in inflation was negative. The independent variables are binary indicators of the president’s party, whether it is an election year (lagged by one quarter so that December of 1991 is considered an election year while December of 1992 is not), and whether presidential approval is within 5 percentage points of 50%. I also include important control variables. Though the dependent variable is binary, in the spirit of a general error correction model (DeBoef & Keele 2008), I include the appropriate number of lags of both inflation and unemployment as well as the quarterly change in inflation and
unemployment. I analyzed the autocorrelations and partial autocorrelations to determine the appropriate number of lags. I also include measures of the partisan composition of Congress, as the president does not unilaterally set policy. I include a continuous measure of the percentage of seats in the House belonging to the Republican party as well as a separate dummy for dominance of the opposition party. This measure is equal to 1 when the opposing party holds more than 55 percent of seat. This is a conservative measure of dominance, as such a super-majority is nonetheless too small to override a veto. However, veto-proof majorities are rare, and indeed, in this time period the Republican party never once attained dominance according to this more permissive measure.

Table 1 presents the results of the logistic regression analysis of the determinants of a decrease in quarterly inflation rates under Republican presidential administrations. Table 2 presents the results of the complementary analysis of the determinants of a decrease in quarterly unemployment rates under Democratic presidential administrations. The second column reports the coefficient estimates, with the robust standard errors surrounding these estimates in parentheses. The third column reports the probability that these coefficient estimates would be uncovered by random error given that the true underlying population coefficient is in fact 0. Due to the presence of interaction terms, the interpretation of the effects is not straightforward. The control variables generally exhibit significant effects, in the direction one would expect. For example, consistent with economic theory regarding the Phillips curve, the higher unemployment (inflation) last quarter, the higher the probability that inflation (unemployment) drops this quarter. Likewise if unemployment (inflation) was increasing in the previous quarter, regardless of the level it was at, the higher the probability that inflation (unemployment) drops this quarter. The results also suggest that inflation drops under Republican administrations less often when the Democrats dominate the House. It would also appear that, given that the Democrats do not dominate the House, the percentage of seats in the House belonging to the Republican Party matters, where more
evenly divided Houses increase the probability inflation drops compared to Houses where Republicans have a larger number of seats. Unfortunately, since the Republican Party never held more than 53% of seats during this time period, it is difficult to know how strong this effect is and whether it is a mirror of the logic regarding the President’s vulnerability. While the Republicans never dominated the House in this period, preventing direct comparison between Table 1 and Table 2, the results do suggest that when under Democratic presidents, the more Republicans there are in the House, the less likely unemployment is to fall.

To illustrate the effects of the key variable, the president’s vulnerability in an election year, I present predicted probabilities of quarterly decreases in inflation (unemployment) under Republican (Democratic) administrations in Tables 3 and 4, respectively. These predicted probabilities were generated holding all other variables constant at their means (or modes, for dummy variables). Notice that in both cases, the effect of vulnerability in a non-election year is moderate (a change of 0.12 for unemployment and 0.16 for inflation), while the effect of vulnerability in election years is much more substantial (a change of 0.2 for unemployment and 0.63 for inflation). The effect is much stronger for Republicans and inflation, despite the nominal independence of the central bank. The reason for the differential effects under vulnerable Republicans versus vulnerable Democrats is not obvious, though it is possible that one reason is that unemployment was managed better during this period overall, leaving less room for accelerating declines during election years. Indeed, the standard deviation on unemployment for this period is 1.56 while the standard deviation on inflation is 3.44, with unemployment dropping under Democratic administrations in 71.5% of quarters in this time period, versus only 49.1% of quarters of Republican rule saw a drop in inflation. Without more variation in the dependent variable and partisan composition of Congress, it is difficult to draw any strong conclusions about the differences across parties with regard to the partisan business cycle, but this analysis is consistent with the notion that popular governments do not try as hard to update the public’s beliefs about their competence
in election years as do governments with more moderate levels of popularity.

### 2.1.3 Competition as Accountability

This discussion highlights the importance of party competition and reelection incentives in creating accountability. I have argued that government accountability stems from threats to replace the government with the opposition if the government does not produce good policy outcomes. Rather than a blind theory of retrospection, I have argued that voters rely on the opposition party to alert them to government decisions that are not in the public interest. However, I do not claim that when this happens, the public automatically sides with the opposition any more than they do with the government. While some portion of the public may indeed do so, those with the strongest partisan attachments, the remainder of the public interprets partisan conflict as a signal that the outcome of the policy in dispute reveals credible information about the government’s competence, while the outcomes of policies that the parties did not disagree over do not reveal such information. In this sense, I have advanced an argument of conditional retrospective voting, where accountability for failed policies depends upon the strategic decision of the opposition to activate the public’s attention to the issue. Further, consistent with extant principal-agent models of accountability, I have argued that the link between policy performance and accountability ironically weakens when the government has presided over especially good times. If these good outcomes are the result of good governance, we might consider the decreased incentive to cater to public opinion as acceptable, perhaps even desirable. But if governments can attain high levels of popularity through accidents and thus be given the gift of the freedom to ignore public demands without fear of repercussion due to dumb luck, the implications for democratic theory are profound and unsettling.

Nor are the implications for scholars of international relations trivial. If accountability
depends critically upon the decision of the opposition party to hold the government accountable (a decision which may not depend solely upon the merits of the government’s policy), and necessarily weakens when governments expect to retain office regardless of whether they cater to public opinion or not, the literature’s tendency to treat accountability as a static characteristic of regime type is inappropriate. If the literature on democracy and war did not contain contradictions or irregularities, but instead managed to explain such outcomes as which nations go to war with one another, the duration and outcomes of wars, as well as conflict outcomes short of war, without producing any logical inconsistencies, we might not concern ourselves with this oversimplification. After all, the scientific approach depends critically upon simplifying reality, which necessarily entails making assumptions we know to be untrue. Increasingly, the science of politics relies upon constructing models (mathematical or otherwise. Models are neither “true” nor “false”. Rather, models are by their very nature known to be inaccurate in certain respects, and so the important question to ask of a model of political behavior, much like one asks of a map, is not whether it is realistic, but whether it is similar enough to reality to be useful for a specific purpose (Primo & Clarke 2007). In other words, the simple fact that a preponderance of evidence suggests there is variation in the degree of accountability faced by leaders of democracies does not itself invalidate the dominant approach in the literature, which tends to black-box domestic political processes within the state. However, as I discussed in the previous chapter and will now demonstrate in greater detail, the literature on democracy and war has a very difficult time explaining why wars involving democracies nearly always begin with the opposition party strongly supporting the decision to go to war or why democracies find themselves in prolonged, losing, unpopular wars. While this is not the modal outcome when democracies go to war, it occurs far too frequently to be dismissed by a theory of domestic politics and war. The current approach is not flawed because it makes a simplifying assumption that can be demonstrated to be untrue, nor am I claiming that it is flawed simply because the
current war in Iraq provides a highly salient counterexample to its predictions. Rather, I contend that the current approach to democracy and war is flawed because its simplified view of accountability has important implications for the ability of this approach to explain the outcomes it was proposed in order to explain. The only appropriate metric by which to judge a model is its usefulness, not its truthfulness. By this measure, the current approach leaves much to be desired.

2.2 Democracy and War

The democratic peace, sometimes called the closest thing to an empirical law in international relations (Levy 1988), emerged inductively, an empirical association that stubbornly refused to be disappear after repeated statistical tests (Maoz & Abdolali 1989, Maoz & Russett 1993, Bremer 1992, Ray 1995). By the early 1990s, few scholars questioned the dual claims that democracies were no less war-prone than other regimes overall but were unlikely to war with one another. However, the search for a satisfactory explanation for this finding remained front and center in international relations scholarship for the better part of a decade (Oneal & Russett 1997, Gartzke 1998, Schultz 1998, Bueno de Mesquita et al. 1999). For many, the debate has ended, and something approaching a consensus has emerged, emphasizing the role of institutional constraints and the information revealed by strategic behavior within these constraints. Empirical critiques continue, but most seek to wash out the significance of what is seen to be a spurious relationship by including measures of the true cause of peace among western nations. These critiques have largely adopted the dominant research design of democratic peace advocates, the weak-link (Henderson 2002, Gartzke 2007). While the decision to critique the theory on the terms of its most prominent advocates is understandable, the weak-link measure is for various reasons a poor way of testing the theory (Bennett 2006), thus casting doubt on those studies that manage to
make the finding “go away”. Theoretical critiques often appear to fail to understand the
nature of the claims made by advocates of the theory (see Rosato 2003 as well as response
(Slantchev, Alexandrova & Gartzke 2005)). I contend that there are indeed considerable
problems with current democratic peace theory, but recent critiques, preoccupied as they
are with disproving the central empirical relationship, miss the point.

Proponents of the democratic peace have sought to demonstrate the usefulness of the the-
ory by constructing logical explanations of the well-known finding that simultaneously pro-
duce further testable implications, which have generally gained empirical support (Bueno de
Mesquita et al. 1999). In light of the number of outcomes advocates of the democratic peace
claim are explained by variation in states’ domestic political institutions, it is unclear what
we learn from studies that seemingly demonstrate that the observed lack of wars between
states in North America and Western Europe can be fully accounted for by financial ties
rather than governance (Gartzke 2007).

The farthest-reaching explanation of the democratic peace comes from the Selectorate
Theory (Bueno de Mesquita et al. 2003). This theory is not a theory of the relationship
between domestic politics and war so much as a theory of the effect of institutional structures
on governance, writ large. The theory argues that all states (and indeed, corporations,
families, and most any organization of people, formal or informal) consist of three groups,
the relative size of which proving to be critically important. First is the overall population.
Second is the subset of the population that plays a direct role in selecting the leader, known
as the selectorate $S$. Third is the subset of the selectorate whose support is essential to attain
office, the winning coalition, $W$. Leaders are assumed to desire attaining (and retaining)
office above all else. In order to do so, they seek to satisfy the minimum number of selectors
necessary, retaining any resources left over after doing so for their own consumption (or to
hedge against unforeseen shocks, which will require them to distribute more resources to keep
together the coalition they assembled upon taking office). The larger this number, $W$, gets,
the more likely it is the leader will choose to satisfy the winning coalition with public goods rather than private goods. Being non-excludable, the leader, through no fault of her own, will have produced good policies that in fact satisfy much of the overall population as well. If winning the wars that one starts is viewed to be a good policy outcome, where taking your nation to war and losing would not satisfy broad swathes of the selectorate, then we would expect nations with a large \( W \) to carefully choose to fight only those wars that they can win. The authors contend that their concepts of \( W \) and \( S \) are distinct from existing regime type classifications, though they are expected to track closely together, such that states we think of as democracies will often (but not always) have a large \( W \). Treating the theory as an explanation of the democratic peace may appear somewhat problematic then, as their theory does not exactly refer to democracy per se, as they insist that the two concepts are different. However, a recent study demonstrates that their statistical tests are flawed, and once correcting for this flaw, the added explanatory value of \( W \) after controlling for democracy disappears (Clarke & Stone N.d.). However, it is worth noting that this critique does not undermine the theory, it merely demonstrates that one can usefully treat democracy as a stand-in for the author’s notion of large \( W \) and find the same results. Democratic governance is thus associated with higher growth rates, lower tax rates, less crime and corruption, greater provision of all basic services, higher standard of living, fewer wars, victory at the end of those wars, and so on. While it may be true that democracy’s association with peace acts through economic variables, given the evidence that democracy is also related to economic liberalism, and given that a theoretical explanation of the democratic peace also facilitates the explanation of so many other outcomes, a narrow critique of whether democracy is the proximate cause of the peace among Western nations does not appear to be very fruitful.

The problem with the Selectorate Theory (beyond the inappropriate empirical measures of \( W \) and \( S \) the authors construct) is that it only manages to explain the occurrence of war
by restricting the bargaining space. In this sense, they essentially assume away the possibility of bargaining as conventionally understood (Fearon 1995, Wagner 2000). As Fearon demonstrates, rationalist explanations for war that do not account for the possibility of bargaining are not logically tenable. If the good in dispute is infinitely divisible and agreements are enforceable, the only logical explanation for states to resort to a costly mechanism such as war to resolve their differences rather than a bargained settlement is that they do not know what agreement to make due to private information and attempts to misrepresent one’s resolve. Since the majority of interstate wars do indeed end in negotiated settlements without the victor coming into possession of the entirety of the good originally in dispute, it is difficult to claim that indivisible goods or the inability to commit to honoring agreements is the primary determinant of most wars, even if such accounts play an important role in some minority of cases (Powell 2006, Wolford, Carrubba & Reiter N.d.). The explanation for the democratic peace offered by the Selectorate Theory is that democratic states try harder to produce victory and only choose to go to war when they know they can win with high probability, which they have no reason to believe when facing one another. However, this theory does not explain why any state would choose to fight a war against a democracy. Knowing democracies behave this way, the rational leader of a state with a small winning coalition, whose tenure can easily survive the granting of extensive foreign policy concessions, should merely grant the democratic state whatever it wishes while demanding some private goods in return to compensate its winning coalition. In fact, the authors have used the same theory to explain how foreign aid can be used by leaders of large W systems to buy foreign policy concessions from leaders of small W states, providing evidence that making concessions poses little threat to retaining office in such systems (Bueno de Mesquita & Smith 2007). There is little room in a theory that holds that leaders of small W states can sell their foreign policies for such leaders to resort to war with a leader who cannot be expected to be willing to fight if there is any reason to expect defeat. A separate study demonstrates that democratic states
rarely lose wars in general, but especially when the democratic state is the initiator, the probability that of victory is nearly 75% (Reiter & Stam 2002).

Schultz (2001) explains why there is unlikely to be any uncertainty over the willingness of a democratic state to escalate to war. Focusing on the announced position of the opposition party at the onset of a crisis, he demonstrates that when the opposition party supports the use of force, the government’s threats are never bluffs. If the opposition opposes the use of force, some probability remains that the government was not bluffing and would indeed be willing to go to war, though its expected value for war is not the highest it could be. This study appears to be at odds with studies that argue that democracies only fight those wars they are most likely to win. For Schultz, democracies, like all states, are more willing to fight the more likely they are to win, but they only expect to have to fight (that is, the targets of their threats are only expected to resist) when they are less likely to win, which the target will know thanks to the behavior of the opposition party. This argument too meets difficulties when faced with the historical record, as the majority of wars involving democracies began with clear support from the opposition.

The Selectorate Theory and Schultz’s signaling model both treat war as a costly lottery, an instantaneous event that players must pay some costs to engage in and produces outcomes according to some probability function without the need for any further decisions on behalf of the players. While both theories emphasize the importance of competition for office as the explanation for why leaders of democracies behave differently than leaders of non-democracies, and thus represent considerable improvements over statist approaches to the democratic peace (Russett & Oneal 2001), they do not treat either war or accountability as sufficiently dynamic. Schultz’s model comes closer to an appropriate treatment of accountability, as his model allows for variation in opposition behavior within a single democratic state across cases. His work demonstrates the value of transparency and open, observable, partisan competition for revealing information about a democratic state’s willingness to use
force. And while the common occurrence of wars involving democracies that began in the face of opposition support poses a problem for his argument, several other implications of his model are bourn out empirically. Therefore, I do not take issue either with the claim that uncertainty is the primary cause of war, nor that democratic institutions reduce uncertainty, at least with regard to the willingness to use force. However, through treating war as a costly lottery and not a process that unfolds over time, Schultz rules out the possibility of variation in opposition behavior (and thus accountability) within a single case that could help explain changes in the level of costs a democratic state is willing to bear in pursuit of its objectives even when there is strong reason to believe they are willing to use force. The logic of Schultz’s argument is persuasive, in that his conclusions follow clearly from his assumptions. The problem is that by assuming away changes in accountability, and therefore long term resolve, within a single case, his approach falls short of being able to account for the full range of empirical patterns a theory linking domestic politics to international outcomes should offer.

2.3 Conclusion

War is not the result of the breakdown of bargaining – rather it is part of the bargaining process itself (Wagner 2000). States use war as a screening mechanism to feel out each others’ resolve, becoming more convinced that the other side is as tough as they claimed to be the longer they hold out (Slantchev 2003b). If wars are fought primarily to learn information about the other sides’ resolve in order to prevent having to make unduly generous concessions, the transparency of democracies should eliminate their need to resort to arms, regardless of their opponent’s regime type. Indeed, Schultz presents evidence that the threats of democratic states are, on average, less often resisted (Schultz 2001). Yet some uncertainty must remain, else it is difficult to account for the frequency of wars involving democracies
whose opposition parties express support for war.

Throughout the remainder of this dissertation, I seek to demonstrate that the willingness to use force and the willingness to bear great costs on the battlefield in pursuit of one’s objectives are distinct types of resolve. Further, the level of resolve the leader of a democratic state possesses vis-a-vis a given crisis can change as the course of the conflict unfolds, owing to changes in her domestic political environment. I will also demonstrate that accountability can have surprising implications, encouraging leaders to do anything and everything to improve their standing among the public and avoid having their policies be judged to be failures. Only through viewing accountability and resolve as dynamic factors can we explain why the support of the opposition is insufficient to remove all doubts about a leader’s resolve from the mind of target states, why democratic states sometimes continue to fight unpopular, divisive, losing wars, and why incumbents expecting very competitive reelection campaigns are motivated to seek out crises despite lacking the resolve to press their demands when the target resists.
Table 2.1: Quarterly Drop in Inflation, Republican Administrations, 1949-2000

<table>
<thead>
<tr>
<th>Variable</th>
<th>Coefficient Estimate</th>
<th>Standard Error</th>
<th>P-value</th>
</tr>
</thead>
<tbody>
<tr>
<td>$\Delta \text{ Inflation}_{t-1}$</td>
<td>0.614</td>
<td>0.159</td>
<td>&lt;0.001  ***</td>
</tr>
<tr>
<td>$\Delta \text{ Inflation}_{t-2}$</td>
<td>0.507</td>
<td>0.156</td>
<td>&lt;0.001  ***</td>
</tr>
<tr>
<td>Unemployment$_{t-1}$</td>
<td>0.368</td>
<td>0.204</td>
<td>0.036   **</td>
</tr>
<tr>
<td>$\Delta \text{ Unemployment}_{t-1}$</td>
<td>2.118</td>
<td>0.741</td>
<td>0.002   ***</td>
</tr>
<tr>
<td>Vulnerable</td>
<td>0.630</td>
<td>0.583</td>
<td>0.140</td>
</tr>
<tr>
<td>Election</td>
<td>-1.694</td>
<td>0.761</td>
<td>0.013   **</td>
</tr>
<tr>
<td>Election*Vulnerable</td>
<td>2.465</td>
<td>1.113</td>
<td>0.014   **</td>
</tr>
<tr>
<td>Republican Seats</td>
<td>-16.232</td>
<td>10.070</td>
<td>0.053   *</td>
</tr>
<tr>
<td>Democratic Dominance</td>
<td>-2.366</td>
<td>0.896</td>
<td>0.004   ***</td>
</tr>
<tr>
<td>Constant</td>
<td>6.375</td>
<td>5.212</td>
<td>0.110</td>
</tr>
</tbody>
</table>

| N                          | 112                  |
| prob $< \chi^2$            | 0.004                |
| Pseudo $R^2$               | 0.247                |

Logit Regression. Robust standard errors. * $p \leq 0.1$, ** $p \leq 0.05$, *** $p \leq 0.01$, one-tailed test.
Table 2.2: Quarterly Drop in Unemployment, Democratic Administrations, 1949-2000

<table>
<thead>
<tr>
<th>Variable</th>
<th>Coefficient Estimate</th>
<th>Standard Error</th>
<th>P-value</th>
</tr>
</thead>
<tbody>
<tr>
<td>Inflation$_{t-1}$</td>
<td>-0.198</td>
<td>0.088</td>
<td>0.012</td>
</tr>
<tr>
<td>∆ Inflation$_{t-1}$</td>
<td>0.185</td>
<td>0.114</td>
<td>0.051</td>
</tr>
<tr>
<td>∆ Unemployment$_{t-1}$</td>
<td>-3.124</td>
<td>1.038</td>
<td>0.001</td>
</tr>
<tr>
<td>Vulnerable</td>
<td>-0.568</td>
<td>0.651</td>
<td>0.191</td>
</tr>
<tr>
<td>Election</td>
<td>-0.473</td>
<td>0.701</td>
<td>0.250</td>
</tr>
<tr>
<td>Election*Vulnerable</td>
<td>1.777</td>
<td>1.157</td>
<td>0.063</td>
</tr>
<tr>
<td>Republican Seats</td>
<td>-8.465</td>
<td>4.444</td>
<td>0.028</td>
</tr>
<tr>
<td>Constant</td>
<td>5.450</td>
<td>2.143</td>
<td>0.005</td>
</tr>
<tr>
<td>N</td>
<td>93</td>
<td></td>
<td></td>
</tr>
<tr>
<td>prob &lt; χ²</td>
<td>0.087</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Pseudo R²</td>
<td>0.170</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

Logit Regression. Robust standard errors. * $p \leq 0.1$, ** $p \leq 0.05$, *** $p \leq 0.01$, one-tailed test
Table 2.3: Predicted Probability of Drop in Inflation, Republicans, 1949-2000

<table>
<thead>
<tr>
<th></th>
<th>Election Year</th>
<th>Non-Election Year</th>
</tr>
</thead>
<tbody>
<tr>
<td>Vulnerable</td>
<td>0.75</td>
<td>0.59</td>
</tr>
<tr>
<td>Not Vulnerable</td>
<td>0.12</td>
<td>0.43</td>
</tr>
</tbody>
</table>
Table 2.4: Predicted Probability of Drop in Unemployment, Democrats, 1949-2000

<table>
<thead>
<tr>
<th></th>
<th>Election Year</th>
<th>Non-Election Year</th>
</tr>
</thead>
<tbody>
<tr>
<td>Vulnerable</td>
<td>0.86</td>
<td>0.63</td>
</tr>
<tr>
<td>Not Vulnerable</td>
<td>0.66</td>
<td>0.75</td>
</tr>
</tbody>
</table>
Chapter 3

Long Term Resolve: Opposition and War

In the next two chapters, I seek to demonstrate the importance of domestic politics in shaping the conduct of war. As discussed in the previous chapter, the central puzzle surrounding the occurrence of wars involving democracies is that democratic institutions are thought to reduce uncertainty, the primary rationalist explanation for war. I argued that the solution to this tension lies in distinguishing between the relative ability of the transparency common to democratic regimes to reveal information about the different types of resolve. That is, I do not dispute the claim that democratic governance generally creates more transparency than is commonly found in non-democratic regimes, nor that this transparency makes it easier to determine when democratic states are willing to commit to using violent means in pursuit of their policy goals. Despite this greater transparency, uncertainty may remain over the willingness or ability of democratic states to bear the costs of prolonged conflict.

I argue that the willingness to bear the costs of prolonged conflict is likely to be at least in part determined by the level of support for the war that is lent by the primary opposition. The opposition’s position on the war is also likely to change over time, particularly if the war drags on for a considerable length of time. If true, uncertainty over the preferences of the opposition, and by extension the long-term resolve of the democratic state as a whole, should
contribute to initial bargaining failure in much the same way that it is argued uncertainty over capabilities does (Fearon 1995). I must first demonstrate that the withdrawal of support by the opposition matters, before establishing that uncertainty over whether an initially supportive opposition would continue to lend its support to a potential war could lead to the breakdown of prewar bargaining. After all, few scholars of international relations would express doubt over ability of uncertainty over resolve to impact crisis bargaining. In contrast, the claim that opposition rhetoric is likely to exert a strong impact on government war policies is likely to encounter considerably more skepticism. Therefore, I focus in this chapter on developing that argument theoretically, through the use of a series of formal models. In the next chapter, I will evaluate the implications of these models empirically.

My goal is to demonstrate that the behavior of the opposition can affect the government’s conduct of war even if we assume the opposition is powerless to directly impact military strategy. While in some electoral systems the opposition might in fact have the ability to directly leverage policy by controlling funding for the war (as in the United States), I do not consider such effects here. Rather, I seek to demonstrate that all the opposition needs to do is go on the record as having called for an end to the conflict in order to alter the government’s decision-making calculus. This power to indirectly alter the government’s payoffs empowers the opposition to influence the willingness of the government to continue fighting. However, this power proves to be a mixed blessing. The formal models developed in this chapter help to illustrate how expressing opposition has the immediate effect of forcing the government to be more discriminating in its decision to continue fighting. At the same time, if the government disregards the opposition’s threat to oppose and chooses to press forward, the public opposition will thereafter serve to entrench the government, forcing it to hold out desperately for progress in the face of costs of fighting that the government would not otherwise be willing to pay. In this light, debate on the floor of the legislature becomes more than mere grandstanding or political theater. What may appear to the casual observer
to be empty words can potentially either encourage an end to, or dramatically lengthen, a war.

The theoretical models developed here further yield the interesting implication that if the opposition’s decision to withdraw versus continue its support were motivated only by the value of foreign policy outcomes and the probability that retrospective voters hold the government accountable, we would never observe politicized wars where the opposition withdrew its support but the government continued fighting anyway. Assuming the opposition knows the government’s value for fighting (a critically important assumption in previous literature), the opposition would always choose to withdraw its support only when by doing so, it is certain to lead to the government ending the war. Given that opposing the war when the government is expected to continue in the short term increases the probability that the government continues the war in the long term, there is no incentive for the opposition to oppose under any other condition. Unless, of course, we consider incentives other than an instrumental act designed to change government policy. I demonstrate that ideological considerations are sufficient to motivate suboptimal opposition decisions, in the sense that a dovish opposition parties may withdraw their support despite knowing that doing so will not make the government quit in this period and will decrease the probability that the government quits in all future periods. Dovish opposition parties may choose to withdraw their support for a war when delaying their withdrawal until some future date might effectively bring about an end to the war, because doing so maximizes their ability to use the war as an electoral wedge issue. This analysis likewise identifies negative consequences of hawkish opposition parties. As I will demonstrate, the government is more likely to continue fighting when the opposition calls on them to continue fighting compared to a world with no domestic political considerations. Given that sufficiently hawkish parties might advocate continuing wars with little prospect for victory and very large costs of continued fighting, their “support” may push the government into continuing a losing war where a more moderate opposition
party would prefer to withdraw their support and thereby prompt the government to quit.

This argument draws on influential work in American politics addressing the power of elites to frame political issues in the public’s mind (Zaller 1992, Berinsky 2007). One of the key insights of this literature is that we are unlikely to observe polarized public opinion absent elite partisan division.¹ This implies that that the outcome of a war, good or bad, is magnified in the public’s mind when the opposition opposes the war, as Arena (2008) has found to be true in the postwar era. Anticipation of this effect makes the government more willing to quit when the opposition initially threatens to oppose the war, but makes the government more sensitive to failure once opposed. In this chapter, I explore both the impact of the opposition’s power to politicize a war if they so desire on the government’s strategy during war as well as the conditions under which the opposition is expected to do so.

First, I motivate the assumptions of the model with a brief discussion of the extant literature on effects of opposition behavior on international conflict. Second, I present a simple model of war-fighting, devoid of any consideration of domestic politics. I then introduce a series of extensions of the basic model, revealing the changes in optimal government strategy under various domestic political contexts compared to the behavior in the model without any domestic opposition.

3.1 The Role of the Opposition

Like others, I assume that the opposition has mixed motives in an international crisis. On the one hand, the opposition values attaining office, and therefore has an incentive to capitalize on the government’s failures, while at the same time valuing the national interest (Schultz 2001, Ramsay 2004). This assumption implies that the opposition will neither always uncritically support the government, passing up an opportunity to translate failed for-

¹The question of who leads and who follows is not a simple one. Zaller (1992) is largely agnostic on the matter. Berinsky (2007) places stronger emphasis on elites.
eign policies on the government’s part into electoral gain, nor blindly oppose all government actions in hopes of eroding popular support for the government and therefore potentially sabotaging foreign policy. An opposition that values both the national interest as well as office-holding will instead pursue a policy of seeking to avoid being on the "wrong side of history".

What this means is that the opposition is forward looking, and wants to have been on the record as having supported those policies which are expected to prove successful and been on the record as having opposed those policies which are expected to prove unsuccessful. To vote in favor of a war that drags on without clear signs of success or against a war that quickly proves to be a resounding success can spell political suicide. Further, anecdotal evidence suggests it may not always be enough to avoid being on the wrong side of history - it may matter how early a politician articulated their position. One interesting characteristic of the 2008 U.S. presidential primaries is Barak Obama’s attempts to use Hilary Clinton’s vote authorizing the use of force in Iraq prior to the war against her, referring to her at one point as “Bush-Cheney lite” (NYT, 7/27/2007). As the Clinton campaign has emphasized, and Obama has not disputed, the differences in terms of current positions on Iraq amongst the leading candidates are negligible.

As the debate amongst the leading candidates for the Democratic nomination illustrates, position-taking has important implications for electoral outcomes. The opposition need not believe their pronouncements will have any effect on policy whatsoever, as they may choose to oppose purely out of purely electoral considerations. Schultz states that, “by going on record as opposed to the conflict, the opposition politicizes the issue and positions itself to exploit the outcome in the next election,” (2001, 80). Gartner, Segura and Barratt (2004) analyze U.S. Senate elections from 1966-1972 and find that incumbents’ and challengers’ positions towards the Vietnam War had an important impact. As discussed in the previous chapter, Arena (2008) demonstrated that war outcomes, good or bad, impact the govern-
ment’s probability of retaining office more so when the opposition opposed the war than when they supported. These results suggest that the opposition has no incentive to systematically undermine the government’s efforts in all crises, or those that are especially likely to be popular, in order to detract from the government’s ability to use the war to its advantage, as has been argued by others (Levy & Mabe 2004). Put simply, the government decides whether or not to go to war and the opposition decides whether or not the war will be a prominent issue in the next election. This may make the opposition’s importance appear to be humble. Indeed, this characterization almost certainly depicts a more restricted role for the opposition than exists in practice, at least some of the time. Yet the analysis in this chapter illustrates that even this limited function can have profound implications for the conduct of war.

One might alternatively argue that the effect of the opposition’s behavior is always to bias the public in their direction. In this view, even when the government is prevailing on the battlefield, it prefers to have the opposition support rather than oppose, lest the criticism erode public support despite their battlefield successes. One might argue that this is the view of opposition behavior held by such politicians as Richard Nixon and Dick Cheney - that opposition always undermines a government’s ability to prosecute a war successfully. The obvious objection to this view would be that elite discord appears to as an informational cue and facilitates the rejection of messages that are at odds with one’s underlying political predispositions, but does not encourage those whose predispositions are at odds with the opposition to accept their critique of government policy (Zaller, 1992). The lack of evidence in support of any direct effect of opposition positions on electoral outcomes noted elsewhere (Arena, 2008) suggests that wars do not automatically become political liabilities for governments simply because the opposition criticizes them. Only in conjunction with failure on the battlefield does opposition threaten the government. Therefore, I conclude that the effect of opposition is not to discredit government policy, but merely to offer the
public differentiation on a salient issue of public policy.

As discussed in the previous chapter, previous research on the role of the opposition has considered the impact of position-taking at the onset of a crisis (Schultz, 2001). Yet Schultz notes, “of course, bargaining does not end once a war starts, and a natural extension of the work here is to consider how domestic political factors influence states’ assessments during wars,” (2001, 69). While his formal model predicts that crises should not escalate to war in the presence of opposition support for the government, and indeed he finds opposition support does decrease the probability of escalation in a sample of cases of extended immediate deterrence, nonetheless we do observe wars that begin despite open support from the opposition. In fact, it is quite common for the opposition to initially support the government only to withdraw their support if the war drags on without sufficient progress. Consider the behavior of the Democratic Party in the U.S. during the Vietnam War. The Gulf of Tonkin Resolution passed 416-0 in the House and 88-2 in the Senate (NYT, 8/8/1964) only to be repealed by only a slightly slimmer margin of 81-10 six years later (NYT, 6/25/1970). In this case, as in the current war in Iraq, the war commenced under near unanimous support but eventually saw widespread criticism from both parties.

The foregoing discussion highlights the impact of disagreement among partisan elites on the prominence of an issue electorally. It also illustrates the possibility of the opposition changing its position in the middle of a conflict. These two factors play an important role in the subsequent analysis. This will be evident when we compare government strategy in the subsequent models of war fighting.

### 3.2 Modeling War Fighting

Before considering the effect of opposition behavior, I will first develop the optimal strategies of the government and the target state in a purely international context through a simplified
model of war fighting. Consistent with the burgeoning literature on war as a continuation of bargaining discussed in the previous chapter, I do not model war as a one shot event. Rather, I model wars as consisting of a series of engagements, or battles.\textsuperscript{2} This structure is similar in many respects to that of Smith (1998). However, unlike Smith’s model, the number of battles that may take place in this model is infinite. In Smith’s model, states battle over “forts”, gaining or losing control in accordance with battle outcomes. If one nation captures all the forts, the war is over. In my model, so long as the two participants both continue to fight, the war proceeds — “victory” does not exist in this model as an exogenous state. The war can only end when one or both sides decide to quit fighting.\textsuperscript{3} The outcome of the war is not arbitrarily divided into discrete states such as “victory”, “defeat” and “draw”. The players extract payoffs after each battle commensurate with their performance in the war to date. These payoffs range from \(-1\), in the extreme case where the state has lost every battle that has taken place so far, to 1, where the state has won every battle that has taken place so far.\textsuperscript{4}

\textbf{3.2.1 Basic Structure of the Model}

The Basic War Fighting Game is a random-walk model, where the key element is a random variable, known as the state variable (Smith, 1998). In this model, the “state” variable,

\textsuperscript{2}I use the term battle because it is common in the literature and has an intuitive interpretation. However, it is worth noting that the model does not require that in each round the two states deploy formal armies to engage in organized military contests taking place upon battlefields which are separated from civilian population centers. Each battle could alternatively be conceived of as a fixed time period.

\textsuperscript{3}I do not wish to overstate the importance of this difference, however. A nation which has lost ten battles in a row and has no expectation of winning the eleventh is unlikely to choose to continue fighting in this model. The equilibrium strategies of this model are not substantively dissimilar from those in Smith (1998). However, the more general structure simplifies the analysis, which becomes particularly important in the subsequent variants of the model where elements of domestic politics are introduced.

\textsuperscript{4}For simplicity, the outcomes of battles are divided into discrete outcomes. Theoretically, battle outcomes could vary continuously over the full range from \(-1\) to 1 as well. However, allowing for this possibility would complicate the solution considerably. I believe that modeling discrete battle outcomes which aggregate into war outcomes that can take on a greater range of values strikes a favorable balance between simplicity and flexibility.
denoted $w_t$, depicts how favorable the course of the war has been to the challenger to date. This equals the average of all prior battle outcomes, where the outcome of a battle is equal to 1 with probability $p$ and equal to $-1$ with probability $1 - p$. If no battles have yet taken place, and the two states are deciding whether or not to engage in a conflict, the current value of $w_t$ would be 0. The number of possible values of $w_t$ varies with $t$. When $t = 0$, $w_t = 0$. When $t = 1$, $w_t$ can only equal either $-1$ or 1. In the limit, as $t \to \infty$, $w_t$ becomes a continuous variable.

Player 1 is referred to simply as the Challenger in the basic game, where both states are unitary actors, but referred to as the Government in subsequent variants of the model, which incorporate domestic politics. Player 1 seeks to maximize $w_t$ while the Target state seeks to minimize it. If at any point one player quits fighting while the other chooses to continue, that player is said to have conceded and the value of the war, $w_t$, goes to 1 or $-1$, depending upon which player quit. If both players quit, the war is said to have ended in a negotiated settlement, and payoffs are determined by $w_t$.

The players' strategies are decisions to fight another battle or to quit in any given period. Such strategies are Markov strategies because the players' decisions are independent of the history of play, except insofar as prior play determines $w_t$, a key component of the players' utilities. In other words, players' strategies are determined by the current state of the war, not how it came about (Smith, 1998).

The payoffs for the Basic War Fighting game are straightforward. If both the Challenger and the Target choose to quit rather than fight another battle, their payoff for this period reflects the course of the war to date, $w_t$ for the Challenger and $-w_t$ for the Target. Since the payoffs are extracted in each period, the Challenger and Target incur no additional costs besides those that they would have paid previously for battles fought in the past. If both players choose to fight another battle, they pay some costs. The per-period costs of fighting are denoted as $c_a$ for the Challenger and $c_b$ for the Target. Further, since another battle
will take place, the value of the war is updated to reflect the outcome of this period’s battle, replacing \( w_t \) with \( w_{t+1} \). Therefore, the net utility to the players if both choose to fight is \( w_{t+1} - c_a \) for the Challenger and \( -w_{t+1} - c_b \) for the Target.

As should be apparent, the difference between \( w_t \) and \( w_{t+1} \) diminishes over time. The more battles that have been fought already, the less an additional battle can sway the overall course of the war. In the limit, as \( t \to \infty \), \( w_{t+1} \) converges to \( w_t \). Irrespective of \( t \), if \( w_t \) is negative, \( w_{t+1} \) cannot exceed 0. To see this, consider the case where \( t = 1 \), where the marginal effect of fighting one more battle is very high. If the Challenger lost the first battle, \( w_t \) equals -1. If the Challenger wins the current battle, \( w_{t+1} \) equals 0. As \( t \) increases, the number of consecutive victorious battles required to transform an unsuccessful war campaign (\( w_t < 0 \)) into a successful one (\( w_t > 0 \)) increases.\(^5\)

If the Challenger chooses to fight but the Target chooses to quit, the Challenger still pays some costs even though no further battles take place. The costs of fighting are not necessarily only the human costs suffered in terms of casualties. Wars also cost money, required to equip and transport troops. Since the model requires players to select their strategies simultaneously, a player that mobilizes for continued fighting only to discover their opponent is willing to lay down arms has nonetheless incurred the costs involved in transporting troops, producing ammunition and other materiel that went into production before it was known that it would not be needed. Costs would also presumably be incurred rounding up the surrendering troops, ensuring that they have disarmed, and so forth. Therefore, the payoffs to each player if the Challenger chooses to fight while the Target chooses to quit are \( 1 - c_a \) and \(-1\), respectively.

\(^5\)This simple observation has important implications. Any explanation of war that hinges on uncertainty implies that wars serve the purpose of revealing information, and the probability of a war ending increases with each battle fought, as more information is revealed (Slantchev, 2003b). The same duration dependence is implied by this basic model, where uncertainty is absent, at least in the sense of private information. I will discuss the issue of duration dependence more in the following chapter.
3.2.2 Optimal Strategy in Basic War Fighting Game

In this section, I briefly sketch the optimal play of the Challenger. Appendix 1 contains a formal solution. I focus here on the basic intuition behind the Challenger’s optimal strategy when the Target is known to be resolute (will fight in this period and the next, regardless of the Government’s strategy), which will serve as an important basis of comparison for the strategies in the subsequent variants of the game that incorporate domestic politics. For ease of comparison, I discuss all the results in terms of the costs each side is willing to bear to continue fighting.

Given the structure of the game, it is inappropriate to discuss strategies in any given round in isolation from the anticipated payoffs of future rounds. For example, if the probability of winning any given battle is arbitrarily high, but short of 1, with some probability the Challenger will lose the first few battles. If we focus only on the payoffs from the current round of play, we will find that the level of costs the Challenger is willing to tolerate to fight a third battle to be considerably higher than we would if we consider the Challenger’s discounted expected payoffs several rounds into the future, which would likely include several victories. When the Challenger has lost every battle that has taken place so far, there is no difference between the value of the war so far, $w_t$, and the value of conceding, $-1$. In the limit, as $t \to \infty$, the Challenger expects the value of $w_t$ to converge to $2p - 1$, where $p$ is the probability of winning any given battle. Therefore, our unfortunate hypothetical Challenger who enjoys a very high $p$ but has nonetheless fared poorly so far due to nothing but sheer bad luck expects her fate to improve considerably if she fights long enough. However, if we only consider the impact of one more battle, we may find the Challenger unwilling to continue fighting.

However, the more future rounds we attempt to factor into today’s consideration, the more complicated the solution becomes, particularly since battle outcomes are stochastic.
This suggests that the added value of considering one more round of play is decreasing with each additional round considered in the solution. Analyzing the players’ payoffs given their decisions in this round and the next brings us closer to the correct answer than analyzing the players’ payoffs while only considering the current round of play. Analyzing the players’ payoffs given their decisions two rounds into the future is even better, but the difference is smaller, and the increase in complexity is considerable. I proceed by analyzing players’ payoffs considering their behavior in both the current round and the next. I contend this approach strikes an appropriate balance between rigor and tractability.

In particular, I consider four possible strategies the Challenger might adopt. The first is to fight in this round and the next, regardless of the outcome of the next battle. I denote this strategy \((FF)\). The next strategy I consider is to fight in this round, but only fight another battle after that if the next battle produces a victory, quitting otherwise. Call this strategy \((FF/FQ)\). I also consider the strategy of fighting in this round and then quitting in the subsequent round regardless of the outcome of the next battle, which I denote \((FQ)\). Finally, I compare the utilities of these strategies to the obvious alternative, which is to quit immediately \((Q)\).

Figure 2 depicts the Challenger’s optimal strategy by the per-period cost of fighting (see appendix for derivation of cutpoints). When the costs of fighting are very low, the Challenger will choose to fight in this round as well as the next, regardless of the outcome of today’s battle. That is, the Challenger’s optimal strategy is \(FF\). As the costs of fighting increase, the Challenger’s optimal strategy changes to \(FF/FQ\), then \(FQ\), then finally \(Q\). In other words, the more costly fighting becomes, the less willing the Challenger is to keep fighting. This result should be very intuitive.

The Basic War Fighting Game tells us little about strategies in war that the literature has not already addressed. However, it is worth noting that this very basic framework that I use as my starting point before introducing complications intended to represent domestic
political considerations yields insights essentially identical to previous work. In particular, the structure and optimal strategies of the Basic War Fighting Game are very similar to those found in Smith (1998). Having demonstrated that the foundation upon which I build conforms to standards in the literature, I now turn to variants of the Basic War Fighting Game that incorporate elements of domestic politics, reflecting the behavior of the opposition. The differences in optimal strategies for the Government in these subsequent variants of the game from those of the Challenger in the Basic War Fighting Game allow us to draw straightforward inferences about the effect of the behavior of the opposition.

3.2.3 War Fighting Game with Opposition Supporting to Date

The first extension to the Basic War Fighting Game that I consider introduces a domestic Opposition that has supported fighting the war to date. In the case where $t = 0$, the Opposition will technically not have taken a previous opposition, so it is perhaps more correct to say this variant of the model includes an Opposition that has not yet opposed the war but potentially may do so. However, as I assume that war, once politicized, cannot be un-politicized, the previous description is sufficiently accurate so long as $t > 0$. Since the more accurate language is somewhat cumbersome, I will refer to this variant as the War Fighting Game with Opposition Supporting to Date.

Consistent with the discussion at the start of this chapter, the Opposition cannot affect the course of the war directly here. Rather, through choosing whether between publicly calling on the Government to fight or to quit, the Opposition has the power to alter the Government’s payoffs and thereby influence their strategy. The assumption here, consistent with the extant literature, is that disagreement among elites is associated with politicization.

\footnote{However, one might argue that if the dominant parties had previously disagreed over the conflict but at some point reach agreement, it is in fact possible for a war to cease being politicized. See Berinksy (2007) for an interesting discussion of how this might in fact have been the case in the US regarding World War II. I will return to this point later.}
and thus polarized public opinion.

Whenever the Government plays the opposite strategy from what the Opposition called for, the value of the outcome of the conflict is amplified for the Government and discounted for the Opposition. This is reflected by two additional terms in the payoff structure of the model. When the Government chooses to fight (quit) when the Opposition plays “quit” (“fight”), the Government’s payoff is multiplied by $\beta$, a constant whose value lies between 1 and 2. The Opposition’s payoff is multiplied by $\rho$, a constant whose value is bounded between 0 and 1.

As a consequence, if the Government quits fighting in the same time period that the Opposition first called on them to do so, the conflict is not politicized. If this appears to be giving the Government a pass too easily, consider the case where $t = 0$. That is, no fighting has yet taken place, but there is an underlying conflict of interest between the two states, and the Government is contemplating the use of force. Suppose further that such a hypothetical military engagement is widely expected to be a failure, or at best, a victory at unacceptable costs. If the Opposition declares that they would not support such an action, and the Government wisely opts not to employ force, would this near-crisis polarize public opinion? Project this logic out, and the Government should only be extra sensitive to war outcomes if they pressed on after the Opposition publicly declared that they would not support continuation of the conflict. The Opposition is assumed here to lack the power to retroactively denounce the initial decision to go to war, thereby holding the Government accountable for its past decisions regardless of whether the Government heeds the Opposition’s call to cease the war at that time. The implications of this assumption are not trivial, so it is important to be clear.

If the Opposition supports a war initially but declares after several battles that they are withdrawing their support and the Government simultaneously concludes that the war is not worth fighting and quits, the model assumes the Government is no worse off than
our unitary-actor Challenger would have been for embarking on a failed foreign venture. That is not to say that the model assigns a particularly desirable payoff to the Government. Indeed, this scenario is only expected to occur when \( wt \) is negative and the costs of fighting are not negligible. This is not a scenario the Government desires to find itself in. All that I have assumed is that the Government would have arrived at a bad outcome in such a scenario, but not as bad as it would have been had the Opposition announced after the previous battle that it would no longer support the war rather than announcing that it is ceasing its support at the same moment that the Government arrives at the same conclusion about the desirability of continuing the war. To reiterate, the outcome of the conflict is only magnified (discounted) for the Government (Opposition) when the Government’s strategy is the opposite of what the Opposition called for. In all other respects, the players’ utilities in this variant of the model are identical to those in the Basic War Fighting Game.

I will delay discussion of the Government’s optimal strategy until after I have briefly discussed the payoffs in the next variant of the game, where the Opposition has already opposed continuing the conflict and no longer has any moves left open to it. I will then discuss the effect of the Opposition’s position on the Government’s strategy.

### 3.2.4 War Fighting Game with the Opposition Opposed

The next extension to the Basic War Fighting Game that I consider removes the domestic Opposition as a strategic player, assuming they have already made their one and only move by advocating an end to the conflict, yet the war has continued. In the case where \( t = 0 \), the Opposition cannot have taken a previous opposition, this variant of the game cannot begin any earlier than \( t = 1 \).

The payoffs to the Target, as before, remain unchanged. The payoffs to the Government are similar to those in the Basic War Fighting Game, except that here the Government’s
payoff is magnified by $\beta$ regardless of its behavior. This game is not played until the Government chooses to fight after the Opposition advocates quitting, so the payoffs here reflect the notion that the war has become politicized and that cannot be undone. Whether the Government ends the war today or tomorrow, the war’s outcome will be influential in the next election.

Later in the chapter, I will discuss these assumptions further, and what the implications might be of extending this model in the future to relax them. For now, I proceed to discuss the effect of the Opposition’s behavior on the Government’s strategy implied by the War Fighting Game with the Opposition Supportive to Date and the War Fighting Game with the Opposition Already Opposed.

### 3.2.5 Effect of Opposition Behavior on Government Strategy

I will illustrate the effect of the Opposition’s position on the Government’s willingness to continue fighting the war in two ways. First I will present a graph of the optimal strategy by the per-period costs of fighting by Opposition position. This graph is similar to that in Figure 2. In fact, the results from Figure 2 are reproduced on the top line of Figure 5. I will then discuss a hypothetical scenario and present the expected utilities to the Government (Challenger) for each of the four main strategies discussed here over time under this scenario. Presenting evidence that the behaviors I claim to follow from the model as a rule operate under one specific set of arbitrarily chosen values for the parameters of the model does not constitute a proof of these claims. Nor is it intended to. A formal proof of the key propositions is presented in the appendix. However, I hope that a detailed discussion of a scenario where these effects are evident will assist the reader to grasp the argument more clearly.

Figure 5 presents the formal results, reporting the optimal Government (Challenger)
strategy as a function of the per-period costs of fighting. Again, this figure illustrates the conditions under which the Government would play certain strategies given that the Target is expected to continue fighting regardless of the Government’s strategy. The first line is identical to Figure 2, illustrating the Challenger’s optimal play in the Basic War Fighting Game. The second line depicts the optimal strategy of the Government when the Opposition is supportive in the War Fighting Game with the Opposition Supporting to Date. Formally, this means the Opposition plays “FF”, calling on the Government to fight in this round and then doing the same in the next round. The third line represents the optimal strategy of the Government when the Opposition first advocates quitting. This occurs when the Opposition plays “quit” this round, and then the following round, play moves into the War Fighting Game with the Opposition already Opposed. The fourth line illustrates the Government’s strategy when they are opposed. This means the Government and Target are already playing the War Fighting Game with the Opposition already Opposed. The dashed lines highlight the shifts in the threshold values of ca that separate the optimal strategies as we move from one Opposition position to the next.

As we move from the top line (a) to the second line (b) we find that a Government with an Opposition that advocates a hardline strategy of continuing to fight regardless of the outcome of the next battle is more willing to continue unconditionally and less likely to quit. That is, the first region, where the optimal strategy is FF, has grown. The region in which FF/FQ, or continuing conditional on victory, remains the same but shifts up the line. The region in which the Government quits immediately shrinks. This leads to the first key proposition, the formal proof of which is reserved to the appendix:

Proposition 1. Given that the Target plays (FF), a Government with an Opposition that advocates continued fighting (“FF”) is more likely to prefer (FF) to (FF/FQ) than a unitary-actor Challenger in the Basic Game.
Note that comparisons to the unitary actor Challenger are purely for theoretical purposes. It is not clear, if we live in a world where leaders are the primary political actors rather than states, how one would compare the behavior of a democratic leader whose opposition advocates fighting to a unitary actor. Nonetheless, it is interesting to make such a comparison theoretically, as the debate between realists and liberals has historically centered on a realist view of the world as conflictual and a liberal view of the world as cooperative. This proposition demonstrates that insofar as liberalism is defined by its adoption of an alternative unit of analysis and consideration of substate actors, the view that liberalism implies greater cooperation and less conflict does not always hold. In order to evaluate these propositions, I will be forced to compare the behavior of the government given a certain opposition position only to the behavior of the government given a different opposition position, without being able to evaluate these behaviors in comparison to that of a unitary actor. Nonetheless, I offer the more general language to emphasize that shifting theoretical focus from self-interested states in an anarchic system to a world where domestic political incentives can alter behavior does not always produce more peaceful outcomes (as in the case of the democratic peace) but can produce different implications from realism in a variety of ways.

The effect on the Government’s strategy when the Opposition initially advocates quitting is less straightforward. We see in Figure 5 that as we move from the second line (b) to the third line (c), the region characterizing an (FF) strategy shrinks relative to a Government with a supportive Opposition, but this region remains larger than it would be for a unitary-actor Challenger. However, we also see that the region associated with quitting immediately grows with respect to both a Government with a supportive Opposition and the Challenger in the Basic War Fighting Game. Thus, while noting the larger region corresponding with an (FF) strategy relative to the Challenger in the basic game, we see that when the Opposition first advocated quitting, the Government is by most measures less willing to fight. This leads to the following two propositions:
Proposition 2a. Given that the Target plays (FF), a Government with an
Opposition that did not previously oppose but now advocates quitting (“Q”)
is less likely to prefer (FF) to (FF/FQ) than a Government with a
supportive Opposition.

Proposition 2b. Given that the Target plays (FF), a Government with an
Opposition that did not previously oppose but now advocates quitting (“Q”),
is more likely to prefer (Q) to (FQ) than the unitary actor Challenger in the
Basic Game or a Government with a supportive Opposition.

Finally, when we move to the last line (d), we find that the Government’s willingness
to keep fighting unconditionally (FF), or only conditional on victory (FF/FQ) remain
unchanged compared to when the Opposition initially calls on the Government to quit.
However, the region in which the Government’s optimal strategy is to quit immediately
shrinks to the smallest on the graph. The intuition here is that if the Government is willing
to fight one more battle when the Opposition first advocates quitting, then it should be
no more or less likely to fight one more battle once the Opposition is already opposed. In
either case, the Government is considering fighting a battle that will be politicized. The
real difference between having the Opposition already opposed to continuing the war versus
having a previously supportive Opposition withdraw its support is only evident in how
willing the Government is to quit fighting immediately. If the Opposition is only now voicing
its opposition to continued fighting, then the Government can safely quit fighting the war
today and avoid politicizing the conflict. But once the Government has found itself playing
the War Fighting Game with the Opposition already Opposed, there is no move that the
Government can make that would not involve the war being politicized. This leads to the
final two propositions:

Proposition 3a. Given that the Target plays (FF), a Government with an
Opposition that has already opposed continuing the war is as likely to prefer (FF/FQ) to (FF) as a Government with an Opposition that just begins to advocate quitting.

Proposition 3b. Given that the Target plays (FF), a Government with an Opposition that has already opposed continuing the war is less likely to prefer (Q) to (FQ) than any other Government (Challenger).

Taken together, these propositions suggest that Governments with Oppositions that demand they keep fighting or have gone on the record as wanting an end to the war are willing to tolerate greater costs in order to keep fighting than would states in an idealized world with no domestic political considerations. In contrast, a Government with an Opposition that had previously supported continued fighting but now withdraws its support and calls on the Government to quit fighting is more likely to quit the war than would a state in such a stylized world.

Though Figure 5 does not help to illustrate this, the appendix contains a proof of the following propositions:

Proposition 4. Holding the strategy of the Opposition constant, the Government is more likely to quit as \( t \) increases.

Proposition 5. The impact of the Opposition’s strategy on the Government’s strategy is strictly increasing in \( t \).

To better illustrate these general effects, I turn now to a brief discussion of a hypothetical scenario, where we shall consider what strategies the Government (Challenger) would adopt holding the context the same and varying only the position (and presence) of the Opposition.
3.2.6 Hypothetical Scenario

Consider a potential crisis between two states. The Target state is relatively evenly matched with the Challenger (Government) in terms of capabilities, strategy, resources, and other factors that might affect battlefield outcomes. Therefore, we expect the Challenger to win any given battle with probability 0.5. The per-period costs of fighting for the Challenger, relative to the issue at stake, are 0.35. The Challenger values future battle outcomes considerably, but not quite as much as current ones. Therefore, we will discount the payoffs of future rounds of play at a rate of 0.8. Domestic political considerations are important, but not predominant. Therefore, the Government’s payoff is magnified by a modest 1.2 if the conflict becomes politicized. These values were chosen to allow for a readily observable impact without stacking deck in favor of domestic political concerns. Numerous other examples were considered (not shown). As might be expected, the lower the cost term, the smaller the impact of a change in the opposition’s position, while the effect is more pronounced for higher cost terms.

With the benefit of hindsight, we as observers know that despite their relatively even capabilities, the Challenger will suffer the misfortune of losing the first three battles (if she chooses to fight that many), after which regression to the mean will kick in and the Challenger will win the fourth battle and every even numbered battle thereafter. However, the Challenger only knows the probability with which it can expect to win any given battle - it does not know before the conflict begins that it is fated to lose the first three engagements.

What would the decision-making of the Challenger look like in this scenario? Figure 6 illustrates the expected utility of adopting each of the four primary strategies I have considered (FF, FF/FQ, FQ, and Q) for the Challenger in the Basic War Fighting Game in each round through the ninth, given the conditions outlined above.

Early in the conflict, the Challenger alternates between preferring (FF) to (FF/FQ),
with the initial edge going to \((FF)\) but quickly switching to \((FF/FQ)\) for the next two rounds. That is, with initial optimism running high, the Challenger would be advised to fight unconditionally. After the initial setbacks (improbable though they were), the Challenger must consider switching to a strategy of continuing only if chance shines on her in the next battle. By the third round, the Challenger becomes indifferent between pressing on contingent upon victory and immediately quitting. As the tide of battle evens out, the Challenger’s best strategy returns to unconditionally fighting.

Figures 7 and 8 illustrate the relative advantage of fighting unconditionally and quitting immediately over the strategy of fighting conditional upon victory, by the position of the Opposition (when there is one).

Here, the story gets more interesting. We see in Figure 7 that only a Government with an Opposition that never wavers in its pressure on the Government to keep fighting would never prefer \(FF/FQ\) to \(FF\). Despite suffering several initial losses, the Government always prefers to keep fighting, regardless of the outcome of the next battle, due to the electoral punishment they would expect to suffer if they chose to concede victory to the Target when the Opposition called on them to keep fighting.

Further, we see in Figure 8 that no Government (Challenger) ever prefers quitting immediately to a strategy of continuing conditional on victory \((FF/FQ)\) unless the Opposition announces in the outset of the third period that they would not support fighting after having lost the first two battles. The Challenger, a unitary-actor not subject to the pressures of domestic competition, is strictly indifferent between \(FF/FQ\) and \(Q\) in the third period, while a Government that has had consistent support from the Opposition or a Government whose Opposition withdrew its support earlier never lose their preference for continued fighting.

This example illustrates the general points established above. Under admittedly arbitrary (but not necessarily unrealistic) values of the parameters of the model, we found that the behavior of the Opposition would make the difference between whether the Government
continued the war or not. I did not even assume the Government was at any meaningful military disadvantage during the conflict - only that they suffered natural setbacks that could easily occur due to random chance.

3.3 Implications

The analysis in this chapter has several important implications, both for the literature on international conflict and for the study of domestic politics. I have argued that a potential resolution to the apparent tension between the claims that uncertainty is the primary explanation for war amongst rational actors and that democratic institutions promote transparency, thereby reducing uncertainty over the government’s preferences, is to focus on the lingering potential for uncertainty over cost tolerance even where there is no doubt about a state’s willingness to use violence. I have not formalized this claim. Rather, I took a step back and sought to demonstrate that, under a set of assumptions about the operation of domestic politics fully consistent with the extant literature, it necessarily follows that domestic political competition affects a state’s cost tolerance during conflict. Having established a mechanism by which a state’s long term resolve might change while a conflict is ongoing, I hope to have convinced the reader that I have also identified a potential source of uncertainty to focus upon in constructing such a claim.

It is important to note that several authors question the importance of uncertainty in explaining conflict.\(^7\) If these critics are right, it does not undermine the substantive conclusions of the analysis here. It certainly suggests that the motivating puzzle - how to reconcile the explanatory power of uncertainty, the uncertainty-reducing role of democratic institutions, and the recurrence of wars involving democracies - may not be in any particular need of solving. Two important points warrant discussion in this regard. First, some of the criti-

\(^7\)See Slantchev, 2003a; Powell, 2006; Slantchev and Levontoglu, 2007; and Wolford, Carrubba and Reiter (draft) for several compelling arguments about the overstated importance of uncertainty.
isms raised by these authors do not rule out the importance of uncertainty. Powell (2006) argues that of the three explanations Fearon (1995) proposed, commitment problems have inappropriately been overlooked, but does not suggest uncertainty is unimportant. Second, while I believe it is important to be able to demonstrate that the recurrence of wars involving democracies need not present a challenge to the power of uncertainty as an explanation for war, the primary claim in this chapter is simply that domestic politics affects long term resolve (conceptualized as cost tolerance). If it is true that the study of conflict is best served by moving away from a myopic focus on uncertainty, the claims made here regarding the importance of the behavior of the opposition remain. Thus, the results might serve to construct a rationalist explanation of war that is in accordance with the (currently) most popular rationalist account, but the conclusions reached here do not rest upon the importance of uncertainty. Whatever the role of uncertainty in rationalist explanations for war, I contend that the behavior of the primary opposition party(ies) in democratic states remains an important and under-theorized phenomenon in international politics.

One question that does remain if one is to use the analysis here to build up an explanation of war that centers on uncertainty is why the Target state would be uncertain over the long term resolve of a democratic state. I have assumed that the behavior of the Opposition can change over time, and pointed to anecdotal evidence of such. I have demonstrated that changes in the behavior of the Opposition have important implications for the Government’s behavior. But one might well counter that there is no reason that the Target would be unable to forecast the behavior of the Opposition if we are comfortable with assuming they are able to forecast other aspects of play. If the Opposition is opportunistic and its strategy is dictated in a straightforward manner by electoral considerations, the Target should have little reason to be uncertain as to what the Opposition’s future play will be.
3.4 Extension: Ideological Opposition Parties

In a stylized world where all types of voters are equally likely to turn out, preferences are uniformly distributed, and parties are unitary actors devoid of coordination problems, efficiently pursuing their goal of attaining office, such a critique would be powerful indeed. Indeed, in the War Fighting Game with Opposition Supporting to Date, two important propositions regarding the Opposition’s behavior must be considered. Again, formal proofs are reserved to the Appendix.

Proposition 6. Given that the Target plays (FF), the Opposition never advocates quitting (“Q”) when the course of the war is favoring the Government \((w_t > 0)\) in the War Fighting Game with Opposition Supporting to Date.

Proposition 7. Given that the Target plays (FF), the Opposition never advocates quitting (“Q”) if the Government is expected to continue fighting regardless of the Opposition’s decision.

These two propositions tell us that the Opposition’s behavior is very easy to forecast. They only oppose losing wars, and withdraw their support when they know that by doing so, they will prompt the Government to quit fighting. This suggests that there is very little reason for the Target to be uncertain over whether and when the Opposition would withdraw their support.

However, recent works on party strategy have emphasize the disproportionate importance of party activists either in turnout decisions or in the provision of the resources vital to mounting an effective campaign (Mueller 2003, Adams, Merrill & Grofman 2006, Schofield & Sened 2006). The adoption of those policies which might maximize a party’s appeal to the mass electorate may not be optimal strategy, since candidates need the support of their core constituents, whose preferences tend to be well away from the center, in order to be
viable. This suggests that the decision calculus of the Opposition, which I have ignored thus far, may not be determined by seeking to influence the Government’s decisions, or avoiding being punished by purely retrospective voters.

I will now briefly consider the effect of introducing a parameter rewarding the Opposition for adopting a position in line with the preferences of its partisan constituents, independent of the Government’s and Target’s strategies. While moderate voters might simply want the Opposition to be on the right side of history, we should not ignore that some parties will be particularly beholden to activists who ideologically oppose (or support) wars with greater probability as a matter of principle, regardless of the individual merits of this particular war in terms of its costs and probability of victory. As discussed in the previous chapter, there are good reasons to believe that partisanship plays a role in the decisions of some voters, while others might vote based more upon the record of policy performance under the incumbent administration.

Figure 9 presents the War Fighting Game with Partisan Opposition. Here, the Government and Target receive the same exact payoffs as in the War Fighting Game with Opposition Supporting to Date. The Opposition’s payoffs are largely identical, save that the Opposition receives \( \pi \) anytime it says “quit”. This parameter ranges from -1 (representing extremely hawkish partisan constituents) to 1 (representing extremely dovish partisan constituents).

A set of important propositions follows directly from this variant of the game:

*Proposition 8a.* If the value of opposing a war (\( \pi \)) is sufficiently high, there are always conditions where the Opposition advocates quitting (“Q”) regardless of the Government’s and Target’s strategies.

*Proposition 8b.* If the value of opposing a war (\( \pi \)) is sufficiently low, there are always conditions where the Opposition advocates fighting (“F”) regardless of the Government’s and Target’s strategies.
Proposition 9. If the value of opposing a war ($\pi$) is sufficiently low, there are conditions where the Opposition will advocating fighting (“F”) even though this will result in worse outcomes for the Government.

These propositions imply that if the Opposition is sufficiently ideological, their strategy is expected to deviate from that outlined above. Hawkish Opposition parties support when they should advocating quitting and dovish Opposition parties oppose when they should advocate continued fighting. The implications of this possibility are not trivial. It might be very easy for the Target to deduce what strategy the Opposition will pursue if the Opposition is moderate. Yet it is less obvious that if the Opposition values satisfying their more extreme partisan constituents at the same time that it values appealing to moderates that the Target can predict which pressure will predominate and can thus anticipate the strategy of Opposition any number of rounds into the future.

The potential importance of partisan preferences over foreign policy in shaping the behavior of the Opposition, with or without the assumption of uncertainty, is considerable. I will briefly address the implications of political orientation on opposition behavior in the next chapter, but a full and more satisfactory treatment must await future work. The primary focus of the next chapter will be to assess the empirical validity of the propositions relating opposition behavior to the government’s willingness to continue fighting.

### 3.4.1 Future Extensions

As with any model, I have made a number of limiting assumptions here. Some, I am relatively comfortable with. Others warrant some brief discussion.

First, I have assumed wars can be characterized one two ways: politicized or not. While the size of $\beta$ might vary from one war to another, allowing us to distinguish cases where partisan disagreement has a pronounced effect versus a more limited one, this rules out the
possibility of within war variation. Once the Government chooses ignore the Opposition’s initial call to end the conflict and pushes forward into the Game with the Opposition already Opposed, the length of time the Government chooses to disregard the Opposition has no effect, nor does the model allow the Opposition to strategically vary the intensity of their opposition to continued fighting.

A natural extension of the work here would be to relax this assumption. One might either compound the $\beta$ and $\rho$ terms with each period that the Government continues. Or one might keep the Opposition as a strategic player even after the Government first disregards their demands, and only compound (or alternatively even remove) the $\beta$ and $\rho$ terms based on both players’ behaviors in future rounds. While such extensions might produce interesting results, I do not consider such effects here for two reasons. The first reason is theoretical, the second practical. Theoretically, I believe a strong case can be made that it is actually useful, if a bit oversimplified, to divide wars involving democracies into two camps: those that are politicized and those that are not. While I accept there is variation both within and across wars in terms of how intense the partisan conflict over the war is, I do believe that it would be very difficult for a party that has publicly condemned a war to switch back to supporting it if the tide of battle turns to the Government’s favor and as a result entirely remove the Government’s advantage over them on this particular issue. We might indeed expect the Opposition to attempt this, and thereby reduce the Government’s ability to use the war as a wedge issue at least to some extent, but so long as the Government would retain some advantage as a result of the Opposition’s previous statements, the substantive results here would largely remain unchanged. Further, I accept that the even once the Opposition has withdrawn its support for continuing the war, they yet face important decisions over how vigorously to press the issue, as can be seen with the U.S. Democratic Party from 2006 onward. However, I contend that the relative effect of maintaining a constant level of moderately intense opposition versus escalating to a more intense position is considerably
sufficiently small in comparison to the change from supporting a war to opposing it that the results outlined here would likely only have to be qualified, but not disregarded, if we modeled such continued debate appropriately.

Another simplification to which I expect the reader may object is that of a unitary Opposition. While I will discuss in detail in the subsequent chapter why this is not tantamount to assuming a formal two-party system, it does nonetheless assume that even if we are willing to speak of blocs rather than parties, that there is yet only one meaningful bloc of opposition parties. Though it would complicate the model considerably (hopefully not insurmountably), it could be worthwhile to allow for two strategic Oppositions, where both, one, or neither may advocate continued fighting. Such a more complex model would be particularly well-suited to addressing certain questions the current analysis cannot. If the Government is pressured by one Opposition to quit and one to fight, what effect will that have? Given that the analysis in this chapter found that facing unrelenting pressure from the Opposition to continue and facing public opposition for not having quit already both encourage the Government not to quit, might having both types of Opposition at once embolden a Government more, less, or essentially the same as having two Oppositions that have both opposed the conflict for some time?

Another important though potentially thorny extension would be to consider the effect of election timing, particularly endogenous versus exogenous election cycles. The current model assumes that at the end of the conflict, regardless of how distant the next election is, the war will have a similar effect. I also do not address the potential for elections during war. While that omission may not too severely threaten the basic results here since I have only considered the payoffs in any given period plus one period into the future, it is well worth considering whether the proximity of elections affects either the Government’s or the Opposition’s strategy, not to mention what effect it might have to consider the ability of the Opposition to hold a vote of no confidence and force an early election during a war.


3.5 Conclusion

I began with the expressed goal of demonstrating that the behavior of the primary opposition party(ies) can affect the willingness of the government to bear the costs of continued conflict. I believe I have succeeded in that regard. I first discussed the optimal strategy of a unitary actor in a simple iterated game, whose basic structure and general implications were quite similar to previous work. I then introduced aspects of domestic politics to determine how the opposition’s position would affect optimal strategy. I presented the effects of the opposition’s behavior on optimal strategy both as a general rule, and in a hypothetical case with arbitrary but plausible conditions, where it was demonstrated that the opposition’s position would effectively determine whether the government continued fighting or not. I also discussed some of the limitations of the model and possible future extensions.

I will address other important issues in the next chapter, where I turn to empirically assessing these claims. There I shall consider such questions as what it means to say the war has favored the government to date and who the opposition is in much more explicit and concrete terms. I will also consider the implications of the findings, with particular attention to the opposition’s dilemma.
<table>
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Figure 1: Basic War Fighting Game
Figure 2. Monotone Markov Strategy for Challenger in Basic War Fighting Game.

0 | k_b | s_b | m_b | 1

- cost of fighting
- FF
- FF/FQ
- FQ
- Q
Figure 3: War Fighting Game with Opposition Supporting to Date

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Figure 3: War Fighting Game with Opposition Supporting to Date

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"Fight"

"Quit"
Figure 4: War Fighting Game with Opposition Opposed

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Figure 3.4.
Figure 5. The Effect of Opposition Behavior on Government Strategies.

From top (a) to bottom (d): Strategy in the Basic Game; Strategy when the Opposition supports; Strategy when the Opposition initially advocates quitting; Strategy when the Opposition is already opposed.
Figure 6. Payoffs for Hypothetical Scenario, Basic Game.

![Diagram showing payoffs for different EU periods](image-url)
Figure 7. Effect of Opposition on Value of Continuing Unconditionally vs Conditionally.
Figure 8. Effect of Opposition on Value of Continuing Conditionally vs Quitting

Difference in Expected Utility between Playing FF/FQ and Playing Q

Period (t)
Figure 9: War Fighting Game with Ideological Opposition

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“Fight”

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Chapter 4

Long Term Resolve II: Empirical Evidence

In the previous chapter, I developed a series of formal models that illustrated the impact of opposition behavior on long term resolve, the willingness to bear the costs of continued fighting. While I did not present a full solution, I derived implications from the models regarding the conditions under which the opposition is expected to withdraw its support for continued fighting and the effect this has on the government’s decision to keep fighting. The analysis suggests that the effect of the opposition’s position is contingent upon the duration of the war. The longer the war has endured, the more true each of the following statements becomes:

1) if the opposition advocates continued fighting, the government is more likely to keep fighting; 2) if the opposition withdraws its support and advocates quitting, the government is less likely to keep fighting; and 3) if the opposition withdrew its support in the past and the government is now fighting a politicized war, the government is more likely to keep fighting.

Further, absent ideological considerations, the opposition’s strategy is always to support when the war is going well for the government. However, the opposition does not necessarily
withdraw its support immediately in wars where the course of battle does not favor the government. The opposition waits until the government’s willingness to bear the costs of continued fighting is already wavering such that withdrawing support, and thus creating an extra incentive to end the war immediately, is likely to make the difference between quitting and continuing for the government. In some cases, this window of opportunity may be open at the very start of the war, and in such cases the opposition would indeed oppose immediately. But in other cases, the opposition will withhold its criticism of a losing war, biding its time for the moment in which switching their position will effectively alter the government’s preferences over continuing the war. However, this result implies that we should never observe wars where the government continues fighting despite continued opposition, which we clearly do. I discussed one possible source for suboptimal opposition timing: incentives to adopt positions that are not intended solely to alter the government’s behavior but also to appeal to the ideological preferences of partisan constituents. The model with ideological opposition parties suggests: 1) the opposition’s position on the war should be a function of the government’s performance, and increasingly so over time, but 2) we should nonetheless observe support for losing wars with positive probability and opposition to winning wars with positive probability.

Finally, I noted that while the government appears to be just as likely to keep fighting when the opposition supports the war as when the opposition withdrew its support some time ago and the war is now politicized, this does not mean the government expects equivalent benefits from continued fighting in each case. Rather, the high cost of conceding defeat once the opposition is opposed forces the government to continue. This suggests that while the probability that the government continues fighting when it enjoys the support of the opposition and the probability that it continues fighting when it is bitterly opposed may be similar, the expected outcomes of such wars need not be similar. If the opposition is partially motivated by ideological concerns, the outcomes of wars that enjoy unwavering
support from the opposition may not necessarily be strictly better than the outcomes of wars that the opposition opposed.

In this chapter, I seek to address the empirical validity of many of these propositions. While it is not meaningful to speak of “testing” or falsifying a model as a whole, given that all models are known to be inaccurate representations of reality to varying degrees, several of the implications derived from the formal analysis in the preceding chapter are novel or counterintuitive. If these implications are shown to be consistent with the empirical record, the models will have proven useful. Other implications are relatively intuitive, and demonstrating support for them will not itself do much to distinguish the approach advocated in the previous chapter from other approaches. However, I am unaware of an existing model or theoretical approach that could, within a single framework, explain all of the patterns identified in this chapter.

A number of simplifying assumptions were made to facilitate the derivation of these results. The outcomes of previous periods of fighting were assumed to have no effect on the outcomes of future periods of fighting, for example. While the models are more general, all of the propositions discussed were derived while assuming that the Target state is known to be willing to continue fighting indefinitely into the future. Again, I stress that it is not meaningful to observe that the assumptions of a model are likely to be false. The more relevant question is whether the model generates useful implications, and whether these implications either help explain known empirical patterns or motivate the discovery of previously unknown empirical patterns (Primo & Clarke 2007). In this chapter, I hope to demonstrate that the formal models developed in the previous chapter are potentially fruitful in both regards. All of the empirical investigations in this chapter lend at least qualified support to the implications of the models from the previous chapter, yielding new insights into the duration and outcome of wars involving democracies and the timing of reversals of opposition positions. The results of these two chapters also offer a plausible explanation to
the second puzzle identified earlier, the routine occurrence of democracies fighting prolonged wars despite public opposition and little hope for military victory. Finally, I hope to have persuaded the reader that while I have not offered a formal proof, the results here suggest that future work focusing on uncertainty over the opposition’s ideological preferences might succeed in offering an explanation of the first puzzle, the routine occurrence of wars involving democracies, even in the presence of opposition support (which should persuade the target state that the government’s threats to use force are not bluffs).

I proceed in four steps. First, I will discuss the method by which the formal propositions will be evaluated. I will address the translation of propositions into testable hypotheses as well as the data and methods used to evaluate the hypotheses. Second, I will present the results of the statistical analyses regarding the duration of war, the duration of opposition support during war, and war outcomes. This section provides evidence that in the post-War era, when democratic states have gone to war, the position of the primary opposition party(ies) has influenced government wartime decisions. The influence of the opposition’s position increases the longer the war endures. When the opposition advocates continued fighting, the war is more likely to continue. When the opposition first advocates quitting, the war is more likely to end. However, if the government continues fighting after the opposition publicly opposes, the effect of this opposition is to make the war more likely to continue. Opposition support for continuing the war is a function of the government’s performance in the war, contingent on the duration of the war. In other words, the less the war has favored the government, the less likely the opposition is to continue supporting the war, but how much outcomes on the battlefield effect the opposition’s decision depends upon how long the war has already lasted and not simply how poorly the war is going. I also demonstrate that the relationship between opposition position and war outcome varies by the length of the war. For very brief wars (as the majority of wars involving democracies tend to be), wars that enjoy opposition support are associated with better war outcomes. However, for
longer wars, a different pattern emerges, and I find that wars that ended in the month the opposition first opposed the war are associated with the best outcomes for the government, followed by wars that end without the opposition ever withdrawing its support, while wars that were politicized (the opposition expressed opposition to the war at least a month prior to the war’s end) are associated with the worst outcomes. In the third section, I present graphs illustrating the substantive effects. I close with a brief discussion of two illustrative cases: the 1982 Lebanon War and the 2006 Second Lebanon War. I argue that many of the relationships implied by the formal models and apparent in the empirical analyses can be seen at work in these two conflicts. Closer examination of these cases reveals important factors overlooked by the model, particularly the role of third parties in shaping regional conflicts. Despite external actors preventing the belligerents from behaving the way they might otherwise, there is still evidence of party competition shaping the dynamics of these two conflicts in ways that are consistent with the model yet problematic for existing theories.

4.1 From Formal Propositions to Testable Hypotheses

In translating the propositions derived from the formal models into specific hypotheses to be tested against empirical data, it is important to discuss exactly what is being tested and what we would learn from the confirmation (or lack thereof) of the individual hypotheses. Further, some of the parameters of the model can be measured in a relatively straightforward manner, while others are more abstract, suggesting a wide range of potential measurements could be proposed. In this section and the next, I motivate the specific research design choices made in subjecting the propositions to empirical assessment, discussing the logic behind each hypothesis and the data and methods used to evaluate them.
4.1.1 Duration of War

Proposition 1 posits a readily observable behavior, that support from the opposition lead the government to fight longer. However, the proof of this proposition made assumptions about the behavior of the target state, and “longer” refers to the behavior of a unitary actor, uninfluenced by domestic politics. The most appropriate way to convert this proposition into a testable hypothesis would be to condition the relationship between the opposition’s support and the government’s behavior on the resolve of the target state, and to directly compare the behavior of governments with supportive oppositions not just to behavior of democratic governments with opposition parties that staked out different positions, but also to governments uninfluenced by domestic political pressure. I address the difficulties of incorporating each of these restrictions in turn. I will then discuss what we stand to learn from observing empirical confirmation of the hypothesis to be tested.

Regarding the target’s willingness to continue fighting, various potential indicators of such a preference might be proposed. I have not chosen to include any such indicators. Indicators based on battlefield outcomes are the most obvious candidates, given that strategies in the formal model were discussed as a function of the costs of fighting relative to the expected outcome of fighting. While there are no data concerns preventing me from conditioning the relationship between opposition support and continuation of the war on the degree to which the performance of the war thus far has favored the target, in order to test the implication of Proposition 5, I must already interact the opposition’s position with a measure of time. The use of triple interaction terms is not standard, and requires the inclusion of a large number of base terms.\(^1\) With only 13 failures in the current data set, estimating a model with all the terms necessary to properly estimate the effects of a three-way interaction term

\(^1\)See (Braumoeller 2004) on the importance of including base terms when estimating models including multiplicative interaction terms
would be very difficult to defend. Thus, I choose to test a hypothesis more general than the proposition it represents. This is a demanding test, and an observed lack of support for this hypothesis would not allow us to conclude that no empirical support exists for the proposition. However, if the hypothesis is supported, it will increase our confidence in the proposition considerably. I therefore offer the first hypothesis:

\[ H1. \text{The effect of the opposition calling on the government to keep fighting (denoted “fight”) on the probability of the war continuing in any given month is positive and increasing over time.} \]

Note that Proposition 5 does not imply a separate hypothesis, but conditions the form of the relevant hypotheses required to test the other propositions. For this reason, I express the first three hypotheses, regarding the effects of the opposition’s behavior on the probability the war continues, as a function of time, incorporating the implication of Proposition 5.

Notice that this expectation is by no means unique to the theoretical approach advanced here, which emphasizes competition as a mechanism for inducing accountability but ascribes no further role to the opposition’s position beyond determining whether the government’s performance will be judged by the outcome of this policy. A theoretical approach that held that support from the opposition consistently increased public support for the government’s policy, regardless of its outcome, while simultaneously enabling the government to more effectively conduct its foreign policy, would also produce this hypothesis. However, a number of the other hypotheses I will offer would not be consistent with such an approach, particularly those regarding war outcome. Nor could such an approach account for the finding discussed in previous chapters that opposition position during war has no effect on the probability of government retention of office absent consideration of the outcome of the war (Arena 2008).

\[ ^2 \text{Nonetheless, I have estimated such a model. While I do not believe this is sound practice, to the extent the results of such a model are meaningful, the results do appear to be roughly consistent with expectations across wide ranges of values of the parameters, and virtually all of the variables attain conventional levels of statistical significance, including, notably, the three-way interactions. Results not reported} \]
Consider now Propositions 2a and 2b. As with the previous proposition, this set of propositions implies a readily observable relationship, but under a restrictive set of conditions. As with the previous hypothesis, I will offer a more general hypothesis to be tested than can be strictly derived from the formal propositions. Again, the motivation here is to avoid poor statistical practice by way of including a triple interaction term. Further, while the propositions establish that the government is less likely to prefer a strategy of unconditionally continuing and more likely to prefer a strategy of immediately quitting, the relative attractiveness of a strategy of conditionally continuing to fight is not necessarily effected by the opposition’s behavior. By including a lagged measure of the opposition’s position, it would be possible to determine whether there is a relationship between the withdrawal of support and the government’s decision to adopt a strategy of conditional continuation of the war. However, the model expects a lack of a relationship, and an appropriate test of the presence of such a relationship would be complicated, and would involve losing almost half of the cases (since 6 of the 13 wars to be analyzed lasted only 1 month). Therefore, I offer the following hypothesis:

\[ H2. \text{The effect of the opposition calling on the government to quit fighting} \]
\[ \text{(denoted “quit”) on the probability of the war continuing in any given month} \]
\[ \text{is negative increasing over time.} \]

As above, I note that this hypothesis is not itself inconsistent with other theoretical approaches. I have emphasized the assumption that the opposition sets the stakes of the war for the government by deciding whether or not to make it a politically active issue, but does not influence the government’s ability to fight the war. Clearly, if the opposition’s support was vital to securing victory, we would expect governments that initially enjoyed the support of the opposition but suddenly lose it to be more likely to quit fighting. However, this would not explain why the withdrawal of opposition only encourages the government to quit at
the time that the opposition first advocates ending the conflict, while subsequently, the probability that the government continues the war in any given month increases. The next proposition is therefore useful in distinguishing the competition as accountability approach advanced here from alternative conceptions of party competition.

Taken together, Propositions 3a and 3b suggest that a war that continues after the opposition first calls on the government to quit becomes less likely to end in any given month. However, it does not suggest that the value to the government of continued fighting is any higher, a point to which I will return later in the chapter. Again, the hypothesis offered to test these propositions ignores the restriction regarding the target’s willingness to fight. As with the other hypotheses, I am forced to assume that if the government is more likely to quit (fight), the war is more likely to end (continue), overlooking the obvious concern that wars involve two (or more) strategic actors. While controlling for which state “ended” the war would be potentially valuable, the limited amount (and quality) of data lead me to make this strong assumption that information regarding one state’s willingness to fight translates into expected duration in a straightforward manner. If this assumption is inappropriate (almost certainly true in at least some cases), it will only serve to reduce the probability of uncovering evidence in support of the hypothesized relationships. If the target state responds strategically to information about the government’s resolve, we might expect that conditions associated with an increased willingness of the government to continue fighting to be associated with shorter wars, albeit ones that end favorably for the government. Therefore, evidence that the governments fights longer but does not produce better outcomes when the opposition advocates continued fighting or has previously withdrawn its support (as per hypotheses H1, H3 and H6 and H7, which I will discuss shortly) will constitute strong evidence in support of the implications of the model.

\textit{H3. The effect of the opposition having withdrawn its support in a previous}
month (denoted opposed) on the probability of the war continuing in any given month is positive and increasing over time.

To my knowledge, existing approaches linking domestic politics to international conflict do not produce this expectation. While the possibility of incentives to “gamble for resurrection” have been previously identified (Downs & Rocke 1994, Goemans 2000), previous studies have assumed the probability of being removed from office as a result of policy failure is constant across time within a given regime. Downs and Rocke (1994) attribute US escalation in Vietnam against the wishes of the US public to the fear of removal from office for admitting defeat after fatalities crossed a certain threshold. Indeed, their model suggests that battlefield outcomes are the primary cause of gambling for resurrection, which is distinct from my hypothesis that gambling behavior is only encouraged if 1) the opposition withdraws its support for the war and 2) the government does not immediately respond to the opposition’s call for an end to the conflict. Goemans (2000) likewise believes gambling for resurrection is the only rational policy once the tide of battle grows sufficiently negative, but does not expect democratic leader to gamble for resurrection at all, instead arguing that only leaders of mixed regimes are prone to such behavior, which he argues explains the decision of Germany to continue fighting World War I long after the inevitability of defeat became obvious. While both studies have made important contributions to the literature and stand in contrast to more optimistic views of the relationship between domestic politics and international conflict, which tend to believe governments are hyper-responsive to the desires of their publics, neither approach would be able to account for the relationship implied by hypothesis $H_3$. Empirical support for this hypothesis then will contrast the approach here not only from theoretical approaches that assume the opposition’s position directly effects the government’s ability to fight, approaches that assume a high degree of responsiveness to public opinion in democracies, and approaches that attribute gambling for resurrection
primarily to battlefield outcomes. If the previous hypotheses enjoyed empirical support but $H3$ did not, it would cast serious doubt on the usefulness of the theoretical models developed in the previous chapter.

The last proposition related to the duration of the war is Proposition 4. In comparison to the previous propositions, this proposition lends itself well to translation from formal proposition to testable hypothesis. Put simply, this proposition produces the expectation that once changes in the opposition’s position are controlled for, we should observe positive duration dependence (indicated when the shape parameter, $p$, is greater than 1).³

$H4. As the duration of the war increases, the probability of the war continuing in any given month decreases.$

This is consistent with informational explanations of war (Wagner 2000, Slantchev 2003b), though contradicts previous empirical studies of war duration (Bennett & Stam 1996). These studies did not distinguish by regime type, however, and some scholars have argued that at least with regard to democracies, we should observe positive duration dependence (Zorn 2000). Therefore, while even the Basic War Fighting Game, which made no assumptions about the domestic politics of the warring states, implied duration dependence, we might expect to find evidence of duration dependence in this analysis simply because all of the wars involve democracies. Therefore, failure to observe empirical support for this hypothesis would prove problematic for the theoretical models developed in the previous chapter, but support for this hypothesis would not distinguish among the multiple potential explanations thereof. Such a result would in essence be over-determined.

³see Box-Steppensmeier and Jones(2004)
4.1.2 Duration of Support

These propositions all relate to the opposition’s decision to advocate quitting the war. Propositions 6 and 7 assume the opposition party lacks ideological motivation. They imply that the opposition only opposes losing wars (though not necessarily all losing wars), and given that they intend to oppose a losing war, they time their decision to ensure that withdrawing their support results in the government quitting. Propositions 8a and 8b state that sufficiently ideological opposition parties might deviate from the strategy outlined by Propositions 6 and 7.

I have not collected data on the ideological preferences of the opposition parties. This would prove a daunting but not impossible task. Various different data sets, using alternative approaches, measure the political orientation of parties. The difficulty for my purposes would be to determine which parties count as the opposition, whether and how to weight each party’s contribution to an overall measure of the opposition’s ideological position, and so forth. Using these data sets to ascribe a position to the government does not pose such a challenge, and in future work I intend to adapt the method proposed by Palmer (1990a, 1990b), extended in recent studies with co-authors (Palmer, London & Regan 2004, Arena & Palmer N.d.).

For now, I make a few simplifying assumptions about the distribution of the ideological preferences of parties within and across the national legislatures. I assume that sufficiently hawkish (dovish) preferences to induce an opposition party to support (oppose) continuing the war when the optimal strategy implied by Propositions 6 and 7 would be to do the opposite have occurred within the legislatures under observation here with probability arbitrarily greater than 0. In other words, I assume that at least in some cases, but not all, ideological preferences were extreme enough to satisfy the conditions of Propositions 8a and 8b. If true, the nonzero occurrence of observations where the government continues fighting despite the
opposition having withdrawn its support would be consistent with the model, yet opposition to war, when it does occur, would still be expected to be responsive to war performance in roughly the manner suggested by Propositions 6 and 7. Rather than never opposing winning wars, as would be expected if ideological considerations were either absent or sufficiently low, I expect opposition to more likely as war performance decreases and less likely as war performance increases. Further, assuming that for at least some subset of the opposition parties in some of the cases, altering the government’s behavior actually was a component of the opposition’s decision-making, I expect that the probability of withdrawing support is strictly increasing over time. Thus, I offer the following hypothesis:

\[ H5. \text{The effect of the government’s war performance on the probability that the opposition continues to advocate fighting in any given month is positive and increasing over time.} \]

Ideological preferences offer one potential explanation for the failure of opposition to universally be associated with war termination. Clearly, there are other possible reasons for this clear expectation of the model to fail to be bourn out empirically: the model may simply not fit reality very well. However, I am unaware of alternative explanations for the opposition to delay expression opposition to a losing war that do not focus on strategic attempts to maximize the probability that a change in position towards the war influences government decision-making. As discussed earlier, the frequency with which early opponents of wars attempt to use their “foresight” against members of their own party (Obama and Clinton in 2007-2008; Dean and Kerry in 2003-2004) suggests that there should be an incentive for politicians to oppose wars as soon as it becomes clear that they are faring poorly, if not even earlier. Tables 1 and 2 illustrate two important patterns regarding opposition decisions and government responses to them.

Table 1 presents a simple cross tabulation of opposition position and war performance.
Specifically, the table shows the number of wars that ended without the opposition ever opposing the war as a function of whether the war went poorly for the government or not. Here, “losing” indicates a negative value on the measure of war performance I have constructed (which I will discuss in greater detail later in the chapter). As the table illustrates, 6 of the 13 wars ended poorly for the government. Of these 1/3 nonetheless saw continuous support from the opposition through the end of the war. By contrast, 7 of the 13 wars ended well for the government, but 3/7 of the time, the opposition opposed the war anyway. The rate of opposition is thus clearly higher among losing wars than wars that favored the government, but something besides the government’s performance in the war appears to enter into the opposition’s consideration.

Table 2 presents a simple cross tabulation of government response to expressed opposition and the current trend in the performance of the war. Specifically, the table shows the number of instances where the war ended when the opposition withdrew their support versus those that continued as a function of whether tide of battle was turning against the government or not. Here, “worsening” indicates a decrease from the previous month in the measure of war performance I have constructed, as was the case in 44.4% of observations. It takes on a value of 1 for all observations in which the current war performance measure is less than the previous war month measure (regardless of whether either month’s measure is greater than 0). It thus represents the trend in war performance, irrespective of the current level. As the table illustrates, in 4 of the 7 wars where the opposition publicly opposed the war, the war ended that month. In all 4 of these cases, the government’s performance in the war had worsened since the previous month. In only 1 of the 3 wars where the government continued fighting despite the opposition withdrawing its support had the government’s performance worsened since the previous month.4

4This was June 1965, four months into the US war in Vietnam. Newspaper accounts suggest by this time, it was already clear that more Democrats favored negotiating an end to the war than did not, though the party was far from unified in this position. However, while I expect that others might take issue with my
I believe these patterns are consistent with the expectations of the model, once ideological motivations are considered, but are difficult to account for otherwise. These two tables suggest that if the opposition party(ies) wait for the right moment to express their opposition, it is exceedingly likely that the government will end the war. A theoretical approach that does not focus on ideological preferences of the opposition would have a difficult time explaining why the opposition would ignore this. Nor would an alternative explanation of these patterns likely produce hypothesis $H_6$, which I will discuss next. Therefore, empirical support for this hypothesis, particularly in combination with the other hypotheses offered here, will provide compelling evidence that the theoretical models developed in the previous chapter yield important insights regarding the link between domestic politics and international conflict, and justifies explicitly focusing on the behavior of the opposition party rather than assuming that opposition is an epiphenomenon of battle outcomes.

4.1.3 War Outcomes

I turn now to the final proposition. In ordinary language, Proposition 9 states that if the opposition is hawkish enough, then we might expect to observe a poor war outcome but nonetheless not observe opposition. Therefore, while we would expect a clear (but decidedly not causal) relationship between war outcomes and opposition positions if the opposition party adopted the simple approach of supporting all winning wars and opposing all losing wars, once we consider the possibility of ideological motivations, we can no longer expect that those wars that end without the opposition ever withdrawing their support will be the wars that end best for the government. The patterns apparent in the previous tables suggest that determination of when the opposition first opposed Vietnam, I note that by my measure of war performance, the US’s performance in Vietnam was worsening month-to-month virtually every month for the first 4 years of the war, and never once worsened over the previous month after that. A decision to code the first expression of opposition later than June 1965 would nonetheless very likely not change the fact that the war was worsening when opposition was first expressed but nonetheless failed to persuade the government to quit, unless one argues that opposition was not expressed until well into 1969.
it’s very likely that opposition parties sometimes express opposition to wars for ideological rather than pragmatic reasons. If the same is true of support for war stemming from hawkish ideology, then we should expect that wars that enjoy support from the opposition all the way to the end should be associated with worse war outcomes for the government, at least for sufficiently long wars. The longer the war lasted, as the probability of opposition is expected to increase over time, suggesting that the posterior probability that expressed support comes from a hawkish opposition party rather than a moderate opposition party is strictly increasing over time. However, we cannot make such inferences about the reason for advocating continued fighting in short wars, and might expect that for very short wars, the relationship between support and war outcomes, though not causal, should be positive.

\[ H6. \text{ The relationship between opposition support ("fight") and the outcome of the war is expected to be positive but decreasing over time.} \]

### 4.1.4 Auxiliary Hypotheses

Finally, I present a few hypotheses that do not follow directly from propositions of the model. These expectations are consistent with the general assumptions of the model and the bargaining model of war, and might follow from the model if I introduced incomplete information. I will consider the implications of introducing incomplete information into the models developed in the previous chapter further in future work, but for now offer a few conjectures, with the caveat that these hypotheses are not based on a formal derivation from the current versions of the models.

First, if the purpose of war is to reveal information about resolve and separate types to facilitate bargaining, wars are more likely to end the more information is revealed by the battle outcomes (Filson & Werner 2002, Smith & Stam 2004, Werner & Yuen 2005, Ramsay N.d.). Thus, a dramatic shift in the course of battle should lead both sides to
update their beliefs about the opposing state’s strength and lead to more rapid convergence of beliefs. This effect is not contingent upon the direction of the shift. While dramatic shifts favoring the government might increase the terms of settlement for the government more than dramatic shifts favoring the target state, suggesting the direction of the shift matters for the outcome, any shift at all increases the probability of the war ending.

Note, however, that this expectation is at odds with the assumptions of one notable study (Goemans 2000). Goemans argues that if the course of battle changes dramatically, the state that suddenly finds itself doing better than expected will increase its war demands, and so bargaining is no easier to achieve. However, this argument assumes that dramatic shifts have no effect on convergence of beliefs, instead leaving the level of disagreement in beliefs constant while both sides update in the same direction. For sufficiently large shifts in the course of fighting, it is impossible for beliefs about the probability of victory for each side should they fight until the absolute end to fail to move closer together if they are moving in the same direction, as probabilities by definition have upper and lower bounds. If state A believes its probability of completely defeating state B in a total war is 0.9 and state B believes state A’s probability of doing so is 0.6, then a shift in battle outcomes in favor of state A which leads both sides to revise their beliefs upward will necessarily decrease the difference between their beliefs so long as neither side increases its estimated probability that A would win a total war by less than 0.1. The minimum increase in the expected probability of victory for side A in the event of total war sufficient to ensure at least some convergence of beliefs is increasing the farther away from 1 the higher of the two beliefs is. This suggests that the effect of shifts on the speed of convergence might be contingent on the optimism of the stronger side, an intriguing possibility worth exploring in future work. For now, I offer the simple hypothesis:

*H7. A dramatic shift in the government’s war performance decreases the*
probability that the war continues.

Second, many scholars have discussed the impact of the balance of capabilities on the duration of war. Some scholars argue that the actual distribution of capabilities should not matter, only rapid shifts in capabilities (Powell 1999, Werner 1999). Others have argued that power parity makes conflict more likely to occur, and longer lasting once it begins (Lemke & Werner 1996, Zagare & Kilgour 2000, Lemke 2002, Reed 2003, Slantchev 2004). This effect is attributed by some to the assumption that uncertainty over capabilities, rather than uncertainty over resolve, is the cause of bargaining breakdown, and parity necessarily involves greater levels of uncertainty.

As discussed in chapter 2, there are reasons to believe that wars involving democracies do not stem from uncertainty over capabilities or short-term resolve. Rather, the argument developed here emphasizes the potential of shifts in the domestic political environment (opposition behavior) to alter the government’s level of accountability for their policy outcomes, and thereby changing the government’s long term resolve. If wars involving democracies occur because the two states disagree in their beliefs about the level of costs the democratic state would be willing to endure in pursuit of victory, we might not expect to find the same relationship between power preponderance and duration of war that Slantchev (2004) demonstrated to hold for interstate wars as a whole. In fact, using the HERO data set (which over-represents democracies), Ramsay fails to find any evidence that preponderance shortens wars overall, but that this result re-emerges once democratic belligerents are excluded (Ramsay N.d.). Thus, I hypothesize that in this sample, which only includes wars involving democracies, power preponderance (parity) should have no impact on the duration of the war, in contrast to some previous findings.

H8. Power preponderance has no significant impact on the probability that a war continues.
The following hypothesis follows directly from the Basic War Fighting Game, but cannot be stated in so straightforward a fashion for the variants of the game involving domestic politics. Nonetheless, this expectation does follow from the basic framework, as well as previous formal models (Smith 1998), and so I offer it, but with the caveat that a more appropriate test would have to condition the expectation on the opposition’s behavior, necessitating a triple interaction (as I am already interacting the opposition’s position with the log of time). Thus, for completeness, I offer the following hypothesis though there are good reasons to expect that it may not receive support since this sample only includes wars involving democracies, where the relationship is not expected to be so straightforward:

\textit{H9. The government’s war performance is expected to exert a U shaped effect on the probability that the war continues in any given month.}

Finally, I offer one further hypothesis regarding the outcome of war. We might reasonably assume that the longer a war lasts (if at all) after the opposition advocates quitting, the less information is revealed by the previous withdrawal of support from the opposition. If the war was going poorly for the government at the time that the opposition opposed but later turns more favorable for the government, the relationship between opposition position and war outcome will be attenuated. While we might expect that politicized wars (those where the government continues despite opposition) that end soon after the opposition withdrew its support will end worse for the government than wars where the opposition supported, this relationship will be less clear the longer the war continues. Thus, I offer the following hypothesis:

\textit{H10. The relationship between fighting after withdrawal of support from the opposition (opposed) and the outcome of the war is expected to be negative and decreasing over time.}
The utility of the models developed in the previous chapter does not depend critically upon evidence in support of these auxiliary hypotheses. The first three do not address the role of government accountability in shaping long term resolve but rather seek to control for important dynamics highlighted by the bargaining model of war literature. Evidence that the distribution of capabilities has no effect on war duration, in contrast to previous findings (Slantchev 2004), but consistent with Ramsay’s result that the distribution of capabilities only has the expected effect for wars that do not involve democracies, will provide indirect evidence in support of the claim that wars involving democracies, due to their transparency, cannot be explained by the same uncertainty that appears to explain other wars. However, while such a finding would be consistent with my theoretical argument, it will not help establish the claim that wars involving democracies do nonetheless result from uncertainty, stemming from a different source (fluctuations in long term resolve induced by party competition effecting the government’s accountability). The last auxiliary hypothesis speaks more directly to the processes central to my argument, but does not follow directly from the current versions of the theoretical models. A lack of empirical support for this hypothesis may suggest a shortcoming in the overall approach, but it may also imply nothing more than the need for caution offering conjectures about implications from a formal model that have not themselves yet been formalized. It would be difficult to determine which conclusions is more appropriate.

Having discussed what specific hypotheses I will rely upon to determine the extent of empirical support for the propositions derived from the model, I turn now to the data and statistical methods that will be used to evaluate these hypotheses.
4.1.5 Data and Methods

The above hypotheses will be test employing data for all interstate wars involving democracies since World War II, a total of 13 wars. In the future, I intend to expand the coverage of the data back to 1816, for a total of 26 wars. For the hypotheses on the duration of war, a total of 171 war-month observations will be analyzed. For the hypotheses on the duration of support, a total of 49 war-month observations will be analyzed. For the hypotheses on the outcome of the war, 13 observations will be analyzed.

Monthly data are employed rather than battle-level data for two reasons. First, duration analysis assumes the dependent variable is discrete time. While it is possible to use non-standard units such as battles, the interpretation of the results of such analysis would not be straightforward. Second, collecting battle-level data is problematic in several ways. Historians do not always agree on what constitutes a battle, or the number of battles within a given war. Even if an acceptable, objective definition of a “battle” was available, there is the problem of uneven availability of key statistics within battles. These critiques have been raised about the US Army’s HERO data set (Reiter & Stam 2002), which records data on individual battles. The coverage of wars, and battles within wars, for this data set does not appear to match the historical record very well. Instead, I relied upon a variety of sources of archival evidence to collect original data on events during war. While I was unable to uncover the same level of detail on disaggregated war outcomes and fatalities over time, in each of the 13 cases, I was able to identify at least 1, and often many more, point estimates of fatalities, allowing me to estimate an interpolated monthly series that allows me to analyze the effect of changes in the course of battle.\(^5\) The data collection was funded by the

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\(^5\)Despite some wars having comparatively few point estimates from which to interpolate the monthly series, overall, there is considerable variation. The average change in fatalities from month to month for the democratic participant is about 475 fatalities, with a standard error of 515, with roughly 15% of monthly observations having an increase in fatalities over the previous month of 1000 or more and a little more than 17% of monthly observations having an increase in fatalities of fewer than 25.
National Science Foundation. The collection effort proceeded in conjunction with a parallel
effort by Kyle Joyce. Our current data collection will serve as a pilot study for a follow up
grant proposal to the National Science Foundation that we hope will cover intrawar data
for the entire Correlates of War list of interstate wars across several dimensions, including
fatalities, geographic location, third-party activity, and negotiation attempts. The current
coverage includes only those wars we each required for our dissertation projects (wars involv-
ing democracies for my dissertation and wars that expanded beyond the original participants
for Kyle Joyce). We hired undergraduate research assistants to consult as many historical
sources as possible, recording specific dates and locations of battles, troops and equipment
sent, fatality estimates (as many as could be uncovered), activity by states other than original
participants, and end dates of active hostilities where different from formal end date
of the war (as in the case of treaties or ceasefires that were negotiated over a period of
time). We uncovered many inconsistencies with the Correlates of War data, most notably
in the case of the Polish-Lithuania War in 1920. We found no evidence that an independent
war took place between these two states in 1920, though there appear to have been some
battles during the Russo-Polish war of 1919 that we suspect were the basis of identifying
this war. We hope in future work to correct for such irregularities, in addition to providing
more detailed data regarding who did what to whom, when and where, over the course of
the interstate wars recorded by the Correlates of War project.

The dependent variable for evaluation of hypotheses regarding war duration is the length
of the war in months. The dependent variable for the hypotheses regarding duration of
support is the length of support in months, with observations right-censored if support is
present until the end of the war. The dependent variable for the hypotheses regarding
war outcome is a categorical coding of war outcomes relative to prewar aims collected by
Slantchev (2004).6 This variable has four categories: complete loss, concessions, gains, and

6I have also performed the same analyses using the Correlates of War war outcome coding, which correlates
complete victory. None of the wars in this analysis ended in complete loss for the democratic participant. Three ended in concessions, nine gains, and one complete victory (the Persian Gulf War).

The key independent variables are original measures of the opposition’s position towards continuing the war, collected by the author with the assistance of the National Science Foundation. The data categorize the primary opposition party’s (or parties’) position according to a five point scale, from which I construct three binary measures for the purposes of this analysis. This scale ranges from strong consensus in favor of immediate withdrawal to significant dissent within the opposition over whether to support or not or near consensus advocating a negotiated settlement to tacit consent (lack of public opposition) to open support for continued fighting to hawkish support, where the opposition not only pushes for continued fighting by criticizing the government for being seen as being too willing to settle or placing excessive constraints or restrictions on the conduct of military operations. For the purposes of this analysis, there are three opposition position variables of interest, “fight”, “quit” and opposed. These variables are coded as follows: “fight” is equal to 1 in any month in which

with Slantchev’s coding at 0.465. The observations were completely determined, rendering the results highly suspect, but the estimated coefficients were consistent with the results reported here.

7 By primary opposition party (parties), I mean those parties that have a realistic chance of replacing the government. In multiparty parliamentary systems, there are typically many effective parties that attain large enough of a percentage of the vote to gain representation in the legislature, yet typically there are only two (rarely three) parties that ever serve as the head of a coalition government. For example, while Israel and India have multiparty systems in the sense that several parties regularly receive sufficient votes to attain representation in the legislature, no government during the period under analysis was led by any party other than either Labor or Likud (or forerunner parties thereof) or Indian National Congress or BJP, respectively. Opposition need not be expressed by the party out of government, however. For example, opposition to the US involvement in Vietnam emerged as early as 1965, expressed by the Democratic Party, under Democratic President Lyndon Johnson. It is worth noting that some of the democratic states included in this analysis were considerably more competitive than others. I have selected the cases according the criteria common in studies of democracy and war, namely the Polity index. Yet Israel and India did not observe their first peaceful transfer of power until 1977 in each case, a key indication of healthy party competition. Turkey also lacked the competitive interactions common in the United States and United Kingdom. Nonetheless, the opposition parties in these systems, though realizing they had very little chance of unseating the government in the next election, were surprisingly vocal in their criticism of the government at times, in ways that are not commonly observed in more traditional one-party states. Thus, including these observations represents a conservative test of the theory, as it is likely that governments respond differently to criticism that only poses a threat to the size of their future majority rather than their ability to retain office.
the opposition’s position is tacit consent or higher (public support, hawkish support); “quit” is equal to 1 only for the first month in which the opposition position variable falls into one of the first two categories (immediate withdrawal or divided opposition/consensus favoring negotiation); opposed is equal to 1 in all months subsequent to any month in which “quit” was equal to 1, if the war is still ongoing. In six of the thirteen wars, the opposition advocates continued fighting up until the end of the war. In the remaining seven which saw the opposition oppose the war, four of these wars ended the same month that the opposition withdrew its support (these wars were: the Assam War; the Turko-Cypriot War; the Lebanon War; and the Persian Gulf War). To evaluate the claim that the relationship between opposition and war outcomes for those wars that continue after the opposition publicly opposes the war, I also introduce a decay function. This variable is equal to $0.99^{t_{opp}}$, where $t_{opp}$ is the number of months that have transpired since the opposition publicly opposed the war. An identical decay function has been used to allow for declining effects of third party interventions into civil wars (Regan & Aydin 2006). Including this measure will allow me to evaluate the relationship between war outcomes and the withdrawal of support over time. Table 3 includes a list of the wars include and the opposition positions, with estimated dates of changes in position, if any.

The other key independent variable measures the war performance of the government to date, a proxy for $w_t$ in the formal models. This measure is the difference between the target state’s share of the total military capabilities in the dyad (taken for the Correlates of War project) and the government’s share of the total fatalities in the dyad, as of the current month. This measure is intended to serve three important functions. First, it normalizes the measure of war performance between -1 and 1, consistent with the parameter from the formal model which it is intended to proxy. Second, it allows for comparisons across wars and across states in a way that any measure based on the absolute number of fatalities would not. Third, it weights the government’s rate of loss by the stakes of the conflict, in that...
the same share of fatalities will produce a higher estimated war performance when fighting against a strong target than when fighting against a weak target. The parameter for the cost of fighting in the formal models is likewise indexed by the stakes of the conflict, suggesting any empirical measure that failed to take this into account would be inappropriate. Others have argued that the distribution of capabilities can not only serve as a proxy for the stakes of the conflict, but may well proxy the government’s war aims (Feaver & Gelpi 2004). This is important, as studies of public opinion have found a strong correlation between the perceived primary political objective of a war and the level of fatalities the public is willing to suffer in pursuit of victory (Sullivan 2008). This measure correlated with the war outcome variable at −0.10, suggesting that the two are capturing different information. Yet, an ordered logit analysis of war outcomes including only the war performance measure, the natural log of time, the interaction between the two, and a measure of the balance of capabilities reveals that over time, better war performance by this measure is associated with better war outcomes.8 Thus, we can be confident that war outcomes are war performance are distinct, but related, concepts.

A construct a binary measure for dramatic shifts in war performance. This variable is equal to 1 if the monthly change in the war performance measure just described is more than a standard deviation above or below the mean monthly change in war performance. Dramatic shifts, so defined, take place in 13 instances, most of which occur in the first month of the war. It is not clear if this tells us that after the first month, clear patterns tend to emerge, or whether the measure is problematic and missing important variation deeper into these wars. As a crude way of addressing this possibility given current data limitations, I constructed an alternative measure of dramatic shift that equals 1 in the first month if the monthly change in war performance was greater than 1 standard deviation above or below the mean of monthly changes during the first month (where war performance is assumed

8results not shown
to be 0 in month 0, so the estimated war performance in the first month effectively is the change in war performance for that month). I set dramatic shift equal to 1 in all subsequent months if the monthly change in war performance is more than 1 standard deviation away from the average monthly change in war performance for all months after the first. The two measures overlap considerably, but not fully, correlating at 0.871. The results discussed below for dramatic shifts do not change if I substitute this second measure of dramatic shift. Even when taking into account the fact that there are larger fluctuations in the first month of a war (which is better interpreted as the gap between war performance initially and prewar expectations of a completely balance distribution of fatalities based upon the relative strength of each side), the effect of dramatic shifts remains the same.

The only remaining variable necessary to evaluate the above hypotheses is power preponderance. This measure is equal to the stronger side’s material capabilities over the sum of material capabilities in the dyad. Thus, the variable ranges from 0.5 (complete parity) to 1.0 (complete preponderance). The capability data are taken from the Correlates of War composite index of national capabilities (Singer, Bremer & Stucky 1972).

I estimate a Wiebull hazard model to evaluate the hypotheses regarding duration. I choose a Wiebull and not a Cox (which is non-parametric) because this allows me to evaluate $H4$. A Cox model would allow me to uncover the baseline hazard rate, more precisely estimating the effect of duration on the probability that the war continues, but would not allow me to test the hypothesis that this probability is monotonically decreasing over time, as suggested by the formal models. The results are reported as coefficients rather than hazard ratios to ease interpretation. A positive coefficient estimate indicates that the variable increases the duration of the event, while negative coefficient estimates indicate that the variable decreases the duration (or increases the probability that the event ends in the current month). Robust standard errors are employed to correct for unmodeled heterogeneity across cases (wars).
I estimate an ordered logit to evaluate the hypotheses regarding war outcome, as the dependent variable is ordered.\footnote{Alternatively, I could estimate a multinomial logit, which would not require me to assume that the difference between concessions and gains is precisely the same as the difference between gains and complete victory, as is assumed when estimating an ordered logit. I have run the same model using a multinomial logit, but due to the small number of observations, many of the observations are completely determined and so the reported coefficient estimates are highly suspect.} Here too I employ robust standard errors.

Having discussed the research design in terms of the hypotheses to be test, the data employed, and the method of analysis, I turn now to a presentation of the results.

### 4.2 Statistical Results

In this section I present the results of the statistical analyses. I will discuss whether each hypothesis receives empirical support, the significance of these results, and the goodness of fit of the statistical models. As the substantive effects of maximum likelihood estimation results are not directly interpretable in the way coefficient estimates from an Ordinary Least Squares regression are, I will present predicted effects for duration and outcome in the next section. I will discuss the results for each dependent variable (duration of war, duration of support, and war outcome) in turn.

#### 4.2.1 Duration of War

The first results I will discuss concern the duration of interstate wars involving democracies. This analysis serves to test hypotheses $H_1 - H_4$ and $H_7 - H_9$. The first four hypotheses follow directly from the model, the latter three are included because of their importance in the extant literature. Table 4 presents the results from the duration analysis. The second column reports the coefficient estimates, with the robust standard errors surrounding these estimates in parentheses. The third column reports the probability that these coefficient estimates would be uncovered by random error given that the true underlying population
coefficient is in fact 0 (p-value). As with any maximum likelihood estimator, the magnitude of the coefficient estimates does not have a straightforward interpretation the way the coefficient estimates from a linear regression (OLS) would. However, the sign and significance are sufficient to evaluate whether the variables have the hypothesized effects, and in the following section I will present graphs of predicted survival rates to help illustrate the magnitude of the effects.

Due to the presence of interaction terms, the results must be interpreted carefully. The insignificance of the coefficient estimates for “fight” and opposed tell us that when the natural log of time is equal to 0, the effect of these positions are not significant. However, in the case of opposed, this is meaningless, as the natural log of time is only equal to 0 in the first month of the war, and the opposed variable is only ever equal to 1 when the government has continued fighting a war the opposition has opposed, which cannot be true during the first month. That the opposition does not increase the probability that the government continues fighting the war by supporting the war in the first month also necessarily implies that the opposition cannot decrease the probability that the government continues fighting by expressing opposition in the first month. Though the theoretical models did not produce this expectation, the more important test is whether the effect of the opposition’s position on the duration of the war is increasing over time.

The interactions between “fight” and the natural log of time and between opposed and the natural log of time are positive and significant. This tells us that the estimated effect of the opposition’s position appears to be increasing over time, and is significantly different from 0 for at least some range of values of the variables. However, we must be careful not to interpret this coefficient as indicating that the interactive effect is significant over all values of the variables, a point often overlooked in interpreting multiplicative interaction terms (\?). However, we can be confident that at least for some range of values, the hypotheses are supported. For example, the coefficients and their standard errors imply that the marginal
effect of opposition support at 12 months is 1.564 with a 95% confidence interval of 0.589 through 2.540. At 36 months, the marginal effect is 2.285, with a confidence interval of 1.009 through 3.562. The marginal effect of support is significantly greater than 0 from 4 months into the war on. The same is true for opposed from the fifth month of the war on. Thus, considerable support is demonstrated for hypotheses $H1$ and $H3$, with the results a little stronger for $H1$. As “quit” is the omitted category (necessary to get the model to converge, given that there are three mutually exclusive and exhaustive states), we cannot directly evaluate $H2$ from Table 4. The effect of “quit” is captured by the constant and the shape parameter. 10 The graphical presentation of results in the following section will demonstrate the magnitude of the impact of the initial withdrawal of support, which is consistent with $H2$.

Hypothesis $H4$ is evaluated by examining the estimated coefficient on the shape parameter, $p$. We see in the first table that $p$ is greater than 1 and is statistically significant. This suggests that overall, the probability that the war continues in any given month is strictly decreasing over time, as hypothesized in $H4$.

We also see in the table that a dramatic shift in war performance has a negative and significant effect on the probability that the war continues. When battle outcomes differ significantly from expectations (under the assumption that prewar expectations are for strong states to shoulder a smaller burden of the total war deaths and expectations at any given point in time during the war being based on current war performance), the war is likely to end. This is consistent with the expectations of much of the literature on the bargaining model of war and offers support for hypothesis $H7$.

Turning to power preponderance, we see that the estimated coefficient has the wrong sign, based upon the expectation of previous research. However, consistent with the expec-

10I estimated a second Weibull model including all the same variables, except replacing “fight” and opposed (and the interactions of these variables with the natural log of time) with “quit” and the interaction between “quit” and the natural log of time. The results (not shown) are similar to those shown here.
tation of \( H_8 \), the effect is not significant. In light of Ramsay’s finding that the traditional expected effect of power preponderance only obtains among wars not involving democracies, this suggests that wars involving democracies do not serve to help the participants resolve informational asymmetries regarding their relative capabilities as wars among non-democracies appear to. The coefficient estimate on power preponderance is likewise in the opposite direction than that expected by extant theories of the effect of the distribution of capabilities on conflict, and here the effect appears to be significant. Thus, overall, there appears to be qualified support for \( H_8 \), though future work investigating the possibility that power preponderance actually lengthens war when one of the participants is a democratic state would be useful.

Finally, we see that war performance does not appear to exert the expected U-shaped effect on the probability that the war continues in any given month. Thus, there does not appear to be even qualified support for hypothesis \( H_9 \). Whether this is due to the measure of war performance employed here or because war performance does effect war duration in as straightforward a manner among wars involving democratic states cannot be determined by this analysis. However, I note that the measure of war performance employed here does exert much of the effects we might expect a reasonable measure of war performance to have. Nonetheless, future work replicating (or overturning) this result with a different measure of war performance might be beneficial.

All but one of the hypotheses regarding the duration of war received at least qualified support in this analysis. However, demonstrating that the factors emphasized by the theoretical approach advanced here “matter”, and demonstrating that these factors explain a considerable amount of the variation in the duration of war are two different things. Assessing the fit of duration models is not as straightforward as assessing the explanatory power of other classes of models, as there is no \( R^2 \) or pseudo-\( R^2 \). Some authors have offered instead a comparison of the mean or median predicted duration and the observed duration. This is dif-
ficult to do for models involving time-varying covariates, as the analysis here does. Instead, I generate the predicted probability that the war continues in each month and compare these probabilities to the observed outcome (continuation vs termination) in each month using the analysis from Table 4. I compared the average predicted probability of continuation for months where the war ended compared to those where it did not. The model over-predicts the continuation of war, as the mean predicted probability for months where the war ended is 0.780. However, this is considerably lower than the predicted probability of continuation for months where the war did continue, which is 0.952. Table 5 presents the frequency of war termination and continuation in comparison to the frequency of predicted continuation (probability greater than or equal to 0.5) and predicted termination (probability less than 0.5). The model correctly predicts 155 observations of continuation and 2 observations of termination, for a total of 157 correct predictions out of 171 observations. The null model (predicting the modal outcome of continuation for every observation) would produce 158 correct predictions. Therefore, the model does not help us explain the precise month in which wars end well, even though we can be confident that the factors thought to effect war duration were largely found to exert a significant effect.

4.2.2 Duration of Support

I turn now to the results regarding the duration of support. This analysis evaluates hypothesis $H5$. Recall that there is strong theoretical reason to expect that opposition decisions do not depend solely upon the course of battle, but also the ideological position of the party’s core supporters. As the current analysis does not account for this important factor, the statistical model is not expected to produce a good fit, even in comparison to the previous model of war duration. In fact, if war performance is found to exert a statistically significant but modest effect on the duration of support, it will be consistent with the theory. However,
the inability of war performance to account for a greater amount of the observed variation does not confirm the expectation that ideological preferences account for the remaining unexplained variation. Future analysis will be necessary to verify that claim. Yet it is worth noting before presenting the results of this analysis that the expectation is simply that war performance have a modest impact on the duration of support.

Table 6 presents the results from the duration analysis. As above, the second column reports the coefficient estimates, with the robust standard errors surrounding these estimates in parentheses. The third column reports the p-value. Again, the magnitude of the coefficient estimates does not have a straightforward interpretation, but the sign and significance are sufficient to evaluate whether the variables have the hypothesized effects. In the next section I will present graphs of predicted survival rates to help illustrate the magnitude of the effects.

Here too, the presence of interaction terms necessitates caution in interpreting the results. The insignificance of the coefficient estimate for war performance indicates that in the first month of the war, the opposition’s decision to support or oppose the war does not depend upon the initial course of battle. The positive and significant coefficient on the interaction between war performance and the natural log of time is misleading, as the effect captured by this term is itself a function of time. What this result tells us is that over time, the effect of favorable war performance on continued support is increasing, and this effect is likely significant over a meaningful range of values. The marginal effect of war performance does not reach conventional levels of statistical significance until month 8. Given that 7 of the 13 wars involving democracies in the postwar era were over by the end of the second month of fighting, this suggests that war performance is unlikely to play a factor in determining the opposition’s position towards the war in most cases. Only 3 of the 13 wars lasted 8 months or longer. I therefore conclude that $H_5$ receives qualified support.

In terms of model fit, the predicted probability that the opposition continues to advocate fighting the war based on this analysis never drops below 0.5. The mean predicted probability
of continued support for months where the opposition first expressed opposition is 0.827, while the corresponding predicted probability for months where the opposition continued to support fighting is 0.880. Thus suggests that focusing on war performance provides limited leverage on explaining the position of the opposition party towards the war.\textsuperscript{11}

Based on this analysis, it appears that opposition parties do not initially base their positions towards the use of force on the performance of the government on the battlefield. As the war drags on, it becomes increasingly likely that the opposition will take the course of battle into account, but very little of the observed variation in opposition positions can be attributed to variation in war performance. The theoretical models develop in the previous chapter would expect precisely this pattern, if opposition parties are sufficiently ideological. Without any measure of ideological orientation, it is difficult to determine whether the theoretical model can truly be said to explain the patterns observed here, but it would also be premature to say that the poor performance of the statistical model undermines the theoretical approach advanced here.

### 4.2.3 War Outcome

Finally, I present the results of an ordered logistic regression of war outcome on opposition position, controlling for war duration and relative capabilities. Again, the second column of Table 7 presents the coefficient estimates and standard errors, with the associated p-value in the third column and significance indicated in the fourth column. Extra caution is warranted in the interpretation of these results. While the previous results likewise rest upon the analysis of only 13 cases, with a large number of war-months, the statistical properties

\textsuperscript{11}The results of a similar analysis substituting the government’s share of fatalities and its interaction with the natural log of time performs even poorer, failing to approach statistical significance at any given duration. A similar analysis using the actual number of fatalities rather than the government’s share of the total war fatalities also failed to produce any significant relationships. The measure of war performance proposed here appears to explain opposition decisions only modestly, but nonetheless considerably better than casualties or relative casualties would.
associated with large data-sets may well be approximated. Here, there are only 13 observations, and the desirable properties of maximum likelihood estimators may not obtain in such small samples (Long 1997). These results are best interpreted as suggestive evidence rather than a true test of hypotheses $H_6$ and $H_{10}$.

The results of this preliminary analysis are supportive of the expectations. As expected, the effect of support (“fight”) is initially positive, but this effect decreases over time. Also as expected, there is initially a negative relationship between war outcomes and politicized wars (opposed), an effect which is decreasing over time. We also see, as might be expected, that the stronger the government is relative to the target state in terms of material capabilities, the better war outcomes they can expect. The positive and significant coefficient on the natural log of time should be interpreted with care. Due to the interaction with opposition support, this coefficient tells us the effect of time when “fight” is equal to 0. The decay effect on opposed is almost perfectly collinear with the natural log of time when opposed equals 1, and thus comes very close to behaving like an interaction. Therefore, the effect of time may be better viewed as the effect of time when opposed and “fight” are both equal to 0, or, put differently, the effect of “quit” (again omitted for identification) over time.

In light of the very small sample size and other concerns (the coefficient estimates all attain statistical significance, but the overall statistical model does not), these results can only be seen as the weakest of evidence in support of the expectations.

While the results of this analysis must not be over-interpreted, the predictions of the model fare better than those of the duration models. The predicted and observed outcomes are displayed in Table 8. The modal outcome is gains, which is observed in 9 of the 13 wars. Thus, predicting the modal outcome would produce 9 correct predictions. The model correctly predicts 11 outcomes, including the only instance of complete victory. Tables 9, 10 and 11 present the predicted probabilities of each outcome by opposition position for wars lasting 1 month, 2 months, and 6 months, to better illustrate the effects. As can be seen from
these tables, in the shortest of wars, better war outcomes are expected when the opposition supports the war then if they oppose it. Complete victory is not expected to occur in wars lasting a single month, but gains are virtually assured when the opposition supports and the government almost certain to offer concessions in one month wars that were opposed by the opposition. The difference disappears when we move to wars lasting 2 months, though here we see that if the opposition opposed the war in the first month and the government fought for one more month before quitting, the probability of gains is considerably lower (and the probability of concessions considerably higher). If we consider slightly longer wars, those lasting 6 months, we see that complete victory is virtually assured if the opposition supported the war for the first five months then advocated ending the war in the sixth month. If the opposition instead continues to advocate fighting, even in the sixth month, complete victory is very unlikely to occur, though nonetheless more likely to occur than if the opposition had opposed the war from the outset. Once again, given the small number of observations, we should exercise caution in interpreting these stark numbers. However, the model appears to fit the data reasonably well, and supports the expectations of hypotheses $H_6$ and $H_{10}$. It appears that for sufficiently short wars (as most wars involving democracies are), opposition support is associated with better war outcomes (though there is little reason to believe this to be a causal relationship), yet the longer the war lasts, the greater the risk that unwavering support is associated with worse war outcomes than would be observed if the opposition initially lent its support but eventually withdrew it. I intend to explore the robustness of this surprising result in future work.

### 4.3 Graphical Results

To ease interpretation of the results discussed in the previous discussion, I present here graphs of the estimated survival rate of war termination (Figure 1) and the survival rate of
withdrawal of opposition support (Figure 3). The survival rate is the conditional probability that the war continues, given that it has lasted until that point.

Figure 1 illustrates that the predicted survival rate (probability that the war continues) over time as a function of the length of support from the opposition party(ies). The survival rates were generated holding all other variables constant at their means (modes, in the case of dummies, such as dramatic shift). The survival rate for a war that the opposition never opposes approaches 1 early in the war, steadily declining over time, dropping to just above 0.6 by month 50. In other words, as long as the opposition continues advocating fighting, the war is likely to last for a long time. The downward spikes represent the immediate effect of withdrawing support, with the probability that the war continues jumping back up again if the government continues the war despite the opposition opposing the war, consistent with the theory. The downward spike is larger the later in the war the opposition withdraws its support. If the opposition turns against the war in the second month, the probability that the war continues in that month remains extremely high. If the opposition waits until the fourth month, the probability that the war continues drops to just above 0.6. This is a substantial effect, but indicates that for the average war, the opposition would have to wait longer than this to be effective. Given that most wars involving democracies do not last 4 months, this suggests that for most wars, opposition will not be an important factor. However, that is not to say that the potential effect of opposition behavior is not large. For wars that last six months, with the opposition supporting for the first five and then opposing the war, the probability that the war lasts past the sixth month is less than 0.3.

To illustrate this point more strongly, Figure 2 illustrates the predicted survivor rate (probability the war continues) for the Vietnam War. Rather than using the mean or modal values of the other independent variables, I have included the observed values for each of the variables, by month. This analysis suggests that the probability that the Vietnam War would have ended when the opposition first opposed the war was only a little below 0.7. The
predicted probability that the war would have ended in the month in which it actually did end (month 97) is just over 0.5. This suggests that there are important factors not included in this analysis that help explain why the war ended when it did, though the factors included here do appear to contain important information. Notice that if the Democrats had waited two more months to oppose the war, the predicted probability that the war would have ended at that point is just over 0.3. If they waited four months, the probability drops to less than 0.1. Recall that the Vietnam War is the only example in the data set of the opposition publicly opposing a war despite positive war performance.

Figure 3 presents the predicted survival rate of opposition support by war performance. The four lines represent the mean war performance, one standard deviation above (“poor”), one standard deviation above (“good”), and the maximum observed war performance (0.742). The effect of war performance on duration of opposition support can be considerable, but as discussed above, for most wars, there will be little relationship between the two. If a war lasts 50 months, the difference between the best war performance observed in this period and even one standard deviation above the mean is stark. However, as most wars in this period did not last more than 2 months, it is worth noting that even when the government’s war performance is poor, the probability of retaining opposition support for a few months is quite high.

These graphs help to highlight the patterns suggested by the statistical results. Taken together, they suggest that many wars will not last long enough for the opposition to begin to react to the course of fighting, nor long enough for changes in opposition position to have a large effect on the government’s decision. However, the potential impact of these relationships for those wars that do last past the first couple of months are profound. Finally, I note that these graphs were generated holding dramatic shift constant at its modal value of 0. However, if we consider wars where the course of fighting shifts in the first month, as was the case in 11 of the wars included here, then the baseline probability that the war continues
would be considerably lower, and the impact of opposition would appear considerably more substantial. Thus, it would perhaps be more appropriate to say that wars that survive past the first month have passed a critical threshold, and the role of the opposition will be muted until a few more months have passed.

### 4.4 Illustrative Cases

Having discussed the statistical results and illustrated the substantive impacts of the variables, I turn now to a brief comparison of Israel’s two invasions of southern Lebanon. These cases highlight some of the important factors missing from the theoretical models developed here, particularly third party behaviors and the importance of non-state actors, yet confirm some of the basic insights of the current approach. Further, extant theories linking domestic politics to international conflict would have a difficult time accounting for some of the dynamics apparent in these cases.

#### 4.4.1 First Lebanon War

Following Syria’s intervention in the early stages of Lebanon’s civil war (1975-1990), southern Lebanon and western Beirut became bases for the PLO and other militias. Following PLO attacks on northern Israel, the Israelis invaded in 1978 to push the PLO north of the Litani river. This brief skirmish ended with the creation of an international peacekeeping force (UNIFIL) and Israel partially withdrew, maintaining control of a 12 mile “security zone”. The ceasefire was routinely violated over the next several years, with Israel supporting the Maronite Christian faction in its struggle against the Syrian-backed Palestinian faction.

Tensions reached a boiling point in June, 1982. On June 3rd, a Palestinian organization attempted to assassinate the Israeli ambassador in London, critically wounding him. The PLO denied any involvement, but the Israeli government responded with air strikes
near Beirut. The PLO countered with heavy shelling of northern Israel. The NY Times reported, “the Palestinian shelling, with artillery and rocket launchers, was the most severe ever directed against Israeli towns” (NYT, 6/6/1982). Israel invaded Lebanon on June 6th.

From the outset of the operation, the Israeli government was aware of the risks. The PLO had grown in strength comparable to that of many conventional armies, and their Syrian allies had more than recovered from the Yom Kippur War with Israel in 1973. The Soviets had been supplying both the PLO and the Syrians with arms and equipment. Further, the cabinet understood that it not only risked Soviet and/or Syrian military intervention, but also strains with Egypt and Western Europe (NYT, 6/6/1982). Consider that by the Correlates of War’s measure of material capabilities, Israel was nearly at perfect parity with Syria in 1982. Engaging in a ground war with the PLO, inviting Syrian intervention and potential Soviet as well, can hardly be interpreted as an example of a democratic state choosing an easy target for a quick, decisive victory, as extant theories of democracy and war suggest. On the first day of the war, Haaretz commented that “it is possible to win on the battlefield there, but sink into a bog from which it will be rather hard to extricate ourselves” (NYT, 6/6/1982).

The Israeli decision to attack is less difficult to explain from the perspective of the theoretical approach advanced here. While quick, decisive victory at low cost did not appear to be expected by the Likud cabinet, the opposition overwhelmingly supported the war. Prime Minister Begin could not have afforded not to act militarily. Former Prime Minister Rabin, who had recently said terrorism cannot be stamped out by force, called on all Israelis to rally behind the government. Shimon Peres, head of the opposition Labor Party, declared, “We believe it is the duty of Israel to do whatever is necessary to stop these attacks upon the population and the life of northern Israel,” (NYT, 6/6/1982).

Yet, after the expected Syrian intervention occurred, bringing along with it the heaviest aerial combat in decades, and Defense Minister Ariel Sharon decided not only to push the
PLO further into Lebanon by lay seige to Beirut, the Labor Party had second thoughts. On August 7th, two months almost to the day after the war started, dissent became clear. Though a majority of the public continued to support the war, the Labor Party approved a resolution “unequivocally opposing any military entry into Beirut as well as any military action geared to facilitating such an entry,” (NYT, 8/7/1982). The resolution was passed in the absence of Rabin and Peres. The opposition had yet to place overwhelming pressure on the government, but throughout the month, criticism grew more intense. By August 18th, the Israeli government agreed to a ceasefire, to be implemented by a multinational force, including as part of its terms the withdrawal of the PLO. However, the PLO was not weakened overall, and many link the devastation of south Lebanon during this war to the rise of Hezbollah. Israel failed in its goal to establish a “legitimate” Lebanese government under the Maronite Christian leader Gemayel, who was assassinated.

4.4.2 Second Lebanon War

In the summer of 2006, six years after Israel finally disengaged from the territory in southern Lebanon occupied since the 1982 war, history repeated itself. Israel was in the middle of a military campaign inside Gaza, prompted by the kidnapping of an Israeli soldier by Hamas. On July 12th, Hezbollah fired rockets into border towns as a diversion for a missile attack on an Israeli border patrol. Two Israeli soldiers were kidnapped and taken in Lebanon. With the brief swell in support sparked by the first kidnapping in Gaza already fading and criticism of the government’s handling of that mission mounting, Kadima Prime Minister Olmert seized upon the opportunity to deflect criticism from the Likud Party (of which he was previously a member), which was increasingly painting him as weak and incapable of protecting Israel from her enemies.

Yet, as with the First Lebanon War in 1982, the government began the war under full
appreciation of the considerable constraints under which they would be operating. Hoping to avoid repeating the mistakes of the previous war, which included massive civilian deaths and investigations into the massacres that led to the resignation of Defense Minister Sharon, the government decided to rely predominantly on air power and delay the use of ground forces until they could be sure that civilians had had ample time to evacuate first. The Israeli government voluntarily handcuffed its military, undermining its chances for success, for political reasons. The NY Times reported that though the government “talks of destroying Hezbollah, that outcome is likely to be impossible without a sustained ground operation that is unlikely. And the clock is ticking: the international community, including the United States, is not going to give Israel a blank check to bomb Lebanon for much longer”, (NYT, 7/15/2006).

After several weeks of restricted fighting, opposition grew. However, in contrast to the 1982 war, the criticism did not reflect a belief that the government was going too far or should end the war. Rather, “the criticism is not that the war is going on, but that it is going poorly...even the bulk of the Israeli left feels this way. There is no real peace camp in Israel right now,” the NY Times reported (7/15/2006). Even before the war’s end, the government recognized that they would face intense criticism and scrutiny of their actions after the war, but not from Labor (their coalition partner), but from the right-leaning Likud, who questioned the early reliance on air power and reluctance to commit ground troops to a more sustained effort.

Even as Israel signed an internationally backed peace agreement and disengaged from Lebanon (not without an aggressive and internationally condemned last push in the 48 hours between signing the ceasefire and it entering into force), Likud called on the government to continue the war. The war was declared a victory by both sides, but members of the cabinet admitted that it was a failure (prompting their dismissal). Israel had failed to produce more than a pyrrhic victory. Hezbollah, though weakened by the campaign, claimed a symbolic
victory and saw a considerable rise in public support, while Prime Minister Olmert has struggled to hold onto power since.

4.4.3 Discussion of the Cases

These two cases, though different in many respects, are strikingly similar in other ways. Both occurred in summer, involved many of the same actors, were prompted by similar events, and were begun under similar constraints. The outcomes were also quite similar, as both conflicts inflicted only limited harm upon the targets and ended with international pressure and peacekeeping missions being sent to the Israeli-Lebanon border. In both cases, the government enjoyed strong support from the opposition at the onset of the war. In one case, this support eroded in the final month of the war. In the other, the opposition criticized the government’s decision to quit fighting. How well does the theoretical argument advanced here account for the events of these cases?

It is difficult to determine the relative importance of the opposition statements and the international pressure to settle, as is emphasized by other approaches (Werner & Yuen 2005). The duration of these wars may be just as attributable to third party involvement as anything else. Further, the theory developed here overlooks the potential importance of non-state actors, and the complicated bargaining framework when such actors have state sponsors (Bapat 2005, Bapat 2007, Bapat N.d.). Bapat (draft) offers an explanation of when states initiate international conflicts against the sponsors of terrorists that suggests governments are often likely to attack weak host states regardless of their ability to crack down on terrorists, while they will only attack overt sponsor states if they are sufficiently incapable of enforcement. This approach may help explain why the Israelis chose to invade Lebanon rather than provide more assistance to their favored faction (the Maronites) and avoid the costs of fighting themselves.
However, other patterns in these two cases conform well to the expectations of the theory. First, both wars begun under full appreciation of the military and diplomatic challenges the government would face. In contrast to the expectations of extant theories that emphasize the selectivity of democratic states in choosing which wars are worth fighting, it is difficult to argue that in either case the Israeli government was confident it would achieve its objectives quickly and easily. In one case, there is clear evidence that the government paid considerable political costs for not trying more vigorously to do so. In contrast to existing approaches, the theoretical argument advanced here suggests that in the face of an opposition party advocating the use of force, democratic states should be quicker to fight than unitary-actor states unconstrained by domestic politics. Though the theoretical models developed in the previous chapter focus on a stylized world where conflicts take place between two parties, ignoring the importance of third parties such as neighboring states, major powers, and international organizations as well as the dynamics of bargaining with non-state actors with state-sponsors, they help explain why the government would be willing to initiate a war despite the obvious challenges it would face in pursuing victory. Further, while the importance of external pressure ought not be understated, it is probably not a coincidence that the First Lebanon War ended within days of the Labor party expressing opposition. After all, there had been attempts to mediate an end to the war almost from the very beginning. While the international pressure may have been greater in August than it was in June, it may well be the case that if the Labor party continued to support the war, it might have lasted another month. The duration of the Second Lebanon War almost certainly must be attributed to international pressure, as the presence of strong support for fighting from the opposition (without any meaningful criticism from the left) should have encouraged the government to keep fighting, and may explain the controversial escalation in intensity and use of cluster bombs in the final hours of the war, once a ceasefire had already been signed. Finally, the domestic political cost paid by the government for ending the war, while fully consistent
with the argument here, is harder to reconcile with existing approaches that suggest that the only relevant domestic constraints are those that encourage governments to limit the costs of fighting.

Overall, the cases highlight both the strengths and limitations of the theoretical models. The exclusion of third parties from the theory overlooks an important factor. Yet the details of the cases do appear to emphasize the importance of party competition, and how attention to such can produce implications at odds with existing approaches that assume constant levels of accountability (which appears to consistently encourage conservative military doctrine).

### 4.5 Conclusion

In this chapter, I detailed the decisions made in seeking to empirically evaluate the implications of the formal models discussed in the previous chapter. I presented a series of statistical tests, nearly all of which were supported. Only one hypothesis failed to receive even qualified support, suggesting that the duration of war is not a function of war performance. However, while this hypothesis would follow directly from the Basic War Fighting Game, I discussed when presenting it that the variants of the model incorporating domestic politics would not produce the same expectation, at least not in so straightforward a manner.

I also discussed two cases, one of which was included in the statistical analysis and one which was not. Though these cases did not perfectly match the expectations of the theory in every respect, some of the more important assumptions of the approach appeared to match the events of these conflicts, and overall, the argument offered here can be argued to produce at least partial insight into the details of these cases. In each case, contrary to the expectations of existing approaches, the Israeli government went to war knowing that they would be very unlikely to be able to achieve their full objectives, but expecting to pay
domestic political costs if they did not act, given the opposition’s public support for the use of force. In one case, the withdrawal of support appears to have played some role in the termination of the war, and in the other, the opposition’s criticism of the government’s decision not to keep fighting appears to have inflicted greater costs on the government than it would have paid otherwise.

Overall, the results in this chapter offer considerable preliminary evidence in support of the theoretical approach advanced here. Given the small number of cases, it is important not to over-interpret the results. However, the predictions of the theoretical models were supported across a variety of outcome variables. This analysis suggests that moving past a static conception of accountability as a characteristic of regime type to a dynamic view that allows for party competition to lead to variation in accountability can yield important insights into the duration and outcome of interstate wars. I hope to have convinced the reader that such an approach offers a plausible explanation of the second puzzle identified at the outset of this dissertation, the routine involvement of democratic states in prolonged, political divisive wars. In future work, I hope to demonstrate that this approach, with appropriate modifications, can also offer an explanation to the first puzzle, the occurrence of wars involving democracies in light of rationalist explanations.
Table 4.1: Opposition Position by War Performance, 1945-1997

<table>
<thead>
<tr>
<th></th>
<th>Losing</th>
<th>Winning</th>
</tr>
</thead>
<tbody>
<tr>
<td>Supported</td>
<td>2(15.4%)</td>
<td>4(30.8%)</td>
</tr>
<tr>
<td>Opposed</td>
<td>4(30.8%)</td>
<td>3(23.0%)</td>
</tr>
</tbody>
</table>
Table 4.2: Government Response to Opposition by Trend in War Performance, 1945-1997

<table>
<thead>
<tr>
<th></th>
<th>Worsening</th>
<th>Steady or Improving</th>
</tr>
</thead>
<tbody>
<tr>
<td>War Ends</td>
<td>4(57.1%)</td>
<td>0(0%)</td>
</tr>
<tr>
<td>War Continues</td>
<td>1(14.3%)</td>
<td>2(28.6%)</td>
</tr>
<tr>
<td>War</td>
<td>Primary Belligerents</td>
<td>Date Opposition Advocated Quitting</td>
</tr>
<tr>
<td>------------------------------</td>
<td>--------------------------------------</td>
<td>-----------------------------------</td>
</tr>
<tr>
<td>Korean War (1950-1953)</td>
<td>United States and North Korea</td>
<td>April 19, 1951</td>
</tr>
<tr>
<td>Suez War (1956)</td>
<td>Israel and Egypt</td>
<td>Never opposed</td>
</tr>
<tr>
<td>Assam War (1962)</td>
<td>India and China</td>
<td>November 21, 1962</td>
</tr>
<tr>
<td>Vietnam War (1965-1973)</td>
<td>United States and North Vietnam</td>
<td>July 1, 1965</td>
</tr>
<tr>
<td>Second Kashmir War (1965)</td>
<td>India and Pakistan</td>
<td>Never opposed</td>
</tr>
<tr>
<td>Six Day War (1967)</td>
<td>Israel and Syria</td>
<td>Never opposed</td>
</tr>
<tr>
<td>War of Attrition (1969-1970)</td>
<td>Israel and Egypt</td>
<td>Never opposed</td>
</tr>
<tr>
<td>Bangladesh War (1971)</td>
<td>India and Pakistan</td>
<td>Never opposed</td>
</tr>
<tr>
<td>Yom Kippur War (1973)</td>
<td>Israel and Egypt</td>
<td>Never opposed</td>
</tr>
<tr>
<td>Cyprus War (1974)</td>
<td>Turkey and Cyprus</td>
<td>July 24, 1974</td>
</tr>
<tr>
<td>Falklands Islands War (1982)</td>
<td>United Kingdom and Argentina</td>
<td>April 28, 1982</td>
</tr>
<tr>
<td>Lebanon War (1982)</td>
<td>Israel and Syria</td>
<td>August 7, 1982</td>
</tr>
</tbody>
</table>
Table 4.4: Duration of Interstate Wars Involving Democracies, 1945-1997

<table>
<thead>
<tr>
<th>Variable</th>
<th>Coefficient Estimate</th>
<th>Standard Error</th>
<th>P-value</th>
</tr>
</thead>
<tbody>
<tr>
<td>War Performance</td>
<td>0.132</td>
<td>0.681</td>
<td>0.846</td>
</tr>
<tr>
<td>War Performance$^2$</td>
<td>-0.509</td>
<td>1.249</td>
<td>0.684</td>
</tr>
<tr>
<td>Dramatic Shift</td>
<td>-0.858</td>
<td>0.421</td>
<td>0.021 **</td>
</tr>
<tr>
<td>Opposed</td>
<td>-0.124</td>
<td>0.827</td>
<td>0.880</td>
</tr>
<tr>
<td>“Fight”</td>
<td>-0.067</td>
<td>0.558</td>
<td>0.905</td>
</tr>
<tr>
<td>Opposed*Ln(Time)</td>
<td>0.686</td>
<td>0.232</td>
<td>0.001 ***</td>
</tr>
<tr>
<td>“Fight”*Ln(Time)</td>
<td>0.656</td>
<td>0.228</td>
<td>0.002 **</td>
</tr>
<tr>
<td>Power Preponderance</td>
<td>1.573</td>
<td>1.211</td>
<td>0.194</td>
</tr>
<tr>
<td>Constant</td>
<td>0.194</td>
<td>0.946</td>
<td>0.838</td>
</tr>
<tr>
<td>p (Shape)</td>
<td>2.732</td>
<td>0.814</td>
<td>&lt;0.001 ***</td>
</tr>
<tr>
<td>N</td>
<td>171</td>
<td></td>
<td></td>
</tr>
<tr>
<td>prob $&lt;$ $\chi^2$</td>
<td>&lt;0.001</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

Duration Analysis, Weibull Distribution. Robust standard errors. * $p \leq 0.1$, ** $p \leq 0.05$, ***$p \leq 0.01$, one-tailed test where appropriate.
Table 4.5: Predicted Versus Observed War Continuation

<table>
<thead>
<tr>
<th></th>
<th>Continue</th>
<th>End</th>
</tr>
</thead>
<tbody>
<tr>
<td>Predicted Continue</td>
<td>155(90.6%)</td>
<td>11(6.5%)</td>
</tr>
<tr>
<td>Predicted End</td>
<td>3(1.7%)</td>
<td>2(1.2%)</td>
</tr>
</tbody>
</table>
Table 4.6: Duration of Opposition Support During War, 1945-1997

<table>
<thead>
<tr>
<th>Variable</th>
<th>Coefficient Estimate</th>
<th>Standard Error</th>
<th>P-value</th>
</tr>
</thead>
<tbody>
<tr>
<td>War Performance</td>
<td>-0.446</td>
<td>0.730</td>
<td>0.541</td>
</tr>
<tr>
<td>War Performance*Ln(Time)</td>
<td>0.968</td>
<td>0.297</td>
<td>&lt;0.001  ***</td>
</tr>
<tr>
<td>Constant</td>
<td>1.745</td>
<td>0.338</td>
<td>&lt;0.001  ***</td>
</tr>
<tr>
<td>p (Shape)</td>
<td>1.358</td>
<td>0.207</td>
<td>0.023   **</td>
</tr>
<tr>
<td>N</td>
<td>49</td>
<td></td>
<td></td>
</tr>
<tr>
<td>prob &lt; $\chi^2$</td>
<td>0.004</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

Duration Analysis, Weibull Distribution. Robust standard errors. * $p \leq 0.1$, ** $p \leq 0.05$,

**$p \leq 0.01$, one-tailed test where appropriate
Table 4.7: War Outcomes and Opposition Positions, 1945-1997

<table>
<thead>
<tr>
<th>Variable</th>
<th>Coefficient Estimate</th>
<th>Standard Error</th>
<th>P-value</th>
</tr>
</thead>
<tbody>
<tr>
<td>Relative Capabilities</td>
<td>19.463</td>
<td>8.772</td>
<td>0.014</td>
</tr>
<tr>
<td>Ln(Time)</td>
<td>13.526</td>
<td>6.468</td>
<td>0.019</td>
</tr>
<tr>
<td>“Fight”</td>
<td>12.030</td>
<td>7.034</td>
<td>0.044</td>
</tr>
<tr>
<td>“Fight”*Ln(Time)</td>
<td>-12.017</td>
<td>6.642</td>
<td>0.035</td>
</tr>
<tr>
<td>Opposed</td>
<td>-167.937</td>
<td>79.131</td>
<td>0.017</td>
</tr>
<tr>
<td>Decayed Opposed</td>
<td>163.810</td>
<td>77.433</td>
<td>0.017</td>
</tr>
</tbody>
</table>

| N                            | 13                   |                |         |
| prob < χ²                   | 0.139                |                |         |
| PseudoR²                    | 0.6920               |                |         |

Ordered Logit Regression. Robust standard errors. *

$p \leq 0.1$, **$p \leq 0.05$,

**$p \leq 0.01$, one-tailed test where appropriate
Table 4.8: Predicted Versus Observed War Outcome

<table>
<thead>
<tr>
<th></th>
<th>Concessions</th>
<th>Gains</th>
<th>Victory</th>
</tr>
</thead>
<tbody>
<tr>
<td>Predicted Concessions</td>
<td>2 (15.4%)</td>
<td>1 (7.7%)</td>
<td>0 (0%)</td>
</tr>
<tr>
<td>Predicted Gains</td>
<td>1 (7.7%)</td>
<td>8 (61.5%)</td>
<td>0 (0%)</td>
</tr>
<tr>
<td>Predicted Victory</td>
<td>0 (0%)</td>
<td>0 (0%)</td>
<td>1 (7.7%)</td>
</tr>
</tbody>
</table>
Table 4.9: Predicted Outcome, One Month War

<table>
<thead>
<tr>
<th></th>
<th>Concessions</th>
<th>Gains</th>
<th>Victory</th>
</tr>
</thead>
<tbody>
<tr>
<td>“Fight”</td>
<td>&lt; 0.001</td>
<td>0.999</td>
<td>&lt; 0.001</td>
</tr>
<tr>
<td>“Quit”</td>
<td>0.986</td>
<td>0.014</td>
<td>&lt; 0.001</td>
</tr>
</tbody>
</table>
Table 4.10: Predicted Outcome, Two Month War

<table>
<thead>
<tr>
<th></th>
<th>Concessions</th>
<th>Gains</th>
<th>Victory</th>
</tr>
</thead>
<tbody>
<tr>
<td>“Fight”</td>
<td>&lt; 0.001</td>
<td>0.997</td>
<td>0.003</td>
</tr>
<tr>
<td>“Quit”</td>
<td>0.006</td>
<td>0.994</td>
<td>&lt; 0.001</td>
</tr>
<tr>
<td>Opposed</td>
<td>0.654</td>
<td>0.346</td>
<td>&lt; 0.001</td>
</tr>
</tbody>
</table>
Table 4.11: Predicted Outcome, Six Month War

<table>
<thead>
<tr>
<th></th>
<th>Concessions</th>
<th>Gains</th>
<th>Victory</th>
</tr>
</thead>
<tbody>
<tr>
<td>“Fight”</td>
<td>&lt; 0.001</td>
<td>0.985</td>
<td>0.015</td>
</tr>
<tr>
<td>“Quit”</td>
<td>&lt; 0.001</td>
<td>0.005</td>
<td>0.995</td>
</tr>
<tr>
<td>Opposed</td>
<td>&lt; 0.001</td>
<td>0.999</td>
<td>&lt; 0.001</td>
</tr>
</tbody>
</table>
Figure 1. Survival Rate by Month of Opposition.
Figure 2. Survival of Support, by War Performance.
Figure 3. Predicted Effect of Delayed Opposition in Vietnam War.
Chapter 5

Short Term Resolve: Approval and Diversion

In the next two chapters, I turn to a particular type of conflict behavior that highlights the importance of distinguishing between long term resolve and the willingness to use force. In this chapter, I develop a bargaining model to illustrate how domestic considerations may encourage leaders to actively seek out crises but not allow these crises to escalate to war. The following chapter presents a series of statistical tests of the empirical implications of this model at several stages of conflict, from the initiation of disputes to their reciprocation and their final outcome.

The diversionary perspective posits that domestically vulnerable leaders are likely to engage in aggressive foreign policy in order to improve their domestic standing (Ostrom & Job 1986, James & Oneal 1991). The analytical usefulness of this perspective has traditionally been as controversial as the behavior it posits. In part, this is due to insufficient precision in the theory, making it difficult to know what behavior it predicts and what evidence is sufficient to falsify it. One of the most prominent critiques of this perspective in recent years does not dispute the incentive to engage in diversionary aggression, but claims that this
incentive does not necessarily lead to conflict, as would be aggressors may find no suitable targets. This perspective, known as strategic conflict avoidance, contends that potential targets of diversion anticipate the conditions that encourage such aggression and choose to keep low profiles, thereby denying opportunities to leaders in need of a victim (Smith, 1996).¹ I argue that this critique is misguided, as potential targets for diversion often do not have any incentive to avoid conflict.

The strategic conflict avoidance argument introduces an international element to what was previously largely a monadic theory of a behavior that by definition involves more than one state. This was an important advancement in the study of diversion. However, this argument suffers from two important flaws. First, it overlooks the distinction between long term resolve and the willingness to use force. Second, it pays no regard to what concessions are required in order to avoid conflict, implicitly assuming that states prefer peace at any price.

Before we can determine whether potential targets will prefer to adopt submissive postures to appease would-be aggressors, we must know what they sacrifice by doing so in comparison to what they could expect if they resisted diversionary aggression. This question is particularly important in light of the possibility that leaders who are willing to use force may nonetheless lack long term resolve. If such is the case, the incentive for potential targets of diversionary aggression to appease leaders in pursuit of opportunities to divert is greatly diminished.

I proceed in two steps. First, I briefly discuss some important disparities between the empirical and formal literatures on diversionary conflict. Second, I present a crisis bargaining model and summarize the key propositions that follow from it. A full solution is reserved to the appendix.

¹See (Leeds & Davis 1997, Clark 2003, Fordham 2005, Foster 2006) for empirical tests of the strategic conflict avoidance perspective.
5.1 Formal and Empirical Studies of Diversion

The assumptions of the model developed here differ in several important ways from previous formal models of diversion. This section seeks to illustrate the logic behind adopting these assumptions. I have attempted in constructing this model to improve the dialogue between formal and empirical approaches to diversion. Previous formal models of diversion rest upon assumptions that admittedly had desirable properties from a modeling perspective, but are often inconsistent with known empirical relationships regarding domestic political vulnerability and international conflict. The value of formal modeling as an analytical tool is that it enforces discipline in the development of an argument, ensuring internal consistency. However, an argument that builds upon premises that are questionable empirically will only yield conclusions that, while logically valid, are unlikely to be empirically supported. In order to ensure that the conclusions of the formal model presented here are not only logical but plausible, I have sought to ground the assumptions of the model in the findings of the extant literature to a greater extent than previous formal work on diversion. The primary difference between the two literatures concerns the effect of foreign policy on public opinion and therefore the conditions under which leaders gain domestically from their actions in the international arena.

5.1.1 Motivations for Diverting

There are several formal models of the diversionary argument (Downs and Rocke 1994, Richards et al 1993, Smith 1996, Tarar 2006). Consistent with the largely monadic orientation of the diversionary perspective prior to the introduction of the strategic conflict avoidance argument, these models capture interactions between the government and the electorate, with only the recent model by Tarar incorporating the behavior of the other state in the crisis. These models speak to the principal-agent problem and the informational
asymmetries inherent therein. The essence of these models generally has been that the government seeks to signal its competency to the public through its foreign policy behavior in order to secure reelection.

Tarar (2006) tacks a bargaining game on to the end of the model developed by Richards et al (1993). He derives the formal conditions under which strategic conflict avoidance is expected to obtain, though he notes that if the value of holding office is sufficiently high, diversionary incentives eliminate the bargaining space and ensure conflict. This suggests that domestic political considerations can provide a further "rationalist explanation for war" beyond those developed by Fearon (1995).²

The critical difference between Tarar’s argument and that presented here is the assumption about what leaders expect to get from diverting. As Tarar notes, his results depend upon the assumption that leaders employ diversionary foreign policies to signal competence to a sophisticated electorate. This is consistent with previous formal models of diversion (Richards et al, 1993; Smith, 1996). However, empirical studies of diversion tend to emphasize the rally effect as a motive for diversion. Focusing on diversion as a means of signaling competence suggests that who the target is and what the outcome of the crisis is will be would factor into the ability of the leader to benefit from diverting. The public is not likely to update their beliefs about the leader’s competence if she initiates a crisis that results in defeat, or prevails in a conflict against a very weak opponent. If signaling competence is the motivation for diversion, the conditions under which we would expect to observe diversionary behavior are indeed quite rare, as observed by Smith (1996) and Tarar (2006).

By contrast, focusing on the rally effect suggests that securing victory against a challenging opponent may not be all that important in order for the leader to benefit. While few

²Fearon himself noted that his three rationalist explanations are exhaustive only if we retain the common neorealist assumption of states as billiard balls. He left open the possibility that domestic political considerations could produce further explanations. The value of Tarar’s contribution then is the formalization of one such explanation.
would argue that the size of the rally effect is entirely insensitive to whom the target is and the outcome of the crisis, the literature on the rally effect generally suggests that there is at least some gain to the leader during any time of crisis, even if that gain is often very small. As I hope to make clear, this subtle difference is of critical importance.

5.1.2 The Rally Effect

In a recent study, Colaresi (2007) argues that the rally effect is neither emotional nor reflexive as much of the previous literature assumes, but is a rational process that can be modeled as a signaling game between the leader and the electorate. This is similar to the formal models of diversion discussed above. However, unlike those models, Colaresi focuses on a surge of support for the leader at the onset of a crisis rather than the public’s decision to reelect the leader following a crisis, and therefore does not model any effects of the outcome of the crisis. He argues that rallies are likely to be larger the more costly the action of the leader, when the leader’s approval is high, and if it is not an election year.\(^3\) Essentially, his informational theory of the rally effect argues that as public confidence that the leader doesn’t have an incentive to engage in a crisis for private benefit, they are more likely to rally behind the leader. He then conducts a series of statistical tests using presidential approval data from the U.S. that demonstrate robust support for the informational theory of the rally effect. Those leaders who need rallies most will be unlikely to secure them, arriving at a result similar to that of Smith (1996), though by a different path. The informational theory further helps explain why the rally effect in the U.S. is quite weak on average, while certain subsets of observations are significantly greater (Lian & Oneal 1993, Oneal & Bryan 1995, James & Rioux 1998).

The modest expected size of any rally effect for those leaders who need them most presents

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\(^3\)He also addresses several other factors that will not be addressed here. Rallies are also expected to be larger during times of divided government, when there is no ongoing scandal, if the president is not a lame duck, if there are greater freedom of information laws, and there is higher trust in government.
a challenge for the diversionary perspective. It is tempting to conclude that we shouldn’t 
expect to observe any systematic relationship between domestic political vulnerability and 
international conflict behavior.

For this to be true, we would have to believe that no conditions exist under which 
leaders would view an increase in approval by even a few points to be sufficient motivation 
to initiate a dispute. That might well be the case much of the time. Yet if a president is 
hovering around 50% approval in an election year, a few percentage points just might make 
the difference between reelection and removal from office. Those with lower approval will 
have little incentive to divert, knowing that the probability that a rally effect will save them 
is very small. Those with approval rates well above 50% will have little reason to divert 
even though the potential rally is much greater, given that their prospects for reelection are 
already quite good. Smith (1996) also argues that when leaders are assured of reelection, 
their decisions are likely to be motivated solely by international concerns. Likewise, when 
leaders are assured of defeat, their decisions are unbiased. It is only when there is sufficient 
uncertainty over the leader’s political fortunes that their decisions will be biased by domestic 
political considerations. The informational theory of the rally (Colaresi 2007) does not rule 
out modest rallies for presidents hovering around 50% approval in election years, particularly 
as stronger actions are taken. These conditions, while not present much of the time, are 
common enough to warrant special attention. Outside of election years, or if the leader’s 
approval is at either extremely high or extremely low ends of the spectrum even during an 
election year, the leader will lack either the ability to procure a rally or any extraordinary 
incentive to pursue one. From this point forward, the simultaneous presence of a proximate 
election and moderate values of approval shall be taken to define the presence of diversionary 
incentives.

The foregoing discussion highlights several important points that inform the model. The 
implications of diversionary incentives can be observed by potential targets and have the
potential to influence the target’s behavior. Those presidents who most need a rally will find it difficult to produce one. Those presidents most able to produce one have no particular incentive to do so - which is of course why a rally would obtain if they entered into a crisis. If presidents sought to exploit the ability to generate large rallies simply because they could, the public would be less likely to rally behind presidents in times of crises. Yet simply entering into a crisis, regardless of the outcome, may produce a modest boost in popularity even when the public has reason to suspect there is a reasonable chance that the president is acting out of private interest. For some presidents, even a modest boost may be of critical importance. Therefore, presidents whose prospects for reelection are most sensitive to the smallest of changes in their popularity may be motivated to provoke crises whether or not they are willing to bear considerable costs in pursuit of their stated objective.

Previous formal models of diversion, while advancing the diversionary literature in important ways, have not incorporated all of these important insights. If the behavior of the potential target is accounted for, diversionary conflicts should not occur unless the value of continuing to hold office is arbitrarily high (Tarar, 2006). I seek now to demonstrate that, given a set of assumptions that is consistent with the extant literature, diversionary conflicts can occur so long as the leader has even a weak preference for retaining office. The model does offer some solace, however: diversionary incentives encourage leaders to make deliberately extreme demands, thereby provoking the target’s resistance and causing a crisis, but these crises generally end with the leader backing down rather than escalating the conflict. As others have noted, the diversionary perspective is more of an explanation of limited military engagements than it is a theory of war (Smith, 1996).
5.2 Modeling Diversionary Crisis Bargaining

The game models bargaining between a leader, who cares both about her reelection prospects and some foreign policy, and a target state, who prefers the status quo on the policy. The policy may be territorial, in which case the conflict of interest is quite simply that the leader wants the target to relinquish control of some disputed tract of land. It may concern the domestic policies or regime of the target, with the leader pressuring the target state to improve its human rights practices. The model does not distinguish between types of issues. The model also does not assume a crisis is actively underway, only that there is some policy dimension along which the leader would view change as preferable while the target would not. Consistent with the diversionary literature, this structure sees constant opportunities for leaders to create crises if they so choose rather than only focusing on those crises that occur more or less as exogenous shocks to which leaders must react.\(^4\)

At the start of the game, nature distributes two terms, one representing the leader’s approval rating, denoted by \(\alpha\), and the other indicating whether the crisis takes place in an election year, represented by \(\epsilon\). The distribution of these terms is observed by both players. The first term ranges continuously from 0 to 1, with \(\alpha = 0.5\) indicating that the leader’s approval rating is 50%. The second term is binary and takes on either a value of 0 or 1 (presidential election year). As the leader is assumed to care about her reelection prospects, consideration of these factors is critical.

The leader then is faced with the decision to make a demand of the target or not. If the leader chooses not to make a demand, the game ends. If the leader chooses to make a

\(^4\)Meernik (2000) argues that opportunities are not randomly or uniformly distributed across time and claims this introduces a concern over selection effects into the search for evidence of domestic impacts on the likelihood of uses of force by the US. He models the occurrence of opportunities separately of the use of force to demonstrate this claim. However, his definition of opportunities is that class of events where characteristics often present in past uses of force are present (p 555). Researchers following this procedure who do not believe domestic factors are related to the use of force will be rather unlikely to find a strong relationship between domestic factors and the opportunity to use force.
demand, she also must choose the size of the demand, setting it between 0 and 1, similar to the bargaining model developed by Rubinstein (1982). In this sense, the leader can ask for modest concessions from the target or can demand its ideal point. The target then chooses whether to concede to the demand or to resist it. If the target concedes, the game ends.

However, if the target resists, a crisis begins. This may or may not involve violence. The model makes no assumptions about the severity of the crisis beyond distinguishing between cases where the leader escalates to war and those where she does not. Regardless of its severity, an international crisis always requires the mobilization of at least some resources and entails some nontrivial costs to both states, albeit fewer costs than would be involved if the crisis escalates to war. These costs may reflect the labor of diplomats and foreign policy personnel that could have been devoted to other issues, the political costs (either domestic or foreign) of initiating a crisis without following through on one’s threats (Fearon 1994, Sartori 2005), or the economic costs of deploying military forces. Once the target chooses to resist, the leader then must choose whether to back down or to escalate the dispute to war. The game ends in either case. If the game ends in escalation to war, there is a lottery over outcomes.

5.2.1 Payoffs

The payoffs for the leader at each node reflect both an international component and a domestic component, similar to Smith (1996). However, here the value of retaining office is normalized to 1, with the domestic component of the leader’s payoff captured by the probability of retention.

The leader’s reelection prospects receive a small boost if the game reaches the level

\[5\] This assumption is not necessary. In fact, it biases the model against producing any equilibria where Leader makes a demand knowing Target will resist and Leader will back down.

\[6\] Likewise, the assumption that if Leader does not escalate the crisis to war she will not gain anything, while clearly empirically inaccurate in some cases, also strongly biases the model against producing any equilibria where Leader makes a demand knowing Target will resist and intending to back down subsequently.
of a crisis. For the sake of simplicity, the size of the rally effect is fixed to 0.05. This choice is arbitrary, and I could have easily selected another value. The important thing is to distinguish between some range of values of approval where the rally effect is politically meaningful from some other range in which it is not. This decision determines fixed cutpoints, discussed later, that are likewise somewhat arbitrary. Adopting this approach rather than modeling the rally as a continuous distribution, with maximum value when approval is at 0.5 and decreasing symmetrically but never quite reaching 0 as approval moves in either direction away from 50, affords a considerable measure of simplicity and eases solution of the model, without substantively altering the results. If the game ends before the crisis escalates to war, the leader receives this boost with certainty. If the game escalates to war, the leader only receives this benefit if she wins the war. If the war is unsuccessful, the leader nonetheless has some probability of being reelected, but it is smaller than the baseline probability of reelection.

As small boosts in popularity have little value when the next election is years away, I set this benefit equal to zero in non-election years. Further reflecting the intuition that foreign policy outcomes (good or bad) will weigh more heavily in the voters minds if they were recent, I assume that the electoral penalty for losing a war is likewise greater in election years.

The probability of reelection is a function of the leader’s approval rating. If the leader’s approval is below 0.5, the probability that she is reelected is 0. If it is between 0.5 and 0.55, the election is likely to be competitive, and the probability of reelection is 0.5. If the Leader’s approval is greater than or equal to 0.55, the leader’s probability of reelection is 1. In election years, the same calculus holds with respect to the leader’s approval after adding the benefit for entering into a crisis, if relevant.

This translation of approval onto reelection probabilities simplifies the model considerably compared to more realistic treatments. The important feature is that approval is not linearly
related to the probability of reelection, making leaders with moderate values of approval most likely to benefit from a small increase. Any extension of the model that relaxed this strict assumption about the functional form of the process translating approval into reelection probabilities would produce similar results so long as the effect of changes in approval was smaller at extreme values of approval.

The probability of reelection after defeat in war is assumed to be 0.5 lower than the baseline. In this way, if the leader would otherwise have been certain to win reelection, she will face competitive elections after losing a war, and if the leader would have faced competitive elections, she is now assured of defeat.

In election years, if the crisis escalates to war and results in defeat, the leader’s approval is first reduced by the size of the rally effect term that was introduced at the start of the crisis before being translated into a reelection probability as described above. This assures that if the leader is near a critical region of approval (0.5 or 0.55), she will suffer more for losing wars in election years than non-election years while if she is far away from these regions she will be punished equally regardless of the electoral cycle.\(^7\)

For convenience, I have included a table summarizing the leader’s net payoffs at each outcome across different values of approval, \(\alpha\), and election, \(\epsilon\). Determining the conditions that are favorable to diversionary aggression from this table is straightforward. When the leader’s probability of reelection is greater if she enters a crisis and backs down than it is if she makes no demand, there are incentives to divert. Note that the payoffs in this table are simplified after distributing terms from the payoff terms listed in the game tree.

\(^7\)Again, the strict assumptions about the sizes of the effects are for convenience, but are not necessary for the results. So long as Leader is more sensitive to failure in election years, the basic results presented here will not be effected by changing the relationship between defeat in war and reelection probability.
5.2.2 Solution to the Model

There are only two subgame perfect equilibria to the model. One always holds during non-election years and also holds during election years if the leader’s approval is such that any rally effect will not significantly improve the leader’s reelection prospects. Call this equilibrium $\sim D$. The second equilibrium holds for election years when the leader’s approval is such that a rally effect would improve her reelection probability. As can be ascertained from Table 1, this is true whenever $\epsilon = 1$ and $0.45 \leq \alpha \leq 0.55$. Call this equilibrium $D$.

In equilibrium $\sim D$, the leader always makes the maximum-sized demand to which the target will concede. The actual size of this demand is determined by whether the probability of victory in war for the leader is above some threshold. The target always concedes to this demand. This result is similar to that of any bargaining model with complete information and endogenous demands, as conflict is ex post inefficient (Fearon, 1995). Substantively, this bargaining may be implicit. This equilibrium may be interpreted as describing what the status quo distribution of benefits on the issue(s) of contention between the states is likely to be. We may not in fact observe any demand or any concession. What this equilibrium tells us is that, under the assumptions of the model, we do not expect to observe crises if diversionary pressures are not present. This result is not intended to suggest that diversionary incentives are the sole explanation for international conflict or that conflict can only occur at the right time during the election cycles of states. Conflict would still be expected to occur for the same reasons offered by Fearon: private information, commitment problems, and indivisibility of issues. What this equilibrium tells us is that, much as we would expect, during those conditions where the mere occurrence of a crisis itself does not advantage a leader domestically, domestic political considerations do not contribute added information for explaining conflicts.

Equilibrium $D$ produces more interesting behavior. If the probability of victory in war
is sufficiently low and the costs of any crisis below the level of war are sufficiently high, the leader makes a demand to which she knows the target will concede, and the target concedes. As above, this may be implicit, and does not necessarily mean we expect to observe a formal demand followed by subsequent concessions. This result is more appropriately interpreted as saying that domestic political considerations give us no reason to expect to observe demands to revise the status quo being that are met with resistance by the target. The leader is also expected to make a demand to which the target will concede for higher probabilities of victory, up until a certain threshold.

However, if the probability of victory is sufficiently low and the costs of conflicts short of war are also low, the leader will make an extreme demand that she knows the target will resist, provoking a crisis, and then back down.¹ The intuition here is that as long as the costs of a low level dispute are not too great, even a modest rally effect is sufficient motivation to tempt leaders whose reelection probability is highly sensitive to small changes to opportunistically create a crisis that is not in the national interest. The target has no incentive to concede to the demand, as it was insincere and strategically chosen to ensure that the target would not want to concede. Further, the leader is expected to back off without pressing for victory.

The model also points to conditions whereby the leader provokes a crisis that escalates to war. This occurs when the probability of victory is high and the sum of the costs of fighting a war for both states is low.

Figures 2 and 3 depict the $\sim D$ and $D$ equilibria formally, with no specific values assumed for any of the parameters of the model. To give a general sense of the relative likelihood

¹Note that the threshold value of the probability of victory that defines this condition, despite being an upper bound, is not necessarily "low". This should not be interpreted as meaning that leaders provoke crises against more powerful states. Without being able to determine the costs of war and the costs of crises short of war in a precise manner, it is difficult to ascribe meaningful values to this threshold. While the model suggests the probability of victory is an important factor, as a practical matter, it would be difficult to derive any empirical implications regarding the role of capabilities from this model.
of the different outcomes, Figures 4, 5, 6 and 7 depict the results graphically for a few theoretically interesting values of the parameters.

Figure 4 depicts the critical region for crises short of war when diversionary incentives are present (equilibrium \( D \)) across all values of the probability of victory and all values of the costs of war for the leader. In this figure, the costs of a crisis short of war for both sides are assumed to be half that player’s costs of war and the costs of war for the target are assumed to be the same as those for the leader. The graph illustrates that a crisis occurs in equilibrium under these restrictions when the probability of victory (should the crisis escalate) is low but so would be the costs of fighting a war.

Figures 5 and 6 depict the same critical regions for crises short of war, with different assumptions made about the relative sizes of the costs of crises short of war and the costs of war for the target. Figure 5 assumes that the costs of crises short of war for both sides are 0 and again assumes that the costs of war are the same for the target as they are for the leader. Figure 6 assumes the costs of crises short of war for both sides are half the costs of war and assumes the costs of war for the target are twice as large as for the leader. Figure 5 illustrates that crises occur in equilibrium for any probability of victory for the Leader in war, provided the costs of war are sufficiently high. As the costs of war decrease, we see that crises short of war will only occur when the probability of victory for the Leader is sufficiently low, as in Figure 4. Figure 6 demonstrates that when the costs of crises short of war are non-zero, there is lower probability of a crisis, as we would expect (compare to Figure 5) and when the costs of war for the Target are greater than for the Leader, the conditions allowing a crisis short of war to occur in equilibrium are more restrictive than when the costs are the same for both sides (compare to Figure 4).

Figures 7 depicts the critical regions for escalation to war for all values of the probability of victory and the costs of war, assuming the costs of war for both sides are equal, and the
costs of a crisis short of war are half the costs of war.\footnote{Varying the costs of crises short of war or the ratio of costs for the target relative to the leader has much smaller effects on this relationship than on crises short of war, so only one graph is presented.}

As can be seen from Figures 4 through 6, as the costs of war increase, the maximum probability of victory for which the leader will provoke a crisis short of war increases, up to a certain threshold. The threshold value of the costs of war beyond which no probability of victory is associated with the leader provoking a crisis short of war depends upon the other parameters of the model.

Figure 7 demonstrates that, as might be expected, the conditions of the model that support escalation to war are rather restrictive. Increases in the costs of war appear to eliminate the possibility of war quicker than do decreases in the probability of victory. Despite this, even when the costs of war are assumed to be 0, which is of course a very stylized condition, the probability of victory still must be quite high to encourage the leader to go to war. While the model does not rule out the possibility of leaders pushing crises all the way to the point of war for domestic gain, it suggests that there is far greater cause for concern over leaders provoking crises short of war.

5.3 Implications

This relatively simple bargaining model produced several important insights, some of which were quite intuitive, others less so. In this section I consider the implications of these theoretical results for the literature and derive empirically falsifiable propositions. Many of these propositions will be tested in the following chapter.

The model suggests that criticisms of the diversionary perspective that question the strength of any relationships between domestic vulnerability and international conflict (Gowa, 1998; Meernik, 2000) have merit, but are overstated. The $\sim D$ equilibrium implies that the popularity of the leader should have no impact on the probability of a crisis at any level of
severity. Given that previous studies have only tested the relationship between approval and the election cycle as independent, linear effects, it is no surprise that support for effects of these variables has been mixed. The $\sim D$ equilibrium would hold 75% of the time in the United States even if every president went into election years with approval near 50%. If some presidents enter the latter stage of the electoral cycle riding either very high or very low in the polls, as some have, then this equilibrium would be expected to operate even more often than that. When diversionary incentives are absent, as will be true much of the time, no relationship is expected between domestic political vulnerability and international conflict. This leads to the first proposition:

**Proposition 1.** When $\epsilon=0$ or $\epsilon=1$ and $\alpha < 0.45$ or $\epsilon=1$ and $\alpha > 0.55$, approval will have no effect on the probability of conflict.

Notice that this result does not imply that domestic political considerations have no meaningful impact on international outcomes, as a realist might argue. To keep the solution simple, strict assumptions about the probability of reelection and how it changes under certain outcomes, were made. If these assumptions were relaxed, the central result that the leader would select a demand such that the target would be certain to concede would still hold. However, domestic political factors, such as the leader’s initial probability of reelection and the probability of reelection after a loss in war, would structure the size of the demand the leader could make. Therefore, domestic politics would not effect the probability of conflict absent diversionary incentives, but it would have an effect on the distribution of benefits between the two states. This is consistent with the bargaining literature, which argues that the distribution of capabilities between two states does not have an effect on the likelihood of conflict, but largely determines the status quo distribution of benefits (Fearon, 1995; Powell, 1999; Wagner, 2000).

On the other hand, the model highlights conditions under which we do expect the di-
versionary logic to hold. The diversionary behavior uncovered here occurs under conditions that are uncommon, yet common enough to warrant scholarly attention. Further, the model suggests that diversionary aggression will most frequently end with the leader backing off short of escalating the crisis to war. These theoretical results tell us that we cannot dismiss the possibility of leaders engaging in conflict for political gain as readily as suggested by the strategic conflict avoidance perspective, but we can at least take solace knowing that diversionary aggression is generally limited, and occurs under very precise conditions.

The strategic conflict avoidance perspective dismisses the possibility of diversionary aggression, contending that potential targets observe the conditions that encourage such behavior and accordingly deny leaders opportunities to engage in it. I have argued that this perspective asks us to believe that potential targets do not care what level of policy concessions would be required in order to placate a leader in need of a victim to the point that she no longer has the incentive to use force. In contrast, I have argued that the promise of the rally effect can encourage leaders to make extreme demands to which the target cannot prefer to concede, particularly knowing that the leader likely lacks long term resolve and will back off shortly after the crisis begins.

As the cost to the target of a conflict short of war approaches 0, the critical value of the leader’s demand that makes the target indifferent between conceding and resisting also approaches 0. As can be seen from Figure 3, the leader is more likely to make an extreme demand and then back down when the cost of a crisis short of war for the leader is below some critical threshold. As demonstrated in the appendix, this threshold value is a function of the target’s cost of a crisis short of war. The smaller the target’s cost for a crisis short of war, the higher this threshold value, and therefore, the greater the leader’s costs must be for the leader to prefer to make a demand to which it knows the target will concede. In other words, the less costly it is for the target to resist, the more likely it will be that the leader will make an extreme demand, as the only demands to which the target will concede
are likely to be less rewarding to the leader than pursuing the rally effect by provoking a crisis. When diversionary incentives are present, as resisting becomes more attractive to the target, conflict becomes more likely. This is one of the more counterintuitive results of the model. Although it would be difficult to identify a satisfying measure of the expected costs of engaging in a crisis short of war, I nonetheless offer the following propositions for the sake of completeness:

Proposition 2a. When $\epsilon=1$ and $0.45 \leq \alpha \leq 0.55$, as $c_{t1}$ decreases, the probability of a crisis increases.

Proposition 2b. When $\epsilon=1$ and $0.45 \leq \alpha \leq 0.55$, as $c_{d1}$ decreases, the probability of a crisis increases.

Equilibrium $D$ also illustrates that the leader’s decision to make an extreme demand, and thereby provoke a crisis, depends upon the value of $p$, the probability of victory in war. These propositions would also be difficult to assess empirically, as the cutpoints for $p$ would be difficult to establish without satisfactory indicators of the cost terms. However, the technical propositions implied by the model are:

Proposition 3a. When $\epsilon=1$ and $0.45 \leq \alpha \leq 0.55$, if $p$ exceeds $p^{***}$, the leader will make a demand that the target will resist and the leader will escalate the crisis to war.

Proposition 3b. When $\epsilon=1$ and $0.45 \leq \alpha \leq 0.55$, if $p^{**} < p \leq p^{***}$, no crisis will occur.

Proposition 3c. When $\epsilon=1$ and $0.45 \leq \alpha \leq 0.55$, if $p \leq p^{**}$, the leader will make a demand that the target will resist and the leader will back down once the crisis begins.
While precise evaluation of these propositions would prove difficult, I can offer the following proposition that is far easier to test empirically and is still consistent with the theoretical results of the model:

**Proposition 4.** When $\epsilon=1$ and $0.45 \leq \alpha \leq 0.55$, the probability of a crisis is greater than if either of these conditions does not hold.

This proposition simply states that when diversionary incentives hold, as is true in election years as approval approaches 0.5, the probability of a crisis increases. The fact that model identifies conditions where no crisis will occur even during election years with the leader’s approval near 0.5 only stacks the deck against finding support for this proposition. If it were possible to parse out the probability of victory in war relative to the critical threshold values and so derive more precise expectations about crisis behavior, the propositions would be that much easier to demonstrate empirically. Therefore, Proposition 4, while crude relative to the actual equilibrium of the model, will provide a conservative test of the implications of the model.

The model yields expectations regarding behavior other than the onset of crises, however. Therefore I also offer the following propositions:

**Proposition 5.** When $\epsilon=1$ and $0.45 \leq \alpha \leq 0.55$, the leader is more likely to back down than if either of these conditions does not hold.

These propositions are stated in terms of the parameters of the model. I will discuss in the next chapter how these propositions are translated into specific hypotheses to be tested empirically.
5.4 Conclusion

The results of the bargaining model presented here offer important insights into the link between domestic political vulnerability and international conflict. Some of these results were consistent with existing arguments while others directly contradict prominent perspectives, in particular the strategic conflict avoidance argument. The model describes a more nuanced link between domestic political vulnerability and international conflict than is commonly argued. Under certain conditions, domestic political vulnerability is not expected to have any impact, consistent with the claims of many of the critics of the diversionary perspective. Under other conditions, the model suggests that vulnerable leaders will be enticed by the rally effect to provoke crises they do not intend to see through to the end, and the targets of this behavior are best served by resisted the extreme demands of the vulnerable leader. Support for the propositions of this model would advance our understanding of the link between domestic politics and international conflict, and of the role of domestic politics in revealing resolve, in meaningful ways. In the next chapter, I turn to the empirical assessment of the claims advanced here.
Table 5.1: Payoffs in Bargaining Game by Domestic Conditions

<table>
<thead>
<tr>
<th></th>
<th>$0 &lt; \alpha \leq 0.45$</th>
<th>$0.45 &lt; \alpha \leq 0.50$</th>
<th>$0.50 &lt; \alpha \leq 0.55$</th>
<th>$0.55 &lt; \alpha$</th>
</tr>
</thead>
<tbody>
<tr>
<td>$\epsilon = 0$</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>~ Demand</td>
<td>0</td>
<td>0</td>
<td>0.5</td>
<td>1</td>
</tr>
<tr>
<td>Target Concedes</td>
<td>$d$</td>
<td>$d$</td>
<td>$0.5 + d$</td>
<td>$1 + d$</td>
</tr>
<tr>
<td>Leader Backs Down</td>
<td>$-c_{d1}$</td>
<td>$-c_{d1}$</td>
<td>$0.5 - c_{d1}$</td>
<td>$1 - c_{d1}$</td>
</tr>
<tr>
<td>Leader Escalates</td>
<td>$p - c_{d2}$</td>
<td>$p - c_{d2}$</td>
<td>$1.5p - c_{d2}$</td>
<td>$1.5p + 0.5 - c_{d2}$</td>
</tr>
<tr>
<td>$\epsilon = 1$</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>~ Demand</td>
<td>0</td>
<td>0</td>
<td>0.5</td>
<td>1</td>
</tr>
<tr>
<td>Target Concedes</td>
<td>$d$</td>
<td>$d$</td>
<td>$0.5 + d$</td>
<td>$1 + d$</td>
</tr>
<tr>
<td>Leader Backs Down</td>
<td>$-c_{d1}$</td>
<td>$0.5 - c_{d1}$</td>
<td>$1 - c_{d1}$</td>
<td>$1 - c_{d1}$</td>
</tr>
<tr>
<td>Leader Escalates</td>
<td>$p - c_{d2}$</td>
<td>$1.5p - c_{d2}$</td>
<td>$1.5p + 0.5 - c_{d2}$</td>
<td>$1.5p + 0.5 - c_{d2}$</td>
</tr>
</tbody>
</table>
Figure 1. Crisis bargaining game with electoral considerations.

\[ N \]
\[ \alpha, \varepsilon \]

\textbf{Leader} \\
\text{~ demand} \\
\( (r_1, 1) \)

\textbf{Targ} \\
\text{concede} \\
\( (r_1 + d, 1 - d) \)

\textbf{Leader} \\
\text{back down} \\
\( (r_2 - c_{a1}, 1 - c_{l1}) \)

\textbf{Leader} \\
\text{escalate} \\
\( (p(1 + r_2) + (1 - p)(r_3) - c_{a2}, 1 - p - c_{l2}) \)
Figure 2. ~D equilibrium.

0 | ........................................ | 1
  |                                 |
  | P*                              |
  | Leader DEM, d=d**               |
  | Target CON                      |
  | Leader DEM, d=d^*               |
  | Target CON                      |

Note: DEM = demand, CON = concede, d = [size of demand]
Figure 3. D equilibrium

0 | \( p^* \) | \( p^{**} \) | 1

Leader DEM, \( d^{**} \) if \( c_{d1} > c_{d1}^{*} \)
Leader DEM, \( d^{*} \)
Leader DEM, \( d > d^{*} \), ESC
Target CON if \( d < d^{*} \)
Target RES if \( d > d^{*} \)

Target DEM, \( d > d^{*} \) if \( c_{d1} \leq c_{d1}^{*} \)
Target CON if \( c_{d1} > c_{d1}^{*} \)
Target RES if \( c_{d1} > c_{d1}^{*} \)

Note: DEM = demand, CON = concede, RES = resist, \( d = \) [size of demand]
Figure 4. D equilibrium crisis outcome conditions for $c_{d1} = c_{t1} = \frac{1}{2}c_{d2} = \frac{1}{2}c_{t2}$
Figure 5. D equilibrium crisis outcome conditions for $c_{d1} = c_{t1} = 0; c_{d2} = c_{t2}$
Figure 6. D equilibrium crisis outcome conditions for $c_{d1} = \frac{1}{2}c_{t1}$; $c_{t1} = c_{d2}$; $c_{d2} = \frac{1}{2}c_{t2}$
Figure 7. D equilibrium war outcome conditions for $c_{d1} = c_{t1} = \frac{1}{2}c_{d2} = \frac{1}{2}c_{t2}$
Chapter 6

Short Term Resolve II: Empirical Evidence

In the previous chapter I developed a bargaining model that suggested that domestic political considerations could encourage leaders to provoke crises despite lacking long term resolve. Potential targets of such diversionary aggression were found to have no incentive to avoid such aggression, as the concessions required to prevent conflict would be considerable. Further, if they choose to resist under such conditions, the leader is very likely to back down.

In this chapter, I assess the empirical validity of several of the propositions derived from that model. I proceed in four steps. First, I will discuss the method by which the propositions will be evaluated. This section will address the translation of formal propositions into testable hypotheses and the data and methods to be used to evaluate them. Second, I will present the results of statistical tests regarding the occurrence of crises and the actions take by the participants in the dispute. This section provides evidence that for the period 1949-2000, in the presence of diversionary incentives (as I have conceived them) the United States is more likely to experience the onset of Militarized Interstate Disputes (MIDs), and the US is more likely to back down during these disputes. To demonstrate the robustness of the
results, I rerun the primary model using alternative dependent variables, demonstrating that diversionary incentives increase the probability that the US will be targeted by other states and the US is more likely to enter into nonfatal disputes. These results confirm the central expectations of the model. I bolster the argument by presenting statistical evidence regarding the consequences of MIDs that occur when diversionary incentives are present. Analyzing changes in presidential approval, I find that the rally effect is smaller following disputes that were begun when the president faced incentives to divert, but the size of such rallies often remains meaningful. In the third section, I discuss the substantive effects of these results. I present graphically the effect of diversionary incentives on the probability of dispute onset, the probability that the US backs down in a dispute, and predicted values of the change in presidential approval when a dispute occurs. Fourth, I briefly discuss two illustrative cases: a dispute initiated by Libya in 1984 and a dispute initiated by Syria in 1996. Drawing on newspaper accounts, I argue that these cases exemplify the theoretical expectations of the model. In both cases, longstanding conflicts of interest were present, yet there is evidence that the US placed increasing diplomatic pressure on these states as electoral considerations grew more pressing for the US president.

6.1 From Formal Propositions to Testable Hypotheses

Each of the propositions derived from the model posits the occurrence of observable behavior. However, matching these formal statements to specific relationships between independent and dependent variables for which data are available is not always possible. Even when it is, the exact variables and relationships among them to best capture the essence of the propositions may not be obvious, and so some justification of the choices I have made in doing so is warranted. I will address the empirical evaluation of each in turn, including whether and how each is to be tested here.
6.1.1 Dispute Occurrence

In everyday language, proposition 1 states that when diversionary incentives are absent, approval will not effect the probability of conflict. The $\sim D$ equilibrium technically predicts no conflict at all, but as discussed in the previous chapter, the model assumes complete information and the results do not preclude conflict stemming from private information, the primary rationalist explanation for war (Fearon, 1995). Therefore, it is more appropriate to interpret the $\sim D$ equilibrium as stating that there is no relationship between approval and conflict when diversionary incentives are absent.

The proposition consists of two parts. The first posits that when $\epsilon=0$, approval will have no effect on the probability of conflict. This is readily transformed into the following hypothesis:

**Hypothesis 1.** During non-election years, presidential approval will not have a significant effect on the probability of conflict.

The second part of the proposition is that when $\epsilon=1$ and $\alpha < 0.45$ or $\epsilon=1$ and $\alpha > 0.55$, approval will have no effect. This follows from the model since the $\sim D$ equilibrium operates under these conditions. However, to test whether or not there is any effect of a variable separately over different ranges of values is not a standard practice. The second part of this proposition is consistent with the hypotheses to be derived from other propositions of the model, which focus more explicitly on the behavior expected during election years. I will therefore return to the relationship between approval and conflict in election years in the discussion of the other propositions of the model.

Propositions 2a and 2b relate the probability of the occurrence of a dispute to the costs of engaging in dispute short of war, given the presence of diversionary incentives, with 2a focusing on the costs to the target and 2b the costs to the leader. The difficulty with assessing these propositions empirically is that there is no obvious measure of the costs of
a dispute. Colaresi (2007) argues that lower levels of hostility (as identified by the MID project) are inherently less costly. That is, issuing a threat imposes fewer costs on the leader than does showing force, which would require mobilizing some military units. This might suggest that leaders are more likely to be involved in MIDs with lower hostility levels when diversionary incentives are present. The problem with offering this as a hypothesis to test Propositions 2a and 2b is that the model leads us to expect the target to be likely to resist under these conditions. If the target is more likely to resist, it is not clear that this should lead us to expect that disputes occurring when the leader faces diversionary incentives are progressively less likely to reach each hostility level coded by the MID project. It is not clear that the mutual use of force on a limited scale, with few to no casualties, would be any less consistent with the expectations of the model than the mutual exchange of threats to use force, particularly since the latter dispute might not produce a significant rally.

A satisfactory test of Propositions 2a and 2b would have to focus on the expected costs of a dispute short of war in a way that is not dependent on the level of hostility. A more appropriate test would be able to assess the empirical validity of statements such as the following: When provoking a dispute that would involve the mutual use of force is likely to impose fewer costs on either or both sides, such a dispute is more likely to occur. As I am unaware of a satisfactory means of assessing this more appropriate claim, I will leave Propositions 2a and 2b untested here.

While it will would not surprise many if Proposition 2b found empirical support, Proposition 2a is more counterintuitive. The implication that leaders facing diversionary incentives are more likely to provoke crises as resistance becomes more inviting to the target is novel, and explicitly portrays leaders as looking for conflict. Devising an appropriate test for this claim warrants future attention.

Propositions 3a, 3b, and 3c offer expectations about the probability of a war or a dispute across different values of the probability of victory in war for the leader. These relationships
depend critically on the precise value of the probability of victory. Unfortunately, it is impossible to assess empirically which of these outcomes is likely to occur, as only crude indicators of the probability of victory are available and there is no reliable way of determining whether any indicator variable is above or below a certain theoretical threshold. As the relationship between the probability of victory and expected conflict outcomes is not linear, reliance on an indicator is insufficient. Therefore I offer no hypotheses to test Propositions 3a, 3b, and 3c.

Proposition 4, in combination with the subsequent one, seeks to capture the essence of Propositions 3a, 3b, and 3c in a more testable form. Rather than isolating specific conditions under which the model predicts crises with certainty, this proposition focuses on conditions that are more readily identified empirically, and could include either equilibrium. As such, the probability of a dispute will be greater where these conditions hold than under those conditions where only the ∼ \( D \) equilibrium holds.

In everyday language, this proposition says that during presidential election years, when the president’s approval is between 45 and 55, the probability of a dispute is greater than if either condition is absent. Identifying presidential election years with precision is not a difficult matter. However, approval data has measurement error, and so it would be inappropriate to test this proposition by focusing on exact values of presidential approval.

Rather, the best approach would be to include quadratic terms, thereby testing for a curvilinear fit. If the model is correct, we should observe an inverted U-shaped curve, with an inflection point somewhere around 50%. By employing a quadratic fit rather than a dummy variable equal to one if approval falls within a specified range, I rely on a more flexible indicator. This approach allows the data to speak for themselves. If the true relationship between approval and dispute occurrence is best captured by an inverted-U with an inflection point around 70%, we will learn nothing by employing a dummy variable for approval values between 45 and 55, but a quadratic fit will uncover this relationship. If the relationship
posed by the model is in fact the most appropriate description of the true underlying process, the quadratic fit is no less likely to reveal this than would a dummy variable. Therefore, I offer the following hypothesis:

_Hypothesis 2. During election years, presidential approval will have an inverted U-shaped effect on the probability of a dispute._

I consider the onset of a dispute to be the most appropriate test of the model. The reason for this is that the model describes conditions under which the Leader will provoke a crisis by demanding more concessions than the target is willing to give. The model does not require that verbal threats of the sort that would be picked up by the MID project accompany such demands, nor does it preclude such. What the model describes as resistance by the Target could just as easily entail a hostile act that was not preceded by any militarized US actions (but may have been preceded by aggressive demands delivered through backdoor diplomatic channels) just as easily as it could imply the reciprocation of US militarized hostility. Therefore, the model does not suggest which side will take the first codeable militarized action as per the rules of the MID project. Nonetheless, it is conventional in the literature to focus on initiation, and it may be informative if the subsequent analysis reveals evidence in support of dispute onset during the conditions I focus on, but reveals different patterns depending up on which state is the initiator of the dispute. This suggests the following hypotheses:

_Hypothesis 3. During election years, presidential approval will have an inverted U-shaped effect on the probability that the US initiates a dispute._

_Hypothesis 4. During election years, presidential approval will have an inverted U-shaped effect on the probability that the US is targeted in a dispute._
I noted in the last chapter that the model does produce conditions under which a leader facing diversionary incentives would be willing to escalate a crisis to war, but that these conditions appear to be quite restrictive. In the interest of demonstrating that the disputes that occur under diversionary incentives are indeed crises short of war, as expected, I disaggregate the dispute variable for an additional test. This next hypothesis will help us determine whether it is true, as many scholars have argued, that the diversionary perspective is not really a theory of war, but a theory of low-level conflicts. I take that logic one-step further. Rather than focusing on disputes resulting in 1000 battle fatalities, as required by the Correlates of War definition of a war, I will distinguish between ”crises” and ”fatal conflicts”. I define crises as those MIDs that involve any level of hostility from a threat to a use of force resulting in 25 or fewer casualties. I will define fatal conflicts as those MIDs that involve greater than 25 casualties, including full-fledged wars.

This constitutes a powerful test of the model. If this hypothesis is supported, it will suggest that leaders facing diversionary incentives not only actively seek to provoke crises, but are quite effective at preventing these crises from escalating into larger scale conflicts. If leaders systematically prevent the crises they provoke when faced with diversionary incentives from reaching the necessary intensity to produce more than 25 casualties, this casts serious doubt on the incentives of potential targets to avoid diversionary aggression. The final hypothesis regarding the onset of crises then is:

_Hypothesis 5_. During election years, presidential approval will have an inverted
_U-shaped effect on the probability of a nonfatal crisis and no effect on the probability of a fatal conflict, relative to no dispute._
6.1.2 Dispute Outcome

The bulk of the model’s implications deal with the conditions under which we expect to see leaders that are facing diversionary incentives provoke crises, producing hypotheses regarding the onset of disputes. This last proposition turns the focus to conflict outcomes. Specifically, the proposition states that the probability that the leader backs down is greater when the leader entered into the dispute under diversionary incentives. As with Proposition 4, this proposition transforms precise expectations from the model into general conditions that are associated with an increased probability of an outcome, as we cannot directly measure the critical parameters of the model that would be necessary to make more precise predictions.

As with Proposition 4, I will test Proposition 5 by focusing on a quadratic fit of approval in election years rather than employing a dummy variable for precise values of approval. The following hypothesis will serve to test Proposition 5:

\textit{Hypothesis 6. During election years, presidential approval will have an inverted U-shaped effect on the probability that the US backs down in a dispute.}

6.1.3 Auxiliary Hypotheses

If all of the above hypotheses are supported, it will constitute considerable evidence that when a leader with uncertain reelection prospects approaches the end of the electoral cycle, crises short of war are likely to occur and the leader is unlikely to be resolved in these crises. However, one might still reasonably question the cause of this pattern. These results are consistent with the model presented in the previous chapter, but they might not be inconsistent with alternative explanations. For example, one might argue that when elections are proximate, leaders who are uncertain of their prospects for reelection might be extremely risk averse, very much the opposite of the diversionary perspective. Knowing this, other states would have an incentive to prey on these vulnerable leaders. This would account
for the occurrence of crises as well as the higher probability of these vulnerable leaders backing down. To the extent that we observe vulnerable leaders initiating disputes under these conditions, the explanation may be that the other state made excessive demands for concessions of the vulnerable leader, prompting that leader to take the first militarized action. None of the evidence presented thus far would distinguish between this alternative explanation and the theoretical model I have developed.

In order to demonstrate that the behaviors I expect are occurring for the reasons I have posited, I present auxiliary hypotheses. These hypotheses do not test propositions that follow deductively from the model, but rather explicitly test one of the key assumptions of the model, concerning the rally effect. Absent an incentive to provoke crises from which she will back down, the leader has no incentive to behave the way I have described, and if we are observing crises under these conditions, it would have to be for alternative reasons (such as the hypothetical alternative explanation above). If, on the other hand, we observe that leaders do enjoy an increase in approval in line with the stylized value assumed by the model (5%) even following disputes that occurred in the presence of diversionary incentives, then my confidence in the model will be strengthened.

As discussed in the previous chapter, there are reasons to believe a rational public would be unlikely to reward a leader for engaging in a crisis when they have every reason to suspect that her motivations for doing so are political (Colaresi, 2007). I have argued that this logic suggests that leaders engaging in crises when their private incentives for doing so are great should enjoy smaller rallies, but it does not preclude a positive, nonzero value. Colaresi’s model focuses on the public’s Bayesian belief that the leader’s behavior is motivated by the public interest. The greater their belief that this is true, the larger the rally effect will be. If a leader enters into a crisis with an election fast approaching, and her approval rating near 50%, the public is certainly more likely to conclude that the leader is acting out of private interest than public, but for this belief to go to 1, it would have to be true that no
leader would enter into a dispute under such convenient timing out of concern for the public interest. The model I have developed allows for the target state to take the first hostile action in these crises (albeit because the leader demanded unreasonable concessions). If the leader does not make the demand that provokes the crisis in a public forum, but rather through diplomatic channels, then the public may be unaware that the crisis leading them to rally behind the leader does not represent unprovoked aggression on behalf of the other state. For these reasons, I contend that the public may have sufficient uncertainty over the leader’s motives in entering a crisis for a modest, but politically meaningful, rally to occur despite the unquestionable presence of diversionary incentives. Nothing in this analysis contradicts the expectation that rallies will be smaller when the public believes the leader is very likely to be politically motivated. The critical question is whether the public believes this to be true so strongly that there is no benefit for provoking a crisis at all. To assess these claims, I offer the following hypotheses:

\textit{Hypothesis 7. The change in approval accompanying disputes that occurred under diversionary incentives will be smaller than the change in approval accompanying other disputes.}

\textit{Hypothesis 8. The net effect of disputes occurring under diversionary incentives on the change in approval will be positive.}

\textbf{6.1.4 Data and Methods}

The above hypotheses will be tested using quarterly data from 1949 – 2000. For hypotheses $H1$ through $H5$ the unit of analysis is the dyad-quarter, and the data set comprises over 28,000 observations. For hypothesis $H6$, the unit of analysis is the dispute-quarter. There are just over 200 observations in this data set. Hypotheses $H7$ and $H8$ will be tested with a monadic quarterly time series data set. This data set also has just over 200 observations.
Quarters are used rather than yearly data in each case because the key independent variable, presidential approval, fluctuates highly within a year, and too much information would be lost by employing that level of aggregation. As for an even more precise level of aggregation, this is unfortunately not possible, given data limitations. The approval data are fitted for quarters from the Gallup organization’s presidential approval polls, which are the longest running approval surveys. Yet Gallup does not conduct the survey at regular time intervals. Some quarters have several observations (which were then averaged), others only have one. Given that some months did not have any approval surveys, it would be impossible to construct a meaningful monthly series.

The dependent variables for all the analyses except those of $H7$ and $H8$, which concern changes in approval, come from the Militarized Interstate Dispute data set (version 3.02). A MID occurs whenever there is a threat, display or use of force by one state against another (Ghosn, Palmer & Bremer 2004). The coding method of the dependent variables is as follows.

The dependent variable for the first two hypotheses is dispute onset. The dependent variable for $H3$ is dispute initiation by the US. A state initiates a dispute when it is an original participant in the dispute rather than a subsequent joiner and was coded by the MID project as having taken the first militarized action. The dependent variable for $H4$ is the initiation of a dispute against the US by the other state in the dyad. The dependent variable for $H5$ is ordinal, taking on a value of 0 when no disputes began, a value of 1 when a “crisis” begins (where a crisis is defined as a dispute with 25 or fewer casualties) and a value of 2 when a “fatal conflict” begins (where a fatal conflict includes greater than 25 casualties). Of the 234 disputes in the data set, 213 qualify as crises and 21 meet the criteria of a fatal conflict. The dependent variable for H6 takes on a value of 1 if the US either yielded or the dispute resulted in a compromise and a 0 if the either side achieved a clear victory.

\footnote{Note that in a few instances, both states in the dyad initiated separate disputes against one another in the same quarter. In those cases, the both dependent variables would take on a value of one.}
the opposing side yielded, or if the dispute ended in a stalemate.\(^2\) I include compromises in addition to those incidences where the US yielded because compromises necessarily entail some concessions on behalf of the US. Insofar as the model seeks primarily to distinguish between cases where leaders did not stand firm behind their demands and those cases where leaders exhibited the resolve to press their demands, I believe compromised solutions are more consistent with the former than the latter.\(^3\) I chose to code victories for the opposing state as 0 because, while such an outcome is clearly desirable for the opposing state, it does not indicate that the US backed down.\(^4\) I set the variable equal to 0 when the dispute ended in victory for the US, yielding on behalf of the opposing state or stalemate as none of these outcomes indicate that the US retreated from its position in the dispute.\(^5\)

The dependent variable for the final two hypotheses is the quarterly change in presidential approval. I focus on changes in approval rather than actual values of approval for two reasons. First, it better captures the notion of a rally effect, which is by definition an increase in approval. Second, this mitigates concerns of serial autocorrelation, a problem raised when current values of a variable are strongly determined by previous values, as is likely to be true with presidential approval.\(^6\)

\(^2\)If the dispute ended in any other outcome, the variable is coded as missing. These other outcomes include “released” for disputes that involved seizures, “unclear” and “joined ongoing war”.

\(^3\)I also ran the model with compromises coded as 0, as a robustness check. Many observations were completely determined, so it is difficult to interpret the results of this model, but to the extent that the results are informative, they are consistent with those reported here.

\(^4\)There are only 3 disputes that end in victory for the opposing state in this data set. As a further robustness check, I recoded “back down” to include these observations as equal to 1. The results were similar to those reported here.

\(^5\)However, as one might reasonably question treating stalemates as equivalent to victories for the US or yielding on behalf of the opposing state, I recoded the variable setting those outcomes equal to -1, stalemates equal to 0, and yields by the US or compromises equal to 1. I estimated multinomial logits with first stalemates then US victory and yields by the opposing state as the reference categories. The results indicate that approval has the hypothesized inverted U-shaped effect in election years on the probability of the US backing down relative to either base category.

\(^6\)I tested the residuals of the model included here for white noise, which is required to establish independence of observations as assumed by OLS. The results indicate that the residuals are white noise. I then estimated the same model with the level of presidential approval as the dependent variable and tested the residuals of that model for white noise. The results indicate that the residuals of the latter model are not white noise, violating a necessary assumption for OLS.
The key independent variables include quadratic measures of presidential approval and a dummy variable for presidential election years, as discussed in chapter 2. The first independent variable is the lag of the averaged quarterly observations of Gallup presidential approval. As discussed above, Gallup does not conduct approval surveys at regular intervals. To create a quarterly time-series of approval, the average of all approval surveys conducted within a quarter is reported. This variable is lagged to avoid predicting conflict behavior with approval numbers that potentially were observed after the dispute.\footnote{The exception to this rule is the first quarter of a new presidency, in which the first recorded quarterly observation of approval for that president is used for both the first and second quarters of their presidency. While this is potentially problematic in that some disputes may occur before the approval numbers that are supposedly predicting them, I consider it even more problematic to allow the approval of an outgoing president to predict the conflict behavior of the sitting president.} This term and its square, interacted with election year, test the hypothesized curvilinear effects during election years. To properly estimate the effect of the interaction between approval, approval\(^2\), and election years, it is necessary to include all the constitutive terms in these interactions (Braumoeller, 2004).

For the two hypotheses regarding the change in approval, I rely on a dummy variable, “vulnerable”, indicating whether the president’s approval was between 45 and 55 rather than the continuous measure of approval used in the rest of the analysis. I chose this approach to simplify the interpretation of the effects.\footnote{Nonetheless, as a robustness check, I performed an analysis identical to the one here with approval and approval\(^2\). The results were complex, but the predicted effects were generally similar to those presented here. Specifically, presidential approval exerts a U-shaped effect on changes in approval following a dispute in an election year, suggesting that the stronger the incentive to divert, the smaller the rally. Despite this, the predicted change in approval is positive for most every value of approval, and is always higher in election years following a dispute than if no dispute occurred.}

Following Gowa (1998), the dummy for presidential election years is lagged one quarter, as the diversionary perspective yields no expectation about disputes occurring after election day (which is roughly one third of the way into the fourth quarter). In this manner, the fourth quarter of the year prior to a presidential election (such as 1995 or 1999) is counted as an election year.
Finally, the analyses of hypotheses $H_1$ through $H_6$, all those dealing with conflict behavior, also include a measure of the US’s relative share of material capabilities. The relative capabilities term equals the U.S. military capabilities as defined by the Correlates of War project (Singer, Bremer and Stuckey, 1972) divided by the sum of the capabilities of both the U.S. and its opponent. This term serves to capture the effect of the probability of government victory, one of the central parameters of the theoretical model. The rationale here is that when the US possesses a majority of the capabilities in the dyad, it is more likely to prevail in a conflict.\(^9\)

Where the dependent variables are binary (hypotheses $H_1$ through $H_4$ and $H_6$), a logit estimator is employed for the analysis. Where the dependent variable is categorical (hypothesis $H_5$), a multinomial logit is employed. Where the dependent variable is continuous (hypotheses $H_7$ and $H_8$), ordinary least squares is employed. Robust standard errors, clustering on the second state in the dyad, are included to adjust for spatial dependence for the dyadic analyses (hypotheses $H_1$ through $H_6$). Following Beck, Katz and Tucker (1998), natural cubic splines of the number of quarters since the last dispute in the dyad are included to adjust for temporal dependence where dyad-quarters are employed (hypotheses $H_1$ through $H_5$). The duration of the dispute and the time since the last dispute are included in the analysis of dispute outcomes ($H_6$).\(^10\)

Having described the research design employed for each stage of the analysis and the operationalization of the key variables, I turn now to the results.

\(^9\)While there are certainly other factors that determine the probability of victory in war, the effect of capabilities has been found to be important, and monotonically increasing (\(?\), \(?\)). Relative capabilities is therefore not a perfect measure of the model’s parameter, but it is a reasonably good proxy.

\(^10\)The duration of the dispute is an important control that could affect the relationship between the key independent variables and the outcome of the dispute. The longer a dispute runs, the less relevant it will be that the president of the US began the dispute facing diversionary pressure, as the probability that the dispute has continued past the election grows.
6.2 Statistical Results

In this section I present the results of the statistical analyses. I will discuss whether each hypothesis receives empirical support and the significance of these results. As the substantive effects of maximum likelihood estimation results are not directly interpretable in the way coefficient estimates from an Ordinary Least Squares regression are, I will present predicted effects for duration and outcome in the next section. I will discuss the results for each dependent variable in turn.

6.2.1 Dispute Onset

The first results I shall discuss concern the probability of dispute onset. This analysis serves to test hypotheses $H_1$ and $H_2$. Note that hypothesis $H_1$ is worded generally, as the expectation will be the same for each dependent variable analyzed. Rather than restating the hypothesis for each dependent variable, I will note whether it is supported or not separately for each portion of the analysis.

Table 1 portrays the results from the logit analysis, with the coefficient estimates in the second column, the robust standard errors in the third column, and the significance levels in the fourth column. Due to the presence of interaction terms between approval, approval$^2$ and election year, the coefficient estimates on the base terms tell us the effect of presidential approval on the probability that the US enters into a dispute in non-election years. This is precisely the effect hypothesized by $H_1$. Neither approval nor approval$^2$ approaches conventional levels of significance, leading me to conclude that in non-election years, presidential approval has no significant effect on the probability of dispute onset. The coefficient estimates on the interaction between election and approval and between election and approval$^2$ indicate an inverted U-shaped relationship, and both appear significant. This suggests that at least for some range of values of approval, the results are consistent with
hypothesis $H_2$.

Consistent with the extant literature, relative capabilities is significant, and indicates a pacifying effect of a preponderance of material capabilities (Lemke 2002, ??). This may not be inconsistent with the model. As the previous chapter demonstrated, crises short of war are more likely to occur the smaller the probability of victory for the leader. However, given the possibility of war when the probability of victory is high and the costs of war are low, and the inability to determine how plausible these conditions might be empirically with any accuracy, it is difficult to definitively say that the model suggests this relationship or not. If these conditions are not prohibitively restrictive, then the model may suggest the observed relationship between relative material capabilities and the probability of crises short of war while suggesting the opposite relationship between capabilities and war. This result cannot speak to that claim, however the analysis of $H_5$, which distinguishes between crises and fatal conflicts, will more directly speak to the potential opposing effects of capabilities on different conflict outcomes.

### 6.2.2 Dispute Initiation

I turn now to an analysis identical to the preceding one except for the dependent variable. All of the independent variables are the same, as is the estimation technique. This analysis provides a test of $H_3$ and a further test of $H_1$. Table 2 portrays the results from the logit analysis. The table is formatted identically to Table 1, the same format that will be used to present the results of all the analyses here.

As with the analysis of dispute onset, neither approval nor approval$^2$ approaches conventional levels of significance. This is further evidence in support of the claim that in non-election years, presidential approval has no significant effect on the probability of conflict, here conceptualized as the initiation of disputes by the US. In contrast to the above
results, the coefficient estimates on the interaction between election and approval and between election and approval\textsuperscript{2} are also statistically indistinguishable from zero. This suggests that while diversionary incentives, as conceptualized here, increase the probability that the US will enter into disputes, but will not necessarily take the first militarized action in these disputes.

It is worth noting that relative capabilities have the same effect on dispute initiation as they do on dispute onset. This is the only variable to achieve significance, however. Even the time since the US last initiated a dispute against the opposing state is not a significant predictor of the probability of dispute initiation, whereas the time since the last dispute was a strong predictor of the probability of dispute onset.

As discussed above, the lack of a relationship between diversionary incentives and US dispute initiation is not necessarily inconsistent with the model. The model only suggests that leaders facing diversionary incentives will knowingly make extreme demands, deliberately provoking a crisis. Whether those extreme demands are backed by a show of force or a threat to use force, which would register as a MID, is not specified by the model. If the US placed diplomatic pressure on an opposing state, expressing clearly an unwillingness to settle for concessions short of some level the opposing state could be anticipated to find unacceptable, that state might take hostile action towards the US. This leads to the next portion of the analysis, where the dependent variable becomes initiation of disputes against the US.

6.2.3 Initiation Against the US

Here I discuss the results regarding the probability that militarized interstate disputes are initiated against the US. This analysis provides a test of $H_4$ and a further test of $H_1$. The results appear in Table 3.
Again, neither approval nor approval$^2$ approaches significance. Regardless of initiator, there does not appear to be any relationship between presidential approval and the probability of conflict during non-election years.

The coefficient estimates on the interactions between election and approval and between election and approval$^2$ are both significant at least at the 0.05 level. This suggests that in the presence of diversionary incentives, the US is no more likely to initiate disputes, but is more likely to find itself in disputes, owing to the fact that other states become more likely to initiate dispute against the US. This result adds a layer of complexity to the literature on diversion. The theoretical model did not clearly specify which state is expected to take the first militarized action, allowing the interpretation that diversionary conflicts sometimes occur because vulnerable leaders back their victims into a corner, prompting them to act first. The empirical results here suggest support this interpretation of the model while offering no systematic evidence that presidents facing diversionary incentives are any more likely to initiate disputes themselves.

It is worth noting that relative capabilities have the same effect here as with the previous analyses. Regardless of the initiator, the relative share of material capabilities held by the US exerts the same effect on the probability of conflict. This is strong and consistent evidence in support of the pacifying effect of power preponderance.

### 6.2.4 Nonfatal Crises

Here I discuss the results regarding the probability that militarized interstate disputes are initiated against the US. This analysis provides a test of $H_4$ and a further test of $H_1$. The results appear in Table 4.

The coefficient estimates on the interactions between election and approval and between election and approval$^2$ for nonfatal crises are highly significant. The coefficient estimates
on approval and approval\(^2\) again do not even approach standard levels of significance. For fatal conflicts, the coefficient estimates on the interactions between election and approval and between election and approval\(^2\) are oppositely signed and significant at the 0.05 level. Again, the coefficient estimates on approval and approval\(^2\) are insignificant.

The strong significance of the results for nonfatal crises in conjunction with the suggestion that approval exerts the opposite effect on fatal conflicts (during election years) strongly suggests that diversionary incentives encourage presidents to enter into crises but not to escalate them. This analysis also suggests that the foregoing results were weakened by pooling fatal and nonfatal conflicts together when diversionary incentives exert different effects on the two outcomes.

The model did not lead us to expect that approval exerts a U-shaped effect on the probability of fatal conflicts, as the results here suggest. However, I have argued that the model does not yield any clear expectations regarding fatal conflicts. The results here are interesting, but are not necessarily inconsistent with the major implications of the model. The effect of diversionary incentives on nonfatal conflicts suggested here strongly supports the expectations of the model. It appears that presidents not only seek out crises when facing diversionary incentives, but succeed in preventing these crises from escalating.

As should come as no surprise by now, relative capabilities is very significant (\(p<0.001\)) and in the same direction as in the preceding analyses. Power preponderance appears to exert a pacifying effect regardless of the level of hostility. In light of the findings in chapter 4 that power preponderance does not have the expected effect on war duration, this is puzzling. Further investigation of the possibility that relative capabilities have differential effects on conflict by regime type would be useful.
6.2.5 Dispute Outcome

This analysis tests hypothesis H6, relating the effect of diversionary incentives on the probability that the US backs down in a militarized dispute. The results of the analysis are presented in Table 5.

The coefficient estimates on the interactions between election and approval and between election and approval$^2$ for are highly significant. The coefficient estimates on approval and approval$^2$ are insignificant. This confirms the expectation that during election years, presidential approval exerts a U-shaped effect on the probability that the US backs down. Interestingly, the effect of relative capabilities is insignificant, as is the length of time since a previous dispute.

The duration of the dispute in days has a positive and significant (p<0.01) effect on the probability that the US backs down, suggesting a general effect whereby the US becomes more likely to back down in a dispute the longer it continues. This effect is not contingent on the domestic political situation, as the duration term was not interacted with either approval or election year. This intriguing result suggests that despite being the most powerful state in the international system throughout the period covered here, the US consistently was less likely to stand firm in a dispute as it dragged on. While this variable was included primarily as an important statistical control, the result is consistent with the general theme developed in this dissertation that states involved in conflicts with democratic states need not believe they are likely to win militarily, rather they often pursue a strategy of holding out long enough to force the democratic state to relent.

6.2.6 Presidential Approval

I turn now to analyzing changes in presidential approval, evaluating whether the alleged motive for the behavior predicted by the model has any empirical basis. The primary ex-
pectations of the model having been confirmed, one might yet remain skeptical that the behaviors occur for the reasons stipulated by the model. This analysis seeks to defuse such potential criticisms by demonstrating directly that presidents do in fact benefit from such opportunistic behavior, despite outward indicators that the president is likely to be politically motivated.

Table 6 presents the results from the linear regression of changes in approval on variables capturing diversionary incentives and dispute occurrence. Notice that all the independent variables have been lagged one quarter. This ensures that the alleged causes temporally precede the supposed effects.

Due to the presence of a large number of interaction terms, interpretation of these results is not straightforward. The first thing to notice is that every coefficient estimate is statistically significant at or below the 0.05 level. The coefficient estimate on dispute suggests that if the president enters into a dispute when her approval is outside the vulnerable range of 45 to 55% and it is not a presidential election year, the expected change in approval on average is a little less than zero (−9.35 plus the intercept of 9.075). While most previous studies did not control for the domestic political context to this extent, this is broadly consistent with previous findings that the average rally effect is very small (Lian & Oneal 1993).

The coefficient estimates on the interactions between dispute and election, dispute and vulnerable, and the three-way interaction between dispute, election and vulnerable tell a nuanced story. These results suggest that individually, the effects of entering into disputes when vulnerable or in presidential election years (both of which ought to increase the public’s belief that the president’s actions are politically motivated) lead to positive changes in approval the following quarter. However, the effect of the confluence of all three conditions, entering into disputes under diversionary incentives, is smaller than the sum of the individual effects. This is qualified support for hypothesis $H7$. I note that it is qualified, since the expectation was that entering into disputes under diversionary incentives would
produce smaller rallies than entering into disputes under any other condition. The results here suggest a more complicated relationship, where the change in approval following some disputes might be smaller than the change following a dispute entered into in the presence of diversionary incentives (such as if the president’s approval was outside the vulnerable range and the dispute began outside of an election year).

Hypothesis $H_8$ stipulates that the net effect of entering into disputes in the presence of diversionary incentives will be positive. If we sum all the coefficients in the model (all of the variables take on a value of one under this condition), the net result is around 0.6, slightly greater than 0. Perhaps more importantly, the results of this analysis suggest that if the president does not enter into a dispute when she is vulnerable in an election year, her approval will drop by 2.4 on average, so the net effect of entering into a dispute in the presence of diversionary incentives versus not entering into a dispute is in fact 3.0. This is well in line with the 5% assumed in the previous chapter. These results suggest that presidents most in need of a rally effect are unable to procure a particularly large boost in approval by entering into disputes, but the change in approval that results if they enter into a dispute is nonetheless likely to be large enough to be politically meaningful under these conditions.\footnote{Due to the presence of a triple interaction term, even where all three variables are binary, extreme caution is warranted in interpreting these results.}

To further illustrate these and the preceding results, I now turn to graphical presentations of the substantive effects of these statistical analyses.

### 6.3 Graphical Results

The predicted probabilities depicted in Figure 1 were generated using the results from Table 1. Relative capabilities, peace-quarters and the respective cubic splines were held constant at their means. In this sample, the mean US share of relative capabilities is 0.97. One will
notice that the predicted probability of a dispute with a given state in a given quarter is quite low. This should not be surprising, as most of the time the US is not in conflict with most states. However, the analysis here is not meant to produce a predictive model. Rather, I am interested in demonstrating a systematic relationship between domestic political vulnerability and conflict behavior. Therefore, the actual magnitude of the predicted probabilities is not particularly interesting. The relative change in the probability of a dispute as we manipulate values of presidential approval in election years is more important.

I do not include in the graph the effect of approval in non-election years, as the statistical analysis indicated that the estimated coefficients on approval and approval² in non-election years could not be distinguished from zero.

As Figure 1 demonstrates, the hypothesized inverted U-shaped effect of approval on the probability of a dispute in election years is born out. Further, the inflection point of this curve appears to be relatively close to 50%, consistent with the theoretical model’s focus on approval values ranging from 45 to 55.

The substantive effect indicated by the graph is considerable. As approval goes from 25% to 50% the probability of dispute increases more than 310%. When presidential approval is at 25%, diversionary incentives will be lacking, while they would be significant at 50%. The probability of a dispute even at 50% remains quite small, but a more than threefold increase in the probability of a dispute is not easily dismissed.

Now consider the predicted probability that a dispute is initiated by another state against the US, based on the analysis in Table 3. Figure 2 presents a graph of the effect of presidential approval on the probability that a dispute is initiated against the US by different values of presidential approval. The election variable is held constant at 1 and the other variables (relative capabilities, peace-quarters and the cubic splines) are held at their means.

Figure 2 shows the expected inverted U-shaped curve. This graph is quite similar to that of dispute onset. The actual probabilities are very similar, although generally a little
bit larger. The inflection point appears to be a little below 50% in this graph, though this difference is probably not statistically significant.

Again, I do not include the predicted probability of dispute initiation against the US in non-election years as approval was not found to have a significant effect outside of presidential election years. I also have not included a graph for the effects of diversionary incentives on the probability that the US initiates a dispute against another state, as the statistical analysis revealed no significant relationship with regard to that outcome.

Next I discuss the predicted probabilities of the nonfatal crisis and fatal conflict outcomes based on the analysis in Table 4. Figure 3 presents a graph of the predicted probabilities of each outcome relative to no dispute by values of presidential approval, with election year equal to 1 and all other variables held constant at their means.

Figure 3 is quite similar to Figures 1 and 2, with respect to nonfatal crises. The curve for fatal conflicts reveals several interesting points. First, the probability of a fatal conflict is always considerably lower than the probability of a nonfatal crisis, as might be expected. Second, while the estimated coefficients suggest a U-shaped effect of approval on fatal conflicts in election years, the graph reveals that the probability of a nonfatal conflict rises as one moves towards higher values of approval, but remains relatively flat even at the lowest values of approval. This suggests that extremely unpopular presidents are marginally more likely to enter into fatal conflicts than moderately unpopular presidents, but the real effect of interest is that the more popular the president is, the more likely a fatal conflict becomes, with this effect largely isolated to the highest values of approval.

Regarding nonfatal crises, the curve is quite similar to that of dispute onset, with probabilities being very similar, although generally a little bit smaller. The inflection point appears to be a little closer to 50% in this graph, though this difference is likely insignificant statistically. A nonfatal crisis becomes about 400% more likely as approval changes from 25% to 50%. The effect of diversionary incentives appears to be a little bit stronger on nonfatal
crises than on all disputes, although the difference is not great.

As Figure 3 illustrates, the effects estimated in the statistical results are substantively meaningful. Further, the effects are generally quite similar regardless of the dependent variable, as Figures 1, 2 and 3 all depict very similar patterns and substantive impacts despite predicting different outcomes.

The predicted probability that the US backs down in a militarized dispute is generated based on the analysis in Table 5. Figure 4 presents a graph of the predicted probability that a dispute ends with the US backing down, given that a dispute occurred during an election year. All other variables held constant at their means.

Figure 4 is quite similar to Figures 1 through 3, though the curve has a higher peak (higher kurtosis). More of the variance is due to infrequent extreme values than more common smaller deviations. The change in the predicted probability of backing down is considerable as we move from extreme values of approval towards more moderate values, with a maximum probability just over 0.14 and a minimum probability approaching 0. This effect is considerably stronger than the effect of diversionary incentives on the other outcomes, suggesting that focusing on diversionary incentives tells us more about the outcomes of those disputes that do occur than it does about whether a dispute with any given state will or will not occur.

The effect of diversionary incentives on the outcome of a dispute suggested by these results is considerable. Approximately 75% of all disputes in the MID data set in the time frame of this study (1949 – 2000) ended in a stalemate. Just over 11% of all disputes in this time period ended with one side or the other yielding, or in a compromise (the criteria by which I defined backing down). It is quite rare for any state to back down in a dispute, according to the criteria employed here. Consider also that the US was the most powerful state in the international system throughout this period. Further, many scholars

\[\text{At least, as observed at the directed-dispute-dyad level}\]
have argued that leaders of democracies are particularly unlikely to back down in disputes
est they suffer audience costs for doing so (Fearon 1994). In light of these considerations, a
predicted probability of 0.14 for the US to back down in a dispute is quite striking. It is not
clear that there should be any incentive for states to make concessions or in any other way
alter their behavior to avoid conflict with vulnerable US presidents in election years.

Here, I discuss two graphs of the expected changes in presidential approval based on the
analysis in Table 6. Figure 5 presents the expected quarterly change in approval by election
year, domestic political vulnerability, and disputes. Figure 6 presents the net expected
quarterly change in approval if the president enters into a dispute by election year and
vulnerability. Figure 5 is thus more complete, as the expected change in approval for every
possible state of the world (based on the variables analyzed here) can be discerned. Figure
6 focuses on a more intuitive interpretation of the one factor over which the president has
control, the decision to enter into a dispute. In this way, Figure 6 better speaks to the
presence or absence of incentives to enter into a dispute under different structural conditions.

One striking pattern revealed by Figure 5 is that, all else equal, presidents who are neither
vulnerable nor facing an approaching election expect their approval to increase by about 9
points. If they choose to enter into a dispute, despite there being little reason to believe
doing so would be driven by political motivations, their approval is expected to decrease by
a fraction of a percentage point. This effect does not follow readily from the model in the
previous chapter, nor is it anticipated by previous research.\footnote{Any explanation I would offer would therefore be quite ad hoc, and atheoretical. I draw attention to this
point because it is unexpected, and I do not wish to appear to have ignored this puzzling result. However, I
leave it to future work to investigate the robustness of this finding, and potential explanations if it is found
to persist.}

Perhaps more intuitively, Figure 5 reveals that presidents who do not enter into disputes
when they are vulnerable or as an election looms tend to gradually drop in popularity. Ap-
proval may tend to decline in election years, regardless of whether the president is vulnerable
or not, for several reasons. Mueller (1970) proposed that all presidents rely on a “coalition of minorities” and argued that the longer a president remained in office, the more inevitable it became that she would alienate some portions of her coalition, and therefore approval is expected to steadily decline from the moment a president takes office. This effect may be driven by presidential election campaigns. In the absence of a critical message, the public tends to accept the prevailing message from the government (Zaller 1992, Berinsky 2007). Criticism of the president is probably most likely to occur when the president is facing a direct challenge during an electoral campaign. Likewise, during non-election years, one would expect criticism of the president to be greater when she is vulnerable, when criticism is most likely to have an effect. When the president’s approval is extremely low, we certainly do not expect many other politicians to be lavishing praise upon the president, but neither do we expect them to devote the same level of resources to convincing the public that the president is not doing a good enough job as they might when the public’s support for the president is in the middle of the spectrum.

To better appreciate the effect of disputes on presidential approval, I focus on the net change (the change in approval in the event of a dispute less the change in approval without a dispute), as illustrated in Figure 6. Here we see that the rally following a dispute in an election year is essentially the same regardless of whether the president was vulnerable or not. This contradicts the expectation of $H7$. Indeed, the only negative rally occurs when both factors that contribute to diversionary incentives are absent. However, $H8$, which posits that the net effect of entering into a dispute under diversionary incentives will be positive, accords well with the graphical results. While Figure 5 shows that the president’s approval is only expected to increase by less than one percentage point if she enters into a dispute in an election year while vulnerable, Figure 6 reveals that, considering the expected decline if the president did not enter into a dispute, the president’s approval will overall be about 3 percentage points higher if she enters into a dispute than if she does not. Recall that
for simplicity, the formal model I developed in the previous chapter assumed a 5% increase in approval. The 3% expected based on the statistical results suggests this assumption is reasonable.

The predicted changes in approval depicted in the two figures facilitate easier interpretation of the results presented in Table 7. They reveal a number of surprises, including the apparent strong incentive for presidents who are neither facing proximate elections nor vulnerable in terms of their popularity to avoid entering into international disputes. Further, the results do not support the rather intuitive notion that those presidents who most need a rally ought to have the hardest time securing one, which previous studies have demonstrated (Colaresi, 2007). Rather, the results here suggest that those presidents who least need a rally would further be punished for pursuing one, suggesting not only the lack of a political motive to enter into disputes, but in fact a strong disincentive to do so. Those who are in greatest need of a rally are by no means greatly rewarded for acting out on their political motives, but they are still better served by entering into disputes rather than passively accepting the decline in their approval that would result otherwise.

This latter effect appears to be at odds with previous studies, but few previous studies of the rally effect or diversion have considered the reverse side of the coin, the effect of doing nothing in the face of a fast approaching election, on approval. This analysis suggests the real value of entering into a dispute when facing incentives to divert is not an expectation of a large increase in approval so much as mitigating the decline in approval that tends to occur when presidents are vulnerable or nearing an election.

The reason for the curious finding that leaders who stand to benefit most from a rally nonetheless succeeding in procuring rallies despite the indicators that their behavior is likely to be politically motivated may not be as puzzling as it first appears. Here I offer two tentative explanations for this pattern.

As suggested by the results in Tables 4 and 5, diversionary incentives do not encourage the
US to initiate disputes against other states, but are associated with an increased probability of initiating disputes against the US. Perhaps presidents succeed in producing rallies when they need them most by taking pains to create opportunities for action through backing target states into a corner diplomatically and then reacting when those states initiate a militarized dispute. The public may be less likely to infer political motivation when they read in the newspaper that the US has reacted to the aggression of other states as it is likely that many members of the public are unaware of the diplomatic moves preceding the crises they would require to know that changes in the US’s policies in the region may have, in part, prompted the aggression.

Another potential reason presidents succeed in producing rallies when the public ought to suspect political motivation is that presidents appear to be careful enough not to drop an “October surprise” on the American electorate. The closer to Election Day an international crisis occurs, the more skeptical the public ought to be, by the same reasoning that the public ought to be more skeptical in election years than non-election years (Colaresi, 2007). Yet when disputes occur in the presence of incentives to divert, they are more likely to occur in the first half of the year. To illustrate this claim, I estimated a logistic regression with the dependent variable being equal to 1 for the first 6 months of the year and 0 for the latter 6 months for all disputes in this time frame. The results confirm that diversionary incentives significantly increase the probability that any dispute that occurs will occur in the first half of the year.

To see this, consider the coefficient estimates on the interactions between election and approval and between election and approval². Both are significant at p<0.001. To facilitate interpretation, Figure 7 presents the effect of presidential approval on the predicted probability that a dispute occurs in the first half of the year for disputes occurring during election years. The inflection point of this curve appears to be farther away from 50% than observed in the other graphs, although this might well be observed by random chance, given that
small changes in the coefficient estimates would produce large changes in the precise shape of the curve.

There appears to be evidence that diversionary incentives do not encourage vulnerable leaders to go and initiate disputes just before elections, which would likely strike the public as incredibly suspicious. Rather, the results here indicate that vulnerable US presidents have found themselves the target of disputes more frequently than they have chosen to initiate them themselves, and these disputes tend to occur months ahead of Election Day, creating an element of plausible deniability of political motivation.

However, I confess that these are attempts to explain an unexpected pattern in the effect of disputes and the conditions surrounding their onset on changes in presidential approval. While I believe they may be useful, they remain tentative, and the topic warrants further investigation.

Having analyzed changes in presidential approval, the alleged motivation for diversionary behavior, I turn now to two illustrative cases.

6.4 Illustrative Cases

The following cases are examples of the patterns of behavior suggested by the model and largely confirmed by the statistical analysis. As in chapter 4, I present these cases to illustrate real world examples of the general trends discussed so far, they are not intended to “test” any of the hypotheses developed here or provide further confirmation of the theory. In fact, these cases are observations included in the statistical analyses performed above, and so cannot possibly add additional information.

In neither case can I claim that the historical record indisputably reveals that the factors I focus on were instrumental in the way events unfolded. However, I have not claimed that diversionary incentives inevitably lead to conflict. The formal model suggests that
diversionary incentives are sufficient to lead to conflict, all else equal - but of course, all else is never equal. The statistical results suggest that diversionary incentives significantly increase the probability of conflict and increase the probability that if a dispute does occur, it will end with the vulnerable leader backing down. I believe the two cases to be discussed highlight several other important factors, but support the claim that the domestic political context in the US also contributed to both the timing and outcome of these incidents. I hope the reader agrees that had presidents Reagan and Clinton been more confident of their reelection prospects in 1984 and 1996, respectively, events in North African and the Middle East might well have played out differently.

### 6.4.1 Libyan Air Strike on Sudan, March of 1984

Late in 1983, Sudanese President Gaafar al-Nimeiry divided southern Sudan into three sub-regions, a move that southerners claimed reduced their influence and autonomy. He also imposed Islamic laws in the south, a primarily pagan and Christian region. These moves, as well as increased training and aid from Libya and Ethiopia, fueled a renewal in the insurgency against the Sudanese government (NYT, 3/26/1984).

Colonel Qaddafi, leader of Libya, said on March 2nd, “We tell the agents in the Sudan that we are allied with the popular revolution in the southern Sudan for the sake of liberating Sudan inch by inch, just as Lebanon was liberated. The United States cannot save that mean man who is hiding in Khartoum...We must force America to fight on a hundred fronts all over the earth. We must force it to fight in Lebanon, to fight in Chad, to fight in the Sudan and to fight in El Salvador,” (NYT, 3/20/1984).

On March 4th, 1984, President al-Nimeiry said he was prepared to go to war against Libya and Ethiopia if they did not stop supporting the rebel groups in southern Sudan. “The Sudan will beat Qaddafi if he opts for war against the Sudan. The Sudan will not remain
fold-handed if Qaddafi tries to instigate our neighbors against us,” (NYT, 3/4/1984).

Yet despite this bold proclamation, it appears President al-Nimeiry was worried about Sudan’s military security. The Vice President of Sudan, General Omar Mohammed el-Tayeb, announced on March 5th that the United States had agreed to fly in military supplies to help the Sudanese government. The Reagan administration denied this report, and two days later, President al-Nimeiry confirmed that no US airlift was approved (NYT, 3/4/1984; NYT, 3/26/1984). The Sudanese continued to request American military assistance to help put down the insurgency. A top aide to Secretary of State Shultz visited Sudan on March 6th to discuss the requests, indicating that the US was receptive to the notion of increasing its support for Sudan. How receptive was unclear, as the aide cautioned, “No specific decisions have been made yet on either weapons systems or mode of delivery,” (NYT 3/6/1984).

In response to increasingly hostile Sudanese rhetoric and indicators that the US might step up its support of Sudan in some capacity, Libya dropped five bombs on Omduran, the second largest Sudanese city on March 16th. Libya officially denied responsibility for the attack, but Jean Kirkpatrick told the UN Security Council that Libya had to accept responsibility, as the US had photographed the TU-22 bomber, which looks like no other aircraft in the region, and Libya was the only country known to have it (NYT, 3/28/1984).

Following the attack, the US deployed two Airborne Warning and Control Systems aircraft, along with tanker planes, to Egypt to join Egyptian fighters in patrolling Sudanese airspace. The United States told Libya there would be “serious consequences” if it interfered with the AWACs surveillance planes, (NYT, 3/20/1984).

Undeterred, Qaddafi issued a speech on March 28th. He criticized Egypt and Sudan for arranging the delivery of the US AWACs planes and said the US should leave inter-Arab conflicts to Arabs. “As long as America challenges and defies us, we shall, as from today, think seriously of upsetting the balance in this region unless America withdraws immediately from Egypt and Sudan,” (NYT, 3/29/1984).
The crisis essentially ends at this point. The United States never withdrew from Egypt or Sudan, yet neither did Qaddafi make good on his promise to “seriously” upset the balance in the region. Nor is there any evidence that Qaddafi ceased supporting rebel groups in southern Sudan. What seems clearer is that the attack dampened domestic criticism of President al-Nimeiry, according to both Sudanese and Western officials (NYT, 3/26/1984). President Reagan’s approval rating in the first quarter of 1984 was 48.2%, but it had climbed to 54% by the second quarter of 1984. How much of this bump, if any, resulted from the crisis with Libya is unclear, but it is interesting that the US returned to ignoring Sudan’s pleas for greater help after Reagan’s approval climbed.

6.4.2 Syrian-Turkish Border Tensions, June of 1996

In November of 1985, Kurdish rebels, members of the PKK (Kurdish Worker’s Party), destroyed a Turkish border post with rocket fire, wounding two Turkish security guards. The PKK had waged a long campaign against the Turkish government, but what made this particular incident unique was that the Kurdish guerrillas, largely operating out of Syria, generally infiltrated Turkey before staging their attacks from within the country. But these rockets had been fired from Syrian soil. Syria promptly denied that Damascus harbored anti-Turkish guerrillas, (NYT, 6/15/1996).

Tension between Turkey and Syria had persisted for some time. Turkish calls for the US to place more pressure on Syria to cut off its support for the Kurdish rebels were likewise not new in June of 1996. But when the Turkish president visited the US that June, saying, “Mr. President, please take note that this is an issue of vital importance for Turkey”, he found the US more attentive than in the past. As the New York Times observed, “the Clinton Administration cannot be in a position to look soft on Syria in an election year,” (NYT, 6/15/1996).
Syria not only supported the PKK in its long-standing campaign against the Turkish government, Syria also backed the Palestinian National Liberation Organization in its campaign against the Israeli government. Adding to regional tensions, Israel and Turkey had tightened their military ties in February of 1996, providing Israeli planes access to Turkish bases, a move Syria feared was part of a strategy to squeeze it from both sides, particularly after Israel elected the hard-liner Benjamin Netanyahu in the first direct election for Prime Minister on May 29th, (NYT, 6/16/1996, 6/20/1996/ 6/24/1996).

Throughout May and into June, explosions wracked Syria, including the capital city of Damascus. The Syrian government did not allow any of the blasts to be reported in the press, and described a United States Embassy advisory as "silly and baseless", (NYT, 6/10/1996). Al-Hayat, the Arabic-language daily newspaper published in London, later reported that the attacks were suspected to have been sponsored by Turkey in attempt to assassinate Abdallah Ocalan, the head of the PKK (AFP, 6/15/1996).

The Syrians then massed troops near their border, as did the Turkish in response. The Clinton Administration warned that Syria risks military confrontation with the US if it goes to war with Turkey, (AFP, 6/15/1996).

Formally, the dispute essentially ends here, as the Syrian, Turkish, and American governments take no further actions directly related to the incident. In particular, no further American involvement in events to immediately follow is evident. However, two weeks later, on June 27th, rebels snuck into the West Bank through Jordan and killed three Israeli guards. Prime Minister Netanyahu and Jordanian King Hussein both asserted they believed the attack to have been sponsored by Syria in an attempt to damage Israeli-Jordanian relations. Prime Minister Netanyahu said in a radio interview that the attack stemmed from a source against not only Israel, but Turkey as well, and that “we will have to work, with international cooperation, to put political and, if necessary, economic pressure on Damascus,” (NYT, 6/27/1996). Then, on July 3rd, less than 48 hours after Turkey’s Islamic Party took
office, a Kurdish woman killed nine Turkish soldiers in a suicide attack. The NYT reports, “Syria is evidently using the PKK as an instrument to press Turkey to accept a series of political demands it also wants Turkey to end its military cooperation with Israel,” (NYT, 7/3/1996).

These border tensions between Turkey and Syria, and the US warnings of potential American involvement if Syria goes to war, are but a chapter in a much larger story. However, it is striking that Turkish demands for greater US pressure on Syria went largely unheeded both before and after this dispute, while the intricate rivalry between Syria, Jordan, Israel, and Turkey were an important feature of Middle East politics for years before and after these events. In the second quarter of 1996, President Clinton’s reelection prospects looked a little uncertain, with his approval rating at 49.6%. Come the third quarter of 1996, however, his approval rating had climbed to 54.7%. Again, it is difficult to demonstrate that this is the direct result of the crisis, but it is telling that once the increase in approval occurred, the United States became far less receptive to Turkey’s continued pleas for greater assistance.

6.4.3 Discussion of the Cases

These two cases exhibit a striking number of similarities. Neither constitutes anything even approaching a smoking gun in establishing that American presidents seek out disputes by placing diplomatic pressure on target states until they react, allowing the US to respond without looking like the instigator. In fact, by its nature, the type of behavior described by the diversionary argument, even the more nuanced version presented here, is unlikely to ever produce a smoking gun.

However, in both cases, the circumstances are suggestive. The underlying tensions that produced the crisis into which the US inserted itself were present for years both before and after the incidents described here. In both cases, a US ally embroiled in an ongoing rivalry
with a neighboring state suffers attacks from insurgent groups allegedly sponsored by that neighbor. In both cases, that ally had been lobbying the US to step up pressure on the state sponsoring the rebel groups, and the US response left its ally disappointed. In both cases, the ally decides to take more aggressive moves of its own, provoking the opposing state and prompting swift threats of US intervention should things escalate, under very similar and revealing circumstances. In both cases, the president’s approval was just under 50%, and a presidential election loomed a few months away.

The circumstances of these cases do not conclusively illustrate the process described by the formal model in the previous chapter. However, they exhibit a number of important characteristics that fit nicely with the predictions of that model, particularly the domestic political context in the US when it decides to come to the aid of its ally, who had requested aid many times previously.

The cases also point to several important elements overlooked by the model. Both cases suggest the importance of rebel groups in inciting international crises. This phenomenon is overlooked not just here, but by much of IR, where interstate conflict is often considered to be a separate and independent process from insurgency or terrorism, with a few notable exceptions (Bapat, 2007; Bapat, draft).

Both cases also highlight the possibility of allies strategically responding to domestic conditions within the US. Others have written of the ability of alliances to free the weaker partner to act more aggressively than they otherwise would (Palmer and Morgan, 2006). However, few have considered this in the context of diversionary incentives in the stronger alliance partner. Just as Smith’s (1996) introduction of attention to the potential target state’s ability to react strategically to domestic vulnerability, the cases discussed here suggest that it might be fruitful in future work to consider the incentives to allies of a vulnerable leader to exploit that leader’s need for a rally to gain greater military assistance.
6.5 Conclusion

In this chapter, I made explicit the decisions I made in seeking to empirically test the formal propositions derived from the formal model in the previous chapter. I presented a series of statistical tests of hypotheses adapted from these formal propositions, nearly all of which were supported. The one hypothesis I was forced to unequivocally reject was H7, which addressed the relative size of a rally effect following different types of disputes.

This hypothesis does not directly follow from the model, but instead was suggested by previous studies (Colaresi, 2007). While the finding that presidents are rewarded about as much for entering into disputes under conditions that would strongly suggest political motivation as they are for entering disputes under other conditions is surprising, and deserves attention in future work, it does not actually detract from the argument made here very much. I also offered a few possible explanations for this finding that I hope will guide future work on motivations for diversionary conflict, including evidence that diversionary incentives increase the probability that any dispute that does occur will occur in the first half of the year rather than the latter half and that diversionary incentives increase the probability that the US will be targeted in a dispute but not the probability that the US will initiate a dispute. These two factors suggest presidents seek to avoid transparently pursuing a rally effect by seeking to prompt aggression on behalf of opposing states through private, diplomatic channels and avoiding the conspicuous occurrence of crisis too close to Election Day.

I also discussed two illustrative cases, which provide suggestive evidence that domestic conditions in the US influenced the occurrence of disputes amidst ongoing tensions. The potential for a dispute in each of these cases was relatively constant and only when the conditions became favorable for the US president to enter into a crisis did the US’s allies take provocative actions that lead the opposing state to initiate a dispute against them,
prompting the US to threaten to intervene.

Overall, the results in this chapter offer considerable evidence in support of the more restricted version of diversionary conflict I have advanced. This analysis suggests that those critics of diversion who question whether there is sufficient motivation for diversion have overstate the case, but have highlighted important points. I have argued that the conditions that encourage diversion, and the behaviors expected to occur under those conditions, are both more nuanced than traditional diversionary accounts portray them to be. However, once the diversionary argument is more carefully specified, with closer attention to the motivations of both the would-be aggressor and potential target, strong support was found for a wide array of behaviors.
Table 6.1: Domestic Political Vulnerability and Dispute Onset, US Dyads, 1949-2000

<table>
<thead>
<tr>
<th>Variable</th>
<th>Coefficient Estimate</th>
<th>Standard Error</th>
<th>P-value</th>
</tr>
</thead>
<tbody>
<tr>
<td>Election Year</td>
<td>-4.826</td>
<td>2.218</td>
<td>0.012 **</td>
</tr>
<tr>
<td>Approval</td>
<td>-0.035</td>
<td>0.047</td>
<td>0.461</td>
</tr>
<tr>
<td>Approval^2</td>
<td>3.77E-04</td>
<td>4.53E-04</td>
<td>0.406</td>
</tr>
<tr>
<td>Election*Approval</td>
<td>0.197</td>
<td>0.077</td>
<td>0.006 ***</td>
</tr>
<tr>
<td>Election*Approval^2</td>
<td>-0.002</td>
<td>0.001</td>
<td>0.004 ***</td>
</tr>
<tr>
<td>Relative Capabilities</td>
<td>-5.239</td>
<td>0.463</td>
<td>&lt;0.001 ***</td>
</tr>
<tr>
<td>Peace-quarters</td>
<td>-0.066</td>
<td>0.012</td>
<td>0.001 ***</td>
</tr>
<tr>
<td>Spline 1</td>
<td>-1.3E-05</td>
<td>1.15E-05</td>
<td>0.259</td>
</tr>
<tr>
<td>Spline 2</td>
<td>6.0E-06</td>
<td>9.59E-06</td>
<td>0.532</td>
</tr>
<tr>
<td>Spline 3</td>
<td>-2.55E-06</td>
<td>4.68E-06</td>
<td>0.585</td>
</tr>
<tr>
<td>Constant</td>
<td>2.472</td>
<td>1.216</td>
<td>0.042 **</td>
</tr>
</tbody>
</table>

N = 28,648

prob < χ^2 = <0.001

Pseudo-R^2 = 0.2006

Logit Regression. Robust standard errors. * p ≤ 0.1, ** p ≤ 0.05,

***p ≤ 0.01, one-tailed test where appropriate
Table 6.2: Domestic Political Vulnerability and Dispute Initiation, US Dyads, 1949-2000

<table>
<thead>
<tr>
<th>Variable</th>
<th>Coefficient Estimate</th>
<th>Standard Error</th>
<th>P-value</th>
</tr>
</thead>
<tbody>
<tr>
<td>Election Year</td>
<td>-2.971</td>
<td>3.657</td>
<td>0.417</td>
</tr>
<tr>
<td>Approval</td>
<td>0.080</td>
<td>0.069</td>
<td>0.243</td>
</tr>
<tr>
<td>Approval²</td>
<td>-0.001</td>
<td>0.001</td>
<td>0.307</td>
</tr>
<tr>
<td>Election*Approval</td>
<td>0.104</td>
<td>0.144</td>
<td>0.235</td>
</tr>
<tr>
<td>Election*Approval²</td>
<td>-0.001</td>
<td>0.001</td>
<td>0.260</td>
</tr>
<tr>
<td>Relative Capabilities</td>
<td>-5.982</td>
<td>0.553</td>
<td>&lt;0.001  ***</td>
</tr>
<tr>
<td>Peace-quarters</td>
<td>-0.021</td>
<td>0.016</td>
<td>0.208</td>
</tr>
<tr>
<td>Spline 1</td>
<td>1.74E-05</td>
<td>1.35E-05</td>
<td>0.196</td>
</tr>
<tr>
<td>Spline 2</td>
<td>-1.6E-05</td>
<td>1.24E-05</td>
<td>0.196</td>
</tr>
<tr>
<td>Spline 3</td>
<td>4.85E-06</td>
<td>5.90E-06</td>
<td>0.411</td>
</tr>
<tr>
<td>Constant</td>
<td>-1.220</td>
<td>1.809</td>
<td>0.500</td>
</tr>
</tbody>
</table>

N               | 28,648               |
prob < $\chi^2$ | <0.001               |
Pseudo-$R^2$    | 0.1461               |

Logit Regression. Robust standard errors. * $p \leq 0.1$, ** $p \leq 0.05$, *** $p \leq 0.01$, one-tailed test where appropriate
Table 6.3: Domestic Political Vulnerability and Initiation Against the US, US Dyads, 1949-2000

<table>
<thead>
<tr>
<th>Variable</th>
<th>Coefficient Estimate</th>
<th>Standard Error</th>
<th>P-value</th>
</tr>
</thead>
<tbody>
<tr>
<td>Election Year</td>
<td>-3.215</td>
<td>2.301</td>
<td>0.081 *</td>
</tr>
<tr>
<td>Approval</td>
<td>0.005</td>
<td>0.066</td>
<td>0.945</td>
</tr>
<tr>
<td>Approval^2</td>
<td>2.96E-05</td>
<td>6.11E-04</td>
<td>0.961</td>
</tr>
<tr>
<td>Election*Approval</td>
<td>0.152</td>
<td>0.087</td>
<td>0.040 **</td>
</tr>
<tr>
<td>Election*Approval^2</td>
<td>-0.002</td>
<td>0.001</td>
<td>0.022 **</td>
</tr>
<tr>
<td>Relative Capabilities</td>
<td>-5.786</td>
<td>0.610</td>
<td>&lt;0.001 ***</td>
</tr>
<tr>
<td>Peace-quarters</td>
<td>-0.013</td>
<td>0.019</td>
<td>0.507</td>
</tr>
<tr>
<td>Spline 1</td>
<td>2.39E-05</td>
<td>1.69E-05</td>
<td>0.157</td>
</tr>
<tr>
<td>Spline 2</td>
<td>-2.31E-05</td>
<td>1.58E-05</td>
<td>0.142</td>
</tr>
<tr>
<td>Spline 3</td>
<td>9.92E-06</td>
<td>7.88E-06</td>
<td>0.208</td>
</tr>
<tr>
<td>Constant</td>
<td>0.735</td>
<td>1.834</td>
<td>0.689</td>
</tr>
</tbody>
</table>

N 28,648
prob < $\chi^2$ <0.001
Pseudo-R^2 0.1476

Logit Regression. Robust standard errors. * $p \leq 0.1$, ** $p \leq 0.05$, *** $p \leq 0.01$, one-tailed test where appropriate
Table 6.4: Domestic Political Vulnerability and Nonfatal
Crises, US Dyads, 1949-2000

<table>
<thead>
<tr>
<th>Variable</th>
<th>Coefficient Estimate</th>
<th>Standard Error</th>
<th>P-value</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Nonfatal Crises</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Election Year</td>
<td>-6.300</td>
<td>2.359</td>
<td>0.004   ***</td>
</tr>
<tr>
<td>Approval</td>
<td>-0.041</td>
<td>0.049</td>
<td>0.403</td>
</tr>
<tr>
<td>Approval²</td>
<td>4.32E-04</td>
<td>4.71E-04</td>
<td>0.359</td>
</tr>
<tr>
<td>Election*Approval</td>
<td>0.266</td>
<td>0.084</td>
<td>0.001   ***</td>
</tr>
<tr>
<td>Election*Approval²</td>
<td>-2.69E-03</td>
<td>7.40E-04</td>
<td>&lt;0.001  ***</td>
</tr>
<tr>
<td>Relative Capabilities</td>
<td>-5.334</td>
<td>0.464</td>
<td>&lt;0.001  ***</td>
</tr>
<tr>
<td>Peace-quarters</td>
<td>-0.062</td>
<td>0.013</td>
<td>&lt;0.001  ***</td>
</tr>
<tr>
<td>Spline 1</td>
<td>-7.70E-06</td>
<td>1.22E-05</td>
<td>0.528</td>
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<tr>
<td>Spline 2</td>
<td>1.13E-06</td>
<td>9.97E-06</td>
<td>0.909</td>
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<td>Spline 3</td>
<td>-1.10E-07</td>
<td>4.94E-06</td>
<td>0.982</td>
</tr>
<tr>
<td>Constant</td>
<td>2.632</td>
<td>1.301</td>
<td>0.043   *</td>
</tr>
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<td><strong>Fatal Conflicts</strong></td>
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<td></td>
<td></td>
</tr>
<tr>
<td>Election Year</td>
<td>3.172</td>
<td>2.468</td>
<td>0.199</td>
</tr>
<tr>
<td>Approval</td>
<td>0.051</td>
<td>0.118</td>
<td>0.666</td>
</tr>
<tr>
<td>Approval²</td>
<td>3.46E-04</td>
<td>9.86E-04</td>
<td>0.726</td>
</tr>
<tr>
<td>Election*Approval</td>
<td>-0.169</td>
<td>0.092</td>
<td>0.067   *</td>
</tr>
<tr>
<td>Election*Approval²</td>
<td>1.92E-03</td>
<td>9.74E-04</td>
<td>0.049   *</td>
</tr>
<tr>
<td>Relative Capabilities</td>
<td>-4.060</td>
<td>1.001</td>
<td>&lt;0.001  ***</td>
</tr>
<tr>
<td>Peace-quarters</td>
<td>-0.103</td>
<td>0.036</td>
<td>0.004   ***</td>
</tr>
<tr>
<td>Spline 1</td>
<td>-5.64E-05</td>
<td>2.98E-05</td>
<td>0.059   *</td>
</tr>
<tr>
<td>Spline 2</td>
<td>4.14E-05</td>
<td>2.71E-05</td>
<td>0.127</td>
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<td></td>
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</tr>
<tr>
<td>--------</td>
<td>------------</td>
<td>------------</td>
<td>------------</td>
</tr>
<tr>
<td>Spline 3</td>
<td>-1.78E-05</td>
<td>1.40E-05</td>
<td>0.201</td>
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<tr>
<td>Constant</td>
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<td>3.904</td>
<td>0.391</td>
</tr>
<tr>
<td>N</td>
<td>28.648</td>
<td></td>
<td></td>
</tr>
<tr>
<td>prob &lt; $\chi^2$</td>
<td>&lt;0.001</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Pseudo-$R^2$</td>
<td>0.1950</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

Multinomial Logit Regression. Robust standard errors.

* $p \leq 0.1$, ** $p \leq 0.05$,

**$p \leq 0.01$, one-tailed test where appropriate
Table 6.5: Domestic Political Vulnerability and Backing Down in Disputes, US Dyads, 1949-2000

<table>
<thead>
<tr>
<th>Variable</th>
<th>Coefficient Estimate</th>
<th>Standard Error</th>
<th>P-value</th>
</tr>
</thead>
<tbody>
<tr>
<td>Election Year</td>
<td>-58.345</td>
<td>21.936</td>
<td>0.008   ***</td>
</tr>
<tr>
<td>Approval</td>
<td>-0.277</td>
<td>0.282</td>
<td>0.325</td>
</tr>
<tr>
<td>Approval²</td>
<td>2.84E-03</td>
<td>2.46E-03</td>
<td>0.249</td>
</tr>
<tr>
<td>Election*Approval</td>
<td>2.285</td>
<td>0.744</td>
<td>0.001   ***</td>
</tr>
<tr>
<td>Election*Approval²</td>
<td>-0.022</td>
<td>0.006</td>
<td>0.001   ***</td>
</tr>
<tr>
<td>Relative Capabilities</td>
<td>-1.445</td>
<td>2.205</td>
<td>0.512</td>
</tr>
<tr>
<td>Peace-quarters</td>
<td>0.007</td>
<td>0.012</td>
<td>0.547</td>
</tr>
<tr>
<td>Duration</td>
<td>1.08E-03</td>
<td>4.10E-04</td>
<td>0.008   ***</td>
</tr>
<tr>
<td>Constant</td>
<td>3.379</td>
<td>8.608</td>
<td>0.695</td>
</tr>
<tr>
<td>N</td>
<td>189</td>
<td></td>
<td></td>
</tr>
<tr>
<td>prob &lt; χ²</td>
<td>0.019</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Pseudo-R²</td>
<td>0.1646</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

Logit Regression. Robust standard errors. * $p \leq 0.1$, ** $p \leq 0.05$, *** $p \leq 0.01$, one-tailed test where appropriate
Table 6.6: Domestic Political Vulnerability, Disputes, and Changes in Approval, US Dyads, 1949-2000

<table>
<thead>
<tr>
<th>Variable</th>
<th>Coefficient Estimate</th>
<th>Standard Error</th>
<th>P-value</th>
</tr>
</thead>
<tbody>
<tr>
<td>Election Year&lt;sub&gt;t-1&lt;/sub&gt;</td>
<td>-9.792</td>
<td>4.399</td>
<td>0.027 **</td>
</tr>
<tr>
<td>Vulnerable&lt;sub&gt;t-1&lt;/sub&gt;</td>
<td>-11.179</td>
<td>4.194</td>
<td>0.008 ***</td>
</tr>
<tr>
<td>Election&lt;sub&gt;t-1&lt;/sub&gt;*Vulnerable&lt;sub&gt;t-1&lt;/sub&gt;</td>
<td>9.481</td>
<td>4.699</td>
<td>0.045 **</td>
</tr>
<tr>
<td>Dispute&lt;sub&gt;t-1&lt;/sub&gt;</td>
<td>-9.350</td>
<td>4.298</td>
<td>0.031 **</td>
</tr>
<tr>
<td>Dispute&lt;sub&gt;t-1&lt;/sub&gt;*Election&lt;sub&gt;t-1&lt;/sub&gt;</td>
<td>12.517</td>
<td>5.199</td>
<td>0.017 **</td>
</tr>
<tr>
<td>Dispute&lt;sub&gt;t-1&lt;/sub&gt;*Vulnerable&lt;sub&gt;t-1&lt;/sub&gt;</td>
<td>11.336</td>
<td>4.400</td>
<td>0.011 **</td>
</tr>
<tr>
<td>Disp&lt;sub&gt;t-1&lt;/sub&gt;*Elec&lt;sub&gt;t-1&lt;/sub&gt;*Vuln&lt;sub&gt;t-1&lt;/sub&gt;</td>
<td>-11.515</td>
<td>5.563</td>
<td>0.040 **</td>
</tr>
<tr>
<td>Constant</td>
<td>9.075</td>
<td>4.135</td>
<td>0.029 **</td>
</tr>
</tbody>
</table>

N | 207 |
prob > F | 0.036 |
Adjusted-R<sup>2</sup> | 0.1509 |

OLS Regression. Robust standard errors. * p ≤ 0.1, ** p ≤ 0.05, *** p ≤ 0.01, one-tailed test where appropriate
Table 6.7: Domestic Political Vulnerability and Occurrence of Dispute in First Half of Year, US Dyads, 1949-2000

<table>
<thead>
<tr>
<th>Variable</th>
<th>Coefficient Estimate</th>
<th>Standard Error</th>
<th>P-value</th>
</tr>
</thead>
<tbody>
<tr>
<td>Election Year</td>
<td>-15.188</td>
<td>4.337</td>
<td>&lt; 0.001 ***</td>
</tr>
<tr>
<td>Approval</td>
<td>-0.066</td>
<td>0.095</td>
<td>0.486</td>
</tr>
<tr>
<td>Approval²</td>
<td>7.04E-04</td>
<td>8.93E-04</td>
<td>0.430</td>
</tr>
<tr>
<td>Election*Approval</td>
<td>0.590</td>
<td>0.169</td>
<td>&lt; 0.001 ***</td>
</tr>
<tr>
<td>Election*Approval²</td>
<td>0.005</td>
<td>0.002</td>
<td>&lt; 0.001 ***</td>
</tr>
<tr>
<td>Relative Capabilities</td>
<td>0.740</td>
<td>0.487</td>
<td>0.129</td>
</tr>
<tr>
<td>Peace-quarters</td>
<td>-0.008</td>
<td>0.003</td>
<td>0.013 **</td>
</tr>
<tr>
<td>Constant</td>
<td>0.919</td>
<td>2.360</td>
<td>0.697</td>
</tr>
<tr>
<td>N</td>
<td>234</td>
<td></td>
<td></td>
</tr>
<tr>
<td>prob &lt; χ²</td>
<td>&lt;0.001</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Pseudo-R²</td>
<td>0.0385</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

Logit Regression. Robust standard errors. * $p \leq 0.1$, ** $p \leq 0.05$, *** $p \leq 0.01$, one-tailed test where appropriate
Figure 1. Predicted Probability of Dispute Onset in Presidential Election Years
Figure 2. Predicted Probability of Dispute Initiated Against the US in Election Years
Figure 3. Predicted Probability of Nonfatal and Fatal Conflicts in Election Years.
Figure 4. Predicted Probability that the US Backs Down in a Dispute, Election Years.

Probability US Backs Down

Presidential Approval

25 30 35 40 45 50 55 60 65 70 75

0 0.02 0.04 0.06 0.08 0.1 0.12 0.14 0.16
Figure 5. Predicted Changes in Presidential Approval by dispute, election year, and domestic vulnerability, 1949-2000.
Figure 6. Net Predicted Changes in Presidential Approval from no dispute to dispute by election year and domestic vulnerability, 1949-2000.
Figure 7. Predicted Probability that Dispute Occurred in First Half of Year for Disputes Occurring in Election Years.
Chapter 7

Conclusion

I began this dissertation observing two puzzles related to patterns of international conflict involving democracies, both of which are typified by the current US war in Iraq. The first puzzle is why wars involving democracies ever occur. Rationalist explanations of war argue that war primarily results from the desire to avoid making excessive concessions and the inability to identify what concessions are needed to avoid war due to incentives to misrepresent one’s value for war. Thus, war is not seen as the failure or breakdown of bargaining, but an unfortunately costly extension thereof, allowing states to learn the true level of resolve of their opponents and therefore make the concessions required to attain peace, but not more than that (Wagner 2000, Slantchev 2003b, Powell 2004a). The implications of this approach are beginning to amass considerable empirical support (Slantchev 2004, Werner & Yuen 2005, Ramsay N.d.). On the other hand, prevailing theories of the link between domestic politics and international conflict argue that democracies are more selective in choosing when to go to war, as they expect to be removed from office with greater probability if they lose (Reiter & Stam 2002, Bueno de Mesquita et al. 2003). Others argue that democratic leaders suffer considerable domestic costs for even threatening to use force without following through on such threats (Fearon 1994, Schultz 2001, Guisinger & Smith 2002). If true, the
threats of democratic leaders are unlikely be seen as bluffs, and there is little reason for the
target state to resist a threat to use force by a democracy. Ramsay (draft) has found that
power preponderance does not explain the duration of wars involving democracies, while it
does have the expected shortening effect in wars that do not involve democracies. Schultz
(2001) has found that democratic threats are less likely to be resisted. Bueno de Mesquita
and Smith have found that leaders of large coalition systems (generally democracies) can
“buy” foreign policy concessions from leaders of small coalition systems, suggesting that for-
eign policy success is not vital to retaining office for non-democratic leaders. If democratic
leaders are more accountable, and therefore more selective, and leaders of other states do
not require success in the international arena to hold onto office, it is difficult to imagine
how war is necessary to reveal information and facilitate bargaining.

The second puzzle, which further casts doubt on existing theories of democracy and war,
is the routine occurrence of prolonged, politically divisive wars involving democracies. While
wars involving democracies are on average shorter than wars involving no democracies, some
of the longest wars in modern history have involved democratic states. The current war in
Iraq is less atypical of general patterns of democratic warfare than we are often comfortable
admitting. Only five interstate wars in the postwar era have lasted more than a year: the
Vietnamese-Cambodian War; the War of Attrition; the Korean War; the Iran-Iraq War; and
the Vietnam War. Of these, three involved democracies. This is not to suggest that the
argument that democracies look for quick and easy victories is entirely incorrect - but it
appears to oversimplify the relationship between domestic politics and international conflict.

I have argued throughout this dissertation that the key to solving these puzzles lies
in adopting a dynamic view of accountability rather than assuming it always inheres in
democracy. A few important previous works have considered the possibility of variation in
accountability stemming from the behavior of the opposition party ( ),(see also (Ramsay
2004)). This important study departs from the conventional practice of assuming that the
institutional structure of a polity defines the level of accountability for policy outcomes facing a leader. However, these studies did not consider variation in accountability over the course of a single international conflict, which would result from the opposition initially supporting the use of force and later withdrawing that support and advocating an end to the war.

Building on Schultz’s work as well as the bargaining model of war, I developed a series of formal models that conceive of war as a series of battles, rather than an instantaneous event. Starting from the assumption that the opposition determines the stakes of the conflict in part by deciding whether or not to politicize the conflict through opposing force, I derived several important conclusions regarding opposition position and international conflict. Each of these implications received at least qualified support from the statistical analysis. If timed properly, changes in the opposition’s position can encourage the government to end a losing war when otherwise it might press on. Thus, I have demonstrated that the pattern assumed to hold for wars involving democracies in general emerges from my theoretical model, but only under certain conditions. The models have done more than provide the microfoundations of existing results, however. I also demonstrated that opposition support can push the government towards war when otherwise the government would not consider the conditions favorable to the use of force, as was arguably the case with the two Israeli invasions of Lebanon. Finally, poorly timed opposition, which I have argued might occur if the opposition not only hopes to capture the votes of moderates who value good policy outcomes but also its base, which rewards ideological purity, can cause the government to “gamble for resurrection”, as the US appears to have done in Vietnam and likely is doing now in Iraq. In contrast to previous “gambling for resurrection” arguments, the theoretical models developed here suggest that this behavior is not caused by regime type (Goemans 2000) or characteristics of the war (Downs & Rocke 1994), but only certain combinations of war performance and opposition positions.

To demonstrate that long term resolve and short term resolve are not only distinct con-
cepts, but need not even have the same causes, I developed a bargaining model that illustrates conditions under which democratic leaders are expected to be willing to enter into crises but are not willing to escalate the crisis to war. The implications of this model received considerable empirical support, highlighting the importance of distinguishing between different types of resolve.

I believe the theoretical models developed in chapter 3 provide a solution to the second puzzle. In current form, the models do not consider the role of incomplete information. Therefore, I am unable to offer a solution to the first puzzle. However, having established the potential of changes in the opposition’s position to influence the government’s accountability, and therefore it’s long term resolve, I believe I have laid the foundations for such an explanation. If the targets of democratic threats do not doubt that the democratic leader is willing to use force, but they do doubt whether the leader is willing to bear the costs of prolonged fighting, then a war whose purpose is to reveal information about long term resolve might result.

7.0.1 America’s Wars with Iraq

Regarding the puzzling change-of-heart with regards to invading Iraq and toppling Saddam that Dick Cheney appears to have experienced sometime between 1991 and 2003, I believe the analysis here presents one possible explanation. Nothing here can rule out the simple (and unsatisfactory) explanation that George H.W. Bush and his son, George W. Bush arrived at different conclusions regarding the wisdom of invading Iraq because they have different levels of intelligence, risk-propensity, or some other personality trait. Nor can we safely say that the international context was entirely comparable. Perhaps Saddam was genuinely perceived as being a greater threat in 2003 than in 1991. Given the tensions between the United States and Iraq throughout the 1990s (with 1998 in particular bearing a disturbing resemblance to
In many ways) and the lack of evidence that Saddam was tightly allied with al’Qaeda, it seems unlikely that a sudden change in the perceived threat posed by Saddam provides much by way of explanation, but it would be dismissive to say such factors did not play any role at all. However, based on the analysis here, I believe that changes in the domestic political context might be sufficient to produce different expected outcomes. Certainly other factors may also have played a role, many of which have received considerable attention, both in academia and punditry. Comparatively little attention has been paid to the change in the behavior of the opposition party. As the United States contemplated deploying troops to expel Saddam’s forces from Kuwait (and prevent them from invading Saudi Arabia), the Democrats adopted an ambiguous position. Many argued that the administration should give sanctions more time to work. But few outright opposed the use of force. The resolution authorizing the use of force split along party lines, but the Democrats agreed to stand behind President Bush if the resolution passed (NYT, 1/13/1991). Once active hostilities began, all criticism virtually disappeared. But as the war drew to a close, many hawks called on the president to invade Iraq and topple Saddam. At this time, opposition emerged for the first time. Even Joseph Lieberman, who failed to receive his party’s nomination to run for the Senate in 2006 due to his support for the current war in Iraq, opposed invading Iraq (NYT, 4/10/1991). With the opposition withdrawing its support for continuing the war after 3 months of fighting, and the course of battle widely expected to worsen if the US invaded, it is perhaps little surprise that President Bush chose to declare victory and end the war.

In contrast, in the long buildup to the 2003 US invasion of Iraq, the Democrats feared the embarrassment those who voted against the authorizing resolution 1991 felt when the war ended in quick victory. They also pointed out that due to the poor state of the economy, President George Bush failed to secure reelection in 1992, and similarly staked their hopes of defeating George W. Bush in 2004 on the state of the economy, rather than the war (NYT, 3/19/2003). Though antiwar sentiment was present from the onset of the war, the vote to
authorize force in 2003 enjoyed far greater support from the Democrats (now in minority in both chambers) than the similar resolution in 1991 (then a majority in both chambers) had. The Democrats did not oppose the war in Iraq to any considerable degree until well after the US took Baghdad. On March 22, Congress passed a bill praising the troops and citing President Bush’s “firm leadership and decisive action in the conduct of military operations in Iraq”, with unanimous support in the Senate and only 11 votes against in the House (NYT, 3/22/2003). Less than a month later, the interstate portion of the war was over, with the government of Iraq effectively dissolved, and President Bush declared major combat operations over. By the time the opposition opposed, the cost of fighting for another month was low, as the US was facing a relatively low-intensity insurgency at that time. Of course, the insurgency only grew in intensity over time, with 2006 in particular proving to be an extremely violent year, but by that time, the government was already committed to the war and was “gambling for resurrection.”

While many other factors were at play in both of these cases, the results of the analysis here suggest that the variation in the opposition’s behavior helps explain why President George H.W. Bush chose not to invade Iraq in 1991 while President George W. Bush decided to do so in 2003.

7.0.2 Extensions

I hope to have convinced the reader of the utility of viewing accountability and resolve as dynamic, continuous concepts rather than static, binary classifications. The focus on regime type in studies of the effects of domestic politics on international conflict forces us to make implicit assumptions about accountability (high in democracies, low in non-democracies, and unchanging over time for both). The tendency to view war as an instantaneous event likewise forces us to treat resolve as a binary measure (either a state is willing to fight a war
over a given issue or it is not). Having relaxed both of these views and treating resolve as a function of accountability, capable of changing over the course of a single event, produced new insights, allowing me to identify new patterns in the duration and outcome of wars, the occurrence, outcome, and political impact of low-level international disputes.

Finally, I believe this approach can offer insights into political outcomes beyond international conflict. I demonstrated in the second chapter that the conditions I later associated with diversionary incentives also explain the political business cycle. More broadly, a greater focus on party competition and previous policy performance (or the popularity that good policy produces) as determinants of accountability might help us explain when governments cling to failing policies and employ policies that are likely to be popular in the short term but might not serve the national interest in the long run. While accountability is generally thought of as a good thing, and lies at the heart of democratic theory, as the analysis in this dissertation reveals, the very fact that governments are highly concerned with their prospects for reelection can create perverse incentives. One important avenue for future research, building on the work here, would be to look for similar examples of opposition behavior encouraging governments to cling stubbornly to controversial policies that are not producing clear results with regards to other policy areas, such as the economy (exchange rates, whether to extend tax cuts, spending levels, etc), education (No Child Left Behind), law enforcement (War on Drugs), immigration, or other issues.

Other extensions aim to yield further insights into the relationship between party competition and international conflict. The first extension I plan to undertake will introduce uncertainty over the ideological preferences of the opposition. Building on previous work by Schultz (2005), I will assume that the opposition has two factions, one moderate, and one extreme. Moderates will be assumed to support all winning wars and only oppose losing wars when they know that by doing so, they will alter the government’s strategy. Extreme doves will be assumed to be willing to support wars for some period of time, but will withdraw
that support after some period of time, regardless of whether doing so is expected to result in the government ending the war. Extreme hawks will be assumed to be willing to support wars even past the point where withdrawing support would result in the government ending the war. I hope to demonstrate the uncertainty over whether the opposition consists predominantly of extremists or moderates, even when their general orientation (hawk v dove) is known can increase the probability that the target resists the government’s initial demand, resulting in an escalation of the conflict. I will then subject the implications of this argument to empirical testing by analyzing the partisan makeup of legislatures and the reciprocation of hostilities in militarized disputes.

The next extension will seek to establish the conditions under which the opposition chooses to oppose knowing that delaying opposition for a few more periods would succeed in prompting the government to end the war while opposing today would not. I will focus not only on ideological preferences but also variation in party systems in terms of encouraging (or discouraging) intra-party competition. This analysis will determine the extent to which democracies with weak parties and more candidate-centered elections, such as the United States, are more prone to suboptimal opposition behavior than are strong party systems. I will subject the implications of this argument to empirical testing by examining the determinants of legislators’ positions towards the War in Iraq over time in both the US and the UK, as well as analyzing the impact of legislators’ positions on their subsequent political careers.

Another extension will focus on relaxing the dichotomies in the current analysis. Rather than forcing governments and targets to either fight or quit in each period, and allowing the opposition to express which of the two it would prefer the government to do, I will endogenize both the goals and the commitment of the government in the war, allowing both to be continuous. The government and the target will make alternating proposals over how to divide the good in dispute, while each period selecting a level of resources to
employ for coercing the other state. The level of resources committed to the war will not only influence each sides’ ability to hurt their enemy, but also the maximum level of costs they will suffer. The opposition, rather than simply advocating fighting or quitting, will announce its preferred level of commitment and goals. I will seek to establish the conditions under which the government might mobilize a militarily suboptimal amount of resources (that is, failing to commit the level of resources that would maximize the probability of victory relative to the costs of fighting) in attempt to minimize opposition. I will subject the implications of this analysis to empirical testing with monthly measures of war goals, troop levels, and an ordinal measure of opposition.

Finally, I hope to construct a generalized model of party competition in multiple dimensions, exploring the implications of opposition and prior policy performance not only in regards to foreign policy but also the economy (and perhaps social issues) for electoral outcomes and policy outcomes (including foreign policy).

Focusing theoretically on variation in accountability induced by party competition or previous policy performance has the potential to produce unique insights into political behavior. In particular, I believe such an approach offers the ability to explain policy choices both by the government and the opposition that do not appear to make sense if we assume the relevant actors are concerned with producing good policy. Recent work in international relations has focused on how incentives to retain office can help explain why “bad policy is good politics” in non-democratic regimes (Bueno de Mesquita et al. 2003), but too few scholars of international relations are willing to challenge the accepted wisdom that good policy is good politics all of the time in democracies. I hope to have begun to fill this gap with this dissertation, and intend to continue to do so in future work.
Appendix A

Proofs for Chapter 3

In each period, players’ payoffs depend on the state \((w_t)\), the actions taken, and the continuation value, or discounted infinite sum of payoffs from future rounds. Let \(v_{at}\) and \(v_{bt}\) represent the continuation values for the Challenger and Target, respectively.

The continuation values are straightforward to calculate when one or more players quit. If the Challenger quits but the Target fights, the Challenger concedes victory in the crisis to the Target. In every subsequent round, the Challenger’s payoff is -1 while the Target’s payoff is 1. The reverse is true if the Target quits while the Challenger fights. If both players quit simultaneously, a negotiated settlement is said to have ended the crisis. In every subsequent period, the Challenger receives \(w_t\) and the Target receives \(-w_t\). The infinite sum of discounted future payoffs is straightforward when the payoffs are constant in all time periods. Thus, the Challenger’s continuation value for quitting this period knowing that the Target will fight is simply: \(-1 - \delta - \delta^2 - \delta^3 \ldots = -1/(1 - \delta)\). The Challenger’s continuation value for fighting this period knowing that the Target will quit is: \(1/(1-\delta)\). The Challenger’s continuation value for quitting in the same period that the Target quits is \(w_t/(1-\delta)\).

Calculating the continuation values when both players choose to fight (and thus the conflict continues) is more complicated. This involves calculating the infinite sum of future
payoffs where those payoffs cannot be known, as they will be determined by the players’ future decisions that will depend, in part, upon the outcomes of the battles, which are stochastic.

It is useful then to solve for the upper and lower bounds in terms of the per-period cost of fighting, $c_a$, that define the region wherein the Challenger’s optimal strategy is to fight in period $t$, but only choose to fight again in $t + 1$ if she wins in $t$, quitting otherwise. Call this strategy ($FF/FQ$). I will also discuss the critical value of $c_a$ above which the Challenger prefers quitting immediately ($Q$), fighting one more period before quitting, regardless of outcome, ($FQ$), and continuing unconditionally ($FF$).

I have assumed that all parameters of the model except the state variable, $w_t$, are invariant in $t$. That is, the cost of engaging in any given battle, and the probability of victory in any given battle, are constant across time. While this is an unrealistic assumption, it is necessary for tractability. The effect of this assumption should be to bias the players towards being willing to continue fighting losing wars, across all variants of the model.

Solving for critical values of $c_a$ does not allow me to produce a full solution to the game. Such a solution would not produce clear implications favorable to empirical testing, as most any behavior can be sustained given the right parameters of the model. However, this solution strategy enables me to compare the size of critical thresholds across the variants of the game that differ in their assumptions about the domestic political context, allowing me to deduce the effect of different domestic conditions in a straightforward manner. The focus in this appendix is on fleshing out the intuition behind the key propositions regarding the effect of domestic politics. See Smith (1998) for a more detailed discussion of existence of Markov Perfect Equilibrium with monotone strategies in a similar context.

**Lemma 1.** The Target’s strategies are symmetrical to the Challenger’s in the Basic War Fighting Game.
Proof: While I have only formally considered the conditions separating different strategies for the Challenger (Government), it is apparent from the payoffs in the Basic War Fighting Game that the Target’s payoffs are structured identically to those of the Challenger. While the Challenger seeks to maximize $w_t$ and the Target seeks to minimize it, there are no important aspects of either’s payoff structure that differ structurally. For example, given that the Target will play $(FF)$, the Challenger prefers $(Q)$ to $(FQ)$ when $c_a > 1 + w_{t+1}$. Given that the Challenger will play $(FF)$, the Target prefers $(Q)$ to $(FQ)$ when $c_b > 1 - w_{t+1}$.

When the war has favored the Government to date, the Government is never willing to quit so long as the Target is willing to fight. The Target will quit and the game will end with the Target conceding if the per-period cost of fighting for the Challenger are below some threshold $c_a^\ast\ast$. If the costs of fighting for the Target are below some threshold $c_b\ast\ast$, the war will continue. If the costs of fighting for each player are above each of these thresholds, both players adopt mixed strategies. The reverse is true if the war has favored the Target to date. In the simplified case where we only consider the payoffs for this round, these critical values follow readily from the payoffs. They are as follows:

- The Challenger will fight given that the Target will fight if: $c_a < c_a^\ast$, where $c_a^\ast = 1 + w_{t+1}$.
- The Challenger will fight given that the Target will quit if: $c_a < c_a^{\ast\ast}$ where $c_a^{\ast\ast} = 1 - w_t$.
- The Target will fight given that the Challenger will fight if: $c_b < c_b^\ast$ where $c_b^\ast = 1 - w_{t+1}$.
- The Target will fight given that the Challenger will quit if: $c_b < c_b^{\ast\ast}$ where $c_b^{\ast\ast} = 1 + w_t$.

QED.

Lemma 2. Given that the Target plays $(FF)$, so long as there are conditions such that the Challenger prefers to play $(FF/FQ)$, there are also conditions such that the Challenger prefers $(FF/FQ)$ to $(FQ)$.

Proof: The Challenger prefers $(FF/FQ)$ to $(FF)$ given that the Target plays $(FF)$ so long as $c_a > k_b$, where $k_b = 1 + (w'_{t+2} - pw_{t+2})/(1 - p)$, where $w_{t+2}$ is the expected value
of fighting a battle in $t + 1$ given that the Challenger won the battle in period $t$, while $w'_{t+2}$ is the expected value of fighting a battle in $t + 1$ without this restriction. Thus $w'_{t+2}$ must necessarily be less than $w_{t+2}$. The above follows readily from solving the inequality $EU(FF/FQ) > EU(FF)$ for $c_a$. Solving the inequality $EU(FF/FQ) > E(FQ)$, given that the Target plays $(FF)$ and $c_a > k_b$ for $c_a$, we find that the Challenger prefers $(FF/FQ)$ to $(FQ)$ when $c_a < s_b$, where $s_b = 1 + w_{t+2}$. If we compare $k_b$ to $s_b$, we find that $k_b$ is less than $s_b$ whenever $w'_{t+2} < w_{t+2}$, which as stated above, must be true. Therefore, the threshold value of $c_a$ below which the Challenger prefers $(FF/FQ)$ to $(FQ)$ is always above the threshold value of $c_a$ above which the Challenger prefers $(FF/FQ)$ to $(FF)$. It follows that if $k_b$ is less than 1, there exists a range of values of $c_a$ such that $(FF/FQ)$ is the optimal strategy.

QED.

**Proposition 1.** Given that the Target plays $(FF)$, a Government with an Opposition that advocates continued fighting (“FF”) is more likely to prefer $(FF)$ to $(FF/FQ)$ than a unitary-actor Challenger in the Basic Game.

Proof: The lower bound on the region that supports $(FF/FQ)$ as an optimal strategy, $k_s$ is higher when the Opposition plays (“FF”) than the corresponding threshold in the Basic Game, $k_b$. The upper bound, $s_s$, is likewise shifted upwards, but by such an amount that the overall size of the region does not change, it merely shifts upwards. The intuition behind the result is that the payoff for quitting in period $t + 1$ given that the opposition advocates continued fighting is worse than the payoff for quitting in the Basic War Fighting Game, so the minimum per-period cost of fighting the Government is willing to tolerate and still play $(FF)$ is higher. Formally, $k_s = \beta + (w'_{t+2} - pw_{t+2})/(1 - p)$, which is clearly greater than $k_b = 1 + (w_{t+2}' - pw_{t+2})/(1 - p)$, since $\beta$ is bounded between 1 and 2. The upper bound, $s_s$ is: $\beta + w_{t+2}$. Therefore, both $k_b$ and $s_b$ have increased by $(\beta - 1)$, leaving the distance between them unchanged. QED.
Proposition 2a. Given that the Target plays (FF), a Government with an 
Opposition that did not previously oppose but now advocates quitting (“Q”) 
is less likely to prefer (FF) to (FF/FQ) than a Government with a 
supportive Opposition.

Proof: The lower bound on the region that supports the play of (FF/FQ) as an optimal 
strategy, $k_q$ is lower when the Opposition advocates quitting (“Q”) than the corresponding 
threshold when the Opposition supports (“FF”), $k_s$. As above, the upper bound, $s_q$ shifts 
by the same amount, leaving the size of the region unchanged. The intuition is that if 
the Government chooses to keep fighting despite the Opposition’s call to cease fighting, the 
stakes of the conflict are raised. The region we are discussing, where the Government is only 
willing to keep fighting if it wins, can only occur if the war has not favored the government to 
date ($w_t < 0$). Therefore, having the outcome of the conflict amplified strictly decreases the 
Government’s payoff, and so the Government is less willing to keep fighting. Formally, the 
cutpoints $k_q$ and $s_q$ are equal to $\beta (1 + (w_{t+2} - pw_{t+2})/(1 - p))$ and $\beta (1 + w_{t+2})$, respectively. 
Since the fraction in the first expression must be negative for $k_q$ to be less than 1, multiplying 
the entire expression by $\beta$ rather than just the 1 at the beginning of the expression (as was 
the case for $k_s$) has the net effect of decreasing $k_q$ relative to $k_s$. It is worth noting that 
the cutpoints $k_q$ and $s_q$ are both higher than the corresponding thresholds in the Basic War 
Fighting Game, $k_b$ and $s_b$. QED.

Proposition 2b. Given that the Target plays (FF), a Government with an 
Opposition that did not previously oppose but now advocates quitting (“Q”), 
is more likely to prefer (Q) to (FQ) than the unitary actor Challenger in the 
Basic Game or a Government with a supportive Opposition (“FF”).

Proof: The critical value of $c_a$ above which the Government (Challenger) prefers (Q) to 
(FQ) is lower when the Opposition calls on the Government to quit than when the Opposition
advocates continued fighting or there is no Opposition (as in the basic game). The intuition is simply that quitting in the period in which the Opposition first advocates quitting avoids politicizing the conflict, and so allows the Government to avoid further decreasing its payoff. Formally, the three cutpoints, \( m_q, m_s, \) and \( m_b \), in order, are as follows: \( w_{t+1} + 1 + \delta - \beta \delta \), \( w_{t+1} + \beta \), and \( w_{t+1} + 1 \). As should be apparent, regardless of whether \( w_{t+1} \) is positive or negative, the first quantity is the smallest, the third quantity is in the middle, and the second quantity is the largest. QED.

**Proposition 3a.** Given that the Target plays (FF), a Government with an

Opposition that has already opposed continuing the war is as likely to prefer

\((FF/FQ)\) to \((FF)\) as a Government with an Opposition that advocates quitting this period.

Proof: The critical values of \( c_a \) defining the region in which \((FF/FQ)\) is an optimal policy, \( k_o \) and \( s_o \), are identical to those when the Opposition initially advocates quitting, \( k_q \) and \( s_q \), respectively. The intuition is that if the Government is willing to fight one more battle, the war will be politicized at that moment. It won’t make a difference if the Government fights two more periods rather than one more. QED.

**Proposition 3b.** Given that the Target plays (FF), a Government with an

Opposition that has already opposed continuing the war is less likely to prefer \((Q)\) to \((FQ)\) than any other Government (Challenger).

Proof: The critical value of \( c_a \) above which the Government prefers \((Q)\) to \((FQ)\) is greater for a Government that is fighting a war that is already politicized. The intuition is simply that admitting failure when the Opposition is poised to capitalize upon your failures is more costly than quitting under any other condition. Formally, the relevant threshold, \( m_o \), is \( \beta(1 + w_{t+1}) \), which regardless of whether \( w_{t+1} \) is positive or negative, is strictly greater than the three previous values discussed in Proposition 2b \((m_q, m_s, \) and \( m_b \)). QED
Proposition 4. Holding the strategy of the Opposition constant, the Government is more likely to quit as \( t \) increases.

Proof: The critical value of \( c_a \) above which the Government (Challenger) prefers \((Q)\) to \((FQ)\), \(m\), regardless of the Opposition’s strategy (if present), is a function of \(w_{t+1}\). As \( t \to \infty \), \(w_{t+1}\) converges to \(w_t\). That is, the more battles have taken place, the less difference one more battle makes. As \(w_{t+1}\) converges to \(w_t\), the Government (Challenger) only prefers to fight this period if they preferred to fight last period, since the critical value of \(c_a\) below which the Government (Challenger) prefers to fight unconditionally, \(k\), which is a function of \(w_{t+2}\) also converges to \(w_{t+1}\) and \(w_t\). In the limit, the Government (Challenger) chooses to fight another battle today only if they are willing to continue fighting indefinitely, as the value of continuing to fight tomorrow will be no different than today. All the cutpoints, \(k\), \(m\) and \(s\) converge, and if the cost of fighting, \(c_a\) is below this unified cutpoint, the Government (Challenger) is willing to fight forever, but if it is above this unified cutpoint, the Government (Challenger) quits immediately. QED.

Proposition 5. The impact of the Opposition’s strategy on the Government’s strategy is strictly increasing in \( t \).

Proof: This proposition follows readily from the previous propositions. Proposition 4 implies that the thresholds separating the Government’s preferred strategies converge as \( t \to \infty \). As \( t \) increases, the necessary increase in \(c_a\) to change the Government’s optimal strategy is decreasing. The cutpoints against which the value of \(c_a\) must be compared to establish the optimal strategy are shifted by \(\beta\) when the Opposition switches from supporting to opposing. As the cutpoints move closer to one another, the same size increase (decrease) stemming from \(\beta\) becomes more meaningful. QED.

Proposition 6. Given that the Target plays \((FF)\), the Opposition never advocates
quitting ("Q") when the course of the war is favoring the Government 

to Date.

Proof: If the Opposition advocates fighting, the Government’s payoffs are identical to 
the Basic War Fighting Game. From Proposition 1, we know that the Government will keep 
fighting, given that the Target will keep fighting, so long as $c_a < c^*_a$, where $c^*_a = 1 + w_{t+1}$. Notice that if $w_t > 0$, then $w_{t+1} \geq 0$. Notice that $c^*_a$ cannot be less than 1 so long as the course 
of fighting favors the Government, thus the conditions under which the Government keeps 
fighting must always hold. If the Government is going to keep fighting, the Opposition’s 
payoff for advocating fighting is identical to that of the government. If the Opposition 
advocates quitting, the Government’s utility for fighting increases, so it cannot be less likely 
to keep fighting. If the Opposition advocates quitting and the Government keeps fighting, 
the Opposition has decreased its utility by $\rho$. It would always be better of advocating fighting 
if the war is favoring the Government and the Government is going to keep fighting. QED.

**Proposition 7.** Given that the Target plays (FF), the Opposition never advocates 
quitting ("Q") if the Government is expected to continue fighting regardless 
of the Opposition’s decision.

Proof: This proof consists of two parts. Consider first the case where the Government is 
expected to continue fighting regardless of the Opposition’s decision and the course of the 
war favors the Government. Then the above proof for Proposition 6 holds.

If the war has not favored the Government but nonetheless the Government is expected to 
keep fighting (perhaps because the costs of fighting are very low), the Opposition is better off 
waiting until the first period in which withdrawing their support will make the Government 
prefer to quit when otherwise it would have preferred to fight. Given that we have established 
with Proposition 5 that the impact of the Opposition’s decision is strictly increasing in time,
this window of opportunity will eventually arrive, if the Government continues fighting long enough.

First, let us establish the conditions under which the Government would be willing to keep fighting even though the Opposition advocates quitting ("Q"). This holds so long as $w_t - c_a > -1$. Looking two periods ahead, the Government is willing to fight today and fight tomorrow rather than quit today, given that the Opposition advocates quitting today, if $c_a < 1 + (\beta w_{t+1} + \beta w_{t+2})/2$. The Opposition prefers to have the Government fight today and tomorrow versus quitting today if $c_a < 1 + (\rho w_{t+1} + \rho w_{t+2})/2$. As $\rho$ is smaller than $\beta$, if $c_a$ is less than this first quantity, it must necessarily be less than the second. However, the Government’s decision to fight or quit is only dependent upon the Opposition’s strategy when $1 + (w_{t+1} + w_{t+2})/2 > c_a > 1 + (\beta w_{t+1} + \beta w_{t+2})/2$. By definition, this condition and the condition where the Government prefers to fight today and tomorrow despite being opposed today cannot both be true. The Opposition always prefers to advocate quitting if doing so will result in the Government quitting. QED.

Proposition 8a. If the value of opposing a war ($\pi$) is sufficiently high, there are always conditions where the Opposition advocates quitting ("Q") regardless of the Government’s and Target’s strategies.

Proof: Lemma 1 established that the Target never quits if the Challenger (Government) is willing to fight when the course of the war has favored the Target. Proposition 6 established that the Opposition only ever advocates quitting ("Q") when the war has favored the Target. Therefore, the challenge is not to demonstrate that there are always conditions such that the Opposition would advocate quitting (provided $\pi$ is sufficiently high) when the course of the war has not favored the Government and the Target is therefore expected to keep fighting. The more challenging task is to demonstrate that there are always values of $\pi$ that allow the Opposition to advocate quitting even when the war is favoring the Government.
Consider the least likely case, where the Government will fight and the Target will quit, regardless of the Opposition’s strategy. Thus, the Government is about to secure a decisive victory, and the Opposition is going to allow the Government to use it as a wedge issue by advocating quitting. If the Opposition could be willing to oppose here, it must be capable of opposing under any other situation.

To see this, compare the Opposition’s payoffs for playing “fight” versus playing “quit” for each of the four possible outcomes of the game. It should be clear that if the Government is going to quit and the Target is going to fight, the Opposition would be better off playing “fight” when there is no intrinsic value to opposing.

However, once ideological preferences are taken into account, we see that the Opposition plays “quit” given that Government will quit and Target will fight if $-1 + \pi > -\rho$. This is equivalent to saying that if $\pi > 1 - \rho$, the Opposition will advocate “quitting” even under the least likely case. If the Government were expected to keep fighting, the condition holds if $\pi > w_t(1 - \rho)$. Since both $\pi$ and $\rho$ have maximum values of 1, there must exist some combination of values of the two parameters where the first condition is satisfied. Since $w_t$ is always less than or equal to 1, if the first condition can be satisfied, so can the second. QED.

**Proposition 8b.** If the value of opposing a war ($\pi$) is sufficiently low, there are always conditions where the Opposition advocates fighting ("F") regardless of the Government’s and Target’s strategies.

Proof: Here, I seek to demonstrate that when the Opposition’s preferences are hawkish and expects to be punished by its constituents for advocating quitting any conflict, regardless of how poorly it is faring, there are always conditions in which the Opposition would advocate fighting. Similar to the proof above, I proceed by exploring the condition in which it makes the least sense for the Opposition to advocate fighting. This is the case where the course of
the war to date has favored the Target and the Government would quit fighting if and only if the Opposition pressured the Government to quit.

The Opposition plays “fight” given that Government will fight (but would not otherwise) and Target will fight regardless of the Opposition or Government’s strategy if \( \pi > c_a - 1 + w_{t+1} \). The minimum value of the right hand side is -2 and the maximum is 1. Since \( \pi \) is bounded between -1 and 1, there are no combinations of values of the parameters on the right hand side that can eliminate the possibility of the expression being satisfied. QED.

**Proposition 9.** If the value of opposing a war (\( \pi \)) is sufficiently low, there are conditions where the Opposition will advocating fighting (“F”) even though this will result in worse outcomes for the Government.

Proof: The Government is made worse off by the Opposition advocating fighting whenever it would prefer to quit if and only if the Opposition advocated quitting. Looking two periods forward, this holds true when \( 1 + (w_{t+1} + w_{t+2})/2 > c_a > 1 + (\beta w_{t+1} + \beta w_{t+2})/2 \). The Opposition prefers to advocate fighting, and thus ensure that the Government keeps fighting, if \( c_a < 1 + (w_{t+1} + w_{t+2})/2 - \pi \). For a hawkish Opposition, \( \pi \) is negative, and therefore this quantity is necessarily greater than the value of the cost of fighting below which \( c_a \) must fall for the Government’s decision to be dependent upon the Opposition’s strategy. Therefore, so long as the Government’s decision depends upon the Opposition’s strategy, a hawkish Opposition must always prefer to advocate fighting and effectively force the war to continue. QED.
Appendix B

Proofs for Chapter 5

The solution to the bargaining model is straightforward, and follows readily from the payoffs.

Let $D$ represent the presence of diversionary incentives. This condition holds when $r_2$ exceeds $r_1$. This condition holds when $\epsilon = 1$ and $0.45 < \alpha < 0.55$. There are two important cases to analyze then, $\sim D$ and $D$.

Strategies under $\sim D$:

Leader plays: (Demand, $d = d^*$) if $p > p^*$ (Demand, $d = d^{**}$) if $p < p^*$

Target plays: (Resist) if $d > d^*$ and $p > p^*$ (Resist) if $d > d^{**}$ and $p < p^*$ (Concede) otherwise.

where $p^*$, $d^*$, and $d^{**}$ are defined below.

Strategies under $D$:

Leader plays: (Demand, $d = d^*$) if $p^{**} < p < p^{***}$ (Demand, $d > d^*$, Escalate) if $p > p^{***}$ (Demand, $d = d^{**}$) if $p < p^{**}$ and $c_{d1} > c_{d1}^*$ (Demand, $d > d^{**}$, Back Down) if $p < p^{**}$ and $c_{d1} < c_{d1}^*$

Target plays: (Resist) if $d > d^*$ and (Resist) if $d > d^{**}$ and (Concede) otherwise.

where $p^{**}$, $p^{***}$ and $c_{d1}^*$ are defined below.

Proof: Most of the elements of the SPE equilibria are straightforward. The backwards
induction solution the model is as follows:

Leader will escalate given demand, resist, $\sim D$ if \( p > p^* \) where \( p^* = (c_{d2} - c_{d1} + 0.5)/(1.5) \).
Leader will escalate given demand, resist, \( D \) if \( p > p^{**} \) where \( p^{**} = (c_{d2} - c_{d1} + 1.5)/(2.5) \).
Target will resist given demand, \( p > p^* \), \( \sim D \), or \( p > p^{**} \), \( D \) if \( d > d^* \), where \( d^* = p + c_{t2} \).
Target will resist given demand, \( p < p^* \), \( \sim D \), or \( p < p^{**} \), \( D \) if \( d > d^{**} \), where \( d^{**} = c_{t1} \).

Leader prefers to set \( d < d^* \) given \( p > p^* \), \( \sim D \) if \( c_{t2} > 0.5(p - 1) - c_{d2} \). Note, this condition must be true. The right side of the inequality must be less than 0, while left side is positive.

Leader prefers to set \( d < d^{**} \) given \( p < p^{**} \), \( \sim D \) if \( c_{d1} > c_{d1}^* \), which must be true.
Leader prefers to set \( d < d^{**} \) given \( p < p^* \), \( \sim D \) if \( c_{d1} > c_{d1}^* \), where \( c_{d1}^* = 0.5 - c_{t1} \).
Leader demands \( d^* \) given \( p > p^* \), \( \sim D \) if \( r_1 + d > r_1 \), which must be true.
Leader demands \( d^* \) given \( p^{**} < p < p^{***} \), \( D \) if \( r_1 + d > r_1 \), which must be true.
Leader demands \( d > d^* \) given \( p > p^{***} \), \( D \) if \( p > (1 + c_{d2})/(2.5) \), which must be true.
This value of \( p \) must be less than \( p^{***} \), and since this condition only applies when \( p > p^{***} \), \( p \) must be greater than this value.

Leader demands \( d^{**} \) given \( p < p^* \), \( \sim D \) if \( r_1 + d > r_1 \), which must be true.
Leader demands \( d^{**} \) given \( p < p^{**} \), \( c_{d1} > c_{d1}^* \), \( D \) if \( r_1 + d > r_1 \), which must be true.
Leader demands \( d > d^{**} \) given \( p < p^{**} \), \( c_{d1} < c_{d1}^* \), \( D \) if \( c_{d1} < 0.5 \), which must be true. The above condition is only relevant when \( c_{d1} \) is already less than \( c_{d1}^* \), which is equal to \( 0.5 - c_{t1} \). If \( c_{d1} \) is less than this quantity, it must also be less than 0.5.
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