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Cumulative Dynamics and Strategic Assessment:
U.S. Military Decision Making in Iraq, Vietnam, and the American Indian Wars

A dissertation presented
by
Jeffrey Allan Friedman
to
The Committee on Higher Degrees in Public Policy

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Cumulative Dynamics and Strategic Assessment:
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Abstract

This dissertation examines why military decision makers struggle to evaluate their policies and why they often stick to unsuccessful strategies for so long. The core argument is that strategic assessment involves genuine analytic challenges which contemporary scholarship typically does not take into account. Prominent theoretical frameworks predict that the longer decision makers go without achieving their objectives, the more pessimistic they should become about their ability to do so, and the more likely they should be to change course. This dissertation challenges those ideas and explains why we should often expect the very opposite.

The theoretical crux of this argument is that standard models of learning and adaptation (along with many people's basic intuitions) revolve around the assumption that decision makers are observing repeated processes, similar to the dynamics of slot machines and roulette wheels – but in war and other contexts, decision makers often confront *cumulative* processes that have very different dynamics, along with a different logic for how rational actors should form and revise their expectations. Empirically, this dissertation examines U.S. decision making in Iraq, Vietnam, and the American Indian Wars. These cases demonstrate how cumulative dynamics affect strategic assessment and how understanding these dynamics can shed light on prominent theoretical frameworks, ongoing policy debates, and salient historical experience.

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Chapter One: Introduction

RATIONAL STRATEGIC ASSESSMENT

Military decision makers often struggle to assess strategic progress. This is one of the central storylines of the Vietnam War, in which U.S. officials consistently underestimated their opponents' resolve and maintained the war was winnable even as the Saigon regime's political legitimacy decayed. Similarly, scholars have often asked why Germany took so much time to moderate its demands in World War I, why Japan did not admit defeat earlier in World War II, why so many colonial powers failed to understand that they could not suppress nationalist resistance, and why so many states seem to resist learning similar lessons today. The notion that leaders make mistakes is not necessarily surprising – much more concerning, perhaps, is how they often seem to find it so difficult to realize and correct those mistakes, and that they often stick to unsuccessful strategies for so long.

Of course, it is just as easy to point out cases where decision makers struggled to foresee strategic gains. The Korean War, for instance, ended with an armistice signed in July 1953 – but just two months earlier, the National Security Council declared in a planning document (endorsed by the Joint Chiefs of Staff and approved by President Eisenhower) that negotiations with the communists had “clearly demonstrated” that “under present conditions” the search for

an acceptable political settlement “is hopeless.”¹ In 2007, much of the United States’ senior military leadership and perhaps a majority of its citizens opposed reinforcing the troop presence Iraq on the grounds that the country could not be stabilized, yet by year’s end violence in that country would fall by ninety percent (at least, for the time being). And when the Berlin Wall came down in 1989, it was a surprise to policy makers, military officials, intelligence agencies, and the general public alike, even though the event signaled the culmination of a grand strategy that had dominated U.S. foreign policy for nearly half a century. On balance, one could therefore argue about whether leaders tend to be unduly optimistic or pessimistic when it comes to assessing strategic progress; either way, strategic assessment is clearly a challenge that confounds decision makers even (and perhaps especially) on questions of first magnitude.

In many instances, the result is that debates about military strategy become mired in ambivalence. When President Barack Obama began withdrawing troops from Afghanistan in 2011, for example, he was harshly criticized by those who believed he was removing critical forces on the cusp of success. But he was also challenged (and just as harshly) by critics making the exact opposite argument, saying that the war was a lost cause and so the troops should come home even sooner. Ten years into the longest war in U.S. history, officials, experts, and the general public were still not remotely close to consensus when it came to estimating how much longer it might take or how much more it might cost to defeat (or otherwise contain) the Taliban.

When these kinds of debates fail to converge, or when military decision makers seem unable to update their expectations, it is not only frustrating – it is also deeply puzzling to scholars of

¹ The document (NSC-147) can be found in *Foreign Relations of the United States, 1952-54*, Vol. XV, pp. 841-42. See Malkasian 2002: 176-177 for a discussion of NSC-147 and how it indicated misplaced pessimism about U.S. strategy in the Korean War.

armed conflict who believe that fighting provides information that reduces uncertainty and facilitates strategic assessment. While there are many reasons why wars can be protracted, it is much less clear why military decision makers should seemingly find it so hard to learn, adapt, and evaluate their policies.

Existing scholarship typically approaches this problem using mechanisms like organizational behavior, culture, domestic politics, and psychology. This dissertation, by contrast, explains why strategic assessment involves genuine analytic challenges that scholars often overlook. These challenges should inhibit strategic assessment on the part of even unitary, rational actors. And because this argument requires no assumptions about the characteristics of armies and their leaders, it may help to explain why military decision makers in many different contexts seem to find strategic assessment so difficult.

The theoretical crux of this argument is that most academic models of learning and adaptation (along with most people's basic intuitions about decision making more generally) revolve around analogies to repeated processes, with dynamics similar to slot machines or roulette wheels – but in war and other contexts, decision makers often confront *cumulative* processes that have very different dynamics, along with a very different logic for how rational actors should form and revise their expectations. This dissertation lays out that argument and its implications in five subsequent chapters.

Chapter 2 provides the basic theoretical groundwork. It draws the key distinction between repeated processes (which characterize most scholarship in the prominent international relations literature on the “bargaining model of war” as well as theories of decision making in other fields), and cumulative processes, which better reflect the dynamics that military decision makers

face. In forming concepts for analyzing cumulative processes, Chapter 2 relies on both inductive and deductive reasoning. Inductively, the chapter examines how U.S. decision makers grappled with cumulative dynamics during the Vietnam War. Deductively, the chapter identifies theoretical principles that generate these dynamics, showing why we should expect them to arise in a wide range of circumstances. From both angles, Chapter 2 develops the conceptual foundations for the remainder of the analysis, while explaining why this framework lies outside the boundaries of existing literature.

Chapter 3 then describes the dissertation's primary theoretical argument, explaining why understanding the cumulative dynamics of armed conflict has fundamental implications for thinking about the way that decision makers should be able to learn and adapt. It is generally believed that the longer rational decision makers go without achieving their objectives, the more pessimistic they should become about their ability to do so and the more likely they should be to change course. Chapter 3 challenges those ideas and explains why we should often expect the very opposite. The logic in this chapter is expressed formally, in order to explain precisely why intuitive premises about cumulative dynamics in war generate surprising predictions, including the idea that rational decision makers might actually become more optimistic about their ability to achieve their goals, even as they continually fail to do so. In showing how and under what conditions this is the case, Chapter 3 demonstrates that we do not need to invoke nonrational factors in order to explain why military leaders often find it so difficult to realize and correct their strategic mistakes. We can explain this behavior simply by taking a careful look at the cumulative dynamics of armed conflict and understanding how they differ from other phenomena.

Chapter 4 presents an empirical application of this argument, showing how it offers a new lens for evaluating past experience by examining U.S. military strategy during the American Indian Wars. This set of cases was chosen for specific methodological reasons. In total, U.S. and British forces fought more than one hundred different Native American tribes; these tribes fought in reasonably similar ways, with reasonably similar technology, against similar opponents, over similar stakes. This is not to say that the American Indian Wars were literally identical, of course, but the units of analysis here are much more comparable than in almost any other cross-section of insurgencies, or conventional wars, or other kinds of political violence. This provides an opportunity to assess the way that U.S. commanders such as George Custer and Winfield Scott formed and revised their expectations in light of a relatively large body of objective, empirical evidence. Rarely is it possible to evaluate beliefs about military strategy in this manner, and *Chapter 4* exploits this opportunity through newly-gathered, event-level data, which along with this dissertation's overarching theoretical framework help to explain why U.S. commanders in the American Indian Wars often acted in otherwise puzzling ways. To the extent that this analysis provides insight into the behavior of commanders like Custer (whose actions have been scrutinized as much those of almost any other decision maker in U.S. history), it indicates the potential promise of the new framework for re-evaluating established scholarship and conventional wisdom.

Chapter 5 then turns to U.S. policy during the occupation of Iraq. The goal of this chapter is partly to show how understanding cumulative dynamics helps to reframe contemporary policy debates, just as *Chapter 4* shed light on the analytic challenges that U.S. commanders faced in the American Indian Wars. The main purpose of examining U.S. policy in Iraq, however, is to motivate a discussion of how decision makers can deal with these analytic challenges. The

central argument in this chapter revolves around the importance of managing what scholars call the “reference class problem,” which requires decision makers to continually update their views about the scenario they are facing, and to understand why different classes of problems require different policy responses. If this sounds truistic, Chapter 5 explains how decision makers, military doctrine, and scholarship on armed conflict place limited emphasis on the issue, often focusing on common patterns that hold across cases, rather than providing information on how to diagnose the ways in which circumstances differ and to assign different policy prescriptions accordingly.

Chapter 6 concludes by expanding the scope of the analysis: it argues that understanding the cumulative dynamics of armed conflict has significant implications for empirical research design and it discusses how cumulative dynamics affect policy assessment in other fields. Armed conflict offers many salient examples of decision makers struggling with cumulative dynamics, trying to determine how long it might take and how much it might cost to achieve their goals. But similar analytic challenges recur in many other areas. Will economic sanctions cause Iran to disable its nuclear weapons program before developing a functional bomb? How much economic assistance will it take to break developing countries out of poverty traps? When is it worth continuing to spend money on expensive scientific projects that have not yet succeeded but could produce significant breakthroughs down the road? When a political campaign’s negative advertisements do not seem to be increasing vote share, does that mean the advertisements are ineffective or that the campaign should run more of them?

These questions emphasize that while military decision making is the primary subject matter in this dissertation, the theoretical framework developed here is ultimately intended to be a contribution to the study of international relations and policy analysis more generally. When

people debate whether policies are not succeeding because of the concept or the execution – whether decision makers are doing the wrong thing or just not doing the right thing *enough* – they are generally dealing with the kinds of cumulative dynamics that make it difficult to decide whether it would be better to switch strategies or stay the course. The following chapters revolve around the difficulties that assessing these dynamics entails.

Section 1.1. Strategic assessment and military decision making

The subject of this dissertation is strategic assessment, defined here as the way that decision makers form and revise their expectations about how long it might take and how much it might cost to achieve a particular goal. Strategic assessment thus entails estimating the potential means required to obtain desired ends. Military decision makers must grapple with this challenge both at the start of an armed conflict and also as that conflict unfolds.

Strategic assessment is just one component of decision making, and performing strategic assessment well does not guarantee that decisions will be sound. For instance, the following chapters generally do not discuss the kinds of value judgments that are crucial for determining whether the prospective costs of some military strategy are “worth it” in order to achieve a particular objective. The following chapters do not discuss the ways that policy decisions can be interdependent (such as how adopting a certain policy in one case can foreclose or otherwise affect decision makers’ options elsewhere), and they generally do not discuss the difficulties of evaluating options in relation to plausible alternatives. Studying how long it might take or how

much it might cost in order to achieve a particular goal is thus substantially more limited in scope than evaluating the quality of decision making writ large.²

With that said, it is *not* possible to make a good decision in the absence of sound strategic assessment. Decision makers who design military strategies or other policies based on flawed assumptions may achieve favorable outcomes, but that does not justify poor reasoning or suggest that others who follow their lead would obtain similar results.³ Douglas MacArthur's landing of U.S. forces at Inchon during the Korean War is perhaps the archetypal example of an excessively risky decision that ultimately succeeded, and few people would argue that this implies the need for military decision makers to discard cautionary advice. MacArthur believed strongly in his personal luck, which worked out well for landing at Inchon but then fared poorly for crossing the Yalu. One of the cardinal principles of decision analysis is the importance of separating ex post knowledge about outcomes from information available to decision makers ex ante.⁴ The way that decision makers assess that information is central to evaluating their performance, and it is the focus of this study.

² See Robert and Zeckhauser 2011 and Keeney 1982 for overviews of basic criteria for analyzing policies and decisions in general.

³ As Richard Betts wrote in a well-known essay on evaluating military strategy, "judgment is often contaminated by hindsight, as good fortune is mistaken for strategic foresight" (2000: 9-10).

⁴ Ronald Howard (1988: 682) argues that this is perhaps *the* fundamental takeaway from decision theory: "I tell my students that if they learn nothing else about decision analysis from their studies, this distinction will have been worth the price of admission. A good outcome is a future state of the world that we prize relative to other possibilities. A good decision is an action we take that is logically consistent with the alternatives we perceive, the information we have, and the preferences we feel. In an uncertain world, good decisions can lead to bad outcomes, and vice versa. If you listen carefully to ordinary speech, you will see that this distinction is usually not observed. If a bad outcome follows an action, people say that they made a bad decision. Making the distinction allows us to separate action from consequence and hence improve the quality of action."

Strategic assessment is also one of the more analytically tractable components of military decision making (though this is not to say that the task is easy to execute). Value judgments are difficult to analyze objectively, especially concerning military strategy and national security, where moral issues tend to be prominent and hotly debated.⁵ In general, one of the central challenges of studying foreign policy is to identify the outcome variable that decision makers should be maximizing. The “national interest” is a notoriously fuzzy concept – it is far harder to define than factors like “profit” or “net present value” that drive the microeconomic analysis of firms – and thus debates about the soundness of foreign policy decisions often fail to get beyond disagreements about how to conceive objectives properly.⁶ A question like “how long might it take and how much might it cost to achieve a particular goal” is more readily accessible. Time, casualties, and expenditures can all be defined conceptually and measured empirically, which facilitates the enterprise of building and testing theory.

Moreover, this dissertation argues that current theoretical frameworks for understanding strategic assessment have important limitations, especially when it comes to dealing with the

⁵ See Schelling 1984 on how apparently intractable disagreements about values can in fact often be reduced to disputes about modeling choices; in general, one of the foundations of utility theory is that, in principle, almost any issue can be incorporated into a rational framework using analytic tools like the comparison of lotteries (Von Neumann and Morgenstern 1944, Luce and Raiffa 1957, Keeney and Raiffa 1976, Keeney 1992). In practice, however, while objective analysis can facilitate the evaluation of value-laden decisions, it is rarely dispositive. See Smelser 1992, Elster 1979, 1993, Levi 1986, and Manski 2011 among others, for discussion of how preferences, discount factors, risk attitudes, regret, and issues of fairness largely lie outside the scope of rational choice theory, implying that in many cases there may be no firm answer to what the rational or optimal decision might be. By excluding these factors from the analysis here, it is possible to analyze a component of decision making in a relatively objective fashion.

⁶ The difficulty of defining concepts like the “national interest,” or “national security” is a central theme in the 2009 history of international security studies by Barry Buzan and Lene Hansen. For more on these debates, see Trubowitz 1996 who focuses specifically on debates over the national interest in U.S. foreign policy, along with Wolfers 1952, Weldes 1996, and Baldwin 1997 for theoretical perspectives.

cumulative dynamics of armed conflict. Though few people would agree that war is a repeated process in which the odds of success are the same in one stage as they are in the next, this is the explicit assumption on which many prominent works are currently based. This assumption tends not to receive much attention or debate, but as Chapters 2 and 3 demonstrate, it plays an important role in defining the way that scholars expect decision makers to assess their strategic prospects, which in turn influences the way that scholars expect these decision makers to behave. It is not possible to form a theory of rational decision making without first building a theory of rational strategic assessment, and that is the goal here.

Section 1.2. Components of strategic assessment

Strategic assessment has two key components: forming prior assumptions about how long it might take and how much it might cost in order to achieve a particular goal, and then revising those assumptions in light of subsequent events. While stating these tasks is simple, carrying them out – or even articulating how a rational decision maker *should* carry them out in principle – is the subject of widespread debate in decision theory.⁷ The following chapters largely revolve around the subject of forming and revising prior expectations in the context of military decision making, and to preface that analysis, it is worth laying out some key concepts.

First, what are prior assumptions? In the context of strategic assessment, prior assumptions represent decision makers' assessments of how long it might take and how much it might cost in order to achieve their desired goals. These assumptions revolve around *subjective probabilities*,

⁷ For reviews of Bayesian decision theory, see Raiffa 1968, Lindley 1971, Keeney and Raiffa 1976, Pratt, Raiffa, and Schlaifer 1995, and Winkler 2003. Hunter 1984 examines several applications in the context of military decision making in particular.

which capture a decision maker's degree of belief that a given statement is true. Before the 2003 invasion of Iraq, for instance, Secretary of Defense Donald Rumsfeld wrote what has become known as his "Parade of Horribles" memo to stimulate discussion about the "potential problems to be considered and addressed" in relation to the coming war. One of the prospects he raised in this memo was that "Rather than having the post-Saddam effort require 2 to 4 years, it could take 8 to 10 years, thereby absorbing U.S. leadership, military, and financial resources."⁸ This is a rough way of stating a prior assumption about how long it might take and how much it might cost in order to achieve U.S. objectives in Iraq. The memo indicates Rumsfeld's belief that the most likely scenario was that the post-invasion occupation would not last long (two to four years) but that there was a chance of the mission dragging on much longer than that (eight to ten years). It is also well-known that several Bush administration decision makers held out hope that U.S. forces would be "greeted as liberators" and so there would be little need for a protracted occupation at all.⁹ Thus while there is no indication that these decision makers ever attempted to stipulate numerically the probabilities of the occupation lasting for any particular period of time, it is possible to get a rough sense of their degrees of belief in different possible outcomes.¹⁰

⁸ Donald Rumsfeld, "Iraq: An Illustrative List of Potential Problems to be Considered and Addressed," memorandum October 15, 2002.

⁹ See Gordon and Trainor 2006 and Wright and Reese 2008 for discussions of U.S. planning for the occupation of Iraq. Chapter 5 will return to this subject in more detail.

¹⁰ The United States intelligence community in fact self-consciously avoids using numbers to assess probabilities on the grounds that doing so would lend an undue aura of scientific precision to their estimates. See Gourley 1997 and Johnston 2005 along with Kent 1964 and Friedman and Zeckhauser 2013 for critiques. But the point remains that, whether formally or not, intelligence analysis and strategic assessment revolve around analysts' and decision makers' degrees of belief in what kinds of scenarios are more likely than others. This is a matter of subjective probability, however analysts choose to articulate it.

Though subjective probabilities about complex political events can rarely be calibrated with the precision of gambling odds or actuary tables, likelihood is an inherently quantitative concept, and subjective probabilities can always be elicited numerically.¹¹ You can try this yourself by picking an ongoing conflict and considering how long it might take for the violence to end. As of this writing, for instance, Syria is in turmoil as opposition forces attempt to unseat President Bashar al-Assad. Roughly speaking, what do you believe are the chances that Assad remains in power for another month? What are the chances that he remains in power for another six months? A year? Two years? Permanently? Now ask yourself how deadly you think this conflict will ultimately be: roughly speaking, what do you believe are the chances that the war ends up claiming fifty thousand lives, or seventy-five thousand lives, or a hundred thousand lives, and so on? By assessing these chances you are articulating subjective probabilities that capture your

¹¹ The standard thought experiment that decision theorists use to demonstrate this point involves the comparison of lotteries. Say that you are asked to predict the likelihood of some event taking place, such as whether the leader of a particular foreign country will be ousted within a year. If this takes place, you will receive a valuable prize, and if this does not take place you will receive nothing. You have the option of taking this lottery, or else accepting an alternative gamble in which an experimenter reaches into an urn containing one hundred marbles, of which thirty are black and the rest are red. If the experimenter draws a black marble then you win the prize, and otherwise, you receive nothing.

Which of these lotteries would you prefer? If you prefer to bet on the leader's ouster, then this means that you believe the probability of this outcome is at least 30 percent, or else you would have chosen to have the experimenter draw from the urn. If you chose to bet on the urn, then this shows you believe that the odds of the leader's ouster are no higher than thirty percent. In principle, we could repeat this experiment using different comparisons of lotteries until you were indifferent between the gambles, and that would indicate how likely you believe it is that the leader will be deposed within a year.

The purpose of this review is not to advocate that betting strategies actually be used in strategic assessment – a topic of substantial controversy in recent years (see Meirowitz and Tucker 2004) – but simply to show that even subjective assessments of probability can still be elicited in a coherent form. See Friedman and Zeckhauser 2013 on this point. For reviews of scholarship on measuring degrees of belief (both in principle and in practice), see Hampton, Moore, and Thomas 1973, Hogarth 1975, Lad 1996, Garthwaite, Kadane, and O'Hagan 2005, Winkler 2003: ch. 2. There is a substantial literature on how to apply these concepts to political-military analysis; some relevant sources include Dalkey 1969, Zlotnick 1972, Hunter 1984, and Schum 1987.

degrees of belief about how long it might take and how much it might cost for the Syrian opposition to depose the government. Together, these estimates form what decision theorists call *probability distributions*, a concept that will recur throughout subsequent chapters.

How should decision makers revise these expectations over time? In many ways, that is the question driving this dissertation, as the following chapters will argue that standard models of learning and adaptation (and perhaps most peoples' intuitions about strategic assessment more generally) make questionable assumptions about how decision makers can update their beliefs when observing cumulative processes. Briefly stated, however, the core concept in rational learning is the notion of *conditional probability*, in which decision makers adjust their prior assumptions in a manner that incorporates any information they obtain. As of this writing, for instance, the Syrian Civil War has been ongoing for about two years, during which it has claimed perhaps forty thousand lives. Whatever our beliefs about the potential scope and protractedness of this conflict might have been heading into the conflict, we now have substantial information that will help us to revise our prior assumptions. There is no longer any chance that the conflict will last any less time or claim any fewer lives than what has transpired already. *Conditional* on this and other information, we can form new assessments about how the Syrian Civil War might play out.

It is important to reiterate that even when decision makers do not *articulate* those beliefs explicitly, they still *possess* beliefs about how long it might take and how much it might cost to obtain their objectives, which they must form and revise as their policies unfold. The purpose of dealing with those beliefs explicitly is not to pretend that these inputs to decision making are any more objective or scientific that they really are, but rather to discuss how to form coherent

inferences based on available information, subjective foundations and all.¹² Moreover, when we examine these inferences systematically, it becomes apparent that some dynamics of learning and adaptation in war can be surprising, even counterintuitive. Chapters 2 and 3 in particular explain how the ways in which rational decision makers should adapt to cumulative processes lie outside the boundaries of much existing scholarship, and often fly in the face of what readers may expect.

Section 1.3. Why study rational action if there are no rational actors?

In particular, the following chapters build theory about the way that unitary, rational actors should form and revise their expectations. In the most general sense, rational action entails maximizing expected utility based on consistent preferences and logical beliefs.¹³ Preferences are rational when they satisfy basic requirements such as being complete, stable, consistent, transitive, and independent of irrelevant alternatives.¹⁴ Beliefs are rational when they follow the

¹² For especially well-articulated statements of this point, see Raiffa and Schlaifer 1960: vii, Keeney and Raiffa 1976: 12, Dawes 1979, Keeney 1986: 130, and Lad 1996: 8-9.

¹³ There are many ways in which this approach can be specified and critiqued (Elster 1986, Friedman 1996, Monroe 1991, Mele and Rawling 2004, Binmore 2009, Manski 2011), but as Becker writes, “everyone more or less agrees that rational behavior simply implies consistent maximization of a well-ordered function” (1976: 153). See MacDonald 2003 for a related argument that rationality essentially entails the purposive and consistent attempt to maximize well-being.

¹⁴ See Winkler 2003: 233 for a list of requirements for rational preferences. There is, however, controversy about how many of these requirements are in fact mandatory for characterizing “rationality” – see Sen 1993, Binmore 2009, and Clark 2013 for important arguments to this effect.

axioms of conditional probability.¹⁵ Actors are unitary when there is no conflict of interest or asymmetric information among decision makers and the groups that they represent.

Once again, these standards are easy to articulate but difficult, if not impossible, to follow. In the real world, decision makers, organizations, and states are all fallible and they operate under a wide range of political, organizational, and cognitive constraints. The purpose of assuming rationality here is thus not that this assumption is descriptively accurate, but rather that it is analytically useful in the following senses.¹⁶

First, theories of rational decision making are prescriptive. They offer guidance about what kinds of information should (or should not) facilitate decision making, along with insight into how that information should (or should not) be processed in order to evaluate decisions objectively.¹⁷ Here, the following chapters provide an especially clear break from existing scholarship. Prominent theoretical frameworks state that war itself provides information allowing combatants to form more accurate perceptions of each others' capabilities and resolve in a way that makes negotiated settlements more likely.¹⁸ This more or less automatic view of rational

¹⁵ Savage 1954.

¹⁶ Schelling 1960a: 108 provides an early discussion of how the assumption of rationality, though often not descriptive, is nonetheless “peculiarly conducive to the development of theory” as argued below. See Walt 1999 and Green and Shapiro 1994 for critiques of the descriptive utility of rational choice theory in security studies and political science, respectively (although their work places less a critique of the rationality assumption per se as opposed to the way that contemporary scholars often develop rational choice theories through the use of formal, game-theoretic models).

¹⁷ See Raiffa 1968: 128.

¹⁸ For instance, see Smith and Stam: “The act of waging war reveals information about the relative strengths of each side. As a war progresses, each side’s beliefs about the likely outcome of continuing the war converge. Once the warring parties’ beliefs have converged sufficiently, they can find a bargained solution to the conflict” (2004: 783). Or Filson and Werner: “War itself

decision makers improving their strategic assessments through trial-and-error clearly clashes with the persistent ambivalence, uncertainty, and lack of adaptation and compromise we observe in many cases. As Chapters 2 and 3 will show, the existing literature's prediction that war is an information-revealing and self-terminating largely revolves around the assumption that armed conflict is a repeated process. Once armed conflict is taken to be a cumulative process, however, it becomes apparent that fighting can actually cause combatants' perceptions of one another to diverge. One of the most important implications of this theoretical framework is thus that decision makers must often use other sources of information to improve their estimates of opponents' capabilities and resolve. In particular, the following chapters explain why decision makers' prior assumptions play a critical but underappreciated role in driving strategic assessment. Both scholars and decision makers could benefit from devoting more attention to these issues.

Second, theories of rational action are normative, helping to define the kinds of behavior we should to expect to see, all else being equal. We *generally expect* decision makers to become more pessimistic about policies that do not succeed. We *generally expect* debates about military strategy to converge towards consensus over time. We expect these things because this is how we think rational people should function, and when people deviate from this model, we typically assume that their behavior is nonrational.

In this respect, theories of rational action serve as important benchmarks for evaluating salient experience. Chapter 4, for example, examines George Custer, Winfield Scott, and other commanders during the American Indian Wars. These commanders are often thought to have

provides the information necessary for disputants to reach a settlement to end the war" (2002: 820). Chapter 2 describes this literature in more detail.

been hubristic, dogmatic, racist, politically motivated, or simply careless in directing their forces. Making, defending, or critiquing these judgments relies on having some idea of what rational behavior would have entailed in these circumstances – it is not possible to assess the rationality of any action without first defining what rationality means, and this dissertation explains why commonly-held beliefs about rational strategic assessment may be mistaken.¹⁹

Rationalist theories thus establish counterfactual scenarios that drive analyses of cause and effect.²⁰ For example, one of the most prominent components of the existing literature on strategic assessment examines the role of psychological factors. In general, these studies demonstrate that cognitive frameworks, confirmation bias, defensive avoidance, overoptimism, and emotional stress influence the way that decision makers process information and evaluate their options.²¹ The mere fact that these mechanisms matter is unsurprising, as all individuals suffer from cognitive distortions.²² The more important issue is the *extent* to which these factors

¹⁹ Charles Glaser expresses similar views: “[I am] skeptical that as a general rule states actually act rationally,” yet understanding their behavior nevertheless “requires a rational theory, even if states do not always act in line with its constraints.... [M]y theory provides a rational baseline against which actual state behavior can be evaluated. We cannot evaluate whether a state is acting rationally/optimally without such a theory.... Therefore, theories of suboptimal behavior, whether built on arguments about domestic politics or errors in individual decision making, rely at least implicitly on a rational theory” (2010: 2-3). Kydd 2008: 437-438 and Elster 1979: 153-156 discuss how rational actor theories are logically prior to their alternatives.

²⁰ On counterfactuals and historical reasoning in international relations, see Fearon 1996, Tetlock and Belkin 1996, Sylvan and Majeski 1998, and Tetlock and Lebow 2001.

²¹ Prominent examples from this literature include Leites 1953, Wohlstetter 1964, George 1969, White 1970, May 1973, Jervis 1976, Lebow 1981, Staw 1981, Janis 1982, Larson 1985, Vertzberger 1990, Khong 1992, Levy 1996, Mercer 1996, Reiter 1996, Heuer 1999, Wrangham 1999, Johnson 2004, McDermott 2004, Rosen 2005, Kahneman and Renshon 2007, Butler 2007, Renshon 2008, Gayer et al. 2009, Woods and Stout 2010, Duelfer 2011, Johnson and Fowler 2011, Johnson and Tierney 2011, Lopez et al. 2011, Mercer 2013.

²² See Kahneman 2011 for a recent review.

affect perceptions and decision making, and it is not possible to make this kind of judgment without first establishing an alternative conception of what strategic assessment would look like absent psychological, organizational, political, or other constraints.²³

Establishing that baseline can be difficult and tenuous. For instance, one of the most famous works on how psychology influences national security decision making is Roberta Wohlstetter's 1964 book *Pearl Harbor: Warning and Decision*, which argues that intelligence analysts and policy makers operated under a "mind-set" that essentially took it for granted that Japan would never attack the United States directly. Wohlstetter showed how, consistent with this mind-set, analysts and policy makers disregarded pieces of information that suggested an impending attack – indications were there, she argues, but they were buried within broader intelligence, and the mind-set of the U.S. national security community impeded its ability to separate the "signals" from the "noise." Wohlstetter's book helped to lay the foundations for a much broader literature on the psychology of intelligence analysis, and the U.S. intelligence community attempts to train its personnel to combat the kinds of obstacles that Wohlstetter and others have described.²⁴ But it

²³ Scholars often advance models of nonrational decision making without specifying what a rational baseline would entail, or how one would determine just how substantially decision makers deviated from that ideal. For example, Scott Sigmund Gartner's 1997 book, *Strategic Assessment in War* is perhaps the most thorough existing study of strategic assessment. Gartner's main argument in this book is that decision makers evaluate their policies through the lens of "dominant indicators," a highly imperfect approach that is the result of bounded rationality and organizational satisficing. Gartner shows that this model fits empirical behavior more convincingly than two alternative, nonrational theories (a "standard-organization" approach and an "action-reaction" model). Gartner does not, however, state what rational strategic assessment would entail – thus to the extent that he demonstrates how his model is perhaps the best nonrational approach to strategic assessment, it is still unclear how much adopting a "dominant indicator" model improves upon assuming that decision makers perform their duties in a fairly reasonable manner that is not distorted by organizational constraints.

²⁴ See Heuer 1999 on the psychology of intelligence analysis more generally along with Davis 1992, 2008 and Marrin 2011 on how the Central Intelligence Agency approaches the matter.

is not at all clear that even perfectly rational analysts could have anticipated the attack on Pearl Harbor. Other scholars have argued that the signals of the Pearl Harbor attack were so faint, and the noise of confounding information so strong, that perhaps analysts' mind-set correlated with the outcome without really causing it.²⁵

One could make a similar argument with respect to the enormous existing literature on the pathologies of U.S. learning and decision making in Vietnam. Among other arguments, scholars have stated that the Army's culture and organization inhibited its ability to adapt to counterinsurgency,²⁶ and that military officials tracked misleading indicators of progress,²⁷ deliberately misrepresented battlefield assessments,²⁸ and failed to correct perceptions of civilian leaders that they knew to be mistaken.²⁹ White House officials are then said to have been disposed to overoptimism³⁰ while processing information based on misleading cognitive filters³¹ in order to form policies guided not only by military considerations but also by concerns over

²⁵ Among others on this issue, see Betts 1976, 2007.

²⁶ Krepinevich 1988, Nagl 2002.

²⁷ Gartner 1997.

²⁸ Adams 1994.

²⁹ McMaster 1997.

³⁰ Johnson 2004.

³¹ Khong 1992.

impending presidential elections,³² distributional politics,³³ and the desire to avoid congressional criticism.³⁴

Once again, there is a difference between asking whether these mechanisms mattered versus how much they actually influenced key outcomes. Assigning causal relevance to these factors requires defining a plausible counterfactual scenario that articulates the way that rational (or at least more reasonable³⁵) decision makers might have behaved. And while almost all of the works cited above address this issue explicitly, they have been unable to establish a baseline conception of rational behavior in Vietnam that scholars generally accept.

Military historian Gregory Daddis, for instance, has argued that the Army was in fact a fairly effective “learning organization” in Vietnam, that it incorporated “best practices” and “lessons learned” about as well as anyone could expect, but that learning was not enough, in itself, to win the war.³⁶ Political scientist John Mueller observed that the communists in Vietnam sustained more casualties than almost any other insurgent movement in history – being such a radical

³² Downs and Rocke 1997.

³³ Caverley 2009/10.

³⁴ Craig and Logevall 2009.

³⁵ Manski 2011, 2013 distinguishes between “rational” or “axiomatic” approaches to decision theory (e.g., Savage 1954), which aim to prescribe specific policies, versus “reasonable” or “actualist” approaches to decision theory, which accept that there are no unambiguous criteria for choosing among undominated actions. (Manski argues that people facing actual decision problems have no inherent preference for consistency per se – all they care about is making a choice that is reasonable given their particular context.) The reasonability standard is thus more flexible, but it can still be difficult to assess how close actual decision makers come to satisfying it.

³⁶ See Daddis 2011, 2012, and especially 2013. Daddis argues that the conventional wisdom about Army learning in Vietnam is unduly influenced by the war’s outcome. Consistent with the argument made above, Daddis argues that strategic failure does not necessarily imply poor strategic assessment, even though existing literature implicitly assumes this to be true.

outlier, this was a situation that decision makers would naturally have found all but impossible to anticipate *ex ante*.³⁷ And despite the inevitably-flawed nature of the policy making process, Leslie Gelb and Richard Betts argued that decision makers actually weighed the costs, benefits, and uncertainties of the situation in a fairly reasonable way such that it might be said, ironically, that “the system worked.”³⁸ Thus even though Vietnam has been one of the most formative and well-studied experiences in U.S. military history, scholars still profoundly disagree on how rational decision makers should have approached the situation. This disagreement, in turn, drives historical assessments and broader questions about what lessons to take away from the experience.

In a similar fashion, entire research programs in the study of international relations can revolve around different views of rational strategic assessment. For instance, scholars have disagreed for nearly a century about Germany’s decision making in the opening weeks of World War I. Some say that Germany’s attempt to knock France out of the war quickly (an adaptation of the infamous “Schlieffen Plan”) was fatally flawed.³⁹ A wide range of scholarship hinges on

³⁷ Mueller 1980. In another argument relating to casualty counts, there is a prominent disagreement among historians as to whether the United States failed to pacify South Vietnam largely due to its excessive emphasis on killing and capturing insurgents, or conversely, whether it placed so many constraints on U.S. commanders that they could not attrit their opponents *enough*. Thus the title of a 2011 biography by Lewis Sorley of the general most closely identified with attrition in Vietnam is titled *Westmoreland: The General Who Lost Vietnam*, while an opposing perspective is articulated in a 2008 article by Dale Andrade entitled “Westmoreland Was Right.”

³⁸ Gelb and Betts 1979.

³⁹ Farrar 1973, Snyder 1984, Van Evera 1984, 1999, Blainey 1988: 35-40, Johnson 2004: 71-84. There is some disagreement about whether an original “Schlieffen Plan” actually existed (see Zuber 2002 and more than a dozen articles debating this idea in the journal *War in History*). But regardless of whether the German attack in 1914 was modeled after a pre-existing document, the question remains as to how well the attack was designed, and whether its main problems had to do with the concept or the execution.

explaining how it was that German leaders could have possibly believed that this strategy was in their interest, invoking ideas about the sources and influence of flawed leadership, bureaucratic politics, or ideas like the “Cult of the Offensive.”⁴⁰ But others believe that Germany’s strategy was basically sound, noting that the German army advanced to the outskirts of Paris and that the French would not have called their victory “The Miracle of the Marne” if the outcome were not seriously in doubt. Many observers have in fact argued that Germany’s main mistake in 1914 was not following the Schlieffen Plan closely *enough*: since the General Staff reallocated forces initially assigned to the main wing of the invasion, it has been criticized for “watering down” the attack.⁴¹

Few military experiences have been studied more thoroughly than this one. Yet historians still hold fundamentally different views about the kinds of mistakes Germany made in the opening stages of World War I and what a rationally-designed strategy might have looked like; political scientists have then developed fundamentally different theories of German behavior (often seen as a window into military behavior more generally) based on these diverging assessments. Without first defining what we should expect to see out of rational actors, it is hard to know how to evaluate past actions, what aspects of past experience require explanation, and what that means for informing broader scholarship.

One could make a similar argument for informing public policy debates. Chapter 5, for instance, examines U.S. policy during the occupation of Iraq. Despite escalating violence from 2003 through 2006, top officials largely stuck to their initial strategy of building Iraq’s security

⁴⁰ See, for instance, Miller, Lynn-Jones, and Van Evera 1991 and Lieber 2007.

⁴¹ Craig 1955, Van Creveld 1977, Turner 1979, Sagan 1986, and Herwig 2009.

forces while minimizing the U.S. military footprint. To much of the public, the administration's refusal to change strategies indicated that it was in a "state of denial," or that it was playing politics with the war.⁴² Yet throughout the occupation of Iraq, the administration had insisted that progress would be cumulative: it was only once Iraqis had reached a political consensus at the top that the country would consolidate and the insurgency would lose momentum. Thus even as critics pointed to unfavorable violence trends, U.S. officials maintained that they were meeting their benchmarks, and that just because their strategy had not worked yet this did not necessarily mean that it would not work soon. Top officials may or may not have genuinely believed these arguments when they made them – perhaps they were simply offered as a defense that could hold up in public debates – but that would only accentuate the notion that these kinds of arguments possess a logic that can be surprisingly difficult to disprove. Simply from the standpoint of effectively critiquing public policy, it will often be important to engage these analytic issues.

Policy initiatives also largely depend on assessments of rationality and nonrationality. For example, one of the largest bureaucratic reorganizations in recent memory was the restructuring of the U.S. armed forces via the Goldwater Nichols Act of 1986.⁴³ This bill aimed to mitigate rivalries among the military services, promoting the concept of "jointness" by changing command structures, encouraging services to develop doctrine, plans, and equipment in tandem, and requiring officers to adopt assignments outside their home service in order to earn promotions. One of the primary motivations for these changes was the widespread belief that interservice rivalry had hamstrung the U.S. military in Vietnam (as well as in subsequent

⁴² The term "state of denial" was the title of a prominent book by Bob Woodward 2006; see chapter 5 for examples of similar assessments at the time.

⁴³ See Locher 2002 for a history of Goldwater-Nichols.

operations like the failed Iran hostage rescue attempt). The Goldwater Nichols Act thus largely turned on a key counterfactual assumption, that if the military had been organized differently, then it would have been able to plan and execute its missions in a more rational and effective manner.

A similar debate is currently taking place in Washington, D.C., where in the wake of criticisms about the invasion and occupation of Iraq, many policy makers and analysts have called for a “Goldwater-Nichols II” process to reform the interagency system as a whole along the lines of how the 1986 bill targeted relations among the military services specifically.⁴⁴ Once again, this effort hinges on the premise that a better-designed system would have achieved better results. But how much better would those results have been? How much more accurately would an ideally-designed bureaucracy have assessed the situation in Iraq and how much more effectively would it have executed its plans? Do these improvements justify the cost of a major institutional restructuring?

The answers to these sorts of questions are often left implicit, but as we have seen, they can be difficult to establish and defend. In this way, evaluating the merits of high profile policies *ex ante* (or their impact *ex post*) requires laying out conceptions of what kinds of actions would be rational, accepting that even if that standard is unrealizable in practice, it serves as an important benchmark for making normative, prescriptive, and causal claims.

This discussion highlights that while the following chapters offer historical judgments, empirical predictions, and policy implications, these arguments are secondary to (and conditional on) a primary theoretical focus. The main goal of this dissertation is to rethink and rebuild

⁴⁴ See, for example, Murdock, Flournoy, Williams, and Campbell 2004 and Bowen 2009.

conceptual frameworks for defining rational strategic assessment, and to show how doing so sheds light on why it is that military decision makers often struggle to evaluate their policies, and why they often stick to unsuccessful strategies for so long.

Ultimately, the notion that armed conflict involves cumulative dynamics is not new. The cumulative dynamics of war have been a central theme of writing on military strategy since Clausewitz, and most people find these dynamics intuitively obvious. What is much less obvious, however, is why those cumulative dynamics *matter*, how they influence learning and adaptation, how they affect decision making, and why existing approaches do not capture key issues. The following chapters show how examining these issues provides an opportunity to question prominent theoretical frameworks, to inform ongoing policy debates, and to re-evaluate salient historical experience.

Chapter Two: Conceptual Foundations

REPEATED PROCESSES, CUMULATIVE PROCESSES, AND THE WAR IN VIETNAM

One of the central themes of Carl von Clausewitz's book *On War* is that armed conflict is a cumulative process. "The course of battle," Clausewitz wrote, is "a slow process of mutual attrition that will reveal which side will first exhaust its opponent." "Losing an engagement" is thus "like the gradual sinking of a scale" as "every engagement is a whole, made up of subsidiary engagements that add up to the overall result." Clausewitz argued that this kind of cumulative dynamic characterized war not only at the level of individual battles, but also at the level of broader military offensives, which gradually lose force and are eventually halted at a "culminating point." To Clausewitz, this implied that one of the key characteristics of a successful general is the ability to "detect the culminating point with discriminative judgment" *ex ante*, and to determine whether it is feasible to achieve strategic objectives before the offensive runs out of momentum.¹

Clausewitz was writing about Napoleonic warfare, but decision makers wrestle with similar dynamics in many areas of national security. In his famous 1947 essay on Cold War grand

¹ See Clausewitz 1832/1976: Book IV, Chapter 7; Book IV Chapter 9; and Book VII, Chapter 5. For centuries, scholars have theorized about the ways in which military outcomes at each level of analysis in war are the combined product of actions at lower levels. While this "levels of war" concept is often cited to Clausewitz and Antoine-Henri de Jomini, it dates back to earlier thinkers such as Paul Gideon Joly de Maizeroy, Henry Lloyd, and G.F. von Templehoff. See Gat 1989: esp. 42, 79 and Handel 2000: ch.3 and app. E for reviews.

strategy, for instance, George Kennan explained that since the communist system “bears within it the seeds of its own decay,” a policy of “patient but firm and vigilant containment” could “promote tendencies which must ultimately find their outlet in either the breakup or the gradual mellowing of Soviet power” – though Kennan was clear that this assessment was shrouded in uncertainty (“This cannot be proved. And it cannot be disproved.”) U.S. military strategy during the Korean War largely revolved around the notion that “continued piecemeal destruction of the offensive potential of the Chinese Communist and North Korean Armies” would eventually push them to make acceptable concessions, though it was by no means apparent how much attrition these opponents would be able to sustain.² When the United States launched airstrikes against Serbia to stop ethnic cleansing in Kosovo in 1999, Secretary of State Madeleine Albright wrote that the administration believed “a NATO bombing campaign directed at [Serbian president Slobodan] Milosevic’s sources of power would weaken him, but we didn’t know how long he would hold out.”³ These statements all reflect Clausewitz’s conception that cumulative dynamics play a central role in strategic assessment as decision makers struggle to determine how long it might take and how much it might cost to achieve their desired goals.

If these dynamics sound intuitive, this chapter shows how contemporary scholarship on military decision making often does not take them into account. In fact, Section 2.1 explains how prominent theoretical frameworks revolve around nearly opposite premises, assuming that military decision makers are observing repeated processes that remain the same from one round of fighting to the next, much like the odds of playing a slot machine or a roulette wheel. These

² This quote is from Ridgway 1967: 167. See Malkasian 2002: chs. 7-9 on attrition strategies (and the difficulty of assessing them) in the Korean War.

³ Albright 2003: 406.

theoretical frameworks reflect broader scholarship on decision making in fields like optimal stopping, dynamic programming, sequential analysis, and bandit problems. Yet these fields were not developed to deal with the kinds of cumulative dynamics that are so central to strategic assessment. Section 2.1 makes this argument in more detail and thus motivates the need for developing new conceptual foundations for analyzing cumulative dynamics in armed conflict.

Section 2.2 begins to develop these conceptual foundations through inductive theory-building, placing special emphasis on U.S. military decision making during the War in Vietnam. For more than a decade, U.S. policymakers struggled to determine how much attrition the Viet Cong could sustain, how difficult it would be to bring Hanoi to the bargaining table, and whether it might be possible to create a stable government in Saigon. These questions all revolved around cumulative dynamics, and the way that U.S. officials approached them constitutes some of the most controversial decision making in this country's military history.⁴ If a theoretical framework is intended to be useful for informing salient debates about national security then it should be relevant to this experience, and so the Vietnam War is a useful place to ground this dissertation's conceptual structure.

Section 2.3 then expands on the *deductive* logic of this theoretical framework in order to demonstrate how it can also be derived from basic principles that apply to military decision making more generally. Section 2.4 closes by discussing the scope conditions associated with this framework in more detail.

⁴ For a review of scholarly debates on Vietnam, see Chapter 1, Section 3.

Section 2.1. Conceptual foundations of existing literature

The standard theoretical framework that political scientists use to model the way that rational decision makers form and revise their expectations in armed conflict comes from the literature on informational asymmetries and the “bargaining model of war.”⁵ This is one of the most prominent developments in the contemporary study of international relations, and it may be the most widely-taught research program that uses formal theory to analyze armed conflict.⁶ This framework assumes that combatants enter conflict with uncertainty about their opponents’ capabilities and resolve. These factors determine a combatant’s overall “type,” which dictates the chances that they will be defeated in each “round of fighting.”

The term “round of fighting” is conceptually more useful than talking about a war’s “duration,” since many armed conflicts contain protracted periods of relative calm. Scholars typically conceive of “rounds of fighting” as taking either of two different forms. Donald Wittman, R. Harrison Wagner, Robert Powell, and James Fearon examine war at the *strategic level*, assuming that each round of fighting induces some probability that one side will defeat the other and take control of whatever stakes the combatants were fighting for.⁷ By contrast, Alastair Smith, Darren Filson and Suzanne Werner, Branislav Slantchev, and Smith and Allan Stam examine war at the *tactical level*, where each “round of fighting” constitutes an individual battle

⁵ As mentioned in Chapter 1 (note 23), Gartner 1997 is one of the most prominent works on strategic assessment in general, but it does not thoroughly discuss what rational strategic assessment would entail.

⁶ The literature on asymmetric information and coercive bargaining is typically traced to Blainey 1973, whose insights were then developed and formalized by Fearon 1995. For reviews of the literature on the bargaining model of war, see Powell 2002, Reiter 2003, and Walter 2009.

⁷ Wittman 1979, Wagner 2000, Powell 2004, Fearon 2007a.

within a broader conflict.⁸ This model (often called “battling for forts”) assumes that combatants enter conflict with a certain number of forts in their possession, each round of fighting determines whether a marginal fort changes hands, and if one side captures all of its opponent’s forts, then it achieves a military victory.

In either of these formulations, uncertainty about an opponent’s type is a crucial determinant of combatants’ behavior. If both sides had complete information (or even if they just had symmetric information) then they should be able to agree on the expected outcome of the war in advance. Since fighting is costly, both sides could make themselves better off if they arranged for this outcome via negotiated settlement and split the remaining surplus.⁹ This was the central insight in James Fearon’s 1995 article on “Rationalist Explanations for War,” a piece that is generally credited with launching the contemporary formal literature on coercive bargaining in armed conflict.

The bargaining model of war thus largely revolves around the way that decision makers wrestle with uncertainty in evaluating their strategic prospects, which largely boils down to estimating an opponent’s “type.” In the simplest models, opponents can be either of two types, strong or weak. Strong types are less likely than weak types to be defeated in each round of

⁸ Smith 1998, Filson and Werner 2002, Slantchev 2003, Smith and Stam 2004.

⁹ Two reasons that this might not be possible would be if combatants face commitment problems in which one side would have a strong incentive to renege on the deal at a later date (Walter 2002, Fearon 2004, Powell 2006, 2012), or if combatants are negotiating over indivisible issues (Goddard 2006, Toft 2003, 2006, Hassner 2009). The “bargaining model of war” covers these issues as well as the problems of asymmetric information that are discussed in the text above. Commitment problems and issue indivisibility lie outside the scope of this dissertation, however, as by definition, decision makers with complete information would face no uncertainty about strategic assessment.

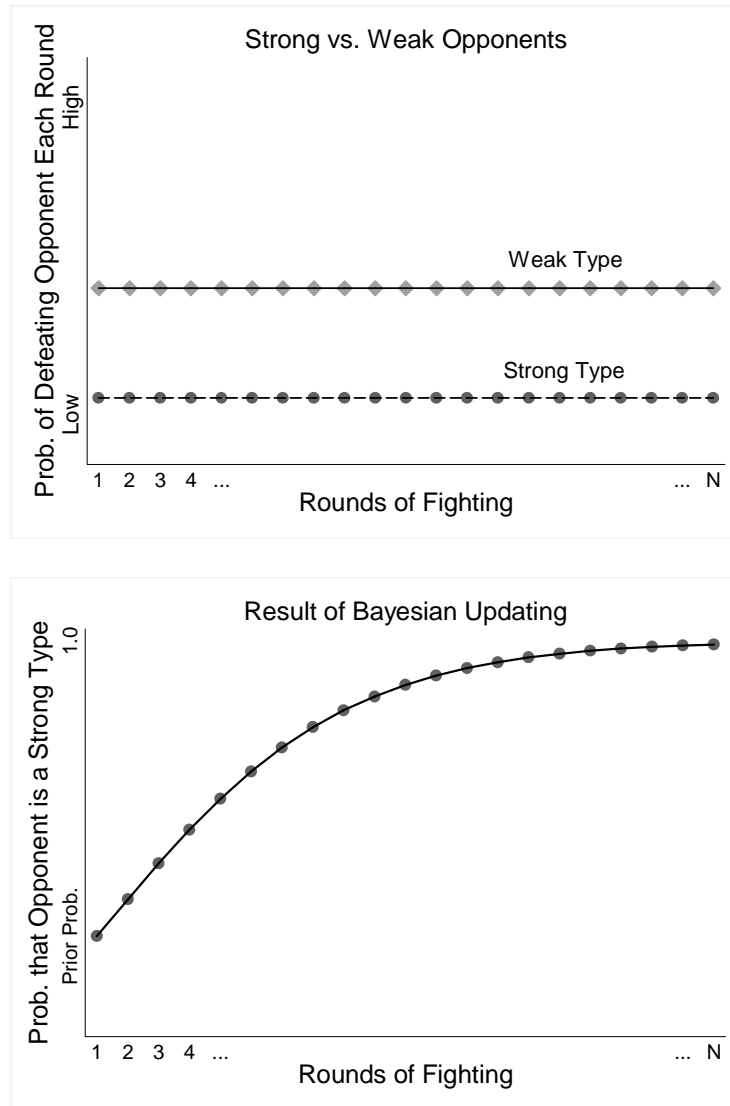


Figure 2.1. Standard Model of Learning in War: Armed Conflict as a Repeated Process

Figure 2.1 represents the standard framework for modeling the way that decision makers form and revise expectations in war. The top panel represents the assumption that an opponent’s “type” dictates its probability of being defeated in each round of fighting. Because those probabilities are fixed and repeated, each round of fighting provides “type-separating information” leading to the smooth, monotonic learning process shown in the bottom panel.

fighting. A common and important assumption, shown in the top of Figure 2.1, is that the probability of defeating an opponent remains the same in every round.

This framework produces an important result, which is that each round of fighting provides information that helps to reveal a combatant's type. After each round of fighting concludes without inducing defeat (in strategic-level models) or each time a combatant loses a battle (in tactical-level models), rational decision makers should become more likely to think that they are facing a strong opponent.¹⁰ This leads to the kind of gradual, monotonic learning process represented in the bottom panel of Figure 2.1. And this learning process is theoretically significant: it indicates that as combatants fight without achieving their goals they should become more pessimistic about their ability to do so, and thus they should also become more amenable to compromise. As Alastair Smith and Allan Stam explain, "The act of waging war reveals information about the relative strengths of each side. As a war progresses, each side's beliefs about the likely outcome of continuing the war converge. Once the warring parties' beliefs have converged sufficiently, they can find a bargained solution to the conflict."¹¹

¹⁰ For example, Powell 2004: 349-350 develops a model in which each round of fighting "generates a risk k_S that [State] S collapses in that round and a risk k_D that [State] D collapses." Those probabilities are assumed to remain constant, and thus Powell explains how if there is a round of fighting in which D did not collapse, then S can use Bayes' rule to update prior expectations and should thus become "more confident that it is facing the more powerful type k_D relative to k'_D This is the sense in which fighting conveys information." Wagner 2000 and Fearon 2007a use similar models, extending logic put forth by Blainey 1973, Wittman 1979, and Fearon 1995. See also the description in the main text above of the "battling for forts" models of Smith 1998 and subsequent authors. Slantchev observes (2003: 627) that almost all formal models of coercive bargaining with asymmetric information exhibit the "screening property" discussed here.

¹¹ Smith and Stam 2004: 783.

This is an important argument because it indicates that war is a self-terminating process, as fighting resolves the uncertainty that led combatants to armed conflict in the first place. As Filson and Werner explain, “War itself provides the information necessary for disputants to reach a settlement to end the war.”¹² The bargaining model also helps to set baseline expectations for thinking about the way that military leaders should form and revise their views. As H. E. Goemans writes in his study of German decision making in World War I, for instance, “War makes agreement possible because war provides information.... As the war progresses, at least one side must discover that its estimate [of the balance of power] was wrong. As unexpected defeats and failures on the battlefield mount, the relatively weaker side learns it overestimated its strength. A rational actor then lowers his estimate of his relative strength.”¹³

Contemporary scholars of coercive bargaining generally base these predictions on formal, mathematical reasoning, but perhaps one of the reasons why this research program has been so successful is that its findings reinforce basic intuitions many people have about the way that military leaders should learn and adapt. In general, it seems reasonable to expect that battlefield outcomes should reduce decision makers’ uncertainty about their opponents’ capabilities and resolve. It also seems reasonable to expect that the longer decision makers go without achieving their objectives, the more pessimistic they should become about their ability to do so, and the more likely they should be to change course.

¹² Filson and Werner 2002: 820. Cf. Reiter 2003: 31: “Combat can reduce uncertainty by providing information about the actual balance of power.... The outcome of combat is observed by both sides and should cause their expectations to converge regarding the likely outcomes of future combat. This increases the likelihood of reaching an agreement that both sides prefer over continued fighting.”

¹³ Goemans 2000: 27-28.

Yet it is important to keep in mind that the predictions of the bargaining model rely on the notion that armed conflict is a repeated process, in which the odds of winning the war (or the odds of winning each battle) are the same in one round as they are in the next. Here, existing scholarship clearly *clashes* with basic intuition.

For example, when the German Army invaded France at the beginning of World War I, it won the early Battles of Charleroi and Mons. The bargaining literature implies that rational German decision makers should thus have become more optimistic about their ability to win the next major battle at the Marne. Yet as the German Army advanced towards Paris, its supply lines were becoming overextended, communication among units was breaking down, and exhaustion was setting in among troops and commanders. Germany's attempt to knock France out of the war was already falling behind schedule.¹⁴ And surely the French Army would fight more tenaciously to defend its capital than to hold peripheral territory. It would thus be tenuous to suggest that Germany had the same probability of winning the Battle of the Marne and the Battle of Mons. Yet this is the key assumption underpinning theoretical scholarship on the bargaining model of war, where rounds of fighting are assumed to have independent and identically distributed outcomes.

Or consider Japanese decision making in World War II. After the Battles of Coral Sea and Midway in 1942, U.S. forces established naval dominance in the Pacific and began successively rolling back Japanese defenses until Tokyo surrendered three years later. Why did Japan hold out for so long, suffering through nearly three years of repeated tactical defeats? This question poses a clear puzzle for the bargaining model framework, which argues that each time combatants lose

¹⁴ See Herwig 2009 for a recent history of the war's opening stages, along with a review of debates about German decision making throughout.

a battle, they should believe there is a higher chance that they will lose the next battle, too. But this argument does not really engage the basic tenets of Japanese strategy. Japan's goal in the Pacific theater was not to capture and hold as much territory as possible – the idea was to establish a defensive perimeter, force the United States to expend heavy losses every time it rolled that perimeter back, and thus eventually to push U.S. decision makers past their threshold for casualty tolerance beyond which they would no longer be willing to continue fighting. In this respect, Japanese strategy revolved around the Clausewitzian notion that every offensive gradually builds towards a culminating point, Tokyo's key mistake was in thinking it could bring this culminating point about, and this dynamic is largely left out of contemporary models of learning and adaptation in war.

To close this section, it is important to be clear that despite the current literature's limitations, its assumptions are by no means idiosyncratic. In many ways, the bargaining model framework is similar to models of learning and adaptation in broader literatures on optimal stopping, dynamic programming, sequential analysis, and bandit problems.¹⁵ These literatures, like the bargaining model of war, capture the way that decision makers learn about repeated processes. Both contain many applications to gambling, and when they model processes of learning and adaptation, the notion that the outcomes of these processes are independent and identically distributed is essentially the assumption of first resort.

The term “bandit problems,” for instance, comes from the example of a gambler playing a slot machine (colloquially known as a “one-armed bandit”), who is attempting to determine the machine's payoff function by playing the game and observing its results. Because a slot

¹⁵ Powell 2004, for instance, explicitly characterizes his model as a “dynamic programming problem.”

machine's payoff probabilities are fixed and repeated, decision makers should be able to form increasingly accurate expectations over time about what those probabilities are. A rational gambler would presumably not begin playing a slot machine unless she believed that it offered an acceptable rate of return – but as she continues to play without winning, she will be more likely to infer that the machine has a low probability of paying off. At some point she may decide that this machine is not as good a bet as she initially thought, and she would stop playing it.¹⁶

This is a reasonable analogy for the dynamics that are captured by the bargaining model of war. And more broadly than this, when officials, experts, and pundits debate military strategy and national security, they regularly invoke terms like “the gambler’s fallacy” and “doubling down” to describe people who favor sticking with policies that do not appear to be making headway. In the context of slot machines, roulette wheels, or other games that involve repeated processes, these models and idioms make sense. Military strategy, however, involves cumulative dynamics that have a very different logic for thinking about the way that decision makers should learn and adapt.¹⁷ The next section will begin a more detailed examination of what these dynamics entail.

¹⁶ On optimal stopping, see Dubins and Savage 1965, Chow, Robbins, and Sigmund 1971, Hill 2009, and *Stochastics*, Vol. 77, Nos. 1-4. On bandit problems, see Berry and Fristedt 1985 and Gittins 1989. On dynamic programming, see Bellman 1957.

¹⁷ Interestingly, Dubins and Savage begin their famous work by explicitly pointing out that it has almost no real-world applicability. Their work is based around the notion that a gambler has arrived in a casino, and must win a certain amount of money by the end of the night. They explain that “The fantasy with which we have introduced the general problem of optimal gambling systems has no immediate practical importance” (1965: 1). They defend this choice based simply on precedent (“the probabilist’s tendency to invoke gambling imagery,” vii) and theoretical interest (“the problem, once proposed, cries out for attention as pure mathematics,” 1). Yet as with many scholarly paradigms, these initial caveats have become much less explicit

Section 2.2. Inductive theory-building: strategic assessment in Vietnam

The purpose of this section is to begin constructing a conceptual framework for strategic assessment that incorporates the cumulative dynamics of armed conflict. The theory-building in this section is largely inductive, based on an analysis of U.S. decision making during the Vietnam War. Grounding concepts in this experience helps to ensure that the ideas developed in this dissertation are relevant to actual analytic challenges that military decision makers face in salient cases. Section 2.3 will then flesh out the deductive foundations of this framework in order to demonstrate how the dynamics discussed here generalize more broadly.

The analysis of Vietnam in this chapter is based on three sets of primary sources. These are the *Pentagon Papers*, which were compiled by the Department of Defense, leaked to the public, and then entered into the Congressional Record by Senator Mike Gravel; the U.S. Department of State's *Foreign Relations of the United States* series, which contain documents that have been compiled and published by the State Department's Office of the Historian; and the National Intelligence Council's *Estimative Products on Vietnam*, which comprise nearly 200 declassified intelligence assessments relating to the war.

All of these collections have at some point been compiled by government bodies, and so it is unlikely that they constitute a truly representative sample of viewpoints. Nevertheless, these documents provide systematic surveys of primary source materials on the Vietnam War from diplomats, military officers, political officials, and intelligence analysts. To the extent that the authors of these documents (or the editors who compiled them for publication) would have had

over time, as standard modeling assumptions acquire inertia and scholars seek for ways of extending them.

an incentive to bias their selection and articulation of these assessments, they would presumably do so in a way that justified their shortcomings by emphasizing the analytic challenges that they faced. Since the goal of this chapter is to examine those analytic challenges, the *Pentagon Papers*, *Foreign Relations of the United States*, and *Estimative Products on Vietnam* are useful source material.

Defining objectives and assessing uncertainty

U.S. military strategy during the Vietnam War was based on three main objectives: to defeat the Viet Cong, to stop North Vietnam from assisting the insurgency, and to create a stable government in Saigon.¹⁸ Policy makers argued extensively about how long it might take and how much it might cost to achieve these goals, and many expressed explicit reservations about their ability to make these estimates in clear and rigorous ways.

When policy makers discussed fighting the Viet Cong (VC), for instance, they often debated the feasibility of reaching a so-called “crossover point,” where the insurgents would start to incur more losses than they could replace. The crossover point would be an important breakthrough because it would suggest that the military balance had tipped against the communists.¹⁹ In order

¹⁸ The Joint Chiefs of Staff describe these “three independent undertakings” in *Pentagon Papers* (henceforth *PP*), Vol. IV, p. 395. Other documents sometimes mention additional objectives (for instance, several discuss a fourth strategic objective as being to keep China from entering the war), and some documents combine defeating the Viet Cong and halting North Vietnamese infiltration into a single objective of ending the insurgency; but since many documents discuss the insurgency in the South and the bombing in the North separately, it makes sense to keep those subjects separate in this analysis as well.

¹⁹ Westmoreland believed that achieving the crossover point was his primary strategic objective. Documents that discuss the crossover point include: William Westmoreland, Notes on Discussions with President Johnson, 27 April 1967: *Foreign Relations of the United States*

to estimate how close U.S. forces were getting to the crossover point, analysts tracked a range of measures. The most well-known of these was the “body count” of insurgents killed or captured. It was notoriously difficult to gather accurate information on insurgent attrition, but it was even harder to estimate the Viet Cong’s ability to reconstitute its ranks. This depended on the number of forces being held in reserve, the remaining pool of potential recruits, desertion rates and morale, and capacity to convert new volunteers into effective soldiers.²⁰ From early on, U.S.

(henceforth *FRUS*), 1964-1968, Vol. V, document 149; Robert McNamara, “Future Actions in Vietnam,” Draft Memorandum from Secretary of Defense McNamara to President Johnson, 19 May 1967: *FRUS*, 1964-1968, Vol. V, document 177; Robert Komer, Memorandum from the President’s Special Assistant to Secretary of Defense McNamara, 29 November 1966: *FRUS*, 1964-68, Vol. IV, document 318 (in which Komer also discusses several other “major turning points”); and Komer, Telegram from the President’s Special Assistant to President Johnson in Texas, 9 July 1967: *FRUS*, 1964-68, Vol. V, document 234. See also Lewy 1978: 78-84 and Drea 2011: 131-137 for discussions of the crossover point and the body count, as well as reflections by Secretary McNamara 1995: 238) and General Westmoreland 1976: 160, 239, 332 themselves. In other documents, decision makers discussed similar tipping points like the chances that “Viet Cong morale cracks significantly” so as to provide the United States with a “decisive break.” McGeorge Bundy and Dean Rusk, “Courses of Action in Viet-Nam,” Memorandum for President Johnson, 9 November 1965: *FRUS*, 1964-68, Vol. III, document 194; see also William Bundy, “1967 and Beyond in Vietnam,” Draft Paper prepared by the Assistant Secretary of State for East Asian and Pacific Affairs, 21 December 1966: *FRUS*, 1964-68, Vol. IV, document 347.

²⁰ For documents describing the difficulty of estimating and combining these factors, see Department of Defense, “Summary Statement on South Vietnam,” Memorandum, 2 March 1964: *FRUS*, 1964-68, Vol. I, document 67; National Security Council, Summary Notes of the 557th Meeting of the National Security Council, 10 May 1966: *FRUS*, 1964-68, Vol. IV, document 135; “The Vietnamese Communists’ Will to Persist,” 26 August 1966: *Estimative Products on Vietnam* (henceforth *EPV*), pp. 353-376; McNamara, Draft Memorandum for the President, 17 November 1966: *PP*, Vol. IV, pp. 365-373; Westmoreland, Notes on Discussions with President Johnson, 27 April 1967: *FRUS*, 1964-68, Vol. V, document 149; Notes from Meeting of the President with Secretary McNamara to Review the Secretary’s Findings during Vietnam Trip, 12 July 1967: *FRUS*, 1964-68, Vol. V, document 238; Special National Intelligence Estimate [SNIE] 14.3-67, “Capabilities of the Vietnamese Communists for Fighting in South Vietnam,” 13 November 1967: *EPV*, pp. 429-455; and SNIE 14.3-69, “Capabilities of the Vietnamese Communists for Fighting in South Vietnam,” 17 July 1969: *EPV*, pp. 473-501. Elsewhere, Daddis 2011, 2012 provides some of the most comprehensive assessments of “the problem of metrics” in the Vietnam War, conveying the breadth of different indicators which Vietnam decision makers tracked. Adams 1994 gives a first-hand account of how bureaucratic politics and politicization also inhibited accurate assessments of Viet Cong casualties and order of battle.

decision makers acknowledged that they could not reliably evaluate these factors. Ambassador Maxwell Taylor wrote, for instance, that “The ability of the Viet-Cong continuously to rebuild their units and to make good their losses is one of the mysteries of this guerilla war,”²¹ while Secretary of Defense Robert McNamara warned that “all of our estimates of enemy strength and variations... contain very great uncertainties,” that “any conclusions drawn from them must be considered to be highly tentative and conjectural,” and thus that “it is impossible to predict the point at which we can expect to attrite enemy forces at the rate he introduces new ones.”²²

Similar uncertainty and intellectual agnosticism surrounded the effort to stop North Vietnam (NVN or DRV) from supporting the insurgency. The United States began bombing North Vietnam in summer 1964 and then launched a sustained air campaign (Operation Rolling Thunder) in March 1965. The bombing was intended to push Hanoi past its “breaking point” where it would no longer be able or willing to send supplies and reinforcements across the border. In Taylor’s words, the administration hoped that by gradually increasing the intensity and destructiveness of the bombing, the United States could “convey signals [to Hanoi] which, in combination, should present to DRV leaders a vision of inevitable, ultimate destruction if they do not change their

²¹ Maxwell Taylor, “The Current Situation in South Viet-Nam,” Paper prepared by the Ambassador in South Vietnam, November 1964 (exact date not given): *FRUS*, 1964-68, Vol. 1, document 426.

²² McNamara, Draft Memorandum for the President, 17 November 1966: *PP*, Vol. IV, p. 370. Similarly, National Security Advisor Walt Rostow wrote to President Johnson that while intelligence assessments could identify “a progressive decline in the morale and the fighting capacity” of the Viet Cong, “no one can tell you when this progressive decline will lead to the breaking up of units or to the ending of the war.” See Rostow, Memorandum from the President’s Special Assistant to President Johnson, 2 August 1966: *FRUS*, 1964-68, Vol. IV, document 198.

ways.”²³ (Put more crudely, National Security Council staffer Robert Komer wrote that the purpose of the bombing was to force Hanoi to “cry uncle.”²⁴) But again, decision makers found it difficult to estimate how long it might take or how much it might cost for these measures to achieve their intended goals. Officials generally believed that the bombing depressed morale throughout Vietnam, damaged industrial and transportation infrastructure, raised food prices, and eroded “discipline” in the communist workforce. Yet what did that ultimately mean for the prospects of breaking Hanoi’s will? Evaluating strategic progress required making conjectures about the nature of North Vietnamese society, the extent to which a predominantly decentralized, agricultural nation could absorb the impact of the bombing, and how the regime in Hanoi might potentially react.²⁵ Analysts regularly acknowledged the difficulty of juggling these factors. As a general rule, there was “no agreement in the intelligence community as to when [North Vietnamese] morale may reach the breaking point.”²⁶ Decision makers did not even have a basic

²³ Taylor, quoted in *PP*, Vol. III, p. 316; see also SNIE 50-2-64, “Probable Consequences of Certain US Actions with Respect to Vietnam and Laos,” 25 May 1964: *EPV*, pp. 202-215; Rostow and Komer, “A Strategy for the Next Phase in Vietnam,” Draft Paper by the President’s Special Assistants, 20 September 1966: *FRUS*, 1964-68, Vol. IV, document 241.

²⁴ Komer, quoted under the heading, “Critical Variables Which Will Determine Success in Vietnam,” in *PP*, Vol. IV, p. 155.

²⁵ Documents exemplifying the challenges in assessing the political and economic effects of the bombing include SNIE 50-2-64, “Probable Consequences of Certain US Actions with Respect to Vietnam and Laos,” 25 May 1964: *EPV*, pp. 202-215; John McNaughton, “Plan of Action for South Vietnam,” 3 September 1964: *PP*, Vol. 3, document 188; National Security Council Working Group on Vietnam, “The Situation in Vietnam,” Intelligence Assessment, 24 November 1964: *PP*, Vol. III, document 240; Rostow, “Status of Civilian Morale in Vietnam,” 19 September 1966: *FRUS*, 1964-68, Vol. IV, document 239; as well as various documents from March 1965 quoted in *PP*, Vol. IV, pp. 254-65. Rostow was perhaps the most zealous advocate of the bombing campaign, and Milne 2008 discusses his assessments in light of how Rostow viewed the North Vietnamese economic system.

²⁶ Rostow, “Status of Civilian Morale in Vietnam,” 19 September 1966: *FRUS*, 1964-68, Vol. IV, document 239.

idea of how much North Vietnamese infiltration they would need to interdict in order to undermine the insurgency: according to a Special National Intelligence Estimate in 1966, it was possible to assess Viet Cong requirements and supply levels to “only rough orders of magnitude,” and so even if the bombing “would probably place an effective ceiling on Communist capabilities to expand their military effort in the South” there were “too many uncertainties to permit estimating at just what level the limit or expansion would be.”²⁷ To summarize, U.S. decision makers saw it as one of their main objectives in the Vietnam War to coerce Hanoi into dropping its supply efforts below acceptable levels – but they could not say how difficult this would be to accomplish, nor could they even define what those acceptable levels entailed.²⁸

A third major U.S. objective during the war was to reinforce the Government of South Vietnam (GVN). Here decision makers often spoke of achieving a “leveling off point” where the government would at least be able to gain new supporters as fast as it lost others.²⁹ Assessments

²⁷ SNIE 10-1-66, “Possible Effects of a Proposed US Course of Action on DRV Capability to Support the Insurgency in South Vietnam,” 4 February 1966: *EPV*, pp. 331, 338; see also Admiral Lloyd Mustin’s discussion of estimating the “minimum necessary sustaining level” of Viet Cong resupply, quoted in *PP*, Vol. III, pp. 213-214. Another useful description of the uncertainties involved with assessing North Vietnamese infiltration can be found in “Summary of Recent MACV and CIA Cables on Infiltration,” 27 November 1964: *PP*, Vol. III, document 243.

²⁸ Leslie Gelb (who directed the *Pentagon Papers* project) and Richard Betts offer a similar characterization in their book analyzing decision making during the Vietnam War: “Much of the most important information about Vietnam was essentially unquantifiable,” they argue. “[Some goals] were so intangible that it was hard to gauge progress or retrogression” (Gelb and Betts 1979: 303, 306-307).

²⁹ Documents which discuss the chances that the loss of political support in South Vietnam will “level off” or “bottom out” include: Secretary McNamara as quoted in *PP*, Vol. III, p. 508; Memorandum Prepared by the Directorate of Intelligence, 15 May 1964: *FRUS*, 1964-68, Vol. I, document 159; William Bundy, “Where Are We Heading?” 18 February 1965: *PP*, Vol. III, document 252; McNaughton, “Proposed Course of Action Re Vietnam,” Memorandum for Secretary of Defense McNamara, 24 March 1965: *PP*, Vol. III, document 153 (see also *PP*, Vol.

of Saigon's legitimacy and the morale of South Vietnamese forces varied widely as analysts struggled to understand and predict the relevant social dynamics. (There is a famous anecdote, for instance, in which President Johnson is briefed by two emissaries just returning from Vietnam who provided nearly opposite assessments. Johnson "looked from one to the other" and replied "You two did visit the same country didn't you?"³⁰) William Sullivan (head of the interagency Vietnam Coordinating Committee) wrote in 1964 that "If we can obtain a breakthrough in the mutual commitment of the U.S. in Vietnam to a confident sense of victory" then it would be possible to achieve the necessary political reforms; but as on other issues, he also conceded that "No one... can define with precision just how that breakthrough can be established."³¹

Thus on each of the war's principal objectives, policymakers and analysts believed that they were making progress toward their desired end point without knowing how difficult it would be to get there or whether this was even feasible at all. One of the most revealing documents from this standpoint is the 1966 "Summer Study" prepared by the Jasons, an independent group of senior scientists who convene for several weeks each year to consult for the Department of Defense.³² The Summer Study was commissioned to evaluate the bombing campaign against

III, p. 348); Rostow, Memorandum from the President's Special Assistant to President Johnson, 5 April 1966: *FRUS*, 1964-68, Vol. IV, document 115; and Military Assistance Command Vietnam [MACV] quoted in *PP*, Vol. III, p. 345.

³⁰ *Pentagon Papers*, Vol. III, p. 23.

³¹ William Sullivan in June 1964, quoted in *PP*, Vol. III, p. 78.

³² The Jason program is still operational: see Finkbeiner 2006, in which pp. 65-70 discuss the 1966 Summer Study.

North Vietnam, and the report is useful because it directly addresses the intellectual challenges of doing so:

While conceptually it is reasonable to assume that some limit may be imposed on the scale of military activity that Hanoi can maintain in the South by continuing the ROLLING THUNDER program at the present, or some higher level of effort, there appears to be no basis for defining that limit in concrete terms or, for concluding that the present scale of VC/NVN activities in the field have reached that limit.... The fragmented nature of current analyses and the lack of an adequate methodology for assessing the net effects of a given set of military operations leaves a major gap between the quantifiable data on bomb damage effects, on the one hand, and policy judgments about the feasibility of achieving a given set of objectives, on the other. Bridging this gap still requires the exercise of broad political-military judgments that cannot be supported or rejected on the basis of systematic intelligence indicators. It must be concluded, therefore, that there is currently no adequate basis for predicting the levels of U.S. military effort that would be required to achieve the stated objectives – indeed, there is no firm basis for determining if there is *any* feasible level of effort that would achieve these objectives.³³

The Jasons' indictment of U.S. strategic assessment supposedly disturbed Secretary McNamara, who became increasingly frustrated with the war effort and eventually resigned in 1968.³⁴ But others had already reached similar conclusions. As early as 1963, for example, National Intelligence Estimates conceded that there was simply “no satisfactory objective means of determining how the war is going.”³⁵ McNamara himself had written in 1964 – in a

³³ *Pentagon Papers*, Vol. IV, pp. 117-119, emphasis in original.

³⁴ This claim is made by the authors of the *Pentagon Papers* (Vol. IV, p. 231); McNamara himself discusses the importance of the Jason report in his book *Argument Without End* (1999: 341). McNamara's views on the war in Vietnam are discussed in detail in two recent additions to the *History of the Office of the Secretary of Defense* series: see Kaplan, Landa, and Drea 2005: chs. 11 and 19 and Drea 2011 which largely focus on the way McNamara and his colleagues struggled with conflicting assessments of the war.

³⁵ NIE 53-63, “Prospects in South Vietnam,” 17 April 1963: *EPV*, pp. 186-198.

memorandum that one historian called the “intellectual foundation” for strategic planning³⁶ – that there was no way to know whether “our objective [in bombing Hanoi] could be achieved by any means within the practical range of our options.”³⁷

Assessing cumulative dynamics

Even if many U.S. officials were overconfident in assessing the Vietnam War, most were nevertheless in agreement that success was unlikely in the short run. When the Johnson administration approved Rolling Thunder in 1965, Secretary McNamara explained that among senior decision makers, “none of them expects the [Viet Cong] to capitulate or to come to a position acceptable to us, in less than six months.... [I]t will take more than six months, perhaps a year or two, to demonstrate VC failure in the South.” In an assessment from 1966, McNamara again wrote that “I see no reasonable way to bring the war to an end soon... there is no sign of an impending break in enemy morale.... The solution lies in girding, openly, for a longer war and in

³⁶ McMaster 1997: 75.

³⁷ McNamara, “South Vietnam,” Memorandum for the President, 16 March 1964: *PP*, Vol. III, p. 499. See also McNamara’s Report from Honolulu Meeting (21 April 1965: *PP*, Vol. III, document 256) in which he described that most key decision makers expected that Rolling Thunder would not succeed in less than a year. Here McNamara is characterizing his views along with those of William Bundy (Assistant Secretary of States for East Asian and Pacific Affairs), John McNaughton (Assistant Secretary of Defense for International Security Affairs), Maxwell Taylor (U.S. Ambassador to South Vietnam), Earl Wheeler (Chairman of the Joint Chiefs of Staff), Ulysses Sharp (Commander of the Pacific Fleet), and William Westmoreland (head of Military Assistance Command Vietnam). For more on the Honolulu meeting, see Drea 2011: 30-31, McMaster 1997: 95-103, and Logevall 1999: ch. 11.

taking actions which will in 12 to 18 months give clear evidence that the continuing costs and risks to the American people are acceptably limited.”³⁸

These statements represent an important way in which the cumulative dynamics of armed conflict influence perceptions of military strategy: decision makers may not expect initial effort to produce immediate returns, but rather to make progress toward reaching a state where success might later be achievable. To mix metaphors from the bargaining literature and Clausewitz, each “round of fighting” is intended to move decision makers closer to their desired “culminating point.”

Documents throughout the Vietnam War share this perspective. Assistant Secretary of State William Bundy, for instance, estimated that “we may have to hang on quite a long time before we can hope to see an improving situation in South Viet-Nam... the most likely prospect is for a prolonged period without major risks of escalation but equally without any give by Hanoi.”³⁹ Undersecretary of State Nicholas Katzenbach described “a consensus that one year from now we will be stronger than we are now, making continued progress against the VC, and slowly building up the GVN—but that there will not have been a decisive and undeniable breakthrough, that the enemy will still be very much with us, and that it will remain difficult to produce dramatic and convincing evidence of a victory in the near future.”⁴⁰ When the Joint Chiefs of Staff

³⁸ McNamara, Memorandum for the President, October 1966 (exact date not given): *PP*, Vol. IV, pp. 348-353.

³⁹ William Bundy, “Where Are We Heading?” 18 February 1965: *PP*, Vol. III, document 252. For a similar assessment, see NSC Working Group on Vietnam, “Intelligence Assessment: The Situation in South Vietnam,” 26 November 1964: *PP*, Vol. III, document 241.

⁴⁰ Nicholas Katzenbach, “Prognosis for Vietnam,” Memorandum for Secretary of State Rusk, 1 November 1967: this document is described in *FRUS*, 1964-68, Vol. V, document 374.

recommended a bombing escalation in 1968, they warned that “it is not anticipated that this impact will be immediately apparent.... The cumulative effects of the air strikes and naval bombardment will gradually increase to significant proportions as erosion of the distribution system progresses.”⁴¹ When national intelligence estimates predicted the impact of ground force commitments, they typically stated that these escalations would not cause the communists to buckle immediately. “The real test,” according to one estimate, “would be that of combat,” and it was only if “the tide of battle runs against the Viet Cong for a substantial period” that the communists might possibly resort to negotiations.⁴²

⁴¹ Earl Wheeler: *PP*, Vol. 4, pp. 254-256.

⁴² Directorate of Intelligence, “Memorandum: Reactions to a Further US Buildup in South Vietnam,” 10 June 1965: *EPV*, pp. 255-260. Similarly, see SNIE 10-11-65 (“Probable Communist Reactions to a US Course of Action,” 22 September 1965: *EPV*, p. 291), which states that “it is not likely that the North Vietnamese would move immediately to the conference table [following a buildup of US ground forces]; they would probably feel that there was still time to test the effectiveness of the Viet Cong against US forces.” In a Telegram from the Embassy in Vietnam to the Department of State (11 November 1965: *FRUS*, 1964-68, Vol. III, document 199), Ambassador Lodge wrote that “In conclusion, we believe the enemy will continue for the next several months at least on his current course of action... We do not expect the Viet Cong to revert to a lesser level of insurgency or to seek a negotiated settlement until they are convinced that their current course will not succeed. The point is not likely to be reached until the capabilities of the forces now being created and deployed have been neutralized or impaired in battle.” Other relevant examples include Lodge, Telegram from the Embassy in Vietnam to the Department of State, 26 August 1965: *FRUS*, 1964-68, Vol. III, document 127, and Directorate of Intelligence, “Memorandum: The Vietnamese Communists’ Will to Persist,” 26 August 1966: *EPV*, pp. 353-376. Lodge reports that MACV held similar views in his Telegram from the Embassy in Vietnam to the Department of State, 8 June 1966: *FRUS*, 1964-68, Vol. IV, document 153.

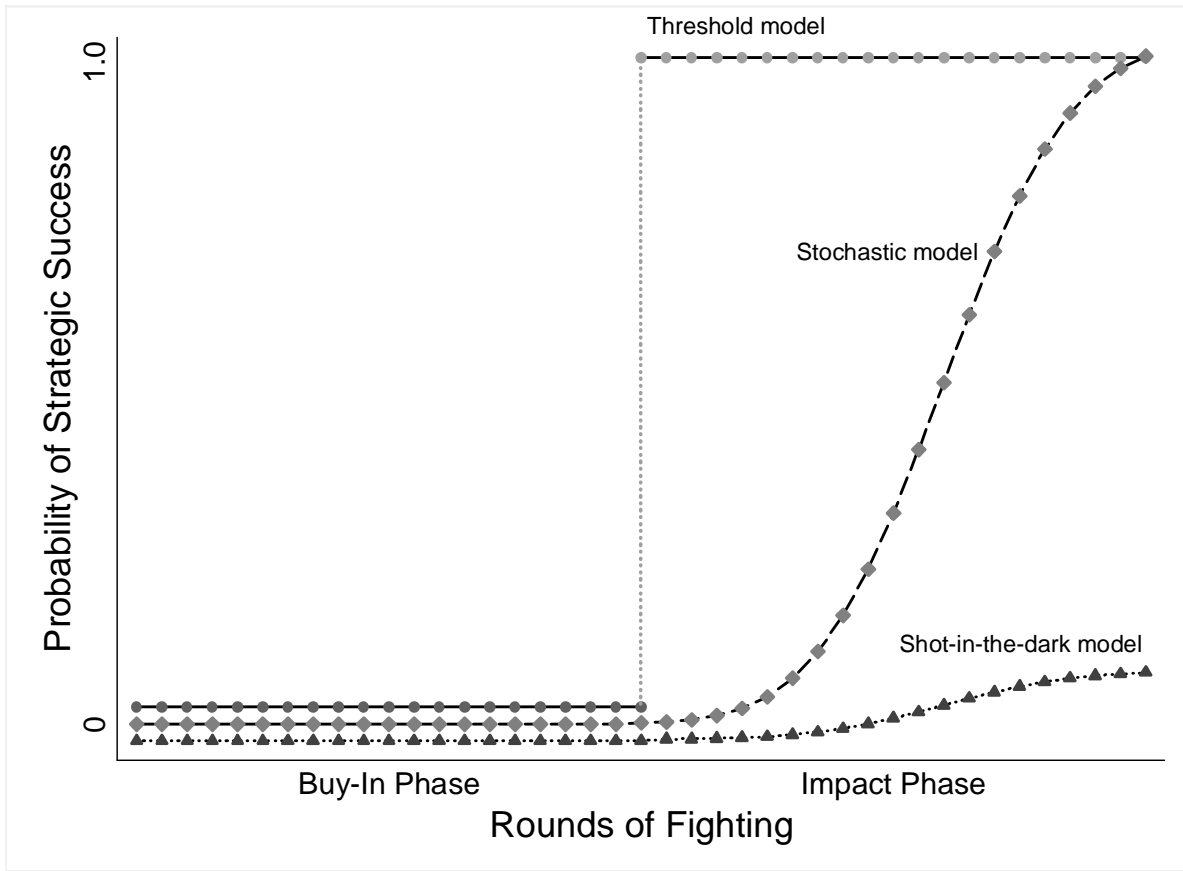


Figure 2.2 Capturing Cumulative Dynamics

Figure 2.2 lays out a schematic for assessing cumulative dynamics in armed conflict. Here, initial rounds of fighting yield no immediate return; this is labeled as the “Buy-In Phase.” Eventually, however, the investment advances to a stage (the “Impact Phase”) where it generates the possibility of achieving intended objectives: the odds of obtaining those objectives can come with certainty at a fixed point (represented here as the “Threshold Model”), they may gradually increase over time (represented here as the “Stochastic Model”), or the chances of success may always remain fairly low (represented here as the “Shot-in-the-Dark Model”).

How might we structure this analytic challenge conceptually? These documents suggest that it is possible to characterize the views of Vietnam War decision makers by dividing their policies into two, distinct phases. The first of these could be called the *Buy-In Phase*. During the Buy-In Phase, there would be little to no probability of achieving strategic goals. The Buy-In Phase is analogous to an entry cost, which does not directly produce profits, but makes it possible to achieve returns at a subsequent point. A better analogy from a lesser-known literature would be the concept of “time-to-build,” which models the way that real estate or research-and-development projects often require protracted expenditures before realizing gains.⁴³ The second stage could be called an *Impact Phase*. Success is not necessarily guaranteed here either, but once decision makers are in the Impact Phase, their investments will generate a positive probability of achieving desired goals.

Figure 2.2 represents these concepts graphically. The x-axis captures rounds of fighting, which we could also represent more generally in terms of the overall amount of costs decision

⁴³ See Grossman and Shapiro 1986, who describe how “Many research projects, as well as some types of investment programs for the installation of physical capital, can be described as follows: measurable progress is achieved over a period of time, but the investment yields no returns until the entire project is completed. Examples of this include laboratory development of a new product or process, the construction of a new building, and the writing of a scholarly journal article. When confronted with investment opportunities of this sort, individuals and firms must decide how many resources to devote to the project at each point in time. Implicitly, this also determines the (expected) duration of the project.” Some works in this field examine situations where “total effort required to complete the research satisfactorily is not known” (Kamien and Schwartz 1971). See also Majd and Pindyck 1987, Roberts and Weitzman 1981, and Dixit and Pindyck’s discussion (1994: chapter 10) on sequential investment. This literature has featured rarely (if at all) in the theory of military strategy, but it connects to the U.S. military’s doctrinal concept of “shaping operations” which are intended to “create and preserve conditions for the success of the decisive operation” (see U.S. Army Field Manual 3-0, *Operations*, paragraph 5-61).

makers have expended in trying to achieve their strategic goals.⁴⁴ As decision makers invest increasing amounts of resources, they will move rightward across the graph, eventually shifting from the Buy-In Phase to the Impact Phase.

Two kinds of uncertainty affect the way that decision makers should approach this situation. The first is uncertainty about where the phase change is located, and whether decision makers expect the Buy-In Phase to be relatively short or potentially protracted. A second crucial uncertainty is just how likely decision makers are to be successful once the Impact Phase begins. Figure 2.2 lays out three different examples of what the Impact Phase might look like: there is a “threshold model” in which a breakthrough comes immediately after completing the buy-in, a “shot-in-the-dark model” in which the probability of success becomes positive but remains relatively low, and a “stochastic model” in between. Any number of other functional forms may be possible as well.⁴⁵

⁴⁴ An advantage of equating “rounds of fighting” with “expended cost” is that this builds into the model the notion that decision makers might face increasing (or decreasing) costs of fighting as the war drags on.

⁴⁵ It is important to have some sense of how likely success will be during the Impact Phase, and how that likelihood will respond to additional investment. For example, William Bundy argued in 1967 that “In short, even if the GVN and we both do the best we possibly can, the odds are on the whole against a major strengthening of the GVN position or a true crack in [Communist] morale during 1967. The possibility of such a morale break is present, but its chances cannot be rated better than about one in three for 1967.” William Bundy, “1967 and Beyond in Vietnam,” Draft Paper Prepared by the Assistant Secretary of State for East Asian and Pacific Affairs, 21 December 1966: *FRUS*, 1964-68, Vol. IV, document 347. For a similar, earlier statement, see William Bundy, “Holding on in South Vietnam,” Memorandum, 30 June 1965: *PP*, Vol. IV, document 259.

This kind of probabilistic thinking may often be appropriate for characterizing the Impact Phase. Figuring out how to estimate these probabilities in a rigorous way is another important challenge for dealing with cumulative dynamics. In fact, one of the most common themes in the literature on intelligence analysis is the trouble analysts face when attempting to estimate and express probabilities. On this point, see Kent 1949, Zlotnick 1972, Heuer 1999, and Friedman and Zeckhauser 2012, 2013.

These uncertainties are double-edged. On the one hand, they can impede forming accurate estimates of how long it will take or how much it will cost to achieve a desired goal (let alone to make precise predictions about the locations of phase changes or the shape of probability distributions). As we have seen, the Jasons, Secretary McNamara, the intelligence community, and a wide range of policymakers repeatedly noted this problem throughout the Vietnam War.

Yet at the same time, these dynamics can offer decision makers grounds for optimism, because they imply that just because some strategy has not worked yet, this does not necessarily mean that it will not work soon. If the entry point into the Impact Phase is uncertain (and especially if strategic success should become highly likely thereafter), then decision makers might often find it plausible to believe that they are sitting on the cusp of a major breakthrough.

Several documents from the Vietnam War offer similar perspectives. For instance, a memorandum written by the Joint Chiefs of Staff argues that “Although there is presumably a point at which one more turn of the screw would crack the enemy resistance to negotiations, past experience indicates that we are unlikely to have clear evidence when that point has been reached.”⁴⁶ Similarly, intelligence reports urged decision makers not to give up hope just because

⁴⁶ *PP*, Vol. IV, p. 65. The Joint Chiefs of Staff wrote that even though “the [bombing] program has not yet successfully interdicted infiltration” this did not imply that the North would be able to resist further escalation (*PP*, Vol. IV, p. 42). *PP*, Vol. III, p. 20 quotes the Chairman of the Joint Chiefs of Staff arguing that even though “up until now the battle against the Viet Cong has seemed endless,” this did not imply that success had to be far off; a similar assessment from the CIA is quoted in *PP*, Vol. IV, p. 137. Secretary of Defense Clark Clifford similarly concluded that even though “the steady and accelerated bombing of the North has not brought North Vietnam closer to any real move toward peace,” it was impossible to discount the possibility that “apprehensions about [escalated bombing attacks] that would destroy Hanoi and Haiphong may at some time help move them toward productive negotiations” (*PP*, Vol. IV, pp. 250-252). See also Lodge (Telegram from the Embassy in Vietnam to the Department of State, 26 August 1965: *FRUS*, 1964-68, Vol. III, document 127), who wrote that even “if they have not done so thus far, the Communists must soon acknowledge their inability either to achieve an early victory or to dislodge the growing military strength of the U.S. forces in the south.”

they had not achieved success thus far. A Central Intelligence Agency assessment once argued, for instance, that “although the progress made in the complex counterinsurgency program in the last six months failed to produce a turning point in the war against the Viet Cong (VC), nothing occurred to change our basic belief that the VC will eventually be defeated.”⁴⁷

This idea will recur throughout the following chapters. In the context of cumulative processes, previous investments may be sunk costs, but that does not mean they are irrelevant to future decisions: even when those investments have not allowed decision makers to achieve their intended objectives so far, they may very well have allowed decision makers to advance closer to their desired goals.

This is a key place where the cumulative dynamics of armed conflict depart from the repeated processes of the gambling table. If you are playing roulette and think that you are “due” for a win because of how many times you have lost in a row, then you are making a mistake. But if you have invested substantial resources in advancing a cumulative process, it can be much less clear what to make of the situation. If you are climbing a mountain, then every step takes you a bit closer to the summit, even if you cannot see that point from where you currently stand.

Summary

Altogether, the Vietnam War documents thus suggest two key features of strategic assessment given the cumulative dynamics of armed conflict:

⁴⁷ Central Intelligence Agency, “Assessment of the Progress of the War Against the Viet Cong in South Vietnam During the First Half of 1963,” Central Intelligence Agency Information Report, 2 August 1963: *FRUS*, 1961-63, Vol. III, document 244.

First, decision makers may believe that there is little to no probability of achieving their goals in the short run. This is important, because it implies that early rounds of fighting would *not* automatically provide the kinds of type-separating information that are central to the bargaining model of war. If no U.S. decision makers believed they would see progress in the war's opening stages, for instance, then the fact that the communists did not concede within that window would have come as no surprise. Since all potential opponent "types" would be able to fight for at least that long, the early phases of the war would not have helped to reduce uncertainty about how long it might take or how much it might cost to win the war thereafter.

Second, learning and adaptation will largely be driven by decision makers' prior assumptions. When decision makers evaluate their prospects, this will depend on whether they believe that the Buy-In Phase will be relatively short or relatively long, and how rapidly they believe the odds of success should rise beyond that point. Prior assumptions that look like the "threshold model" in Figure 2.2 will have very different implications for the way that decision makers adapt and behave than if they held prior assumptions that looked more like the "shot-in-the-dark model." Yet this is another aspect of strategic assessment that scholars generally do not take into account, as existing theoretical models generally express uncertainty in terms of a single parameter, namely the chances of defeating an opponent in each round of fighting, which remain constant as fighting unfolds.

Both of these properties will play an important role in Chapter 3, which explains why taking these dynamics into account can generate surprising predictions about the way that military decision makers should form and revise their expectations. The next section, however, fleshes out the deductive foundations of this conceptual framework in order to show how it is not

idiosyncratic to Vietnam, and how the same ideas can be derived, in a general form, from basic theoretical principles.

Section 2.3. Deductive foundations of cumulative dynamics in war

One of the central tenets of scholarship on military strategy is that the basic purpose of fighting is to alter an opponent's cost-benefit analysis. Clausewitz, for instance, wrote that "if the enemy is to be coerced you must put him in a situation that is even more unpleasant than the sacrifice you call on him to make."⁴⁸ Thomas Schelling similarly explained that "coercion requires finding a bargain, arranging for [an opponent] to be better off doing what we want."⁴⁹ This also seems to be a fair characterization of the perspective that Vietnam War planners took: as Walt Rostow and Robert Komer explained, "Our problem is to present [North Vietnam] with a situation where, whatever their will to hold on and sweat us out, they have no realistic option but to accept our terms."⁵⁰

⁴⁸ Clausewitz 1832/1976: 75.

⁴⁹ Schelling 1966: 2, 4, 8.

⁵⁰ Rostow and Komer, "A Strategy for the Next Phase in Vietnam," Draft Paper by the President's Special Assistants, 20 September 1966: *FRUS*, 1964-68, Vol. IV, document 241.) Similarly, John McNaughton wrote that the purpose of the air campaign was to "increase the actual and portended cost of the war to DRV," and thus to make "a convincing demonstration made of the great costs and risks incurred by a country which commits aggression against an ally of ours" ("Plan of Action for South Vietnam," 3 September 1964: *PP*, Vol. III, document 188). A National Security Council Working Group on Vietnam stated that "Increased US pressures on North Vietnam would be effective only if they persuaded Hanoi that the price of maintaining the insurrection in the South would be too great and that it would be preference to reduce its aid to the Viet Cong and direct at least a temporary reduction in Viet Cong activity." "Intelligence Assessment: The Situation in South Vietnam," 26 November 1964: *PP*, Vol. III, document 241. Additional examples of this approach include Lodge, Telegram from the Embassy in Vietnam to the Department of State, 11 November 1965: *FRUS*, 1964-1968, Vol. III, document 199; Rostow, "Some Observations As We Come to the Crunch in Southeast Asia," Memorandum for

Contemporary studies of military strategy typically adopt a similar starting point. Robert Pape, for example, built his prominent analysis of air power on the premise that “The problem in coercion is to persuade the target state that acceding to the coercer’s demands will be better than resisting them.”⁵¹ This assumption also lies behind recent formal theory on the bargaining model of war. As Branislav Slantchev explains, “although fighting can result in complete military victory, its more important function is coercive: to convince the opponent to accept a settlement. This happens after opponents learn enough about their prospects in war to decide that continuation is unprofitable.”⁵²

Slantchev’s distinction between resolving war through “settlement” and through “total military victory” is important, and it drives much of the literature on the bargaining model of war. Recall, for instance, that one of the central assumptions of the “battling for forts” model used by Slantchev and others is that once a combatant has lost enough battles, it will have to concede the entirety of the stakes for which it is fighting;⁵³ similarly, in strategic-level bargaining models, each round of fighting induces a chance that one side will be totally defeated,

the Secretary of State, 23 November 1964: *PP*, Vol. III, document 238; NSC Working Group on Vietnam, “Intelligence Assessment: The Situation in Vietnam,” 24 November 1964: *PP*, Vol. III, document 240; and discussion in *PP*, Vol. III, pp. 70-71, 381-382, and 482, as well as *PP*, Vol. IV, p.292.

⁵¹ Pape 1996: 15.

⁵² Slantchev 2003: 621.

⁵³ For instance, see, Slantchev: “The game continues until an agreement is struck or until one of the players is decisively defeated” (2003: 623). Smith: “Over time, one nation’s advantages could accumulate until it completely overwhelms its foe... Put simply, nations fight battles until one nation decisively defeats the other or until one nation surrenders” (2000: 302). Filson and Werner: “Wars end when one side is defeated militarily or when the attacker alters her negotiating position sufficiently such that the defender is willing to accept the proposed settlement” (2002: 820).

and will have to concede the full stakes to its opponent.⁵⁴ This ultimate threat of losing everything (along with the potential gains of winning everything and the material costs of fighting each round) is what drives decision makers' perceptions of the expected value of continuing to fight.

A potentially confusing aspect of these models is that their bargaining dynamics are thus contingent on the prospect of military outcomes that involve no bargaining at all. Historically, it is difficult to find many examples that correspond to this assumption.⁵⁵

Japan in World War II, for instance, is often held up to be one of the cardinal examples of “unconditional surrender” – but in fact, Japan retained important bargaining leverage up through the end of the war, which it used to extract concessions from the United States (including convincing the Truman administration to relent its earlier insistence that Japan abolish its emperor). The Axis occupation of France in World War II is also often thought to be one of the most decisive military victories in history, and yet the western front did not simply close once France had lost all of its “forts” – the French resistance simply transitioned to partisan warfare while waiting for a conventional counteroffensive, just as Confederates in the southern United States turned to guerilla methods once their conventional forces were defeated in the Civil War,

⁵⁴ For instance, Powell 2004: 345: “The present study sees war as a bargaining process during which the states run a risk of military collapse.” To Wagner, war is fought “to influence expectations about the outcome of a contest in which states try to disarm each other,” and where a disarmed state would thus have to cede all of the stakes in dispute (2000: 473). Fearon: “fighting may result in the government eliminating the rebel group completely, in which case the strategic interaction ends” (2007a: 7).

⁵⁵ Bargaining model scholars typically acknowledge this. Slantchev, for instance, writes that “although it is possible for a war to end with a complete military defeat of one side... most wars do not terminate with the obliteration of the losing side but are settled long before that” (2003: 628).

and just as Spanish forces adopted an insurgent strategy once Napoleon had conquered Iberia in the Peninsular War. Even in the most extreme examples of “military victory,” the weaker side can retain substantial military capabilities and bargaining power. And even if one side were fighting for limited objectives that it could entirely capture by means of brute force alone, the war would not be over until opponents decided that it was no longer worth fighting in order to take those objectives back. Wars simply do not end through the kinds of exogenous, involuntary mechanisms that prominent theoretical models invoke.

One way to address this issue while staying close to previous work is as follows. Assume that combatants go to war having made a set of political demands. During the War in Vietnam, for instance, the United States demanded that Hanoi stop supporting the communist insurgency, and that the insurgency stop challenging the government in Saigon. (Seen from the opposite perspective, the communists demanded that the United States cease supporting the Saigon regime and allow the country to hold free elections.) The objective of fighting is to obtain these concessions by imposing costs on an opponent, convincing the other side that fighting is a costlier option than they originally anticipated, and thus persuading the opponent that it would be in their interest to make the desired concessions. The central challenge of strategic assessment is to determine how costly it will be to do this, and whether decision makers would be better off pushing for more limited (or more expansive) objectives. The war ends when once a combatant agrees to its opponent’s demands (an outcome less extreme than total disarmament and collapse), or both sides agree to a compromise settlement, or neither side determines that it is worth continuing to fight.

This conception of military strategy is not original – there is little in this formulation that does not appear in the writing of scholars like Clausewitz, Schelling, and others cited at the top of this

section. The central point of departure from contemporary theoretical frameworks is simply dropping the notion that there is some exogenously-determined, probabilistic chance of obtaining total military victory outside of the bargaining process. This reformulation has important implications for thinking about strategic assessment.

Figure 2.3, for instance, presents stylized ways of capturing how a combatant might react to a given demand for concessions. The x-axes in these plots represent rounds of fighting, as in standard theoretical frameworks. The y-axes represent the combatant's expected utility for refusing to grant the concessions their opponent is demanding. This calculation will be based on combatants' subjective assessments of a number of factors, including the costs of war in current and future periods, the prospects that the opponent might reduce their demands, and the value each combatant places on the stakes in dispute. The horizontal lines in Figure 2.3 represents the combatant's "reservation value" for fighting – if they believe that the expected utility of resisting concessions is above this point then they will refuse the deal, and if they believe that the expected utility of resisting concessions is below this point then they would naturally grant them.⁵⁶

Both plots in Figure 2.3 shows the results of a military strategy that is working. With each round of fighting, the combatant is becoming progressively pessimistic about their prospects for refusing to grant the demanded concessions. Eventually, the combatant will be in a situation where they would prefer to concede.

⁵⁶ Assume that these calculations capture estimates of expected net present value. Thus if the opponent's expected utility of continuing the war is negative, it would not make sense to say that the opponent might wait a few more rounds of fighting in order to see whether the situation changes – rationally calculated estimates of expected net present value would already account for these prospects.

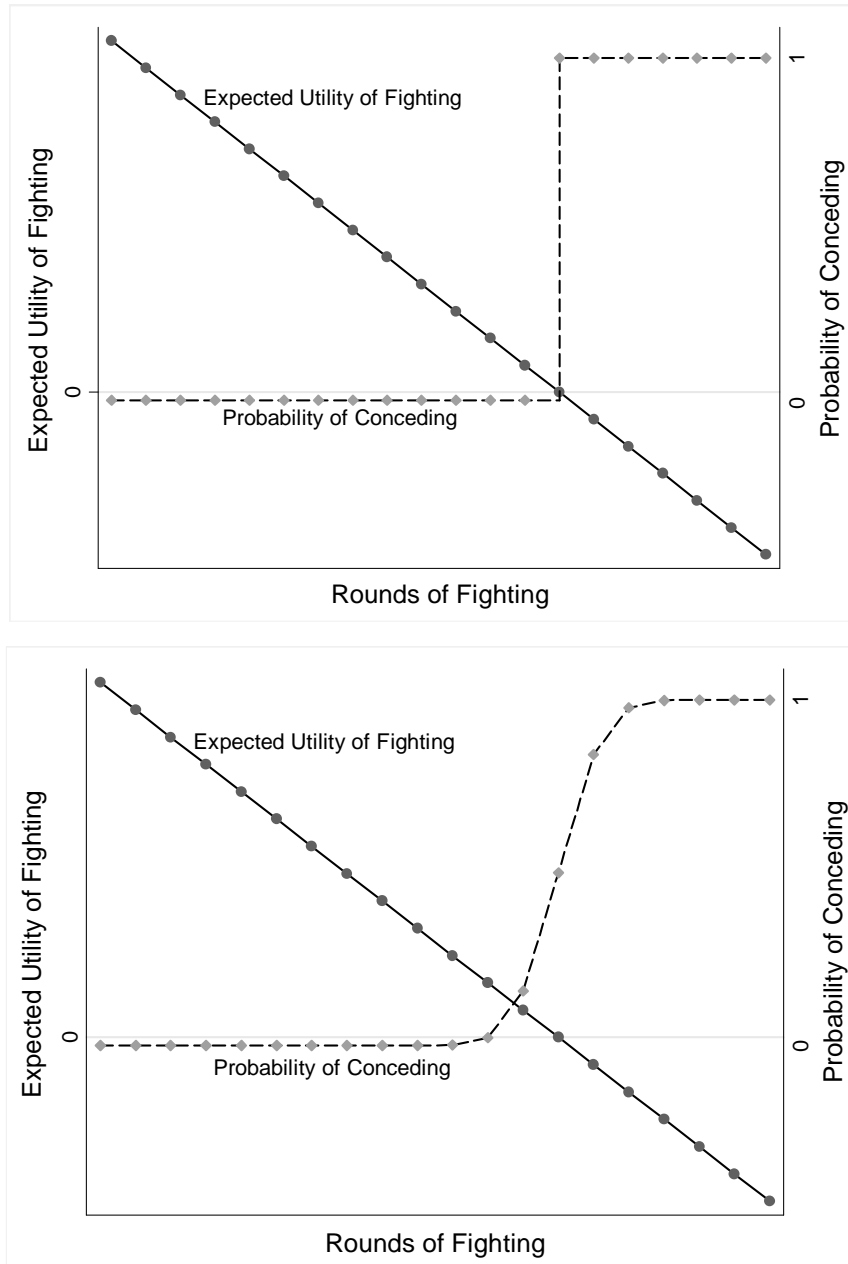


Figure 2.3. Expected Utility and Thresholds for Conceding

Figure 2.3 captures the key challenge of assessing cumulative dynamics in war. Assuming that an opponent will fight so long as the expected utility of doing so is positive, then the opponent will only concede once the expected utility of fighting crosses their reservation value. This implies that there may be no change in observed behavior, even as the opponent continually revises its cost-benefit analysis: even if their expected utility of continuing the war gradually decreases over multiple rounds of fighting, opponents will not have an incentive to concede until the reservation value threshold has been crossed. If the opponent's cost-benefit calculation has a stochastic component, then the dynamic may be more along the lines of the bottom panel.

Though simplistic, this framework helps to motivate some of the key dynamics developed in the previous section. The dashed line in the top panel of Figure 2.3, for instance, shows how combatants would respond to this situation, so long as they operate under the basic decision rule that they will resist granting concessions if they believe they are better off doing so. The result is that the probability of obtaining the intended concession reproduces the “threshold model” we saw earlier. The important point is that even though the opponent is assumed to be continually revising its cost-benefit analysis, its observed behavior may not change for a significant period of time.⁵⁷ As the opponent’s expected utility of fighting grows progressively smaller, it still has an incentive to avoid making concessions until the reservation value has been breached. Before that point, a military strategy might very well be working, and it might very well be making cumulative progress towards its desired goal of convincing opponents to give in, but there is not necessarily a reason to expect that this will result in any observable changes in behavior.

This provides a deductive foundation for the concept of distinguishing between a Buy-In Phase and an Impact Phase, and it reflects the dynamics discussed above in the Joint Chiefs of Staff assessment of the Vietnam War that “Although there is presumably a point at which one more turn of the screw would crack the enemy resistance to negotiations... we are unlikely to have clear evidence when that point has been reached.”

An important component of the discussion in Section 2.2 was that there is often uncertainty about where this kind of phase change will occur. It is thus important to be explicit where that uncertainty comes from. There are a number of possibilities, and they are not mutually exclusive. For example, Clausewitz famously argued that combat outcomes are inherently probabilistic.

⁵⁷ See Fearon 2007a for additional reasons why combatants have an incentive to continue fighting and avoid bargaining even as they learn about their opponent’s capabilities and resolve.

Clausewitz is perhaps best known for describing how the “friction” of war can lead to substantial, irreducible uncertainties, and that how “in the whole range of human activities, war most closely resembles a game of cards.”⁵⁸ (Though Clausewitz presumably did not mean for this analogy to extend to the notion that each battle, like each draw from a shuffled deck, is independent and identical. If that were true, then there would be a fundamental contradiction in Clausewitz’s crucial argument: if rounds of fighting were independent and identically distributed, then the more of them there were, the *less* uncertainty there would be about overall outcomes, as probabilistic draws are more predictable in larger samples.)

Compounding battlefield friction is the notion that decision makers surely operate under the constraints of imperfect information and bounded rationality: it is unreasonable to expect that they will be able to make perfectly accurate predictions about a complex social phenomenon such as armed conflict.⁵⁹ Moreover, contemporary scholars have emphasized how even perfectly rational actors might still be unable to agree on their expectations about the outcome of combat. The idea is that combatants have incentives to misrepresent their capabilities and resolve; this makes it difficult to gather credible information, and it means that combatants often have to

⁵⁸ Clausewitz 1832/1976: 86. A related line of thinking summarized by Beyerchen 1992/93 holds that war is a “nonlinear” phenomenon, in which the same action can produce very different results across different times and contexts. Beyerchen writes that war is analogous to systems studied by chaos theorists, in which “feedback loops, delays, ‘trigger effects,’ and qualitative changes over time produce surprises, often abruptly crossing a threshold into a qualitatively different regime of behavior” (p. 93). On nonlinear dynamics and military thought, see also Mann 1992, Czerwinski 1999, and Moffat 2003.

⁵⁹ Jervis 1976 provides one of the best-known works on the ways in which psychology and misperception can influence foreign policymaking; Heuer 1999 examines the psychology of intelligence analysis in particular.

guess about how long their opponents will be able to resist granting concessions.⁶⁰ Each of these arguments helps to explain why it can often be difficult to estimate how long it will take or how much it will cost to convince an opponent that they would be better off granting some concession.

Factors like friction, bounded rationality, and incentives to misrepresent can also help to explain why anticipated behavior need not always take the form of the threshold model that is represented in the top panel of Figure 2.3. This figure assumes that combatants will be able to revise their cost-benefit analyses in an immediate, accurate, and continuous fashion, but that assumption is almost certainly too strong. It is therefore reasonable to think that the likelihood of a combatant granting concessions involves a probabilistic component, which captures all of these various obstacles to strategic assessment in war. The bottom panel of Figure 2.3 captures this dynamic: as the expected utility of fighting nears the reservation value, then the chances that the opponent concedes rise and then gradually taper off. Needless to say, there are any number of ways in which to model this process – this line of thinking implies that the likelihood of success can take a number of functional forms, and that is consistent with the argument in the previous section.

Summary

We can summarize the deductive argument here in the following way. Decision makers should develop prior assumptions about how long it might take and how much it might cost to

⁶⁰ See Fearon 1995: 395-401 and Meirowitz and Sartori 2008 on private information and incentives to misrepresent.

obtain their political demands. Almost all opponents should be able to resist making concessions for at least the opening stages of armed conflict – this is the Buy-In Phase, where decision makers should not expect that fighting will lead to any changes in observed behavior. Decision makers will presumably face uncertainty about how long this initial phase will last, and what the probability of obtaining concessions will look like thereafter.

These propositions are straightforward and consistent with long-standing theoretical scholarship on coercion, but they deviate from existing models which revolve around the assumption that war is a repeated process which induces an exogenously-determined threat of total military victory absent negotiation. Chapter 3 will go on to show how the alternative framework of war as a cumulative process developed here entails a substantially different set of predictions for how rational decision makers should learn and adapt. Before moving on, however, the chapter will close by describing scope conditions for this theoretical argument.

Section 2.4. Scope conditions

The conceptual framework developed in this chapter applies to the way that decision makers assess uncertainty about how long it might take or how much it might cost to achieve their strategic objectives. This framework applies when two conditions hold: first, decision makers must be observing a cumulative process in which they are attempting to move progressively closer to their intended goals; and second, decision makers must not be able to observe their progress directly (or else there would no uncertainty that they needed to resolve).⁶¹

⁶¹ As stated earlier, this helps to make clear how the theoretical argument in this dissertation does not generally speak to models of coercive bargaining in which combatants have “complete

A general rule of thumb for considering where this framework is most likely to be relevant is to think about situations where people disagree about whether their policies have not been successful as a result of the concept or the execution: whether the problem is that decision makers are doing the wrong thing or that they are just not doing the right thing well *enough*. These kinds of debates often revolve around uncertainty about cumulative dynamics, and this uncertainty is what makes it difficult to understand whether the right response to an unsuccessful policy is to switch strategies or stay the course.

To motivate the conceptual framework, this chapter used the example of U.S. military decision making in Vietnam. In general, counterinsurgency is an area where the challenges of assessing war's cumulative dynamics are especially difficult. Insurgents do not usually attempt to hold ground; it is not possible to benchmark strategic progress in terms of closing in on an opponent's capital, or by how much territory an opponent has left to defend. Since insurgents generally draw their personnel and resources fluidly from the population at large, it is often difficult to know how close they are to running out of manpower or materiel. Strategic assessment in counterinsurgency is largely an abstract challenge: estimating an opponent's willingness to continue fighting despite the mounting costs of doing so. In recent years, U.S. decision makers have struggled greatly in performing this kind of assessment with respect to insurgencies in Iraq and Afghanistan.⁶²

information" – an assumption that is unrealistic, but which helps to draw out important dynamics of armed conflict, such as commitment problems, time horizons, and issue indivisibility.

⁶² Unsurprisingly, the recent wars in Iraq and Afghanistan have thus generated a broad literature on the challenges of assessing strategic progress in counterinsurgency: among others, see Kagan 2006, Patel and Ross 2006, Baker 2006/07, Clancy and Crossett 2007, Westerman 2008, Schroden 2009, Kilcullen 2010: ch. 3, Daddis 2011, 2012, and Kapstein 2012. For official doctrine on the subject, see U.S. Army Field Manual 3-24, paragraphs 5-90 to 5-112.

To what other forms of war should the concepts in this dissertation apply? In order to understand the scope of the analysis, it is useful to employ Thomas Schelling's distinction between achieving strategic objectives through coercion versus brute force. Schelling described this distinction as being between "taking what you want and making someone give it to you." Brute force generally entails physically seizing and holding material assets such as territory or natural resources: it thus revolves around tangible factors which tend to be relatively easy to conceptualize and assess. Coercion, by contrast involves using the threat of imposing costs on an opponent in order to convince them to grant concessions voluntarily. Because the success or failure of coercion is thus contingent on altering an opponent's cost-benefit calculations for behaving in certain ways, it is something that is relatively abstract and difficult to assess with precision. Generally speaking, we can expect that the more military strategy relies on coercion relative to brute force, the more this will exacerbate the uncertainties surrounding strategic assessment that are the subject of this dissertation.⁶³

Strategic bombing, for instance, is similar to counterinsurgency in the way that it often revolves around coercion. We have seen that the principal U.S. goal in bombing North Vietnam was to escalate the pressure on Hanoi gradually, so as to "convey signals which, in combination, should present to DRV leaders a vision of inevitable, ultimate destruction if they do not change their ways." Similarly, we saw Madeleine Albright describe that the goal of bombing Milosevic's power base in 1999 was to convince him that it would be in his interest to halt ethnic cleansing in Kosovo. When the Israeli Air Force bombed Lebanon in 2006, the stated intention

⁶³ Schelling developed the distinction between brute force in his book *Arms and Influence* (1966: ch. 1) Byman and Waxman 2002 provide a more recent review of broader literature on the subject. Biddle and Friedman 2008 use the distinction between brute force and coercion as way to structure assessments of strategic choices.

was to impose costs that would convince the government that it would be in its interest to somehow restrain Hezbollah. Because strategic bombing tends to be so heavily coercive, and because its objectives are so closely tied to influencing an opponent's cost-benefit analysis, this is another form of military strategy that will presumably involve a great deal of the uncertainty that is the subject of this dissertation.⁶⁴

Even military campaigns that rely heavily on brute force, however, typically involve at least some coercive component, and some substantial uncertainty about the kinds of cumulative dynamics discussed here. Take Napoleonic warfare, for instance: conventional combat with massed armies and set-piece battles to sever enemies' lines of communications and control their territory. Commanders in this context could track many tangible indicators of progress. After the Battle of Austerlitz, for instance, it was fairly clear that the Third Coalition had suffered a major blow, just as after the Battle of Waterloo, it was clear that the French power had been severely curtailed. Yet Napoleonic warfare was exactly the subject that Clausewitz wrote about in *On War*, where he characterized armed conflict as a cumulative process and described the difficulty of anticipating the way offensives gradually build towards culminating points. Conventional land warfare may be more analytically tractable than many elements of counterinsurgency or strategic bombing, but it is still a context with substantial scope for uncertainty in evaluating cumulative dynamics.

⁶⁴ On coercion and air power, see Pape 1996 and Byman, Waxman, and Larson 1999. Pape points out that some strategic bombing strategies revolve more around brute force than others – in fact, his central argument is that bombing campaigns that focus on “denial” rather than punishment tend to be the most successful – but these denial strategies constitute a minority of cases, and even they involve at least some coercive element of convincing an opponent that it would not be in their interest to continue resisting.

Naval warfare is an area in which the concepts in this dissertation may not apply as well. Naval battles are often thought to be the epitome of Jominian strategy, with commanders maneuvering into positions where they can strike each others' decisive points and thereby eliminate the enemy fleet with a rapid stroke. There have been no major naval battles between great powers since World War II, so it not entirely clear if this model carries over to the present day; and as several strategists have emphasized, even the most decisive naval victories play only one part among many in determining the outcomes of broader wars. But naval warfare is a domain of armed conflict which, if viewed by itself, revolves so heavily around brute force and measurable capabilities that may lie largely outside the scope of this dissertation.⁶⁵

Finally, counterterrorism is a domain of conflict that could fall within or outside the scope of this analysis depending on what aspects one wishes to examine. There are some elements of counterterrorism that decision makers can observe and assess directly: for instance, when missions are designed to apprehend a certain set of targets, the relevant outcome variables are easy to measure. One could even make the case that some kinds of intelligence leads (such as tips about the locations of enemy personnel or improvised explosive devices) are roughly analogous to a repeated process, where the odds of different pieces of information paying off are relatively independent and similarly distributed.

At the same time, other aspects of counterterrorism directly engage the kinds of cumulative dynamics discussed in this dissertation, and of this there is no better example than the ten-year hunt to capture Osama bin Laden. Since the attacks on September 11, 2001, it was one of the

⁶⁵ Alfred Thayer Mahan is particularly associated with the view that naval strategy should be seen in Jominian terms; Julian Corbett was perhaps Mahan's most prominent critic, arguing that decisive battle at sea was much less feasible than Mahan had suggested.

U.S. government's top national security priorities to find al Qaeda's leader. To say the least, there was substantial uncertainty about how long it might take and how much it might cost to do this. The process of tracking bin Laden was a cumulative one, with intelligence analysts gathering information from a wide range of sources, combining them together, and eventually developing the hypothesis that bin Laden was hiding in Abbottabad, Pakistan. Even after tracking a suspected bin Laden courier to the compound, intelligence agencies spent months trying to pare down remaining ambiguity about whether they had truly found their target, with President Obama insisting that he would not assault the compound unless the intelligence community could pass a reasonable threshold of certainty in making this assessment. From start to finish, the hunt for bin Laden was a story in which decision makers wrestled with uncertainty in assessing cumulative dynamics.⁶⁶

This review serves to frame the analysis in this dissertation by making clear that it does not apply to all forms of armed conflict, nor does it apply to different kinds of military strategy in equal measure. Yet almost any military action involves at least some substantial element of uncertainty about how long it will take or how much it will cost to achieve strategic objectives. This uncertainty may be correlated with how much certain strategies depend on coercion versus brute force, and the extent of this uncertainty may vary across cases; but most of the time, military decision makers will face at least some salient challenges in assessing strategic progress. How decision makers can deal with this uncertainty is the subject of this dissertation, and as Chapter 3 will demonstrate, many common assumptions about this subject are worth assessing themselves.

⁶⁶ The hunt for bin Laden has rapidly become the subject of a large literature, which prominently includes Bergen 2012, Bowden 2012, and Sanger 2012: ch.4.

Chapter Three: Theoretical Argument

FORMING AND REVISING EXPECTATIONS

This chapter demonstrates how simple assumptions about the cumulative dynamics of armed conflict can generate surprising predictions for strategic assessment and military decision making. Existing scholarship (and perhaps most people's basic intuitions) suggests that the longer decision makers go without achieving their objectives, the more pessimistic they should become about their ability to do so and thus the more likely they should be to change course. This chapter challenges those ideas and explains why we should often expect the very opposite.

In showing how and under what conditions rational actors should behave this way, the following sections offer a new perspective on prominent puzzles in existing literature. When scholars ask why military decision makers seem to find it so difficult to realize their strategic mistakes, or why they often stick to unsuccessful strategies for so long, they typically turn to nonrational explanations. For instance, scholars often argue that militaries are built to fight in certain ways, while bureaucratic politics and standard operating procedures can inhibit self-evaluation and change.¹ Cognitive frameworks, confirmation bias, defensive avoidance,

¹ For example, Wildavsky 1972, Lewy 1978, Snyder 1984, Van Evera 1984, 2002, Cohen 1984, Herwig 1987, Krepinevich 1988, Cable 1988, Rosen 1991, Zisk 1993, Avant 1994, Sorley 1999, Gartner 1997, Nagl 2002, Lyall and Wilson 2009, and McAllister 2010/11.

overoptimism, or emotional stress may impede military decision making.² Political constituencies may inherently prefer some methods of fighting over others, and officials may have incentives to “gamble for resurrection” in risky wars so as to boost their electoral prospects.³ These ideas have all been the subjects of valuable research programs, and nothing in this chapter challenges the notion that these mechanisms affect military decision making in important ways. Yet each of these arguments requires making assumptions about why groups might act in ways that violate their overall interests. They either assume that decision makers knowingly adopt inefficient policies in response to some other incentives, or that something prevents decision makers from evaluating their strategies in clear-headed fashion.

This chapter, by contrast, shows that we do not need to invoke nonrational factors in order to explain why some actors find it so difficult to realize and correct their strategic mistakes. We can explain this behavior simply by examining the cumulative dynamics of armed conflict and understanding how they differ from other phenomena. This argument proceeds in four sections.

Section 3.1 begins by demonstrating why military decision makers might actually become more optimistic about achieving their objectives even as they continually fail to do so. This argument is developed using formal theory, which explains precisely why it is that relatively

² For example, Leites 1953, Wohlstetter 1964, George 1969, White 1970, May 1973, Jervis 1976, Lebow 1981, Staw 1981, Janis 1982, Larson 1985, Vertzberger 1990, Khong 1992, Levy 1996, Mercer 1996, Reiter 1996, Heuer 1999, Wrangham 1999, Johnson 2004, McDermott 2004, Rosen 2005, Kahneman and Renshon 2007, Butler 2007, Renshon 2008, Gayer et al. 2009, Woods and Stout 2010, Duelfer 2011, Johnson and Fowler 2011, Johnson and Tierney 2011, Lopez et al. 2011, Mercer 2013.

³ For example, Weigley 1973, Mack 1975, Downs and Rocke 1997, Kier 1997, Reiter and Meek 1999, Huth and Allee 2002, Goemans 2002, Merom 2003, Freedman 2004, Gray 2006, Berinsky 2007, Stanley 2009, Craig and Logevall 2009, Goemans and Fey 2009, Caverley 2009/10; Debs and Goemans 2010, Trubowitz 2011.

intuitive premises can lead to these potentially surprising predictions. Section 3.1 builds this logic in stages, starting with a simple model in which decision makers are trying to distinguish between two types of opponents, and then generalizing into broader and more complex situations. This section constitutes the dissertation's core theoretical argument.

Section 3.2 fleshes out this argument by going into more detail about where decision makers' prior assumptions come from, and explaining why these assumptions play a crucial role in understanding the way that these decision makers should learn and adapt. This discussion is important not just for clarifying the theory's underpinnings but also for drawing further distinctions with respect to existing literature, which generally does not focus much attention on the importance of prior assumptions in the manner shown here.

Section 3.3 then discusses what the theoretical framework implies for models of strategic interaction. A wide range of theoretical scholarship on armed conflict uses game theory to examine strategic interaction explicitly. While these bargaining dynamics are not the principal focus of this dissertation, Section 3.3 provides at least a brief discussion of what rethinking strategic assessment in light of cumulative dynamics implies for broader, game-theoretic scholarship. Section 3.4 then concludes the chapter by summarizing its intended theoretical contributions.

Section 3.1. Cumulative dynamics and the costs of fighting

This section explains why taking the cumulative dynamics of war into account leads to predictions that sharply deviate from existing scholarship. To draw this out in a simple case, Figure 3.1 begins by comparing the way that rational decision makers would adapt their

expectations in a situation where opponents can be either of two types, “strong” or “weak. Figure 3.1a represents the way that existing models would capture this situation, under the standard assumption that war is a repeated process. As Chapter 2 discussed, contemporary literature on the bargaining model of war generally assumes that an opponent’s “type” determines the odds that they will be defeated in each round of fighting. Those odds remain the same from round to round. This means that each round of fighting provides “type separating information”: each time a round of fighting concludes without defeating an opponent, a rational decision maker should raise their prior probability that the opponent they are facing is a strong type. Using a straightforward application of Bayes’ rule, Figure 3.1a shows how this leads standard models to predict a gradual, monotonic learning process: as rounds of fighting mount and decision makers continually fail to achieve their goals, they should continually become more pessimistic about their ability to do so.

Figure 3.1b shows how matters differ if we assume that war is a *cumulative* process. As Chapter 2 discussed, this viewpoint suggests a different way of conceptualizing an opponent’s “type.” Rather than assuming that an opponent’s capabilities and resolve dictate the chances that they will be defeated in any round of fighting (and that these odds stay the same from round to round), Chapter 2 argued that it might be more appropriate to think of a combatant’s “type” as the amount of fighting they can sustain before giving in to an opponent’s demands. In this formulation, stronger opponents are still able to hold out for longer periods, but even “weak types” should be able to conduct some amount of fighting without being defeated. This is also a stylized way to think about the issue – but it is no more stylized than existing models, and Chapter 2 offered both inductive and deductive reasons why this formulation may be more viable than the underpinnings of standard frameworks.

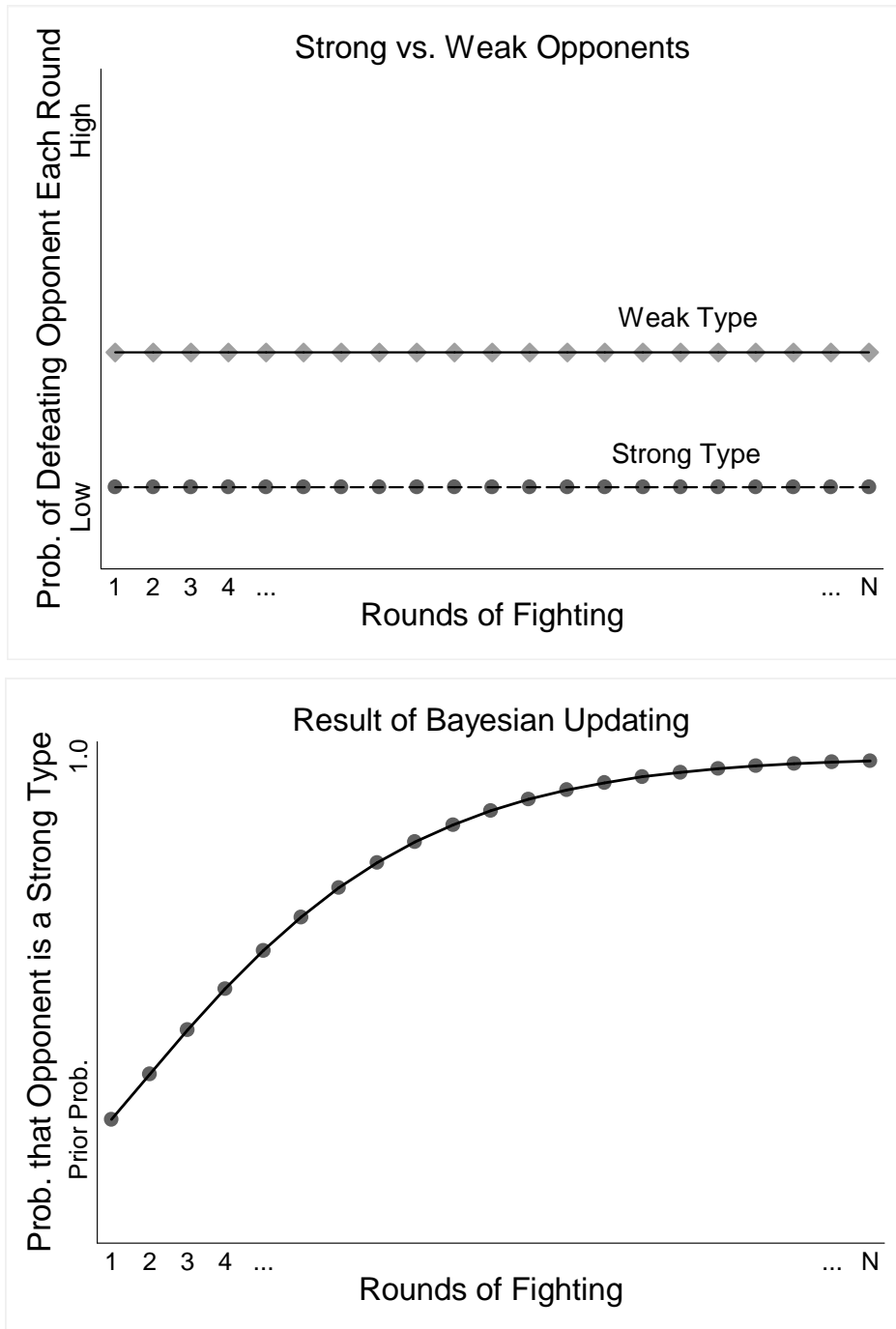


Figure 3.1a. How Decision Makers Learn if War is a Repeated Process

When war is a repeated process, such that every round of fighting generates an independent and identical probability of success, then every round is also type-separating: each time an opponent resists defeat, a rational decision maker should become more convinced that they are facing a “strong type.”

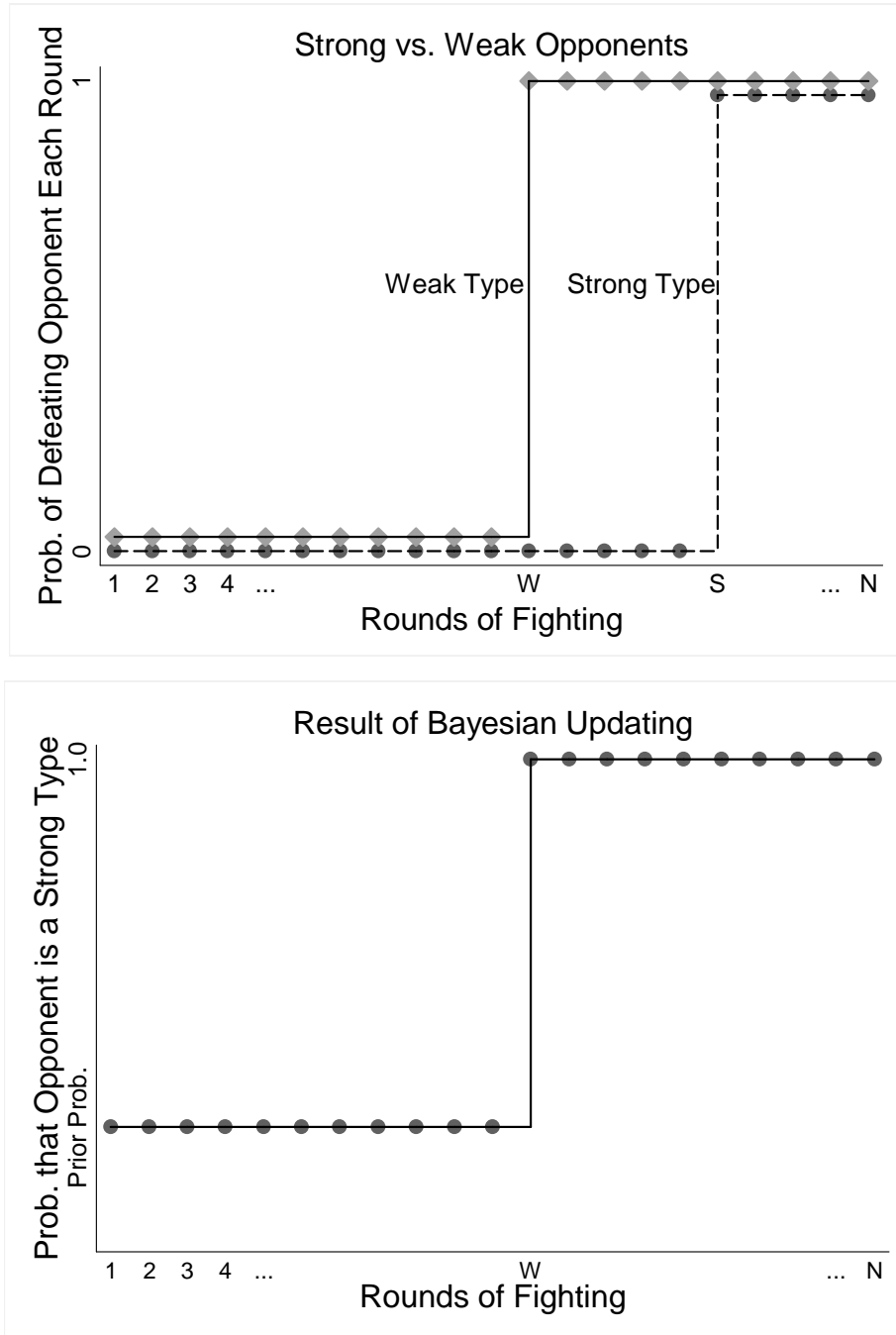


Figure 3.1b. How Decision Makers Learn if War is a Cumulative Process

When war is a cumulative process, such that multiple types of opponents should be able to withstand initial rounds of fighting, then those rounds do not necessarily provide type-separating information. In this simple, “two-type” model, it is not possible to distinguish between strong and weak opponents until the latter is forced to concede.

Figure 3.1b also shows how this alternative framework produces a learning process that is very different than the one proposed in the standard literature. Because both strong and weak types can withstand some initial rounds of fighting, those rounds do not provide type-separating information. The updating process in this model is therefore discontinuous – there is a point at which decision makers will be able to rule out the notion that they are facing a weak type, but otherwise they should not have the ability to revise their prior assumptions. And once decision makers reach this point where they can be sure that they are facing a strong opponent, it may make sense for them to continue fighting the remaining rounds it will take to achieve their objectives, even if they might *not* have begun the war if they had known that they were facing a strong opponent *ex ante*.

A model with only two types of opponents is overly simplistic of course, but this example makes clear how accounting for cumulative dynamics provides a clear point of departure from existing theory on learning and adaptation in armed conflict. The remainder of this section will expand this analysis in order to show how decision makers might respond to a much more general set of circumstances in which they must account for a wider range of possibilities of how capable and resilient their opponents might be.

The linear case

Consider, for instance, if decision makers enter conflict with a “flat prior” about how opponent types are distributed: this is to say they believe it is equally likely that their opponents will be able to resist defeat for one round of fighting, for two rounds of fighting, or any other number out to some theoretical maximum denoted x_{max} . (It is important to define a theoretical

maximum here, or else the expectation of the number of rounds of fighting an opponent could sustain would be infinite. As shown below, it is not necessary to define this maximum for most other kinds of prior distributions.) In this case, we can define the Expected Total Cost (ETC) of fighting as the number of rounds that the average opponent could withstand: we can express this quantity as $ETC = \frac{x_{max}}{2}$. Figure 3.2 demonstrates that this is simply the midpoint of the range of possible opponent types.

Now consider what happens after combatants have already conducted a certain number of rounds of fighting, which we denote as x' . After having fought this number of rounds, decision makers can update their prior expectations. Specifically, they can rule out the notion that they are facing an opponent who is weak enough to have been defeated already. Conditional on having already fought for x' rounds, a rational decision maker could thus re-estimate Expected Total Cost as $ETC(x') = \frac{x_{max} + x'}{2}$. Expressed visually in Figure 3.2, this corresponds to the midpoint of the range of remaining opponent types.

This is not the quantity of interest, however. Keep in mind that Expected Total Cost represents the *total* amount of fighting that the opponent can be expected to withstand. By this point, however, combatants have conducted a portion of that fighting already, and those costs are now sunk. The quantity of interest here is thus the Expected *Remaining* Cost of fighting, which we can express as $ETC(x') - x' = \frac{x_{max} - x'}{2}$.

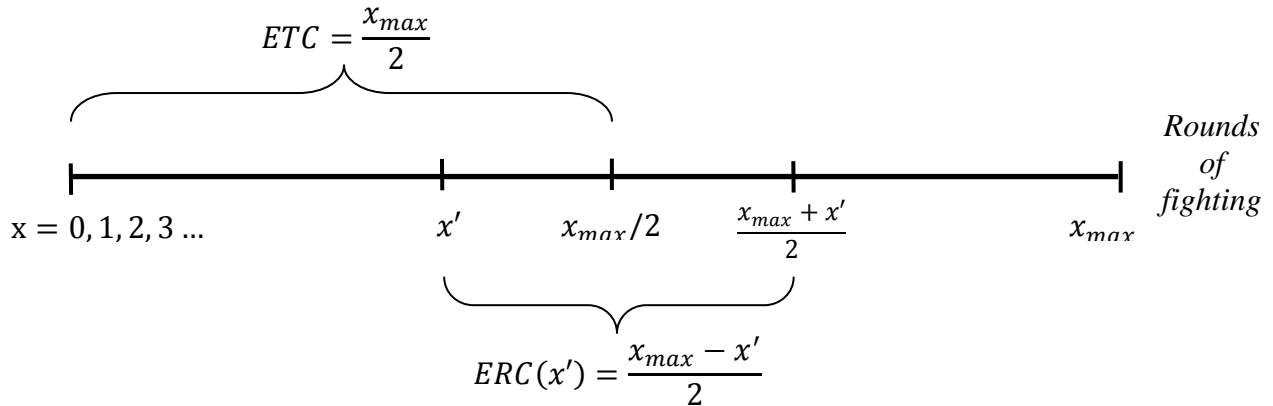


Figure 3.2. Expected Total and Remaining Cost for the Linear Case

Figure 3.2 represents the way that decision makers should form and revise their expectations about the expected total and remaining costs of defeating their opponents, in cases where these decision makers begin with a “flat prior” where they believe it is equally likely that their opponents can hold out for one round of fighting, two rounds of fighting, or any other number of rounds out to some theoretical maximum.

The important thing to note is that Expected Remaining Cost is strictly decreasing. This means that the longer combatants fight without achieving their objectives, *the more optimistic*⁴ they should become about their prospects moving forward. The uniform distribution is again a highly stylized way to represent prior expectations, but it is also theoretically significant, as it reflects a state of total uncertainty about an opponent’s potential type – it thus serves as the baseline case for judging the way that rational decision makers should behave absent any additional assumptions about which types of opponents are more common than others. And in

⁴ Following Slantchev 2003, the words “optimism” and “pessimism” refer simply to a decision maker’s views about whether the expected marginal costs of achieving their objectives are higher or lower than what they previously believed.

this baseline case, the prediction for how rational decision makers should update their views is exactly the opposite of the conventional wisdom.

The general case

From here, we can extend the framework in order to cover a much broader range of assumptions about the way opponent types might be distributed. Generally speaking, we can say that a decision maker's estimated probability that an opponent will be defeated in the x^{th} round of fighting can be represented by the function $p(x)$.⁵ The decision maker's overall expectation of how many rounds of fighting it will take to achieve their strategic objectives will then be $ETC = \int_0^{\infty} p(t) \cdot t dt$. After fighting for x' rounds, the new Expected Total Cost will be

$ETC(x') = \frac{\int_{x'}^{\infty} p(t) \cdot t dt}{\int_{x'}^{\infty} p(t) dt}$.⁶ Backing out sunk costs, Expected Remaining Cost (ERC) is

$$ERC(x') = \frac{\int_{x'}^{\infty} p(t) \cdot t dt}{\int_{x'}^{\infty} p(t) dt} - x'$$

This expression is useful, because we can take its first derivative with respect to x' , and thereby determine the range of cases where a rational decision maker would become more

⁵ In this way, the function $p(x)$ represents the way that a decision maker has captured all the different kinds of uncertainties discussed in Chapter 2 in order to form different degrees of belief about how likely it is that an opponent can hold out for each possible round of fighting.

⁶ The numerator in this expression “truncates” the prior distribution, ruling out the possibility that the decision maker is facing an opponent that is weak enough to have been defeated already. The denominator then reweights the remaining probabilities such that they sum to 1.

optimistic or more pessimistic while attempting to achieve a desired goal. It turns out that the range of cases where decision makers become more optimistic is fairly broad, spanning several commonly-used distributions.

For example, the normal distribution is probably the most common tool that scholars use to define prior expectations. Yet when decision makers have normally-distributed priors about their opponent's potential type, then Expected Remaining Cost will always decline. Figure 3.3 shows this graphically. The prior assumption about the distribution of potential opponent types is represented by the shaded density function. The x-axis represents rounds of fighting; as those rounds of fighting proceed, the solid and dashed lines in Figure 3.3 show the way that a rational decision maker will form new estimates of Expected Total Cost and Expected Remaining Cost. The key takeaway from this figure is that Expected Remaining Cost continuously falls. This will be the case for all normal distributions, regardless of their parameters.⁷

It is worth considering several other ways to model the distribution of opponents' potential types. Figure 3.4 presents the logistic, Laplace, gamma, and exponential distributions. These distributions have different shapes and they are used for different purposes, yet each produces a similar result: Expected Remaining Cost never increases, giving rational decision makers no reason to become more pessimistic about their policies based on how much they have already invested in trying to achieve their intended goals.

⁷ Expected Total Cost under normally distributed expectations can be expressed as the first moment of a truncated normal, which is the sum of a constant μ and the Inverse Mills Ratio. Thus, $ERC(x')$ can be expressed as $\mu + \lambda(x') - x'$, where $\lambda(x')$ is the Inverse Mills Ratio evaluated at x' . A known property of the Inverse Mills Ratio is that $\lambda'(x')$ is always between 0 and 1. Thus $dERC/dx'$ is always between -1 and 0.

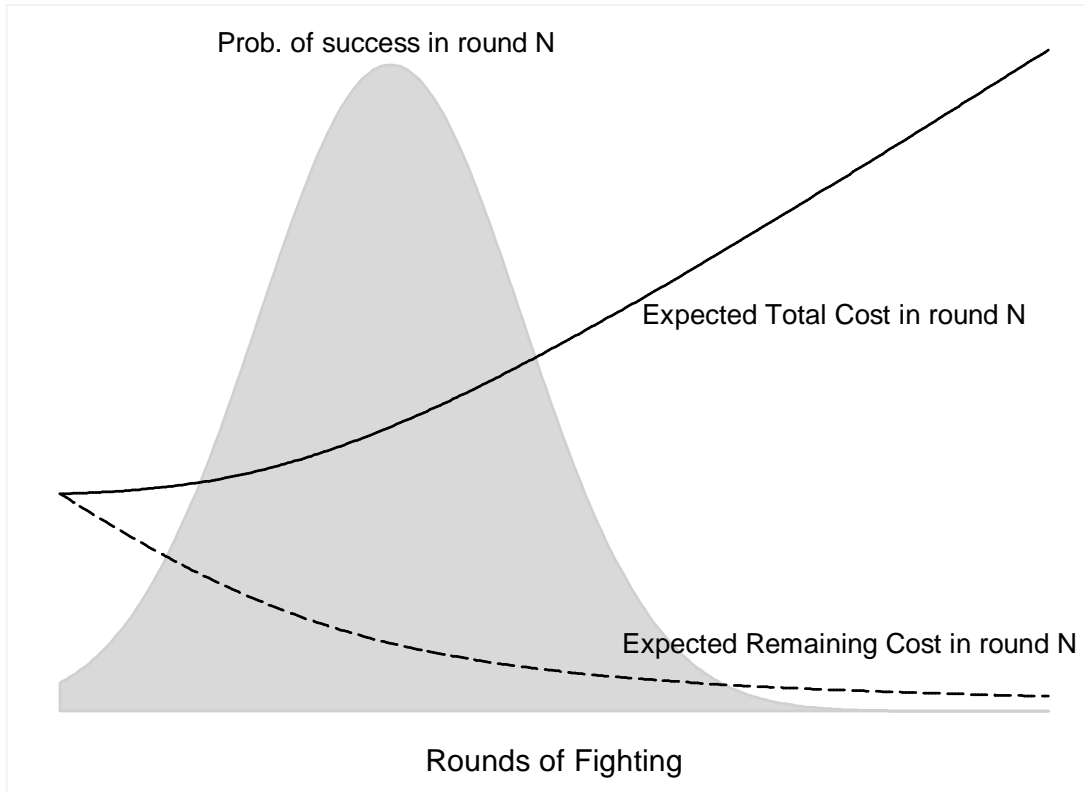


Figure 3.3. Expected Total/Remaining Cost with a Normally Distributed Prior

The gray area in Figure 3.3 represents a decision maker’s initial expectations about the likelihood of facing an opponent who will be defeated in each round of fighting. In this figure, those expectations are normally distributed. Conditional on reaching a certain round, the solid and dashed lines then represent the way that decision makers should revise their expectations of the Total and Remaining Costs of fighting. Note that Expected Remaining Cost continuously declines.

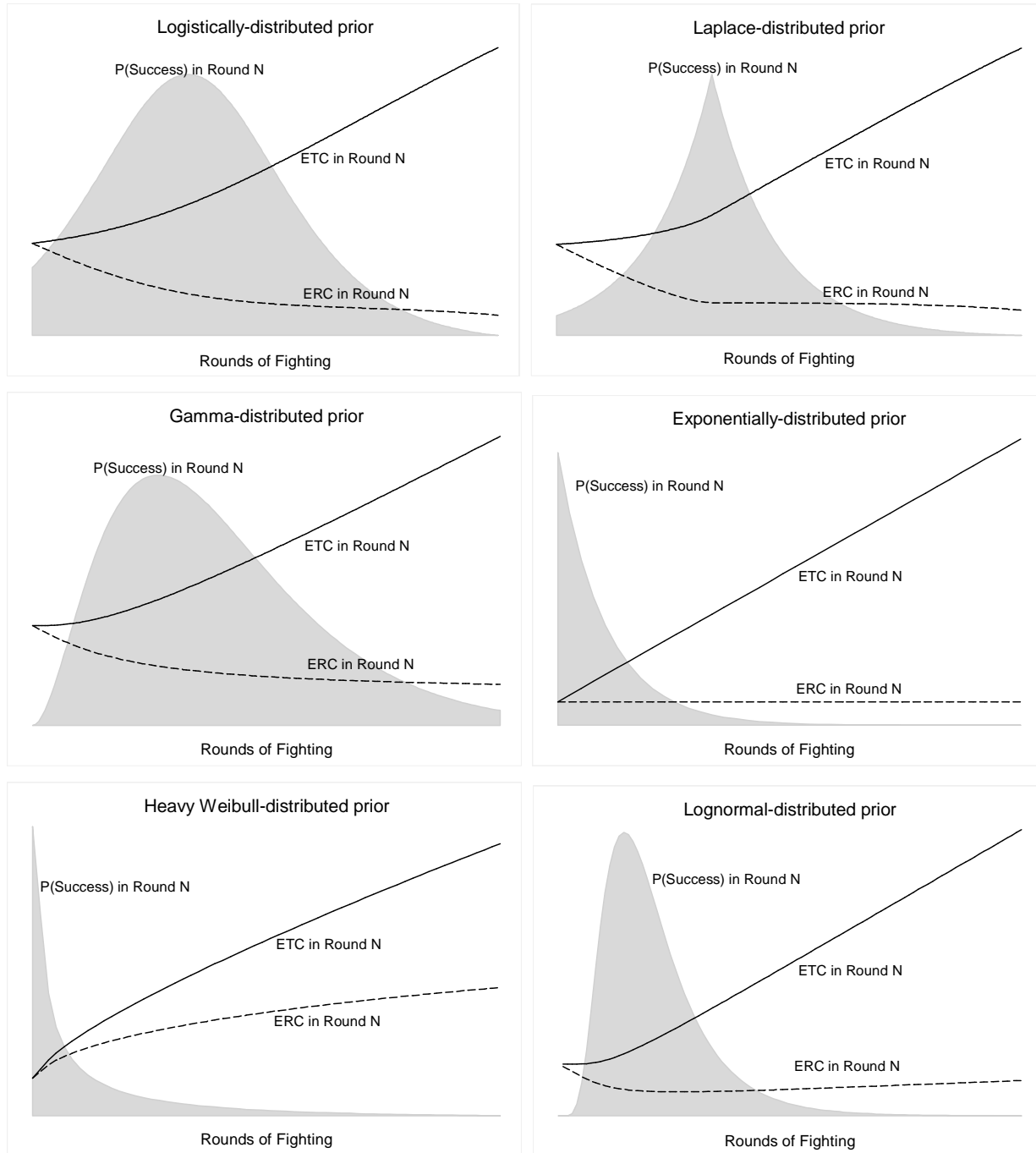


Figure 3.4. Expected Total/Remaining Cost across Additional Distributions of Types

Figure 3.4 shows how a decision maker’s prior expectations about the distribution of opponent types (shown here by the gray areas representing the chances that an opponent will be defeated in each round of fighting) affect the way that the decision maker would revise her expectations about Total and Remaining Costs as fighting proceeds. Expected Remaining Cost can only increase if the decision maker’s prior expectations fall into the class of “heavy-tailed distributions.”

The exponential distribution is an important case: as shown in Figure 3.4, Expected Remaining Cost would be the same in every round of fighting if decision makers believed opponents' potential types were distributed exponentially.⁸ This gives a more general result, which is that Expected Remaining Cost can only rise when a decision maker expects that the distribution of opponent types has “fatter tails” than the exponential. In probability theory, this is the definition of a “heavy-tailed distribution.” Some examples include the Heavy Weibull distribution and the lognormal distribution – these are shown in at the bottom of Figure 3.4, and in these graphs, Expected Remaining Cost does indeed increase at certain points.

There are several caveats to consider here, however. One is that many heavy-tailed distributions (such as the Cauchy or the Lévy) do not have finite expectations, and so they cannot be used to estimate the potential costs of fighting. A second issue (as shown at the bottom of Figure 3.4) is that even with a heavy-tailed distribution like the lognormal, it may take a significant period of time before Expected Remaining Cost begins to rise, and even longer before it rises *enough* to exceed its original value. This means that even with heavy-tailed prior assumptions about the distribution of opponents' potential types, it may take a significant amount of fighting before rational combatants become more pessimistic about the chances of achieving their goals relative to their expectations when the war started. Until this happens, there is no reason to expect that they would wish to alter or abandon their chosen strategies.

⁸ The exponential's probability density function is $f(x) = \lambda e^{-\lambda x}$ and its cumulative distribution function is $F(x) = 1 - e^{-\lambda x}$. This means that $ERC(x') = \frac{\int_{x'}^{\infty} f(t)t \cdot dt}{1 - F(x')} - x' = \frac{\frac{1}{\lambda}[-e^{-\lambda x'}(-\lambda x' - 1)]}{e^{-\lambda x'}} - x' = \frac{1}{\lambda}[1 + \lambda x'] - x' = \frac{1}{\lambda}$. Since this is a constant, it follows that $dERC(x')/dx' = 0$.

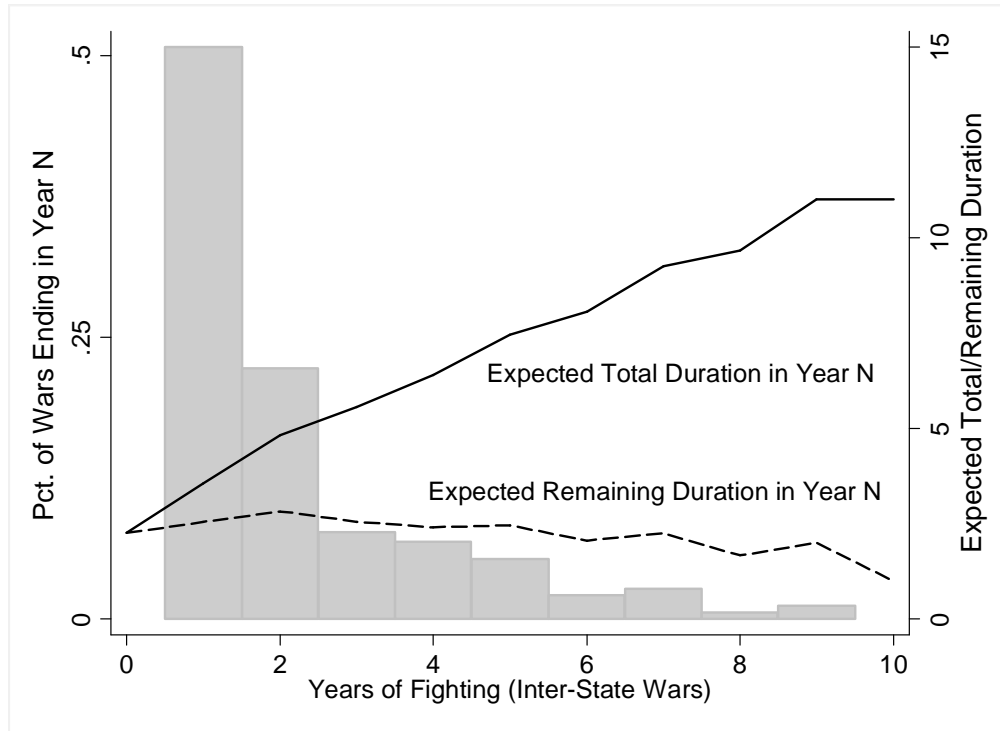
Applications to actual data

Discussing theoretical probability distributions helps to establish basic conceptual properties of learning and adapting to cumulative processes, but how does this framework relate to actual data? The following chapters will examine this issue in more detail, but for the moment, Figure 3.5 demonstrates how the expected total duration and the expected remaining duration of wars evolve over time given the empirical distribution of four different kinds of armed conflict.⁹

In each of the four panels of Figure 3.5, shaded bars represent the percentage of wars that terminate in a given year of fighting. The solid lines then capture how many years a randomly-selected war in each category would be expected to last, conditional on already being active for a certain period of time. The dashed lines then capture the conditional expectation of how much time *remains* in a randomly-chosen conflict. Since we are dealing with actual data in these figures, the patterns are neither smooth nor monotonic. But consistent with the argument laid out in this section – and contrary to prominent existing theoretical frameworks – these graphs indicate that the expected remaining duration of armed conflict does *not* strictly increase as these conflicts unfold. For inter-state wars, in fact, expected remaining duration in fact is almost strictly declining. For the other forms of violence the pattern is decidedly mixed, but it certainly does not support the commonly-held belief that the longer a war has lasted already, the longer we should expect it to continue moving forward.

⁹ Data are from the Correlates of War project (Sarkees and Wayman 2012). All wars in the data set must cause a minimum of 1,000 total battle deaths to be considered for inclusion, a point that will be discussed in more detail in Chapter 4. “Extra-state wars” are conflicts that take place between a recognized state and a nonstate entity outside its borders. “Non-state wars” are conflicts that take place between groups none of whom are recognized as being state entities. All four sets of data involve cases between 1816-2007. These data comprise 337 inter-state wars, 250 intra-state wars, 185 extra-state wars, and 186 non-state wars.

Inter-State Wars



Intra-State Wars

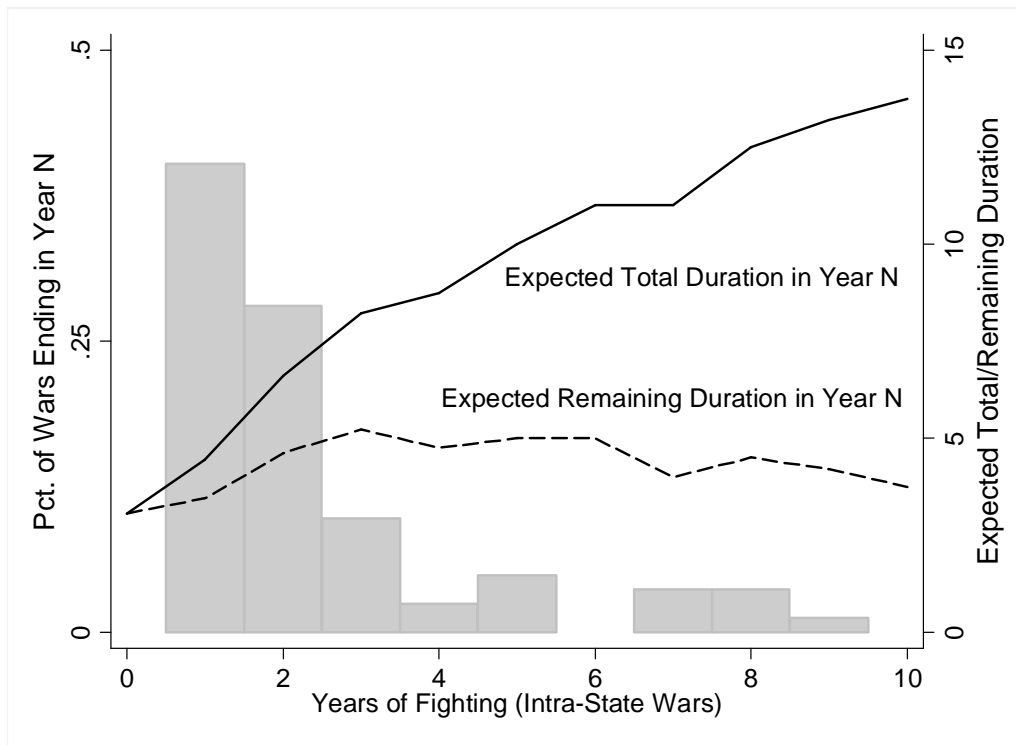
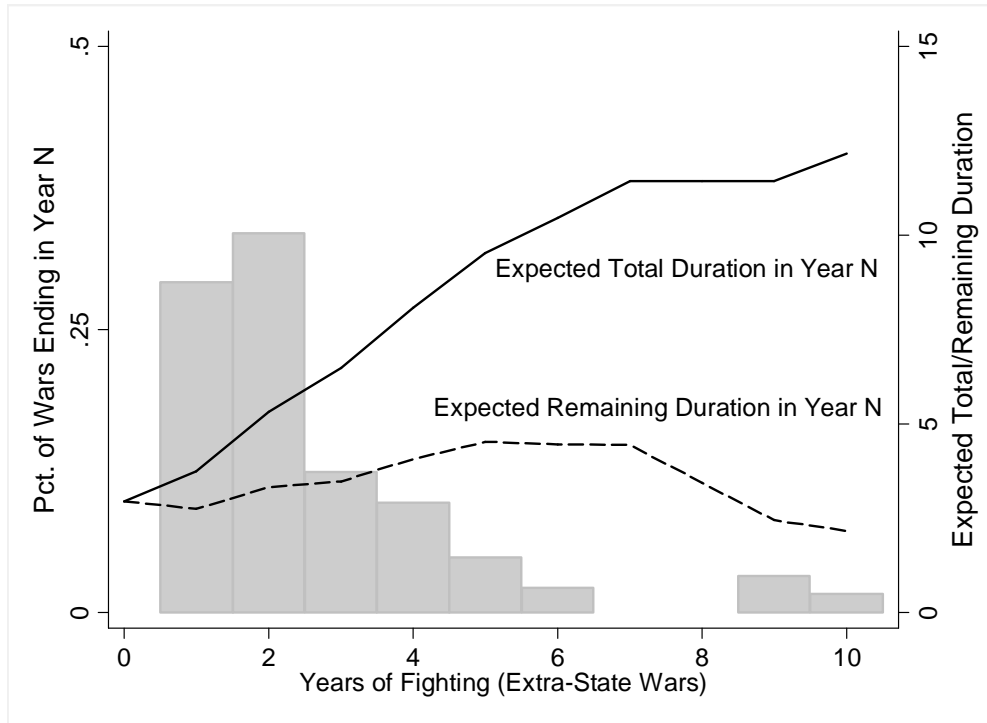


Figure 3.5. Expected Total/Remaining Duration Across Empirical Distributions of Conflict

Extra-State Wars



Non-State Wars

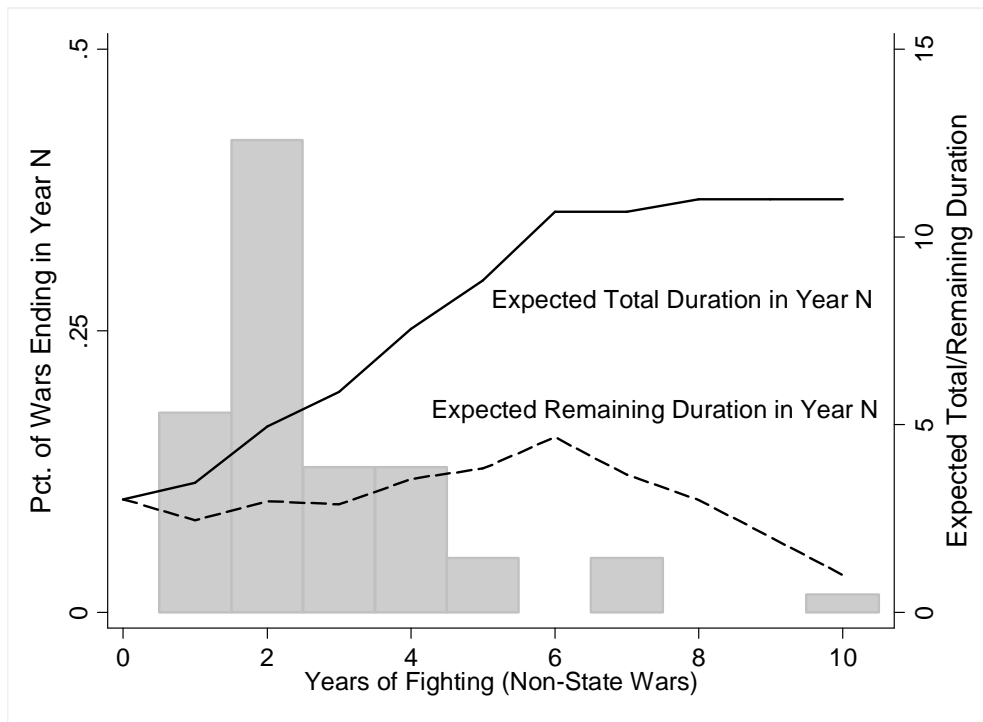


Figure 3.5 (continued). Expected Total/Remaining Duration Across Empirical Distributions of Conflict

Summary

This section has advanced four theoretical arguments: 1) that rational decision makers should not necessarily become more pessimistic about their policies, even as they continually fail to achieve intended goals; 2) that the way decision makers revise their expectations when observing cumulative processes is largely driven by what their prior assumptions were to begin with; 3) that it is possible to derive the specific conditions under which decision makers should become more pessimistic or more optimistic as they proceed; and 4) that the latter condition contains many commonly-used theoretical distributions *and* the empirical distributions for inter-, intra-, extra-, and non-state wars.

These arguments contradict a wide range of scholarship on strategic assessment in war by suggesting the prospect of rational inertia in military decision making. When decision makers continually express optimism and continue to “stay the course” despite failing to demonstrate success, this is generally assumed to constitute nonrational behavior. As the introduction to this chapter explained, political scientists have developed wide-ranging explanations for how organizational constraints, domestic politics, psychology, and other factors can prevent decision makers from adapting to unexpected situations. Yet this section has demonstrated that we do not need to invoke these arguments in order to explain why military decision makers often fail to realize and correct their strategic mistakes. That kind of behavior is also potentially consistent with rational beliefs and decisions, so long as it is understood how cumulative dynamics affect strategic assessment.

Moreover, if rational learning depends on decision makers’ prior assumptions, then this is another important point of departure between the theoretical framework developed in this

dissertation and models offered by the existing literature. Existing models tend not to afford much weight to prior assumptions – by assuming that war is a repeated process in which every round of fighting is independent and identically distributed, existing scholarship essentially assumes that decision makers should be able to revise their expectations based on trial-and-error, which will eventually cause rational actors to converge on common beliefs. When viewing war as a cumulative process, this is not the case: just because a policy has not worked yet this does not necessarily mean that it will not work soon, and decision makers' initial assumptions about potential opponent types exert a fundamental impact on the way they will perceive their prospects, even after many rounds of fighting have transpired. The next section will explain this issue in more detail, saying more about where prior assumptions come from, and providing a more in-depth comparison of how decision makers should update their priors depending on whether armed conflict is considered to be a cumulative or repeated process.

Section 3.2. The importance of prior assumptions

Where do prior assumptions come from?

In practice, decision makers do (and should) form prior assumptions based off a combination of inductive and deductive reasoning. The simplest form of inductive reasoning is the use of analogies. Heading into World War I, for instance, Germany's chief of general staff Helmuth von Moltke based his plans for the invasion of France on an attempt to achieve the kind of swift and decisive victory that Carthage had achieved over Rome at Cannae.¹⁰ Similarly, Chapter 5

¹⁰ In planning for the war, Moltke ordered his staff to produce a volume, *Cannae Studies*, which would demonstrate how the principle of envelopment could lead to rapid and decisive victory over France. (Rothenberg 1986.) The irony, of course, is that while Carthage won the Battle of

will demonstrate how U.S. military planners based their expectations for the occupation of Iraq around analogies to Germany, Japan, and South Korea, all cases of “post-war reconstruction” where occupying forces faced relatively little organized military resistance. In general, analogical reasoning provides a rough heuristic for grounding strategic assessments in empirical evidence.¹¹

Of course, the examples of German planning for World War I and U.S. planning for the occupation of Iraq come to mind largely because the analogies they invoked were so flawed. An analogy is ultimately just a data point – it is one possible comparison that might be relevant for structuring decision makers’ expectations, but there are bound to be other cases to choose from as well, and the lessons they offer are bound to vary.

For instance, when it comes to determining how long insurgencies tend to last, it is possible to derive one’s expectations from a broad evidence base. Over the last two centuries there about 300 insurgencies on record.¹² Figure 3.6 demonstrates that about 13 percent of these insurgencies collapsed within their first year of fighting, about 15 percent collapsed in their second year of fighting, and then the probability of defeating an insurgency tends to decline each year

Cannae, it did *not* win the Second Punic War, which dragged on for another fifteen years, and is now known just as well for the Romans’ “Fabian strategy” of exhausting an opponent through protracted attrition.

¹¹ On analogical reasoning in national security affairs more broadly, see Hoffmann 1968, May 1973, Jervis 1976, Neustadt 1986, Khong 1992, and Reiter 1996.

¹² Data presented below are from Lyall and Wilson 2009, who track 286 insurgencies from 1808 to 2005.

thereafter.¹³ If we instead conceptualize “rounds of fighting” with respect to the amount of attrition that insurgents can sustain (which is consistent with the way that some U.S. decision makers approached the Vietnam War) then we see that roughly a quarter of insurgencies collapse before sustaining five thousand casualties, about ten percent collapse between sustaining five and ten thousand casualties, and again the distribution continually decreases as it extends to the right.¹⁴ As with the data presented in Figure 3.5, these constitute purely inductive prior assumptions about the potential resilience of an insurgent opponent.

Basing priors on such a broad range of data is surely too broad-brush of an approach, however, since the information in Figure 3.6 represents nearly three hundred cases spanning two centuries, involving many contexts that may not be especially relevant to thinking about contemporary cases. There are many ways to refine this information in order to form a more relevant assumption about the distribution of potential opponent types in any given situation. Bennett and Stam, for instance, developed a statistical model in order to make explicit

¹³ These data represent the proportion of insurgencies who are defeated, an outcome that Lyall and Wilson define as occurring “when the insurgency is militarily defeated and its organization destroyed or the war ends without any political concessions granted to insurgent forces.”

¹⁴ Data on insurgent attrition were coded by the author: they are rough measurements of insurgents killed in battle, as well as those captured and executed, but they do not include civilians killed by collateral damage during the war, nor do they include estimated deaths due to displacement or disease. (In this respect, the definition of insurgent attrition employed here differs from other data sets that aim to capture “war-related deaths” more broadly.) These data were coded through an analysis of more than 500 secondary sources. Some of these sources were prominent compilations of historical statistics: Clodfelter’s *War and Armed Conflicts* (2008), for instance, chronicles casualty statistics specifically; some other examples of anthologies used for this project include Condit et al., *Challenge and Response in Internal Conflict* (1967-68), Asprey, *War in the Shadows* (1975), Dupuy and Dupuy, *Harper Encyclopedia for Military History* (1993), Clayton, *Frontiersmen: Warfare in Africa since 1950* (1999), Beckett, *Modern Insurgencies and Counter-Insurgencies* (2001), Phillips and Axelrod, *Encyclopedia of Wars* (2005), and Arnold, *Historical Dictionary of Civil Wars in Africa* (2008). The large majority of sources for these data, however, are histories of individual wars. References for each individual data point are catalogued within the data set itself.

predictions about how long the occupation of Iraq might last.¹⁵ More informally, decision makers often limit their analysis to cases they believe to be most relevant for whatever reason. The task of defining the relevant case universe for structuring expectations is a crucial one, and it will be the main subject of Chapter 5.

Finally, a large part of military planning is essentially deductive, working through the specific problems decision makers believe they are confronting through a close analysis of desired ends, available means, and opponents' capabilities. There are a wide range of simulation technologies and wargame formats that aim to sort through these factors in order to draw inferences about how long it might take and how much it might cost to achieve strategic objectives based on case-specific reasoning. In a different context, Clausewitz discussed the importance of a commander's "genius": the intuition that would allow commanders to "rise above the rules" and the strictures of past experience in order to perform strategic assessment and guide their forces successfully.¹⁶

Decision makers thus form their prior expectations based on a wide range of inputs, both inductive and deductive, broad-based and ad hoc. These forms of reasoning are often left implicit, and they form the basis of subjective probabilities about how long it might take and how much it might cost for decision makers to achieve their intended goals. The purpose of discussing those prior assumptions formally is not to provide a descriptive picture of how decision makers perform strategic assessment, but to explain precisely why it is that those prior assumptions play such an important role in structuring the process of learning and adaptation in war, and how existing scholarship often neglects key dynamics on this score.

¹⁵ Bennett and Stam 2006.

¹⁶ On "Clausewitz, Genius, and the Rules," see Sumida 2001 and Rogers 2002, among others.

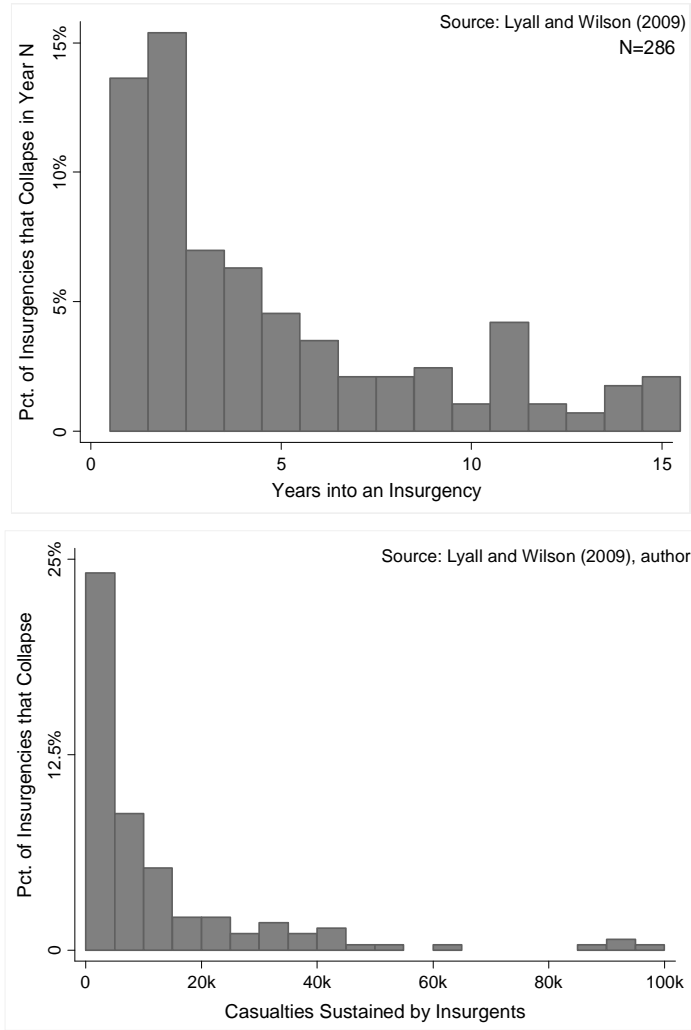


Figure 3.6. Empirical Distributions of How Long Insurgencies Last

Based on data from Lyall and Wilson 2009 and codings by the author, Figure 3.6 demonstrates the empirical distribution of how long it generally takes to defeat an insurgency. See Chapter 5 for a more extensive discussion of these data and their subsets.

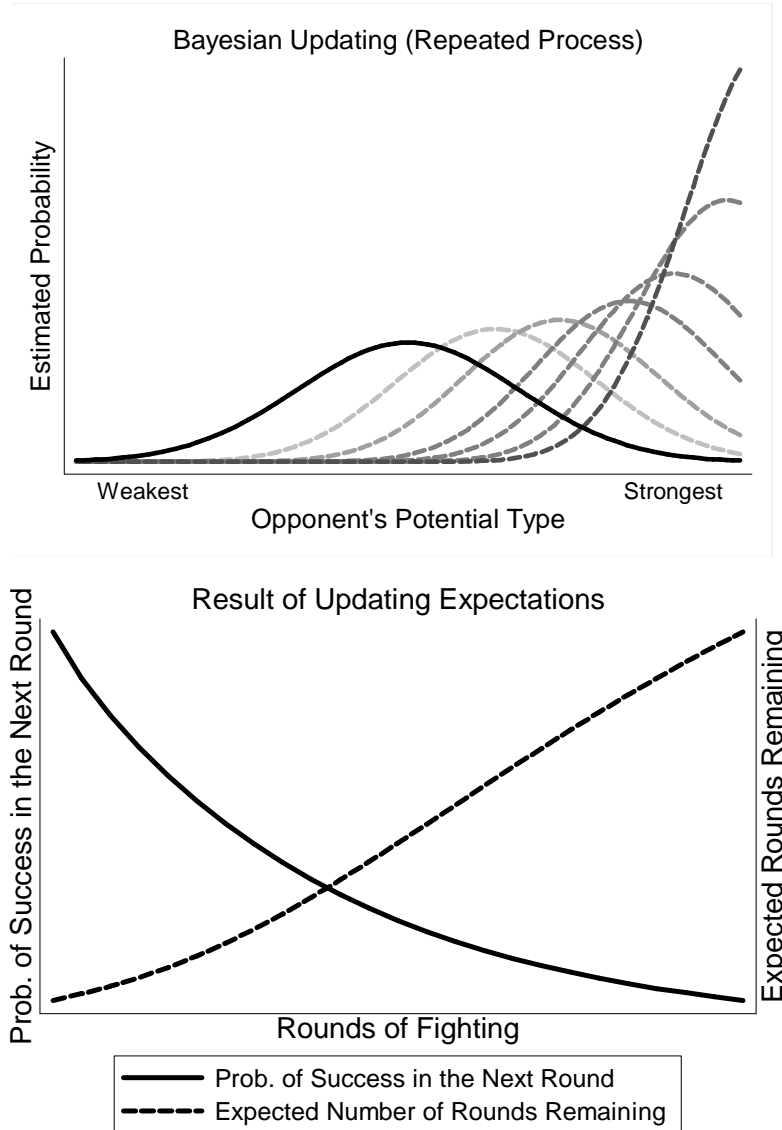


Figure 3.7a. How Decision Makers Would Revise their Expectations If War is a Repeated Process

When war is a repeated process such that the chances of defeating an opponent in each round of fighting are independent and identically distributed, each round of fighting allows decision makers to learn that there is a progressively smaller chance that they are facing a “weaker” type, and a relatively higher chance that they are facing a “stronger” type. The top panel shows how this will lead to a gradual shifting of prior expectations. The bottom panel shows that decision makers will become progressively more pessimistic about the chances of defeating their opponent in the next round of fighting, and they should believe that it will take longer to defeat their opponents at the margin.

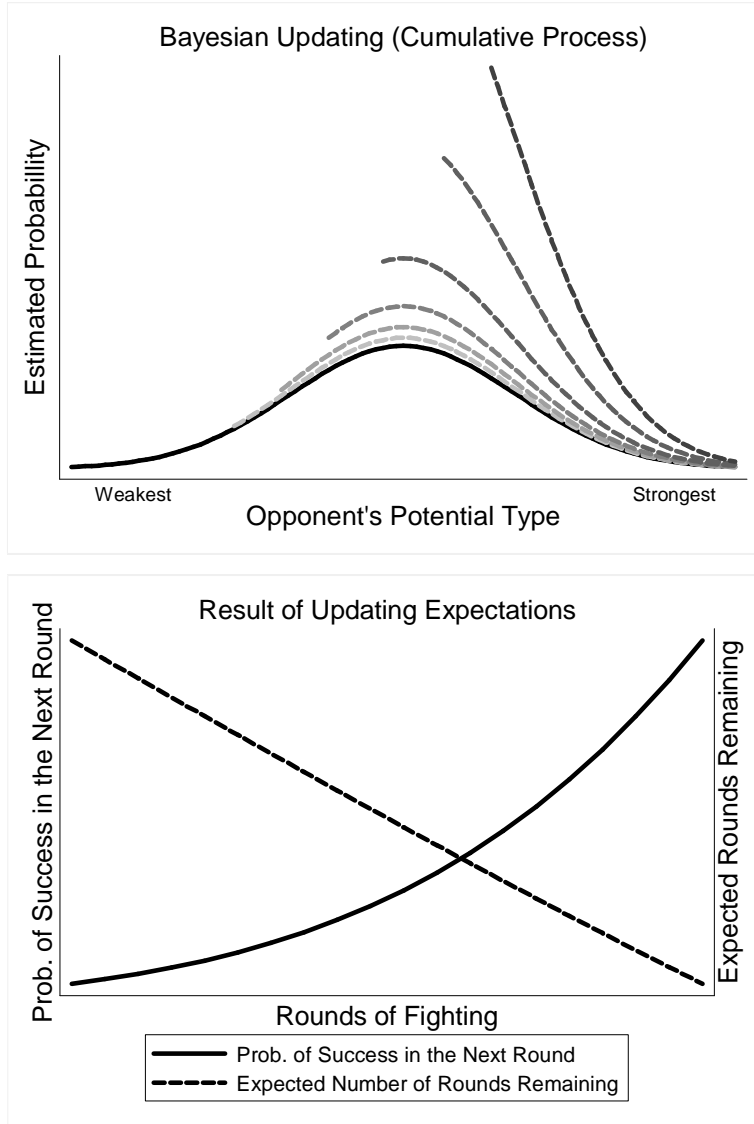


Figure 3.7b. How Decision Makers Would Revise their Expectations If War is a Cumulative Process

When war is a cumulative process, the learning process is very different. In each round of fighting, decision makers can rule out that they are facing an opponent that was weak enough such that they would have conceded already – but this does not necessarily provide any way to separate the potential types that remain. As a result, decision makers should update their expectations by truncating their prior assumptions, not by shifting them gradually. When prior assumptions are normally distributed, this has the complete opposite effect as the process shown in Figure 3.7a: here, rational decision makers will become progressively more optimistic about their chances of defeating their opponents in the next round of fighting, and the expected cost remaining to complete the war will continually decline.

A closer look at updating expectations

Of course, the notion that decision makers form prior assumptions is not unique to the theoretical framework in this paper – in the existing literature on the bargaining model of war, for instance, the way that decision makers form initial expectations of an opponent’s potential “type” is a central topic of interest, especially because of the notion that war might often result from combatants being “mutually optimistic” about their military prospects based on private information.¹⁷ Where the theoretical framework in Section 3.1 deviates from the existing literature is primarily in the way that decision makers *revise* these prior assumptions, based on the notion that war is a cumulative process and not a repeated process.

Figure 3.7 shows graphically how these two kinds of learning processes differ. The top panels of this figure show how decision makers should update expectations about an opponent’s potential type if war were a repeated process. Consistent with contemporary scholarship on the bargaining model of war, an opponent’s type here represents the likelihood that they will be defeated in each round of fighting. The prior assumption here is that opponent types are distributed normally, such that there is some probability that the odds of defeating a particular opponent in each round of fighting are very low, there is some probability that those odds are very high, and the most likely kinds of opponents are somewhere in between. Every time a round of fighting transpires without the opponent being defeated, decision makers can revise these assumptions, assigning progressively less weight to the notion that the opponent is relatively

¹⁷ For examples, see Fearon 1995, Ramsay and Fey 2007, Meirowitz and Sartori 2008, and Slantchev and Tarar 2011.

weak, and assigning progressively more weight to the notion that the opponent is relatively strong.¹⁸

The top panels of Figure 3.7 capture the dynamic nature of this process by displaying several revised distributions, which move from lightest to darkest each time they are updated. Shown graphically in Figure 3.7, the updating process in standard models essentially consists of gradually shifting expectations to the right: every time an opponent is able to survive a round without conceding, decision makers should think it is increasingly likely that they are relatively resilient. As a result of this learning process, rational decision makers should think it is progressively less likely that the next round of fighting will produce concessions, and by the same logic, their estimate of the total number of rounds required to defeat their opponent will continually increase.

The bottom panels of Figure 3.7 show, in contrast, how decision makers would revise their expectations under the assumption that war is a *cumulative* process. In this framework, a combatant's "type" represents the number of rounds of fighting they can hold out before giving in to their opponent's demands. Once again we start with a normally-distributed prior – there is some chance this combatant will concede quickly, some chance they will hold out for a protracted period, and it is most likely that their breaking point is somewhere in the middle. But the process of updating this prior assumption is quite different from what we saw earlier. Conditional on reaching a certain round of fighting, it is possible to rule out the notion that the opponent is weak enough such that they would have conceded already, but since all remaining types should have been able to resist thus far, fighting does not automatically provide any

¹⁸ See Powell 2004: 358 for a similar discussion of how prior assumptions "shift" across rounds of fighting in this fashion.

additional information allowing decision makers to determine which of these types is more likely than the others. Figure 3.7 thus shows how the updating process here essentially consists of *truncating* the prior assumption, and then raising all remaining probabilities proportionally so that they always sum to 1.

Note also how, in the bottom of Figure 3.7, probability mass becomes progressively concentrated to the left of the remaining distribution each time the prior is updated. This provides a graphical sense as to why, when decision makers believe that opponent's types are normally distributed, they will perceive that the Expected Remaining Cost of continuing the war is continually declining. These dynamics lead to predictions that are the exact opposite of standard models of strategic assessment: after each round of fighting, rational decision makers should expect that there is a progressively *higher* chance of defeating their opponent in the next round, and their estimate of the total amount of fighting it will take to achieve their objectives will progressively *decline*. Comparing the output of these models on the right side of Figure 3.7, it is apparent how defining armed conflict as a repeated or a cumulative process essentially inverts expectations about how rational decision makers should learn and adapt.

A closer look at why the shape of prior assumptions matters

Section 3.1 formally demonstrated that normally-distributed priors generate continually decreasing perceptions of Expected Remaining Cost. The reason this is the case is that if you pick a point on the normal distribution and start moving to the right, then the remaining probability mass will become increasingly concentrated near the point you are considering. This is not easy to see when looking at the normal distribution. In fact, first impressions appear to

suggest that the opposite, since the right-tail of this distribution “flattens out” as it reaches more extreme values. This impression is misleading. It is true that the slope of the normal distribution declines in absolute terms as we move along the tails; but in *relative* terms – if we renormalize the remaining probability mass as we go along – the distribution gets progressively steeper. To show this, Figure 3.8 “zooms in” on different windows of the normal distribution’s tail. Each window has the same width. Focusing on each window individually and rescaling the axes each time shows how as we move rightward along the normal distribution, we keep getting closer to the mean of the probability mass that remains, and thus Expected Remaining Cost will decline.

Using a similar technique, we can see why the exponential distribution is a “boundary case” for predictions about learning and adaptation – as Section 3.1 explained, if decision makers believe that opponent types are exponentially distributed, then Expected Remaining Cost would be the same in every round of fighting. Figure 3.9 shows this graphically by again zooming in on four different windows of the distribution’s right tail and renormalizing the axes each time. Each of these subsets is identical: no matter which point we select along the exponential distribution, the remaining probability mass is always spread out in precisely the same way. Thus any point along this distribution is always the same distance away from the mean of the probability mass to its right, and this is why Expected Remaining Cost remains constant.¹⁹ (By a similar logic, if priors have thicker tails than the exponential distribution, then probability mass will continually shift further to the right as we move along, which is why Expected Remaining Cost will *increase* with each round of fighting when decision makers possess heavy-tailed priors.)

¹⁹ In the literature on probability theory, this property of the exponential distribution is called “memorylessness”: no matter where you are on the distribution, you are always the same distance from the mean (or any percentile) of the remaining probability mass to your right. See Figure 3.8 for a graphical representation.

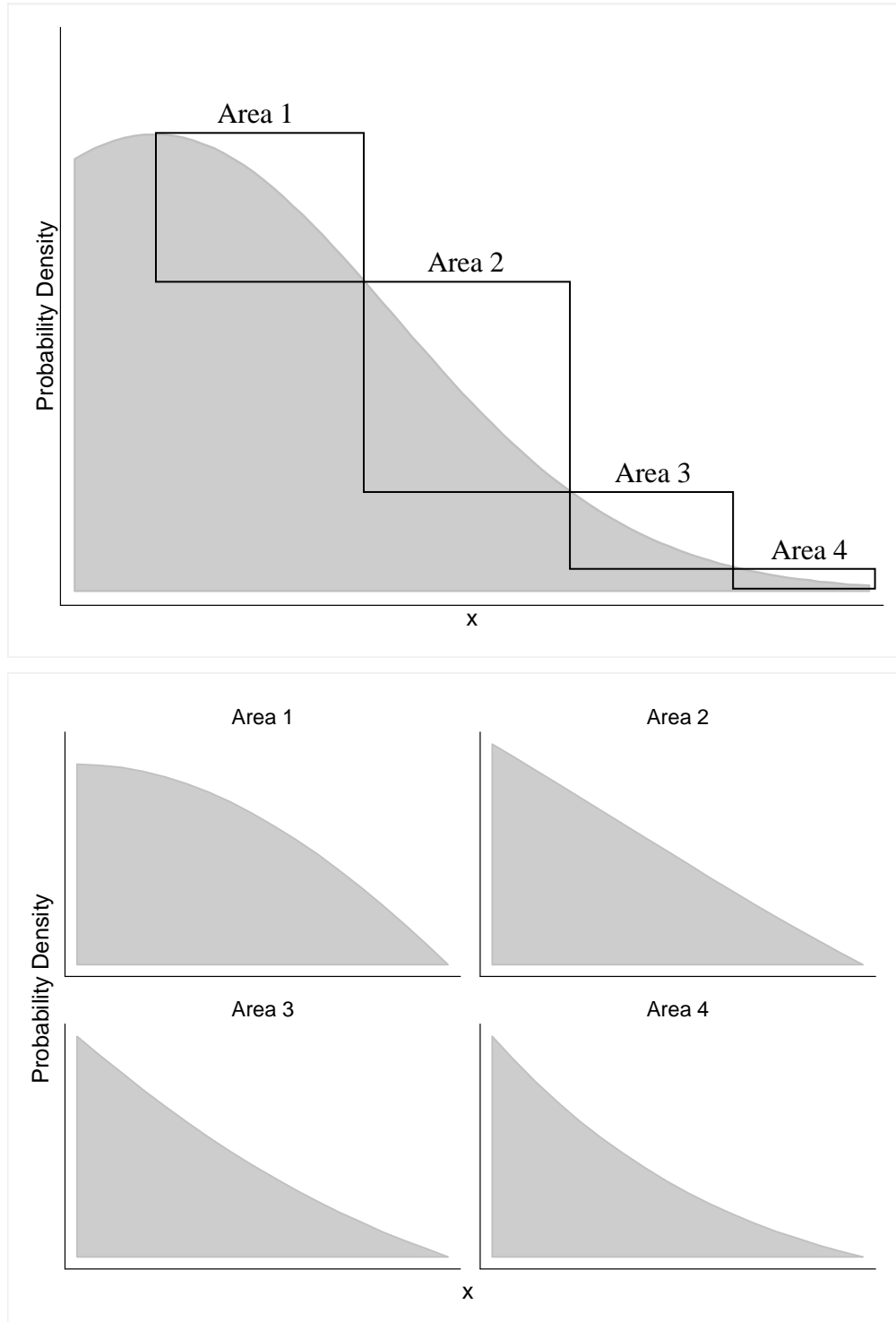


Figure 3.8. Probability Mass along the Normal Distribution's Tail

By zooming in on different windows of the normal distribution and renormalizing the axes each time, it is evident that as those windows move to the right, the probability mass within them shifts to the left. This shows graphically how as we move out along the distribution's tail, we get progressively closer to the center of the distribution that remains.

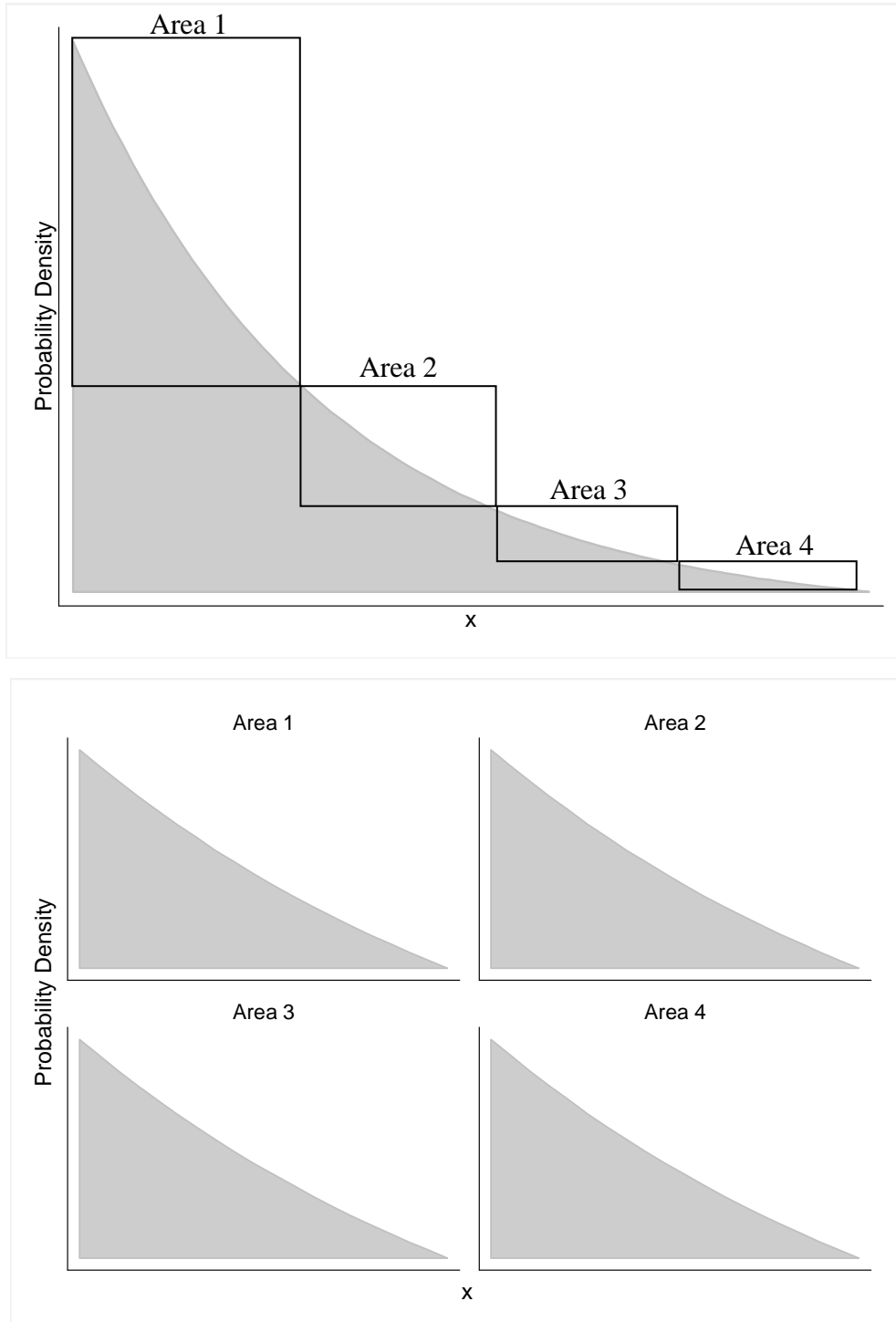


Figure 3.9. Probability Mass along the Exponential Distribution's Tail

Expected Remaining Cost is the same at all points along an exponential distribution: zooming in on different areas of this distribution's tail helps to explain that its distribution of probability mass always remains constant, a property that is called "memorylessness."

The main takeaway from this discussion is that the shape of the prior assumption is critically important for understanding how rational decision makers should learn and adapt over the course of armed conflict. If this sounds obvious, keep in mind that this is not something that existing models typically take into account. The standard prediction of the bargaining model of war is monotonic: one of its central insights is that each round of fighting brings combatants' perceptions closer into alignment, regardless of what those perceptions were to begin with. Here, by contrast, we see a learning process that is much more contingent, implying that it is not possible to understand how rational decision makers will learn and adapt unless we can understand the kinds of prior assumptions that structured their original assessments.

Section 3.3. Implications for strategic interaction

The principal subject of this dissertation is strategic assessment, defined as the way that military decision makers form and revise their expectations about how long it might take or how much it might cost to achieve their political objectives. The analysis in this chapter so far has been “one-sided,” focusing on the way that rational decision makers might learn about their opponents' capabilities and resolve as they fight. Most of the contemporary literature on the bargaining model of war, by contrast, seeks to go further, leveraging one-sided decision analysis in order to create game-theoretic models of how combatants might optimally negotiate with each other as conflict unfolds. While this kind of bargaining behavior lies outside the primary scope of this dissertation, it is such an important component of the contemporary theoretical literature that it is worth discussing what viewing war as a cumulative process implies for strategic interaction.

In doing so, it is useful to begin by providing a brief overview of how the current game-theoretic literature on bargaining and war developed. This literature is generally grounded in the early work of Thomas Schelling, Geoffrey Blainey, and Donald Wittman, who developed the notion that war is a tool for coercive bargaining and that fighting can advance this bargaining process by revealing information about combatants' capabilities and resolve.²⁰ James Fearon formalized the idea that asymmetric information about combatant's "types" (along with incentives to misrepresent than information) can constitute a rationalist explanation for war, but he left it for other scholars to explain precisely how fighting would resolve this informational problem.²¹

Two works were influential in helping to "open up the black box of war" in this respect. Alastair Smith developed the "battling for forts" model described earlier, in which he assumed that war consisted of a number of individual engagements fought to control a discrete number of objectives, that the odds of each side winning a given battle were fixed and repeated, and that fighting therefore allowed combatants to resolve uncertainty about their relative balance of power.²² R. Harrison Wagner expanded the foundations of strategic-level bargaining models, arguing that it is possible to think of war as having multiple "rounds of fighting," in which each side ran a fixed and repeated risk of military collapse; thus as war continued without obtaining a military victory, rational combatants could revise their views of what this risk entailed.²³ Importantly, these early works on learning and adaptation in war were one-sided analyses much

²⁰ Schelling 1960b, 1966, Blainey 1973, Wittman 1979.

²¹ Fearon 1995.

²² Smith 1998.

²³ Wagner 2000.

like the theoretical framework advanced in Section 3.1 – rather than constituting full-blown game theory from the start, the early stages of developing the bargaining model of war largely consisted of defining assumptions about what fighting entailed (including the notion that war could be modeled as a repeated process) and then drawing conclusions about how individual combatants would revise their views of their opponents accordingly.

The contemporary, game-theoretic literature on coercive bargaining then constituted what Robert Powell called a “second wave of formal work on war,” building off the foundation of scholars who examined learning and adaptation on the part of individual combatants.²⁴ Powell’s 2004 article on “Bargaining and Learning While Fighting,” as mentioned in Chapter 2, is probably the most prominent strategic-level model in the bargaining literature, and it is explicitly framed as a game-theoretic extension of Wagner’s earlier framework.²⁵ Similarly, Alastair Smith extended his battling-for-forts framework into a model of strategic interaction in his 2004 article with Allan Stam on “Bargaining and the Nature of War.” Filson and Werner, Slantchev and others built their game-theoretic treatments of coercive bargaining on assumptions that were similar to Smith’s earlier, one-sided analysis.²⁶

In some sense it is trivial to point this out, because no game-theoretic model can be constructed without first defining how each side will respond to a given set of circumstances on

²⁴ Powell 2004: 345.

²⁵ Powell writes: “The present formulation is closest to Wagner. He also bases his analysis on a model in which rejecting an offer generates a risk that the game will end in that round because one state or the other collapses. Wagner, however, does not formally derive the equilibria of the game when there is asymmetric information...” (2004: 347).

²⁶ Filson and Werner 2002, Slantchev 2003. See Smith and Stam 2004: 784 and Powell 2004: 347-348 for a review of these models and their origins.

its own: finding game-theoretic equilibria requires defining “best response functions” for each relevant actor, and then rewriting these functions in terms of one another in a way that identifies situations where all actions are optimal and no one would be better off adjusting their behavior. In this respect, the validity of game-theoretic models fundamentally depends on core assumptions about how each actor will assess its options – and in particular, strategic assessment in the bargaining model of war depends on the assumption that war is a repeated process. By questioning that basic assumption, and by showing how viewing war as a cumulative process has important implications for rethinking strategic assessment, the argument in this dissertation will naturally influence the output of game-theoretic models as well.

In the standard setup for the bargaining model of war, for instance, two combatants – call them players 1 and 2 – are making demands over how to divide some stake X . We can think of this stake as a piece of territory, a stock or flow of material resources, or any other form of value that is possible to exchange. In order to think about the way that combatants would negotiate over the division of X , it is first necessary to define how well each of them believes they would do if they attempted to decide the issue through war. This will determine each side’s reservation value for negotiations, as they should not accept a settlement that gave them less utility than what they expect to obtain through fighting.

Since war is costly, players 1 and 2 would prefer to divide X peacefully and avoid the deadweight loss of fighting. But in cases where players possess asymmetric information about their military capabilities, it is possible for each side to be mutually optimistic about their military prospects, such that neither player is willing to offer the other a deal that they would find acceptable. This is the framework which serves as the basis for the contemporary literature on

the bargaining model of war.²⁷ As mentioned in Chapter 2, the key takeaway from most of the existing literature on this subject is that fighting helps to resolve this asymmetric information. As combatants' perceptions of the military balance converge, so will their reservation values and bargaining demands, and eventually this will open up a space for negotiated settlement.

Powell, for instance, describes a situation where a satisfied state S is fighting a dissatisfied state D , each round of fighting induces some probability that each side will collapse, and states enter war with uncertainty about what these probabilities are: "Suppose," Powell writes, "that the satisfied state is trying to distinguish between types k_D and k'_D where k_D is less likely to collapse than k'_D ($k_D < k'_D$) and therefore is more powerful. At the outset of the game, S believes that D 's probability of collapse is distributed according to θ with density θ ." Using Bayes' rule, combatants can thus update their perceptions of how their opponent's type might be distributed, and as a result of the dynamics laid out earlier, this learning process always pushes in the same direction. As Powell writes, "the odds of facing [the stronger type] k_D rise with each round of fighting. If these types have fought r times without collapsing, the odds rise to $(1 - k_D)^r \theta(k_D) / [(1 - k'_D)^r \theta(k'_D)]$. This is the sense in which fighting conveys information." As combatants incorporate this information into their bargaining positions in Powell's model, it will make them more amenable to a negotiated settlement: since they will become progressively more likely to think that they are facing a strong opponent, they will also determine that the expected costs of fighting are growing progressively higher – thus their reservation value for a negotiated

²⁷ Fearon 1995.

settlement will progressively decline, and it will be more feasible to reach a mutually acceptable deal.²⁸

This chapter has demonstrated, however, that Powell's key assumptions here do not necessarily hold. Section 3.1 showed that once we take war to be a cumulative process and not a repeated process, then rational combatants should not necessarily perceive the expected remaining costs of warfare to be rising as rounds of fighting mount. We in fact saw how, for several possible distributions of an opponent's potential type, decision makers should progressively become more *optimistic* about their ability to achieve their wartime demands as armed conflict drags on. This implies that combatants' reservation values in negotiations might increase, not decrease, which would reduce the bargaining space, not expand it.

It is possible to rethink Slantchev's prominent model in a similar fashion. Slantchev's setup assumes that combatants are fighting for a discrete set of "objectives"; each round of fighting entails a battle for control of a marginal objective, and the war ends if one side captures all of its opponent's objectives. Slantchev writes how, conditional on going to war, combatants can be either of two types: strong or moderately strong (as "weak types" in his model would never go to war). Heading into battle, combatants will thus have prior expectations about the probability that they are facing each of the relevant types. "Intuitively," Slantchev writes (as he assumes that war is a repeated process), "winning a battle should make [a combatant] more optimistic about the chance of facing a weak opponent, while losing a battle should make [a combatant] more pessimistic." The connection to bargaining behavior is clear. "Victories make [a combatant] more optimistic about its chances," Slantchev writes. "Consequently, if [a combatant] wins the

²⁸ Powell 2004: 349.

fight, it demands more than it does if it loses it.” And “it is always the case” that the reverse is true, such that if a combatant *loses* a fight, then they would demand *less* than if they had been successful.²⁹

A problem with this formulation, as Section 3.1 showed, is that it is entirely possible for *both* combatants to believe that they are making strategic progress simultaneously. Depending on their prior assumptions, it is *not* necessarily true that everyone will learn the same lessons from each battle. In the Pacific theater of World War II, for instance (as discussed in the last chapter), the U.S. objective was to successively roll back Japan’s defenses while the Japanese goal was to successively exhaust U.S. casualty tolerance – it was thus perfectly logical for both sides to believe after each battle concluded that they were making cumulative progress towards achieving their intended objectives.

A similar situation characterized the 1916 Battle of Verdun, one of the costliest battles of World War I in which France and Germany attempted to wear each other down through cumulative attrition. (The French referred to the battle as “the meat grinder”; German commander Erich von Falkenhayn said that his objective was to “bleed the French white.”) In all, the Battle spanned ten months of German offensives and French counteroffensives, producing nearly three-quarters of a million casualties combined. How could the battle have dragged on for such a protracted and costly period if fighting provides information which resolves uncertainty? Both French and German forces were observing the same information, so how could they both have maintained mutually optimistic beliefs for so long? Existing models of strategic interaction have trouble explaining this, but when viewing war as a cumulative process,

²⁹ Slantchev 2003: 624, 626, 631.

this behavior is no longer surprising. There is nothing inherently contradictory in the notion that rational actors observing battlefield outcomes would both believe that they are making progress towards achieving their goals.

If this is true, then an important takeaway is that there are circumstances under which combat might in fact hinder – not help – combatants to define an acceptable negotiated settlement. If opponents are becoming progressively more optimistic about their chances of extracting concessions through force, then all else being equal, this should make them less likely to reduce their demands or to make concessions of their own. Here the theoretical framework developed in this dissertation clearly departs from prominent scholarship in the contemporary study of international relations.

To reiterate, there is nothing here to say that this behavior is inevitable: while the existing literature makes a strong prediction that combatant's perceptions and bargaining demands should strictly converge as war proceeds, Sections 3.1 and 3.2 demonstrated that the way rational combatants revise their views is largely contingent on the kinds of prior assumptions that they make about their opponents *ex ante*. The framework developed here thus reinforces the argument developed in Chapter 2 about how viewing war as a cumulative process suggests placing much more attention on the content of these prior assumptions than existing models. This will continue to be a central theme of the analysis in subsequent chapters.

It is possible that scholars could model strategic interaction in a way such that combatants somehow account for the plausible kinds of prior assumptions under which their opponents might be operating, and then incorporate those assessments into their own strategic calculus such that they anticipate subsequent divergences in bargaining demands and adjust their own

negotiation positions accordingly. Ramsay and Fey, for instance, proposed revising standard bargaining models in a manner such that decision makers account for the prospects of mutual optimism, essentially reducing their demands on the eve of war in the knowledge that they might have mistakenly set their demands too high.³⁰ Slantchev and Tarar showed, however, that these sorts of adjustments can be quite dependent on assumptions about game structure, while Fearon more generally argued that if combatants are allowed to bluff or manipulate their demands strategically, then they may not reveal information through offers and counteroffers in the way that other literature expects.³¹

The purpose of this dissertation is to focus on strategic assessment, and it is not oriented towards the broader, game-theoretic literature per se. But since models of strategic interaction treat bargaining behavior and battlefield outcomes to be interdependent, then to the extent that this chapter has shown how rational decision makers do not necessarily learn from battlefield outcomes in ways that the existing literature assumes, this opens the possibility for re-evaluating broader literature. This is yet another reason why it is important for scholars to understand how war is a cumulative process, and how that influences learning and adaptation in a manner than departs from existing theoretical frameworks.

Section 3.4. Summary of the theoretical framework

To summarize the argument up to this point, Chapter 2 described the standard theoretical framework for capturing the way that rational decision makers form and revise their expectations

³⁰ Ramsay and Fey 2007.

³¹ Slantchev and Tarar 2011, Fearon 2007a.

in war; showed how this framework relied on the assumption that war is a repeated process; and offered an alternative way to conceptualize combatant types that is more consistent with the notion that war is a *cumulative* process. This chapter then showed how making this change resulted in theoretical predictions that are in some cases the very opposite of what existing scholarship predicts. In particular, the new framework challenges the common assumption that rational decision makers should become more pessimistic about their policies as they fail to achieve their intended goals: there are a wide range of conditions under which we should in fact expect the very opposite.

This framework provides a new vantage point for understanding why so many military decision makers struggle to realize their strategic mistakes, and why it is that they often stick to unsuccessful strategies for so long. This chapter has shown how the cumulative dynamics of armed conflict raise genuine obstacles to strategic assessment that do not rely on assumptions about organizational behavior, domestic politics, psychology, or any failure of rational evaluation. For that reason, it is plausible to think that the discussion here can generalize widely across time, space, and subject matter, helping to explain why decision makers in war and many other fields are often so reluctant to change their policies, even when those policies do not seem to be succeeding. This framework is also relevant to understanding why wars are often so protracted if fighting provides information that supposedly promotes compromise – this is one of the main puzzles confronting the literature on the bargaining model of war, and this chapter indicated why understanding the cumulative dynamics of armed conflict may explain why this is the case.³²

³² On the puzzling nature of protracted conflict, see Fearon 2007a and Powell 2012.

Ultimately, the purpose of this chapter is to help reformulate baseline expectations for how rational decision makers should learn and adapt in armed conflict. Much of the existing literature on this subject assumes that decision makers who “stay the course” when their policies are not succeeding are obviously behaving in an irrational fashion; this has driven a great deal of research on the sources of strategic errors, and this literature has produced many valuable insights.³³ It may very well be the case that militaries possess organizational cultures that emphasize conventional warfare, that leaders have biases that hinder self-evaluation, and that domestic politics might create perverse incentives. But this chapter demonstrates that *we do not need* these factors in order to explain why some actors seem to be so unwilling to adapt their policies.

What the existing literature treats as a problem of nonrational behavior may thus simply be a natural reaction to the cumulative dynamics of war. Understanding these cumulative dynamics suggests that when decision makers fail to achieve their goals this does *not* logically mean that they should become more pessimistic about the policies that they are pursuing. In some cases, the theoretical argument in this chapter may thus provide a vantage point for re-opening debates about whether some forms of military behavior are truly as irrational or as puzzling as previous scholarship has argued. This is the subject of the next chapter, which applies the theoretical framework developed here to offer a new interpretation of U.S. military behavior during the American Indian Wars.

³³ See the sources reviewed in the opening section of this chapter.

Chapter Four: Empirical Application

STRATEGIC ASSESSMENT IN THE AMERICAN INDIAN WARS

From the founding of Jamestown in 1607 to the Battle of Wounded Knee in 1890, British colonists and U.S. citizens were in continual conflict with one Native American tribe or another. The American Indian Wars receive relatively little attention from contemporary political scientists, but they occupied the U.S. and British Armies for centuries and significantly shaped the development of the “American way of war.”¹ Moreover, as this chapter will explain, there are specific, methodological reasons why the American Indian Wars provide exceptional analytic traction for the study of military decision making.

Section 4.1 sets the stage for the analysis by describing how U.S. commanders often struggled to form and revise their expectations during the American Indian Wars, while Section 4.2 discusses case selection and research design. Section 4.3 then demonstrates how a theoretical understanding of cumulative dynamics, combined with an original data set spanning roughly 3,000 frontier engagements, sheds new light on U.S. behavior, explaining why officers had a puzzling tendency to underestimate tribal forces, and why they maintained those optimistic beliefs even as some conflicts dragged on. Section 4.4 discusses how, to the extent that this approach provides new insight into the beliefs and actions of commanders like George Custer (whose actions have been scrutinized as much as almost any other decision maker in U.S.

¹ On this thesis, see Prucha 1969, Weigley 1973, Lee 2001, Grenier 2005, and Cohen 2011.

history), it indicates the potential promise of the new framework for re-evaluating established scholarship and conventional wisdom.

Section 4.1. Underestimating tribal forces

Throughout the American Indian Wars, U.S. and British officials generally articulated their policies in terms of coercive punishment: they believed that if they could inflict enough costs upon the tribes, then their opponents would conclude that it was not in their interest to continue resisting. As Major General George Crook once put it, reflecting on his experience as one of the country's most renowned frontier commanders, sometimes "there was nothing else to do but go out and kill them until they changed their minds."²

Military and civilian leaders regularly expressed similar sentiments. Secretary of War John Floyd ordered that operations against the Navajos "should have for their object to inspire them with fear, by a few decisive blows." Brigadier General Newman Clarke's battle plan against the Coeur d'Alene tribe was to "make their punishment severe, and persevere until submission of all is complete." Brigadier General John Pope's intention in the Red River War was to "wear down the fugitives so that they would hasten back to their agencies and submit." Indian Superintendent Alfred Meacham's stance on the Modocs' refusal to leave their land was that "We will whip you until you are willing." In the Dakota War, Governor Henry Sibley aimed to convince his opponents that "the long arm of the government could reach the most distant haunts and punish them." Colonel Henry Carrington's objective against the Cheyennes was to send out "active operations in different directions, as best affords chance of punishment." Colonel Edwin

² Bourke 1891: 213.

Sumner's plan for the Utes was to "inflict upon them a severe chastisement," and Lieutenant General William Sherman's impression of the Arapahos was that "these Indians require to be soundly whipped."³

Of course, the U.S. Army held the military upper hand in these conflicts over the long term. While the United States was in the process of becoming one of the world's largest, wealthiest, and most powerful countries, North America's indigenous population was sharply declining as a result of violence, displacement, and disease.⁴ Many tribes' economic foundations were gradually eroding as well, through overhunting furs and the depletion of the great buffalo herds (which the United States Army at times facilitated in an attempt to undermine Plains tribes). And as the United States developed a network of railroads throughout the continent's interior, this diminished the tribes' main tactical advantage of being able to operate from areas that were far from Army bases of supply.

The puzzle therefore is thus not why the United States commanders believed that they had the ability to overwhelm their opponents with force, an outcome that was ultimately overdetermined. The puzzle is why so many commanders were so ineffective at judging how long it might take and how much it might cost to achieve these military objectives – for even if the United States ultimately "won" almost all of the American Indian Wars, their performance with strategic assessment was often decidedly mixed.

³ For these quotes, see Utley 1967: 86, 172, 203; Utley 1973: 219, 103, 144; Dillon 1973: 103; Thrapp 1967: 118; Clodfelter 1998: 115; Carroll 1976: 6.

⁴ Diseases were especially problematic – Native Americans were acutely susceptible to diseases like smallpox and influenza, which could easily kill half of a given tribe in a single wave. See Calloway 1997: ch. 2.

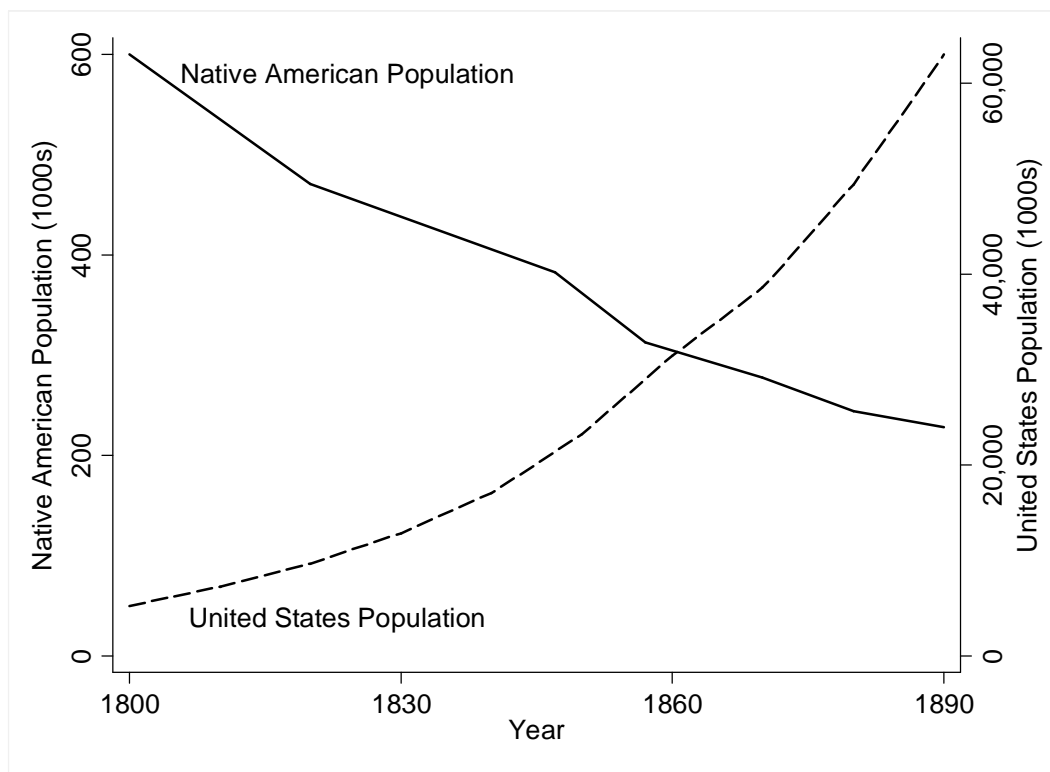


Figure 4.1. Nineteenth Century Native American and U.S. Population Shifts

Over the course of the nineteenth century, the total population of Native Americans living in the continental United States declined from roughly 600,000 to roughly 200,000, while the U.S. population grew from roughly 5 million to more than 60 million. (Data from Reddy 1993 and the U.S. Bureau of the Census.)

For instance, at least twelve tribes – Apaches, Cheyennes, Comanches, Creeks, Delawares, Narragansetts, Nez Perces, Pequots, Rogues, Seminoles, Tuscaroras, and Wampanoags – held out against U.S. military expeditions until a majority of their military-aged males had been killed or captured in battle. In two of these wars (the Creek War of 1813-14 and the Second Seminole War from 1835-42, which will be discussed below), the fighting did not end until the U.S. Army

had essentially killed or captured the entire tribe.⁵ In other cases, tribes only surrendered when the U.S. Army posed a credible threat to eliminate the group at once. The Modocs defended their positions among California's lava beds in 1872-73 until they were largely surrounded and the Army had begun to bombard them with howitzers. The Nez Perces fought a running string of engagements in 1877 as they attempted to escape from Idaho into Canada, and they only gave up when they made a break for a mountain pass and the tribe was surrounded by U.S. forces on either end. In a sense, the Modocs and Nez Perces succumbed to a coercive threat. But that threat had to escalate to an all but existential scale before they were willing to surrender. The expectation that any of these tribes could be "humiliated," "whipped," "demoralized," or otherwise "punished" into submission with a moderate show of force was completely out of line with reality. Understanding how U.S. commanders could have held these kinds of mistaken beliefs is a central question confronting historical scholarship on the period and it is the empirical puzzle that motivates this chapter.

Underestimating opponents in the Great Sioux War

The "Great Sioux War" of 1876, for instance, was fought for control of South Dakota's Black Hills. This land was home to Sioux⁶ and Cheyennes; it also possessed valuable timber, and when an 1874 Army expedition declared there was gold in the region as well, settlers rushed in to mine it illegally. At first the Army attempted to restrain the settlers, but by the end of 1875 the Grant

⁵ In the Creek War, there were an estimated 3,000 "Red Sticks" fighting the United States, of whom roughly 1,200 were military-aged males. The U.S. Army killed or captured 1,000 Red Stick warriors on the battlefield (including 600 in a single engagement at Horseshoe Bend, Alabama), and captured another 1,000 women and children.

⁶ Predominantly the Teton branch of the broader Sioux family.

administration decided to press the Sioux and Cheyennes to cede the territory instead. When the tribes refused to do so, and Native Americans began to leave neighboring reservations in order to join bands in the Black Hills led by prominent individuals such as Sitting Bull and Crazy Horse, the Army was ordered to evict them.

As in most of the American Indian Wars, the U.S. Army expected the coming fight to be quick and cheap, if the tribes offered military resistance at all. Lieutenant General Philip Sheridan (commander of the Division of the Missouri and thus the principal figure directing the operation) chose to deploy his forces in three columns intended to encircle his opponents. “In this way,” Sheridan argued, “the Indians would be deprived of the means of escape... and be forced to fight until subdued or annihilated.”⁷ Sheridan was not particularly worried about how this fighting would turn out, explaining to Brigadier General Alfred Terry (one of his two principal field commanders) “the impossibility of any large number of Indians keeping together as a hostile body for more than one week.” Terry shared Sheridan’s optimism, replying that “I have no doubt of the ability of my column to whip all the Sioux whom we can find.” And George Crook (then a Brigadier General and Sheridan’s other principal subordinate) expected to “strike a blow at once which will demoralize the savages from the start.”⁸

⁷ Headquarters Military Division of the Missouri, Office of the Chief Engineer. *Annual Report of the Chief of Engineers for the Fiscal Year Ending June 30, 1876*. July 11, 1876. See also Headquarters Military Division of the Missouri. *Report of Lieut. Gen. P. H. Sheridan*. House of Representatives Executive Document No. 1, Part 2. 44th Cong., 2nd Sess. November 25, 1876.

⁸ Sheridan to Terry, 16 May 1876 and Terry to Sheridan, 16 May 1876; Crook interview with *Denver Rocky Mountain News*, 23 Feb 1876. See Gray 1976: 45, 90 and the documents appended to the Secretary of War’s 1876 report to Congress: these include Sheridan’s *Report of Lieut. Gen. P. H. Sheridan* (Headquarters, Military Division of the Missouri, 25 Nov 1876), where he recalls that Terry believed a “quick movement... might be decisive”; and Crook’s recollection, even after the fact (*Report of General Crook*, Headquarters, Department of the

The course of this campaign was, of course, very different. Both Crook and Terry picked up the trail of Sitting Bull's forces, but the first major battle at the Rosebud was indecisive – despite engaging roughly one thousand opponents in a fight lasting six hours, the Army was unable to kill or capture more than a few dozen. Eight days later at Little Bighorn, cavalry under Lieutenant Colonel George Custer located another Sioux and Cheyennes camp. In a move that historians have generally interpreted as demonstrating a lack of regard for his opponents' military capabilities, Custer decided to split his forces and assault the camp in broad daylight. Outnumbered roughly three to one, the event turned into one of the most infamous defeats in U.S. military history. The Sioux and Cheyennes counterattacked, exploiting Custer's inability to coordinate units he had divided, and eliminating five companies entirely.

Persistent optimism in the Second Seminole War

If commanders entered warfare against the tribes with these sorts of misperceptions, they often exhibited extreme difficulty in adapting to unexpected events. The Second Seminole War is perhaps the clearest example. From the start of the war in 1835, more than 90 percent of Seminoles in Florida were either killed, captured, or forcibly relocated and yet the remainder continued to fight until President Tyler ordered a unilateral ceasefire in 1842.

Yet despite the fact that the Seminoles proved to be such exceptionally resilient insurgents, U.S. commanders were consistently optimistic about their prospects for terminating the conflict in short order. In December 1835, Secretary of War Lewis Cass reported that while some

Platte, 25 Sep 1876) that "I did not ask for reinforcements because I felt that we were abundantly able to take care of ourselves."

Seminoles were preparing to resist relocation, “They will, probably, when the time for operations arrives, quietly follow their countrymen.”⁹ When hostilities did in fact break out, the War Department expected a quick fight, relying on militia who were only mobilized for a three-month term of service. Brigadier General Winfield Scott took command of forces in Florida with a plan (much like Sheridan’s forty years later) for using three columns to surround his opponents such that they would be “hemmed in and captured.”¹⁰ Scott estimated that he would need just twelve days to do this.¹¹ But his first attempt was waylaid by poor coordination among the columns, and especially when one of them was brought under siege. A second attempt ran out of supplies before the U.S. Army captured even 60 Seminoles.¹²

Over the next six years the Seminoles continually frustrated this kind of “column and detachment” strategy, but the U.S. Army rarely attempted to alter its playbook. Though Scott’s campaign led to no avail, he argued that the failure was in the execution and not the concept – that that strategy was sound, but that he had not been given time to launch the expedition early enough in the fighting season, while being undermined by inferior subordinates. A court of inquiry investigated the matter and agreed with Scott’s assessment, saying that the “plan of campaign was well devised.”¹³

⁹ *Annual Report Showing the Condition of the War Department in 1835*, 8 Dec 1835. In *American State Papers: Military* (henceforth *ASP*) Vol. V §613.

¹⁰ This is how Scott described his strategy in his 1864 memoirs; cf. Sprague 1848: 115, Cohen 1836: 230.

¹¹ Cohen 1836: 230, cf. Scott to President Jackson, 3 Jan 1836.

¹² Mahon 1967: 157.

¹³ See the documents in Sprague 1848: 114-157.

Florida Governor Richard Call succeeded Scott as commander of the Army of the South. His columns and detachments failed, too, but the prevailing view was that this was because Call was “insufficiently vigorous” in directing them. Call had in fact been able to locate more than 600 Seminoles, and to bring them to battle at Withlacoochee and Wahoo Swamp. Both battles ended before Call’s forces could inflict more than a few dozen casualties on their opponents. But the Battle of Wahoo Swamp closed when U.S. forces decided not to pursue the Seminoles across a stream that appeared too deep to ford. When it later turned out that the stream was indeed quite passable, this once again precipitated widespread belief that a conventional offensive had put the Seminoles on the ropes, only to let them escape at the last moment.¹⁴

Thus Call was replaced by Major General Thomas Jesup, who received the standard instructions, along with the expectation that “should you succeed in bringing the Indians to a general engagement, and in defeating them therein, the ready submission of the tribe may probably be expected.”¹⁵ In spring 1837, Jesup reported that he had the Seminoles located and surrounded, that he had induced their leaders to capitulate, that “the war is no doubt ended,” and that “there is no danger of a renewal of hostilities.”¹⁶ The War Department issued orders for

¹⁴ Interim Secretary of War Butler to Call, 4 Nov 1836 (*ASP VI* §716); Sprague 1848: 162-67; Mahon 1967: 185; Doherty 1961: 104-5.

¹⁵ Butler to Jesup, 4 Nov 1836: *ASP VI* §716, *VII* §760.

¹⁶ Jesup to War Department, 7 Feb 1837, 6 Mar 1837, 26 Mar 1837, and 5 May 1837: *ASP VII* §760.

withdrawing the majority of Army forces from Florida.¹⁷ But by June, the war was back in full swing.¹⁸

In total, the Second Seminole War persisted through seven commanding generals, five secretaries of war, and three presidents, most of whom repeated this pattern of unfulfilled expectations. As historian Francis Prucha summarizes: “One commander after another tried his hand at bringing the embarrassing affair to a successful conclusion, yet the war dragged on, despite optimistic announcements from the commanding generals and the War Department, which periodically proclaimed that the war had finally been brought to an end.”¹⁹ To go along with these failures of strategic assessment were genuine costs: the conflict was politically embarrassing and financially draining,²⁰ it killed more U.S. servicemen than the first decade of

¹⁷ Commanding General A. Macomb to Jesup, 7 April 1837 and Secretary of War J. R. Poinsett to Jesup, 17 May 1837: *ASP VII* §760.

¹⁸ Jesup reported that “this campaign, so far as it relates to Indian emigration, has entirely failed,” and he requested to be relieved. Jesup to War Department, 5 June 1837: *ASP VII* §760.

¹⁹ Prucha 1969: 268. Jesup’s successor, Zachary Taylor (then a Brigadier General and later the 12th U.S. president), was the only commander who significantly challenged the soundness of the column-and-detachment strategy. After failing to subdue the Seminoles during the 1838-39 winter season, he suggested a “Squares Plan,” which involved dividing Florida into districts, each of which was controlled by a centrally-placed military post from which the Army could methodically sweep the area for Seminole holdouts. This system was never implemented, however. In May 1839, the Army’s commanding general Alexander Macomb arrived in Florida to negotiate with the Seminoles, after which he relayed his “satisfaction of announcing to the army in Florida, to the authorities of the territory, and to the citizens generally, that he has this day terminated the war with the Seminole Indians” (General Orders cited in Sprague 1848: 228-29, cf. Macomb to Poinsett, 22 May 1839). After this prospective peace fell apart, Taylor was succeeded by Brigadier General Walker Armistead, who was followed by Brigadier General William Worth. Their adherence to the standard column and detachment formula (and optimism about its prospects) are summarized in Mahon 1967: 276, 285, 295 and Sprague 1848: 256, 273.

²⁰ Financially, the Second Seminole War cost roughly \$35 million in 1840 USD (Covington 1993: 72), which may have made it the second most expensive war in U.S. history, as a fraction of the contemporary federal budget.

the War in Afghanistan,²¹ and militant Seminoles literally lived to fight another day – thirteen years later the U.S. Army would be summoned to fight a Third Seminole War.

Native American decision making

If U.S. commanders often struggled with strategic assessment during the American Indian Wars, what can we say about Native American decision making in these conflicts? To some degree, the answer is not much, since the tribes almost never left a written record of strategic deliberations. In a general sense, however, historians have emphasized several reasons why tribes may have been willing to fight such protracted campaigns against U.S. and British forces.

Some of the most common explanations for Native American military resilience are that tribes were intensely averse to leaving their ancestral lands; that many knew their relocation would result in attacks and assimilation by their rivals;²² and that some Native Americans believed that supernatural powers would offer them battlefield protection.²³ Many scholars have emphasized

²¹ 1,535 members of the U.S. Army and Navy were killed in the Second Seminole War (out of a total of 10,169 who served in Florida, for an attrition rate of 15 percent). In addition, 55 militia members were killed in action, and far more volunteers presumably died from wounds or sickness. Mahon 1967: 325. By comparison, 1,446 U.S. soldiers died in Afghanistan between 2001-2010, out of roughly one million who served in that country.

²² On the fate of some relocated tribes, see Hoig 1993 and LaVere 2000.

²³ There are many stories of Native American leaders motivating their followers to fight large battles with the U.S. Army by promising that their spiritual “medicine” would render warriors invincible to bullets; the Shawnee Prophet, the Comanche leader Isatai, and the leaders of the Sioux “Ghost Dance” at Wounded Knee are three prominent examples. It is worth noting, however, that faith in supernatural powers often led the tribes to tactical successes. Before the Battle of Little Bighorn, for example, Sitting Bull famously had a vision that involved U.S. cavalrymen falling upside down from the sky – he and his followers took this to be an omen that the Sioux would achieve a major victory in their next battle, which of course is exactly what happened.

that Native American warfare against the United States should be viewed against the backdrop of centuries of intertribal conflict that developed social and cultural norms about how war should be waged.²⁴ Native American warfare was often highly individualistic: forces were generally not organized into cohesive, hierarchical armies and demonstrations of personal bravery were often a primary means of social advancement. Many Native Americans thus sought combat despite (and perhaps because of) its lethality.²⁵ At the very least, many tribes had a limited ability to restrain “spoilers” who believed – for whatever reason – that fighting was in their interest.²⁶

²⁴ Anthony McGinnis 2010, 2012, Stan Hoig 1993, and John Ewers 1975 make this argument, which resonates with the broader research program of military historian John Keegan, who wrote in *A History of Warfare* (1993) that warfare is powerfully shaped by cultural constraints that develop over time and often resist adaptation. For example, the widespread practice of mutilating corpses is an example of a cultural practice that exerted a significant, negative impact on the tribes’ relations with whites. Many tribes believed that individuals retained their bodily form when they entered the afterlife – destroying the corpse was thus “regarded as the ultimate revenge” and it was a standard practice among many plains tribes (McGinnis 2010: 28). Europeans were shocked by these acts, which reinforced the kinds of animosity and fear that underscored the push for Indian removal. Scalping was another practice that can be seen in similar terms. Especially in the Southeast, obtaining enemy scalps was a sine qua non for social advancement within some tribes, and this also doubtlessly enhanced interethnic animosity (Lowie 1954: 106). In general, however, the practice of scalping was probably more limited than many people believe – in tribes such as the Ponca and Kiowa, for instance, simply touching an enemy was considered the highest form of valor, more so than inflicting any bodily harm (Mishkin 1940/1992: 3, 39 cf. Smith 1938: 426-34).

²⁵ See, for example, Hamalainen’s discussion of the Comanche “warrior cult”: “If there was an all-embracing internal force behind the rise of the Comanche empire, it was the relentless competition for social prestige among Comanche men. Violent seizure of livestock and captives through pillaging represented the main path to social acceptance: it gave them access to wealth and women and lifted them toward full manhood. For Comanche men, raiding was a matter of social life and death, and it was a mixture of anxiety and raw ambition that pushed them to repeatedly risk life for loot, devote much of their lives to arduous raiding” (2008: 269). Needless to say, this “all-embracing internal force” was often at odds with maintaining peaceful social relations with the Comanches’ neighbors.

²⁶ A good example is the beginning of the war with California’s Modoc tribe (1872-73). The war was preceded by increasing hostilities between Modocs and white settlers. The Modoc leader Kintpuash favored a peaceful resolution of the dispute. However, his stance was challenged by a younger and more militant Modoc leader known as “Scarface Charley.” Dillon 1973 explains

Yet these factors, while presumably important, do not really tell us much about why tribes would have varied in their capability or willingness to resist U.S. forces. As a later section of this chapter will demonstrate, many tribes did not offer the kind of lethal or protracted military resistance demonstrated by the Seminoles and the Sioux, and this should hardly be taken as a sign that those tribes were any less committed to their homelands, or any less spiritual, or any less influenced by cultural norms of warfare. Ultimately, the key challenge for social scientists aiming to understand the American Indian Wars is to identify ways of systematically predicting variation across cases. Of course, predicting how long these conflicts might last and how much they might cost was also the principal challenge for Army commanders performing strategic assessment in the American Indian Wars – and the next section explains why this set of cases provides scholars with an unusual opportunity to evaluate the way that commanders dealt with this uncertainty.

Section 4.2. Case Selection and Research Design

There are two main methodological reasons why the American Indian Wars provide a useful base of evidence with which to evaluate the plausibility and usefulness of the theoretical framework developed in this dissertation. First, it should be a tough test of this dissertation's theoretical framework to explain U.S. military behavior in these conflicts with concepts that characterize the decisions of unitary, rational actors. This is especially true since the experience

how Kintpuash understood that he did not have the ability to restrain Scarfaced Charley, that Charley's faction would eventually retaliate against the settlers in force, and that war was thus essentially inevitable. As a result, Kintpuash reluctantly agreed that it was best to initiate combat on Modoc terms, laying a trap to kill Major General Edward Canby under the pretence of peace talks. Canby was the highest ranking U.S. military officer to be killed in the American Indian Wars.

is typically seen as being relatively easy for alternative, nonrational theories to explain. George Custer, for instance, had a legendary flair for risk-taking, which many scholars have attributed to hubris, insubordination, or desire for public acclaim and political advancement.²⁷ Winfield Scott was a prominent proponent of Napoleonic warfare, which historians have argued may have shaped his insistence on employing columns and detachments in the Second Seminole War.²⁸ Developing a plausible explanation for how U.S. Army commanders in these conflicts may have had a much more rational basis for their actions and expectations than is commonly believed thus runs against the weight of conventional wisdom.

By a similar logic, we should also expect the written record of commanders' expectations to be biased in favor of optimism. The quotes from the Sioux and Seminole Wars mentioned above largely came from commanders who were writing for public audiences, to whom they would have an incentive to boast of their own capabilities; or writing to subordinates, for whom they would have found it important to express optimism in order to maintain morale; or writing to superiors for whom they have been inclined to exaggerate their own abilities. These are yet additional reasons why it would be a tough test for the theoretical framework developed in this dissertation to explain that these statements are plausibly consistent with rational beliefs.

The second methodological justification for focusing on the American Indian Wars is that these conflicts offer a relatively large cross-section of experience with a relatively high degree of unit homogeneity. In total, U.S. and British forces fought against 114 different Native American

²⁷ See Hutton 2004 for assorted views on Custer.

²⁸ In fact, Scott had recently translated a French manual into English so that it could be taught at West Point. See Scott 1864: 258 and Wooster 1988: 10. Knetsch 2011: 129, 132 describes U.S. strategy in the Second Seminole War against the backdrop of Scott's commitment to Napoleonic, Jominian principles.

tribes. Despite some important variations in firearms and whether they fought on horseback, most tribes waged war in relatively similar ways.²⁹ These tribes fought against either of two opponents (the United States or the British). In almost all cases, the stakes were the same, with the U.S. and British attempting to stop the tribes from raiding and to eject them from contested territory. In every instance, the U.S. and British ultimately succeeded in achieving those objectives – by the end of the nineteenth century, even the most militant tribes had been coerced into reservation life.³⁰

Of course, this is not to imply that all of the tribes and all of the conflicts in this sample were literally identical. The point is that, relatively speaking, these units of analysis are more comparable than those in most existing data sets on interstate wars, civil wars, insurgencies, or other kinds of political violence. The most widely-used data set on interstate conflicts³¹ involves multi-party wars of the most destructive magnitude (e.g., World War II) along with much more limited conflicts in vastly different contexts (e.g., the Ecuadorian-Colombian War of 1863). The most widely-used data set on insurgencies³² involves one case (the Chinese Civil War) in which million-man armies employed advanced weaponry throughout a huge East Asian country, along

²⁹ For more on the tribes' fighting methods and the influences upon them, see Grinnell 1910, Smith 1938, Mishkin 1940, Newcomb 1950, Secoy 1953, Ewers 1975, and McGinnis 2010, 2012.

³⁰ This lack of variation in strategic outcomes is extremely important for facilitating analysis of the data. If the U.S. Army had won some conflicts and lost others, for instance, then we would need to stratify the sample in order to interpret variables like the campaign's duration or loss rates: being able to hold out for a longer period of time in a losing campaign is generally an indication of capability and resolve, but by contrast, when a combatant *wins* a war, then shorter and less costly wars are more indicative of their military potential.

³¹ The Correlates of War Project: Sarkees and Wayman 2012.

³² Lyall and Wilson 2009 (see the discussion in Chapter 3).

with another case (the Costa Rican Revolution of 1948) in which a few thousand rebels toppled a Latin American regime in less than two months. In relation to these bodies of evidence, the American Indian Wars offer a much more comparable cross-section of cases, which facilitates identifying meaningful empirical patterns.³³

To be specific, this analysis draws on original data that aim to capture a reasonably comprehensive sample of armed encounters between the United States (or, prior to independence, the British) and each tribe, including anything from scattered raids to major battles. In all, there are roughly 3,000 engagements in the data set, based on roughly 5,000 event reports. Across these engagements, the United States inflicted more than 33,000 recorded casualties on 114 different tribes. The appendix to this chapter provides more detail on how these data were gathered and collected, and Figure 4.2 contains summary information.³⁴

These data provide an opportunity to take military commanders and their statements about how long it might take or how much it might cost in order to defeat their opponents – such as those laid out in the historical overview above – and then compare these expectations to a relatively large body of objective, empirical evidence. Rarely do data on armed conflict make it possible to evaluate beliefs about military strategy in this manner. To give a contrasting example,

³³ See Friedman 2013a for a discussion of how the American Indian Wars offer additional methodological advantages to scholars conducting empirical research on the dynamics of armed conflict.

³⁴ See the Appendix and Friedman 2013b for more information on the data and a discussion of their comprehensiveness. The latter work estimates that these data capture roughly half of the total casualties inflicted on Native American forces during the American Indian Wars, along with perhaps ninety percent of casualties inflicted on U.S. forces.

For standards of comparison, these tribes had a combined total of roughly 400,000 people based on population estimates corresponding to the time when each tribe entered armed contact with the U.S./British; most treatments assume that roughly one-quarter to one-fifth of a tribe would have consisted of military-aged males.

DATA ON THE AMERICAN INDIAN WARS

Temporal coverage: 1607-1890 (founding of Jamestown to Battle of Wounded Knee)

Unit of analysis: violent engagements between U.S. and Native American forces

Sources: Axelrod, *Chronicle of the Indian Wars* (1993), Michno, *Encyclopedia of Indian Wars* (2003), Michno and Michno, *Forgotten Fights* (2008), Nunnally, *American Indian Wars* (2007), Ratjar, *Indian War Sites* (1999), Webb, *Chronological List of Engagements* (1939), U.S. Army Adjutant General, *Chronological List of Actions, &c., with Indians* (1890s), U.S. Army War College, *Compilation of Indian Engagements* (1925).

Independent events recorded in the data: 2,958

Total recorded Native American casualties: 33,207

Total recorded U.S. casualties: 18,044

Tribes enumerated in the data set: 114

See the chapter's appendix for more details.

TIME SERIES OF RECORDED CASUALTIES

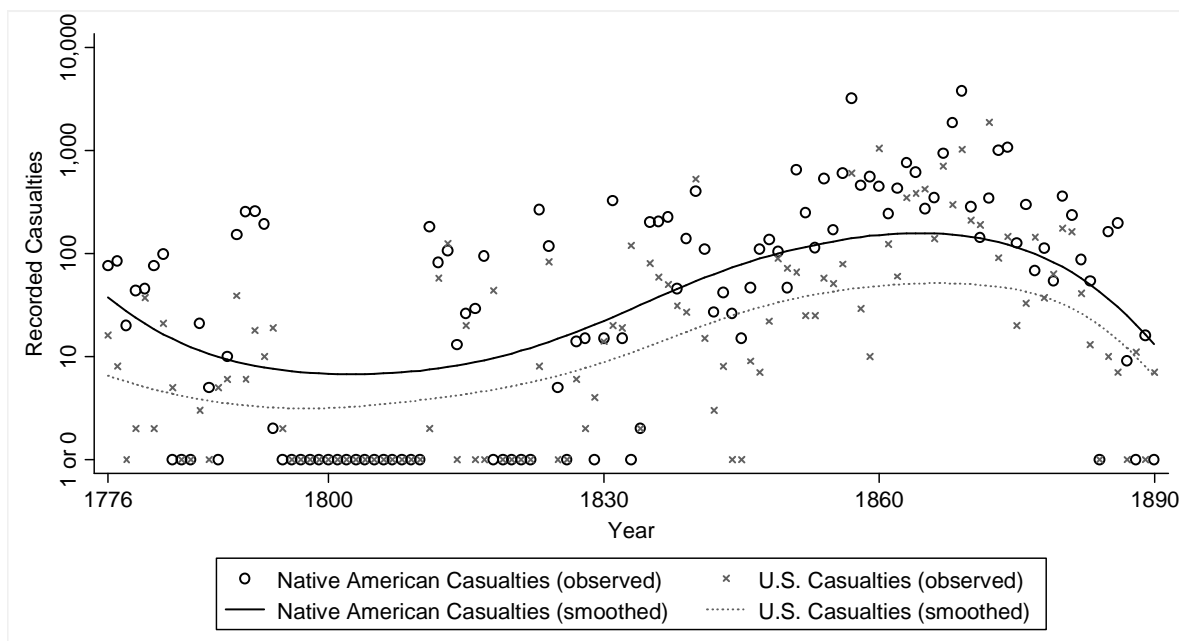


Figure 4.2. Overview of the Data

recent literature about “what works” in counterinsurgency and what that means for informing U.S. military strategy in Iraq and Afghanistan almost always runs up against contentious arguments about the relevant case universe for comparison.³⁵ Similarly, scholarly work on intelligence analysis generally assumes that it is impossible to test the accuracy of political predictions because they often deal with phenomena that are sufficiently unique that it is impossible, even *ex post*, to critique probabilistic assessments objectively.³⁶ Within the field of strategic studies, the American Indian Wars provide rare leverage for addressing this inferential problem, and the following section demonstrates how this provides a new (and potentially surprising) perspective on U.S. military behavior.

Section 4.3. Forming and revising expectations in the American Indian Wars

For instance, there is little doubt *ex post* that Custer, Crook, Sheridan and other U.S. commanders underestimated Sitting Bull’s forces in 1876. *Ex ante*, however, their belief that most tribes would not fight protracted wars was supported by historical experience. Figure 4.3 shows this by presenting the 114 tribes in order from fewest to most recorded casualties

³⁵ For example, many analysts strongly disagree about which cases are relevant for thinking about counterinsurgency in Iraq and Afghanistan. Krepinevich 2005 and Laird 2005 drew insights from the insurgency in Vietnam, while Biddle 2006 argued that Vietnam’s “Maoist Peoples’ War” dynamic was inapplicable to Iraq, and he structured his own recommendations for U.S. policy around cases of “communal civil war” such as the ethnic cleansing campaigns in the Balkans. Sepp 2005 provides a list of 48 “relevant” cases for establishing best practices in counterinsurgency, all of which come from the 20th century. Luttwak 2007 and Peters 2007 base their own analyses of counterinsurgency in part on the experience of the Romans under Caesar.

³⁶ Rieber 2004 surveys the relevant literature (see also Gentry 2010). Tetlock 2005 provides a broader study of calibration and discrimination in political and economic analysis. See Betts 2000 on the difficulty of separating *ex ante* and *ex post* analysis in strategic studies.

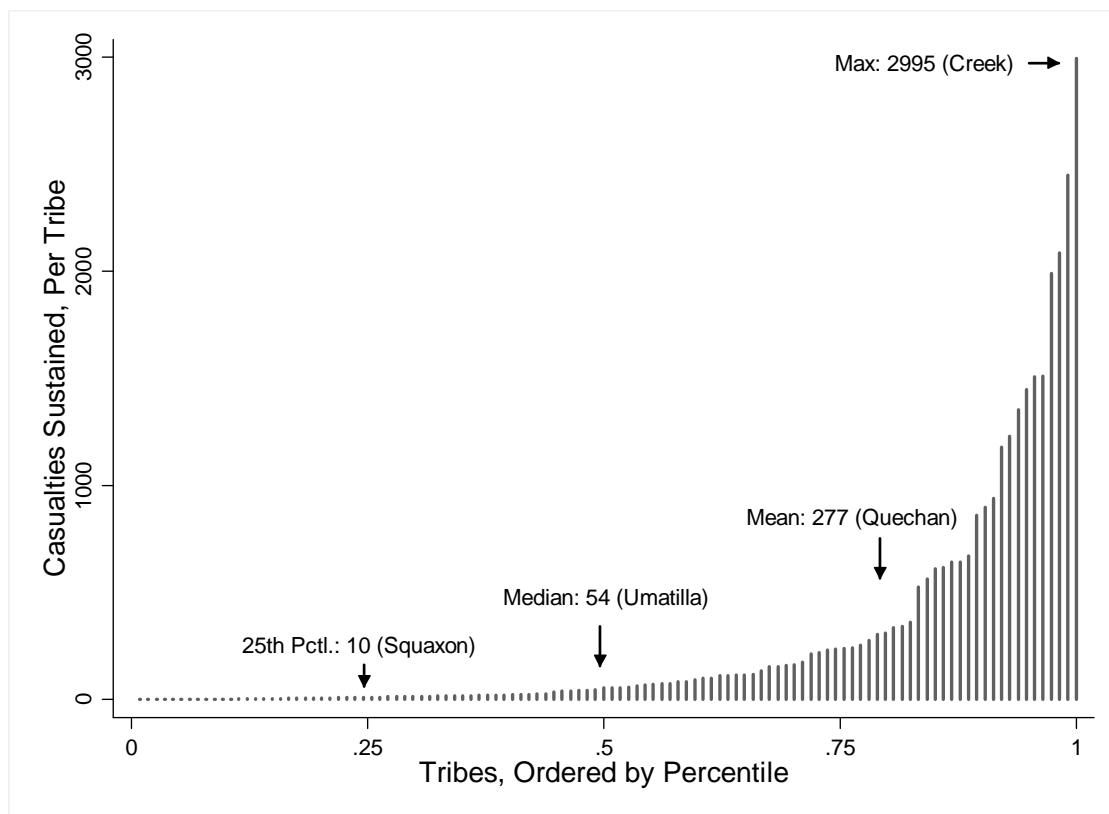
sustained.³⁷ These data place the Great Sioux War into a useful perspective. The right side of Figure 4.3 indicates that a few tribes (such as the Sioux, Seminoles, and Creeks) were indeed able to continue fighting despite sustaining relatively large numbers of casualties. But Figure 4.3 also demonstrates how these tribes are outliers. Sixty-nine tribes (61 percent) sustained fewer than 100 recorded combat casualties. The median tribe in these data lost only 54 members in battle. Thus when U.S. Army commanders such as Custer and Sheridan asserted that the tribes could not withstand large amounts of coercive punishment, they were, in expectation, correct.³⁸

The data offer further insight into the context of the Great Sioux War. For example, George Custer is often criticized for dividing his forces in assaulting Sitting Bull's camp, leaving his regiment unnecessarily vulnerable. But it was extremely rare for this sort of vulnerability to be exploited in the manner of Little Bighorn. Of the 2,958 engagements in the data set, only thirteen caused more U.S. or British battle deaths than Custer's Last Stand. Only four of these engagements involved tribes who did not fight alongside European regulars; of this subset, only one had occurred in the nineteenth century, and the majority of casualties there were civilians.³⁹

³⁷ In order to interpret these figures, note that the mean population of each tribe in the data set is roughly 3,000, with a standard deviation of 4,000. Rescaling this figure with per capita casualties on the y-axis produces results that are substantively similar, but much less precise due to the difficulty of estimating tribal populations. Moreover, aggregate counts are more conceptually appropriate here. When commanders stated their expectations that these conflicts would be relatively brief and cheap, this is inherently a statement about the aggregate costs of fighting.

³⁸ Friedman 2013a enumerates an additional 53 tribes who are not recorded as having engaged in any organized fighting with U.S. or British forces. It is unclear how many of those tribes belong within the scope of the analysis here, but surely some of them were relocated through the use of military coercion, even if this did not ultimately result in the use of violence.

³⁹ This was the 1813 Fort Mims Massacre during the Creek War (Nunnally 2007: 66).



0-10 casualties: Achumawi, Cahuilla, Catawba, Cayuga, Chetco, Chickasaw, Chinook, Choctaw, Haida, Havasupai, Kansa, Kichai, Koasati, Luiseno, Menominee, Miami, Nanticoke, Niantic, Nipissing, Pamunkey, Ponca, Puyallup, Squaxon, Taos, Tonkawa, Tututni, Weanock, Winnebago, Yuchi, Yuki.

11-50 casualties: Arikara, Atsina, Cayuse, Chippewa, Coeur d'Alene, Crow, Hupa, Kalispel, Kitanemuk, Klamath, Mingo, Mohawk, Nipmuc, Osage, Pawnee, Potawatomi, Seneca, Spokane, Susquehannock, Tahltan, Tenino, Wallawalla, Wichita, Wintu, Wyandot, Yakima.

51-100 casualties: Caddo, Coquille, Fox, Gosiute, Jicarilla Apache, Modoc, Nisqually, Nomlacki, Palouse, Quapaw, Shasta, Umatilla, Waco, Yokut.

101-200 casualties: Abenaki, Arapaho, Karankawa, Lipan Apache, Nez Perce, Pocumtuck, Pomo, Sauk, Tolowa, Yamasee, Yankton Sioux.

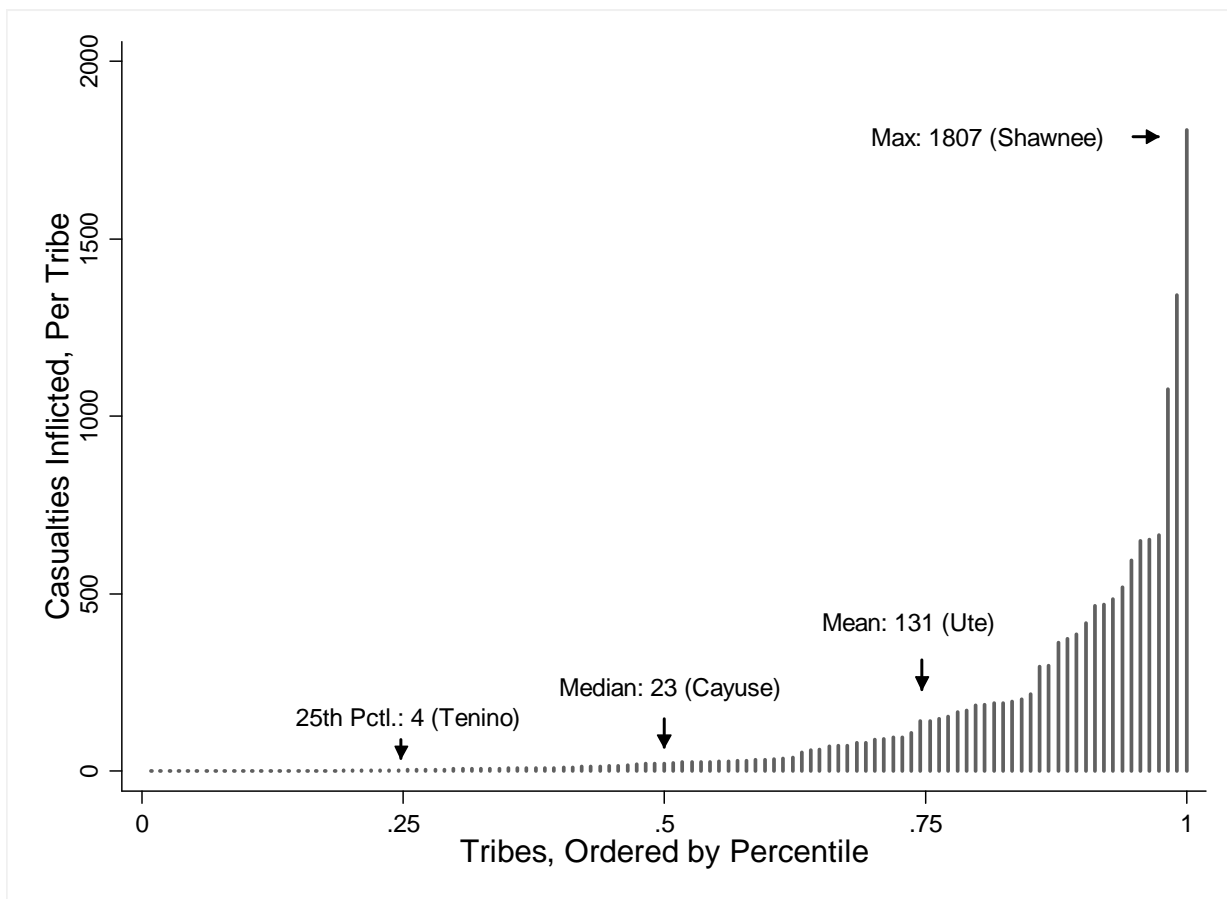
201-500 casualties: Blackfeet, Delaware, Kickapoo, Mescalero Apache, Mohave, Powhatan, Quechan, Shawnee, Takelma, Ute, Walapai, Wiyot, Yavapai.

501-1000 casualties: Apalachee, Bannock, Cherokee, Kiowa, Navajo, Ottawa, Pequot, Seminole, Wampanoag, other Apache bands.

1001-2000 casualties: Cheyenne, Chiricahua Apache, Narragansett, Paiute, Teton Sioux, Tuscarora, Western Apache.

2001-3000 casualties: Comanche, Creek, Santee Sioux.

Figure 4.3. Recorded Casualties Sustained by 114 Tribes during the American Indian Wars



0-10 casualties: Achumawi, Chetco, Chickasaw, Chinook, Choctaw, Coeur d'Alene, Crow, Haida, Havasupai, Hupa, Kalispel, Kansa, Karankawa, Kichai, Klamath, Koasati, Luiseno, Nanticoke, Niantic, Nipissing, Nisqually, Nomlacki, Palouse, Pawnee, Pomo, Ponca, Puyallup, Quapaw, Spokane, Squaxon, Susquehannock, Tahltan, Tenino, Tolowa, Tonkawa, Tutuni, Umatilla, Walapai, Wallawalla, Wichita, Wintun, Wiyot, Yuchi, Yuki.

11-50 casualties: Apalachee, Arikara, Atsina, Blackfeet, Caddo, Cahuilla, Catawba, Cayuse, Coquille, Fox, Gosiute, Lipan Apache, Menominee, Modoc, Mohave, Navajo, Osage, Pequot, Quechan, Shasta, Taos, Waco, Yakima, Yankton Sioux, Yavapai, Yokut.

51-100 casualties: Arapaho, Cayuga, Jicarilla Apache, Mescalero Apache, Mingo, Pamunkey, Pocumtuck, Sauk, Wampanoag, Weanock, Western Apache, other Apache bands.

101-200 casualties: Bannock, Chippewa, Kiowa, Miami, Narragansett, Nez Perce, Seneca, Takelma, Ute, Winnebago, Wyandot, Yamasee.

201-500 casualties: Cheyenne, Chiricahua Apache, Comanche, Delaware, Kickapoo, Mohawk, Nipmuc, Paiute, Powhatan, Santee Sioux, Tuscarora.

501-1000 casualties: Cherokee, Creek, Ottawa, Seminole, Teton Sioux.

1001-2000 casualties: Abenaki, Potawatomi, Shawnee.

Figure 4.4. Recorded Casualties Inflicted by 114 Tribes during the American Indian Wars

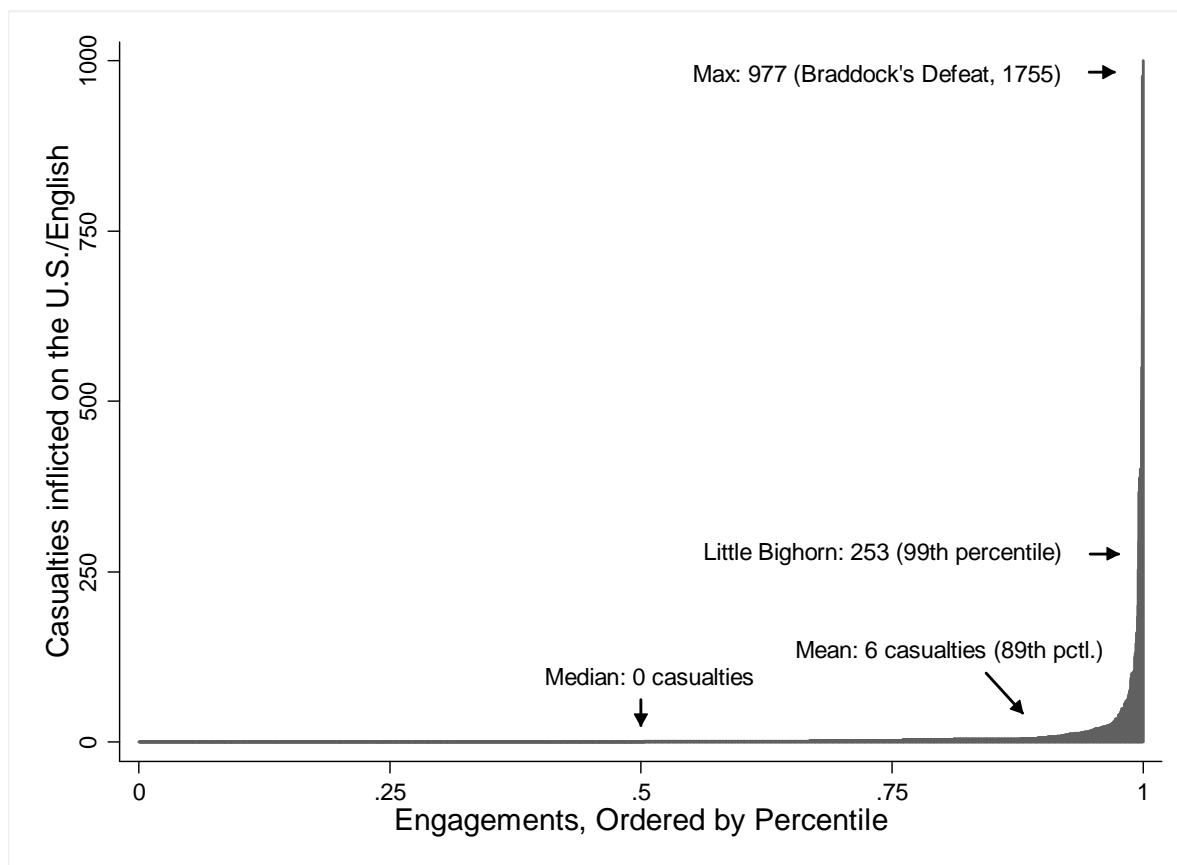


Figure 4.5. Recorded Casualties Inflicted by Tribes per Engagement

Figure 4.5 orders the 2,958 engagements in the data set from fewest to most recorded casualties inflicted on U.S. or British forces. This makes clear how the vast majority of armed engagements with the tribes resulted in relatively few casualties for the United States or the British – Custer's defeat at Little Bighorn is in the 99th percentile, and the median number of recorded U.S./British casualties per engagement is zero.

Custer, for his part, had participated in six previous engagements against the Sioux and Cheyennes. Across these engagements, Custer had lost a total of 27 men killed in action, an order of magnitude less than what his regiment lost in a single day at Little Bighorn. Custer's previous experience included an attack on a Cheyenne village on the Washita in 1868, which many historians believe was the model for the assault at Little Bighorn. Custer had been criticized for taking too many risks in this attack, too, but it had still succeeded at a cost of only 21 U.S. combat deaths. Custer's regiment, the Seventh U.S. Cavalry, had participated in 41 recorded engagements in the decade since it was formed. Most of these engagements involved Sioux or Cheyennes, and they had generated a total of just 43 U.S. combat deaths. So when Custer arrived at Little Bighorn, past experience overwhelmingly suggested that his opponents would not stand, fight, and exploit his vulnerabilities.

Figure 4.4 demonstrates this pattern more broadly, showing how the distribution of casualties inflicted *by* Native American tribes is similar to the distribution of casualties inflicted *on* Native American tribes: it is skewed and it has a few prominent outliers, but most tribes caused relatively little damage to U.S. or British forces. The mean tribe in the data set inflicted 131 recorded casualties on its opponents, and the median is just 23. Keep in mind, moreover, that these numbers describe the total casualties inflicted by each tribe across the entire data set, while Little Bighorn was a single battle. Thus if we instead examine the 3,000 individual *engagements* on file (as shown in Figure 4.5) then the average frontier fight led to six U.S./British casualties, with the median engagement generating no recorded casualties at all. These measures help to place in proper perspective how the fight at Little Bighorn was truly a radical outlier.

In order to predict the extraordinary performance of Sitting Bull and Crazy Horse's followers, U.S. commanders would thus have needed some way of predicting how extensively they differed

from the average tribe. (And even from the way that the Sioux and Cheyenne had themselves fought in the past.⁴⁰) This would naturally have been quite difficult. At the time – and this was true in most of the American Indian Wars – U.S. Army commanders possessed little information on the specific tribes that they were fighting. In most cases, the central challenge was simply *finding* the tribe in question, to say nothing of assessing the complex military, social, and political dynamics underlying its resiliency and combat effectiveness. In 1876, these dynamics would have been especially difficult to pin down. The campaign took place at the same time that thousands of Sioux left their reservations for the annual hunting season. Determining just how many of these “Summer Roamers” intended to fight with Sitting Bull was severely challenging. Intelligence estimates of Sitting Bull’s force size varied widely, and according to one staff officer, they were “wildly conjectural.”⁴¹

One way in which organizational factors could have hindered assessments of the Sioux and Cheyennes in 1876 was that commanders did not have the benefit of institutional wisdom in sizing up their adversaries. The U.S. Army did not release military doctrine for fighting the tribes, and West Point generally did not include the subject in its curriculum.⁴² At the same time, even if there *were* more institutional wisdom on frontier warfare, it is not clear what lessons it

⁴⁰ Prior to the Battle of Little Bighorn, the data record Teton Sioux and Cheyenne forces inflicting an average of 3.7 combat deaths over 309 total engagements. Other than Little Bighorn there were just two engagements where these tribes inflicted more than 25 battle casualties on the U.S./British: the 1854 Grattan Fight (29 casualties) and the 1866 Fetterman Fight (79 casualties).

⁴¹ The quote is from Bourke 1891: 246. See Gray 1976 on the “Summer Roamers.” In *Report of Lieut. Gen. P. H. Sheridan*, Sheridan wrote that he initially believed his opponents comprised a small band of 30-40 lodges, with roughly 70 warriors under Sitting Bull and roughly 200 warriors under Crazy Horse.

⁴² See Utley 1973: chapter 3, Wooster 1988: 39, 143, 211, and Birtle 1998: 10, 13, 61.

Table 4.1. Time, Population, and Region as Predictors of Tribes' Casualty Sustainment

	<i>Hazard Ratio</i>	<i>Std. Err.</i>	<i>p</i>	<i>95% Conf. Int.</i>	
Year of 1 st Conflict	1.011	0.003	0.00	[1.00	1.02]
Tribe's Population	1.000 [†]	0.000	0.07	[1.00	1.00]
<i>Region</i>					
California	0.832	0.332	0.65	[0.38	1.82]
Great Basin	0.367	0.177	0.04	[0.14	0.95]
Northeast	1.898	0.965	0.21	[0.70	5.14]
Northwest	0.692	0.343	0.46	[0.26	1.83]
Plains	0.728	0.277	0.40	[0.34	1.54]
Southeast	0.876	0.525	0.83	[0.27	2.84]
Southwest	0.362	0.135	0.01	[0.17	0.75]
Dummy variable for Plateau region dropped					
† Coefficient and standard error bounds slightly below 1				N=114	

Table 4.2. Time, Population, and Region as Predictors of Tribes' Casualty Infliction

	<i>Hazard Ratio</i>	<i>Std. Err.</i>	<i>p</i>	<i>95% Conf. Int.</i>	
Year of 1 st Conflict	1.002	0.003	0.46	[1.00	1.02]
Tribe's Population	1.000 [†]	0.000	0.00	[1.00	1.00]
<i>Region</i>					
California	2.342	1.008	0.05	[1.01	5.44]
Great Basin	0.543	0.252	0.19	[0.22	1.35]
Northeast	0.207	0.108	0.00	[0.07	0.58]
Northwest	0.723	0.344	0.50	[0.28	1.84]
Plains	0.567	0.227	0.16	[0.26	1.24]
Southeast	0.265	0.162	0.03	[0.08	0.88]
Southwest	0.541	0.201	0.10	[0.26	1.12]
Dummy variable for Plateau region dropped					
† Coefficient and standard error bounds slightly below 1				N=114	

Tables 4.1 and 4.2 use Cox proportional hazard models to examine whether there are any consistent relationships between time, population, geography, and the tribes' military behavior. If these relationships are estimated using ordinary least squares regression, then even fewer of them appear to be statistically significant. Ordinary least squares regression models are not appropriate for modeling cumulative dependent variables like casualty counts, but they demonstrate how the basic findings of Tables 4.1 and 4.2 are not simply being driven by model choice. Negative binomial regressions for predicting event counts also do not return substantively different results.

would have drawn, and how these would have helped to predict the behavior of the Sioux, the Cheyennes, or any other tribe *ex ante*. Tables 4.1 and 4.2, for instance, examine how the tribes' sustainment and infliction of recorded casualties varied by time, region, and population.⁴³ These tables show that tribes in the Great Basin and the Southwest sustained more casualties than the norm; tribes in the Northeast and Southeast inflicted more casualties than the norm while tribes in California dealt less damage to U.S. and British soldiers, on average and all else being equal. But no broad lessons present themselves here that might have indicated that the Sioux would have been especially problematic in 1876. Indian Wars were actually becoming *less* violent, on a per-tribe basis, over time. Larger tribes were able to sustain and inflict more casualties on balance, but only marginally so.⁴⁴ Plains tribes were not consistently more violent than the norm, as shown by the wide confidence bands around the estimated hazard ratios for this variable.⁴⁵

⁴³ Tables 4.1 and 4.2 represent the output of survival models, where the dependent variable is the number of casualties that each tribe was able to sustain or inflict in fighting the U.S. or British Armies. Survival models are appropriate tools for this analysis because casualty counts are cumulative, and because the quantity that these models are estimating is how far these counts could progress before the tribes were unable to resist any further. Thus the "duration" variable in the survival models captures the number of casualties inflicted on or by each tribe, and the moment of "failure" is when the war ends. In cases where a tribe received or inflicted 0 casualties, this value was replaced with a coding of 1 – otherwise, the statistical model cannot interpret the observation.

⁴⁴ The hazard ratios on tribal population as a predictor of casualty sustainment and infliction are 0.99995 and 0.99991, respectively. Cox proportional hazard models measure relative risk, and the standard deviation for the population of tribes in the data set is 4,175. A standard deviation increase in a tribe's population would thus correspond to decreasing the hazard rate on casualty sustainment and infliction by roughly one-third, an effect size substantially smaller than the hazard ratios on a majority of the regional dummies in Tables 4.1 and 4.2.

⁴⁵ One of Crook's aides reports a moment where he at one point directly considered regional factors in forming his strategic assessments. Comparing the Sioux to the Apaches (a southwestern tribe Crook had recently fought against), Crook "expressed himself freely in regard to the coming campaign," saying that "while the Sioux and Cheyenne were brave and bold people they would never stand punishment as the Apaches had done. The tribes of the plains had accumulated too much property in ponies and other things, and the loss of that would be felt

These data therefore suggest it may often have been perfectly reasonable for commanders like Custer or Scott to enter conflicts with the perception that it would not be particularly costly to achieve their objectives – though there were many instances in which those perceptions proved to be false, *ex ante* they were supported by substantial empirical evidence. And as the previous chapter demonstrated, if military decision makers enter conflicts with the belief that they can obtain their objectives at reasonable expense, we should not necessarily expect them to become more any pessimistic as those conflicts drag on.

This is another claim that we can directly evaluate in the context of the American Indian Wars, because with the empirical distribution of tribal “types” in hand, we can plug this information back into the theoretical framework from Chapter 3 in order to examine the way that commanders updated their expectations about the resilience of the opponents they were facing. This is the purpose of Figure 4.6: the shaded area in the figure represents the epanechnikov kernel density for how many casualties a tribe could sustain. The lines in Figure 4.6 capture the way that Expected Total Cost and Expected Remaining Cost would have varied along this distribution. In other words, Figure 4.6 explains how, conditional on already inflicting some number of casualties on a tribe, decision makers might have updated their expectations about how much *more* coercive punishment that tribe might be able to sustain.

Because Figure 4.6 is dealing with real data, the patterns here are neither smooth nor monotonic. Nevertheless, they provide a useful lens through which to view U.S. military behavior in the Second Seminole War. In particular, Figure 4.6 shows why it might not be so

most deeply” (Bourke 1891: 286). Table 4.1 shows that Crook’s reasoning here was correct in a general sense: Plains tribes were, on the whole, less casualty-resistant than tribes in the Southwest.

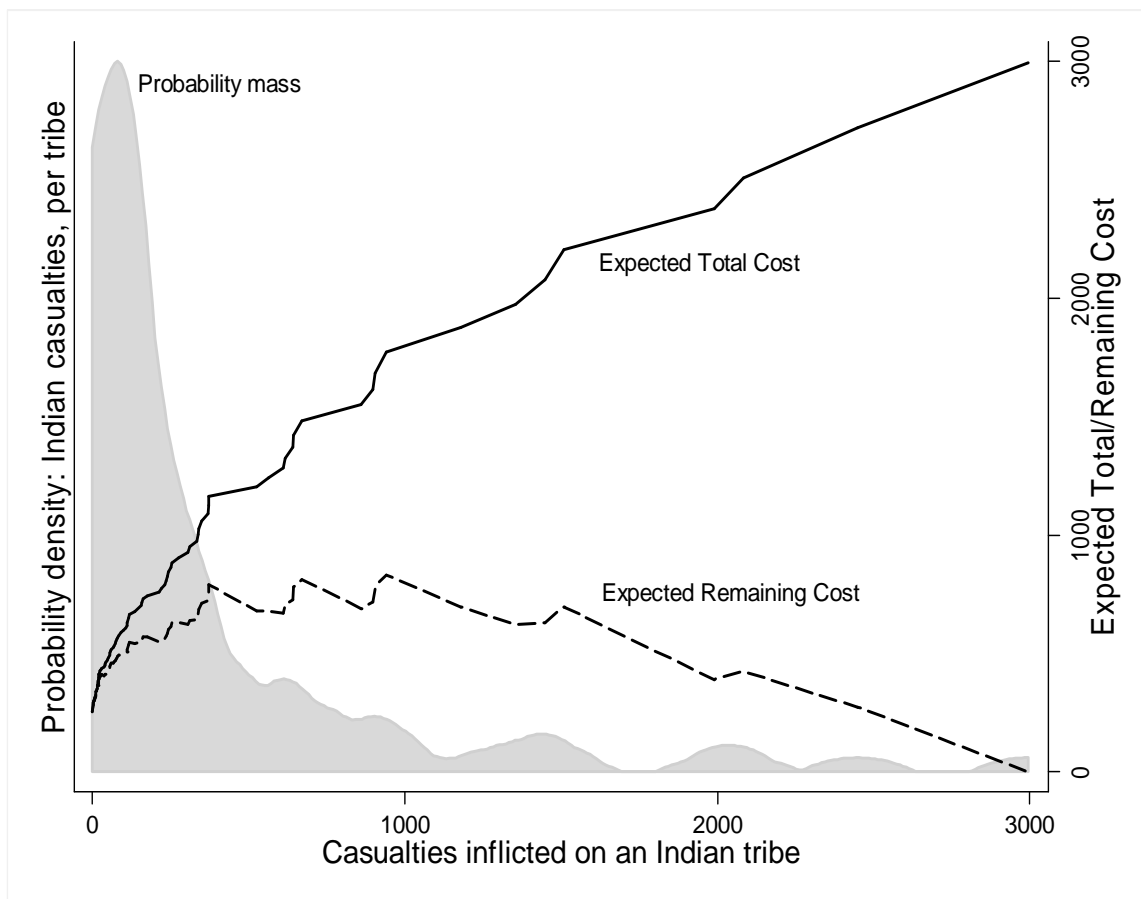


Figure 4.6. Expected Total/Remaining Cost Progression during the American Indian Wars

Figure 4.6 combines the empirical distribution of tribal “types” (shaded in gray, representing the proportion of tribes that conceded after sustaining a certain number of casualties) with the theoretical framework from the previous section. Based on this distribution, the figure shows how many casualties a randomly-chosen tribe could sustain, conditional on already conducting a certain amount of fighting. The main takeaway from Figure 4.6 is that, across most of the distribution, Expected Remaining Cost does not increase. This is in contrast to standard theoretical models, which predict that military decision makers should become more pessimistic about their future prospects as they fail to defeat their opponents.

surprising that these commanders did not become more pessimistic about their prospects as the war continued. In fact, across much of Figure 4.6, Expected Remaining Cost either declines or stays relatively constant.

Of course, this is not a descriptive model of how any decision makers actually formed their views, and these data alone cannot say whether it was reasonable for commanders ever to think that one more large battle could have caused the Seminoles (or any other tribe) to concede.⁴⁶ But at the very least they help to show that this expectation would not necessarily have become any more implausible as the war progressed, even though prominent theoretical frameworks and historical scholarship have argued the very opposite. This contrast demonstrates the way that the concepts in this paper diverge from the existing literature, and where they can potentially shed light on salient experience.

⁴⁶ Though it is important to point out that there were plenty of reasons to believe, *ex ante*, that Scott's column-and-detachment strategy might work. For instance, while one of the war's most prominent historians criticized the idea for being "somewhat European," he also mentions that Andrew Jackson had used a similar strategy in the *First Seminole War* (1817-18), which was widely viewed as an overwhelming success (Sprague 1848: 145-146). Jackson had also been able to decimate the resistance of the Creeks – a neighboring tribe that was closely related to the Seminoles – in the 1814 Battle of Horseshoe Bend, a conventional fight which led to the death or capture of roughly one thousand Indians and brought the war to a rapid close (Nunnally 2007: 68). In fact, the use of converging columns to strike enemy camps was one of the only tactics that commanders widely believed to be effective in fighting hostile tribes (Wooster 1988: 212). So while one of Scott's biographers criticized the general for using "Napoleonic tactics," he also concedes that the plan was "not without merit." (Peskin 2003: 92.) Another Scott biographer writes that "from the strategic point of view [the plan] was a perfectly sound one" and that its failure was less a matter of the concept, and more due to a failure of its execution (Elliott 1937: 299-300).

Section 4.4. Connections to broader scholarship

A popular adage states that “the definition of insanity is doing the same thing over and over again and expecting different results.” It is a catchy phrase that repeatedly recurs in debates about military strategy – but its logic is based on the dynamics of slot machines and has little to do with the actual analytic challenges that military decision makers face. For in many cases, it would also be inappropriate to abandon a course of action just because it does not succeed immediately. Rational behavior lies somewhere between the extremes, but it can be difficult to say exactly where. The theoretical discussion in Chapter 3 combined with the empirical discussion here have aimed to show that conventional judgments of strategic assessment may be in need of further analysis themselves.

It is frustrating when military strategies do not succeed quickly, or when decision makers do not seem to be changing their policies or their expectations as a result. Military leaders who do not abandon unsuccessful strategies are often portrayed as being in a state of denial or under the influence of other nonrational factors. U.S. commanders during the American Indian Wars are generally thought to fit this mold. But this chapter showed why, in contrast, why might actually *expect* these decision makers to have remained optimistic about their chosen strategies, even as they did not succeed. There is no *prima facie* reason to presume that this behavior is irrational – that kind of presumption is common in historical analysis, political debate, and theoretical scholarship, but it is not necessarily sound.

To be clear, this chapter did not provide a critical test of one theory versus another; its goal was to show how this dissertation’s theoretical framework for connecting cumulative dynamics to strategic assessment plausibly explained military behavior in the “least-likely” context of the

American Indian Wars, rather than to assign causal weight to those dynamics in relation to other factors like organizational behavior, domestic politics, or psychology. A chapter that intended to distinguish the explanatory power of rival theories would look very different than this one: it would need to focus the majority of its attention on deductively identifying observable factors that would help to discriminate among these mechanisms in specific cases, and then conduct an empirical search for which of those factors indeed exist. Yet to the extent that it is difficult to distinguish just how much explanatory power to assign to the genuine analytic challenges of assessing cumulative dynamics in relation to nonrational factors, this is *precisely* why we should not assume that those nonrational hold pride of place in explaining why decision makers often fail to realize and correct their strategic mistakes.

Perhaps the most fundamental implication of this exercise is that it therefore shows how the theoretical framework in this dissertation provides an opportunity to re-evaluate whether some forms of behavior are truly as irrational or as puzzling as they appear. U.S. Army commanders during the American Indian Wars such as George Custer and Winfield Scott are good examples, as they are widely believed to have acted in a manner largely influenced by organizational inertia, cultural prejudice, or personal hubris. Yet this chapter provided both theoretical and empirical reasons to question those judgments, suggesting that many strategic decisions we consider faulty *ex post* may have been reasonably justified *ex ante*. George Custer could doubtlessly have been more careful in planning his attack on Little Bighorn, but it would have strained credulity had he presented a judgment to General Sheridan that he expected the Sioux and Cheyennes to kill more than two hundred and fifty of his cavalymen in a single afternoon. Similarly, it would have seemed ludicrous if General Scott had predicted at the outset of the Second Seminole War that his opponents would continue to fight after nine-tenths of their tribe

had been killed or captured. Because these cases were so extreme, they would have been inherently difficult to understand and predict. And as we have seen throughout this dissertation, the cumulative dynamics of warfare can make it difficult for decision makers to adapt their prior assumptions in a way that allows them to realize the extent of these initial misperceptions, no matter how rational they may be.

With that said, it is important to point out that cumulative dynamics are properly viewed as a complement – and not a substitute – for alternative explanations. Commanders like Custer and Scott were human and fallible, and they operated under the direction of a War Department and a Congress whose decisions were surely influenced by nonmilitary considerations. These factors undoubtedly played important roles in structuring U.S. policy during the American Indian Wars, just as they undoubtedly affected U.S. decision making in Iraq, Vietnam, and other conflicts. And it actually becomes *more* likely that these factors could have played decisive roles in these cases if it turns out that this apparently puzzling behavior is much closer to rationality than what is commonly believed. Just as “trembles” are more important in game theory when it is easier to divert players onto a nonequilibrium path, strategic assessment and military decision making should become more sensitive to nonrational factors when the analytic problems they engage are genuinely hard.⁴⁷ And ultimately, it is not possible to evaluate whether or not some leader, organization or state is acting rationally without having a sense of what rational behavior would

⁴⁷ For a similar line of reasoning, see Fearon 1995: 409: “a better understanding of what the assumption of rationality really implies... may actually raise our estimate of the importance of particular irrational and second-image factors.”

actually entail – from a theoretical standpoint, the fundamental purpose of this dissertation is to help rethink and re-establish that baseline.⁴⁸

The misleading nature of conditional perceptions

From an empirical standpoint, another broad implication of this chapter is the importance of assessing decision making in light of the full array of relevant past experience. The data used in this chapter were informative largely because they contained information on *all* of the American Indian Wars, regardless of their intensity.

Most data sets on inter- and intra-state conflict, however, are left-censored, as many of them examine only cases which meet a threshold of causing 1,000 battle deaths. This can give the impression that certain kinds of armed conflicts tend to be more violent than they really are. Figure 4.7 helps to demonstrate this point by presenting information on battle deaths in intra-state conflicts published by the Peace Research Institute in Oslo.⁴⁹ These data are revealing because their threshold for inclusion is only 25 battle deaths. As Figure 4.7 shows, more than half of the conflicts recorded in these data (122 or 51 percent) caused fewer than 1,000 total battle deaths. The *median conflict* is thus censored out of data sets that impose a 1,000 battle-death threshold for inclusion. By relaxing this threshold from 1,000 to 25, the median number of casualties inflicted in internal wars between 1946-2008 drops by nearly an order of magnitude, from roughly 9,000 to 1,000; the mean drops by half, from roughly 37,000 to 19,000.

⁴⁸ See Glaser 2010: 2-3: “theories of suboptimal behavior, whether built on arguments about domestic politics or errors in individual decision making, rely at least implicitly on a rational theory.”

⁴⁹ Lacina and Gleditsch 2008.

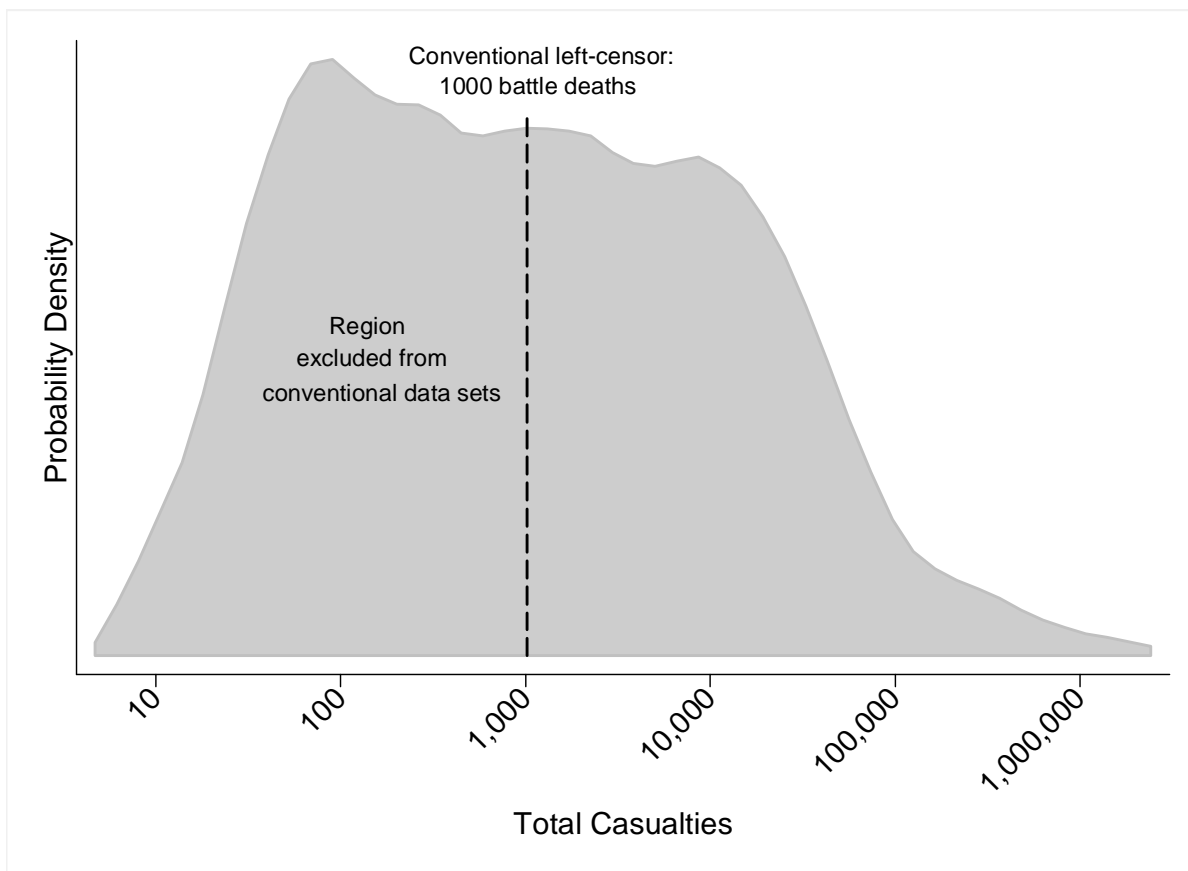


Figure 4.7. What Conventional Casualty Thresholds Exclude

Figure 4.7 presents the kernel density for total casualties across 240 intrastate conflicts from 1946-2008 (data from Lacina and Gleditsch 2005). The figure shows how conventional data sets that exclude conflicts causing fewer than 1,000 casualties censor out a substantial portion (and in fact the slight majority) of recorded experience, leading to misleading inferences about how long these wars typically take and how much they typically cost.

Broader perceptions of armed conflict may often be biased in much the same way. Take another look, for instance, at the tribes listed in Figure 4.3. Readers will presumably be familiar with almost all of the groups who fought protracted wars (such as the Creeks, Seminoles, and Sioux) but almost none of the tribes (such as the Atsinas, Nisquallys, and Umatillas) that did not.

Yet these less violent tribes were far more common. They constituted the bulk of the opponents that a U.S. commander in the frontier army would have faced, and this kind of context is important for evaluating these commanders' prior assumptions. Yet historical memory tends to be censored just like scholars' data sets, focusing on the cases that are salient but not necessarily representative.⁵⁰

Just as understanding the full distribution of American Indian Wars was important for understanding why U.S. Army commanders found it difficult to foresee and adapt to extreme cases like the Great Sioux and Second Seminole Wars, understanding the full distribution of armed conflicts is important for framing judgment of military decision making elsewhere. The Vietnam War was such a formative experience in U.S. military history, for instance, that it is easy to forget just how unusual the case was. Communist forces in Vietnam sustained roughly one million casualties in fighting the French and the United States, giving them perhaps the highest threshold for casualty tolerance (at least in absolute terms) for any insurgent movement in the last century.⁵¹ Similarly, a vast amount of research in international relations focuses on decisions and military planning leading up to the World Wars. Because these experiences are so important in their own right, they deserve extensive study. But in conducting those studies, it is important to keep in mind that they are outside the norm and thus in many ways at odds with

⁵⁰ For comparison: the Correlates of War data on intra- or extra-state conflict do not include any tribes besides the Sioux; Lyall and Wilson's data on insurgencies contains observations of eight Native American tribes; the data used in this chapter indicate that at least 114 different tribes fought against U.S. or British forces.

⁵¹ Some data sets estimate that Afghan forces sustained more losses in fighting the Soviet occupation and that Yugoslavs sustained more losses in resisting the Nazis, but in both instances, it is difficult to separate military and civilian casualties.

reasonable expectations.⁵² Rather than asking why decision makers did *not* understand the nature of these challenges, a more relevant question may be to ask how they *could* have understood them.⁵³ This chapter explains why that can be genuinely difficult.

Switching strategies

How then *can* decision makers deal with these difficulties? What then *might* drive decision makers to change their strategies when they do not seem to be succeeding? This will be the subject of the following chapter, and as a way of foreshadowing this discussion, it is useful to return briefly to the experiences of the Great Sioux and Second Seminole Wars.

After the U.S. military disaster at Little Bighorn, Army strategy changed course in short order. In a political climate that now favored making almost any expenditure to defeat Sitting Bull and stave off further embarrassment, Congress and the War Department quickly appropriated additional money and forces to the campaign. With these resources, General Sheridan constructed two new posts that allowed his units to exert territorial control over the Yellowstone area rather than sending detachments on prolonged excursions to find opposing forces and fight

⁵² See Boot 2013 for a recent argument about how defense analysts may seriously overrate the military potential of insurgencies by focusing their attention on outlying cases.

⁵³ Thomas Schelling made a similar argument in a roundtable on the lessons of Vietnam, writing: “I am still impressed by the enormous quantity of money, people, and technology that was poured into Vietnam, and by the fact that it did not do the job. If in 1964 one had added up what the United States was going to commit – everything from B-52s to the latest in fighter aircraft and ultimately ‘smart bombs’ and electronic fences – anybody would have said, ‘you don’t need sixty-four times as much as you think it will take.’ We committed enough to sink the country, and I think America slowly realized that it would always take more. One would have to go back and imagine 1964, when McNamara had his little pictures of barbed wire fences around those ‘pacified strategic hamlets,’ to realize how astounding it is in retrospect that all that material and all those people and all that money could not do the job” (Hoffmann et al. 1981: 9). John Mueller (1980) extends a similar argument in more detail.

them far from bases of support. In addition, the U.S. Army was given authority to administer the nearby Indian agencies, where it could control the Sioux's food supply and monitor population movements more effectively. As Sheridan reflected later, "It seems to have required some disaster like that which happened to Custer" before the U.S. Army could switch to a different strategy.⁵⁴ The Sioux and Cheyennes surrendered the following summer.

By contrast, the Second Seminole War never precipitated the kind of sharp, political crisis that could make the costs of occupying the Everglades seem worthwhile. Quite the opposite, one of the government's foremost priorities at the time was keeping the federal budget low.⁵⁵ In 1835, there were just seven thousand soldiers in the entire U.S. Army, spread across 53 posts nationwide.⁵⁶ This force was far too small to occupy Seminole territory, especially since much of this territory consisted of swampland that impeded movement and logistics. Congress did approve three marginal expansions of the Army during the Second Seminole War, but in each instance, the main effect was less to increase the number of soldiers in Florida than to replace militia with regular troops. Total manpower (regular and militia) was at 5,901 in 1836; it peaked at 8,411 in 1837, and then hovered between four and six thousand for the remainder of the war.⁵⁷ The cost of switching to a more manpower-intensive strategy was far beyond what Congress was willing to pay at the time – and as we saw earlier, military commanders and political leaders

⁵⁴ Division of the Missouri, Office of the Chief Engineer, *Annual Report of the Chief of Engineers for the Fiscal Year Ending June 30, 1876*.

⁵⁵ See Mahon 1967: 114 on this context for the Second Seminole War in particular, along with Utley 1967: 17, 71, Wooster 1988: 30, and Fehrenbach 1974: 379 on Congress's general reluctance to appropriate funds for the American Indian Wars.

⁵⁶ Mahon 1967: 116.

⁵⁷ Sprague 1848: 103.

continually maintained that they were on the verge of defeating the Seminole resistance anyhow – so the war continued to drag on with U.S. military strategy in stasis.

Unlike Custer's defeat at Little Bighorn in the Great Sioux War, the Second Seminole War thus offered no significant pivot point that caused decision makers to rethink the basic nature of the war they were fighting and whether their methods for doing so were appropriate. In the next chapter, we will see that similar issues play a prominent role in understanding U.S. policy during the occupation of Iraq, another context in which a war dragged on far longer than many people originally anticipated without seeming to cause any significant change in the way decision makers viewed the problem or how they sought to fix it.

Appendix. Data on the American Indian Wars

The data used in this chapter come from an event-level compilation of military engagements between Native Americans and U.S. or British forces that took place between the founding of Jamestown on May 14, 1607 and the Battle of Wounded Knee on December 29, 1890. These data track several variables listed below, along with the number of recorded casualties – defined as the number of people killed, captured, or mortally wounded – that occurred as a direct result of these engagements. The data include engagements within the continental United States or those that involved pursuits into neighboring territory (such as expeditions into Mexico to capture Geronimo). The analysis includes armed engagements of any intensity, ranging from small-scale raids to large battles. The data include engagements fought by regular and militia forces, and they include noncombatant casualties.

In some cases, it is ambiguous whether a particular engagement should be seen as occurring “between U.S. and Native American forces” (thus warranting inclusion within these data) as opposed to isolated interpersonal or intercommunal violence that was essentially nonpolitical in nature. Scholars disagree as to what constitutes “political violence” in general, and similar conceptual ambiguity applies to many aspects of fighting on the U.S. frontier. This study adopts an inductive approach to dealing with this issue by including information from a broad range of sources, and thus letting the sources “say” which engagements belong in the data. To the extent that there is disagreement here, the present study thus errs on the side of inclusiveness, while following the lead of the literature on which it aims to build.

The sources for this data collection effort comprise several anthologies that record violent engagements during the American Indian Wars. One of the most comprehensive is George W.

Webb's *Chronological List of Engagements between the Regular Army of the United States and Various Tribes of Hostile Indians* (1939) which lists 1,177 engagements between the United States Army and Native American tribes from 1790 to 1890. Webb's book is itself a compilation of two official records: the U.S. Army Adjutant General's *Chronological List of Actions, &c., with Indians* (printed in the early 1890s) and the U.S. Army War College Historical Section's *Compilation of Indian Engagements* (1925). These records are certainly incomplete and inaccurate in some respects, but they are generally well-regarded.⁵⁸

Several additional sources help to flesh out the data used in this analysis. Among these, perhaps the best-known is Gregory Michno's *Encyclopedia of Indian Wars* (2003), which describes 787 engagements occurring after 1850; Gregory and Susan Michno's follow-on work, *Forgotten Fights* (2008) adds another 334 engagements to this list dating to 1823. Alan Axelrod's *Chronicle of the Indian Wars* (1993) surveys 222 engagements after 1607. Though Axelrod's work contains the smallest collection of engagements used for constructing data here, it is still regarded as being relatively comprehensive, and it was the source which Spirling (2011) used to define the case universe of armed conflict between the United States and the Native Americans in a previous study published in the *American Journal of Political Science*. Steve Ratjar's *Indian War Sites* (1999) and Michael Nunnally's *American Indian Wars* (2007) also provide information spanning the full period of observation, covering 773 and 1,278 engagements, respectively. Each of these anthologies includes transparent sourcing; consistent

⁵⁸ See Peters 1966. In a related discussion, Michno 2003: 353 discusses the potential accuracy of government event reports. He writes: "[T]here is reason to be confident that the army estimates were reasonably accurate.... [T]he army lived by a strict code of honor, particularly in its official reports, and even if the unit leader tried to inflate numbers, he would face correction and perhaps ostracism by other soldier-witnesses." The appendix in Delay 2008 offers a similar discussion in describing the potential accuracy of his event-level data set covering engagements between Native Americans and Mexicans.

information on the date, location, casualties, and tribe(s) involved in each engagement; and broad temporal and geographic coverage. Together, they comprise material from more than one thousand unique references. Based on these sources, the data set contains information on the following variables:

Engagement: Identified by date (month/day/year), location (e.g., “Little Bighorn, MT”) and, where applicable, name (e.g., “Custer’s Last Stand”).

U.S./British commander: The leader of U.S./British forces in the engagement, available for 1,918 observations (65%).

Tribe commander: The leader of Native American forces in the engagement, available for 561 observations (19%). This information is primarily useful for determining what tribe fought in the engagement if that information is not explicitly provided. For instance, Native Americans fighting under Geronimo are presumably Chiricahua Apaches.

Tribe(s): The name(s) of the tribe(s) involved in the fighting, available for 2,630 observations (89%). 3,725 casualties accrued to tribes that were not identified by the sources. In these instances, the relevant tribe was interpolated based on (1) whether an engagement had been fought in the same location with a known tribe in the last three years; (2) if the U.S./British commander involved in the engagement had been fighting a particular tribe in that state during this period; (3) if the location of the engagement could be identified on a map, whether that location was clearly within the homeland of a tribe as recorded in Gerlach (1970) or Waldeman (1985). Observations with interpolated tribes are explicitly marked in the data set, comprising 2,133 casualties. After interpolation, only 1,592 casualties (less than 5%) were inflicted on unidentified tribes.

Tribe population: Estimates of each tribe's population, primarily from Sturtevant (1978-), and supplemented with information from Malinowski et al. (1998) and Swanton (1952). Population estimates were coded as close as possible to the year of the first recorded military engagement with the U.S./British.

U.S./British casualties: The number of U.S./British soldiers or civilians killed, captured, or mortally wounded in each engagement. In 55 cases, U.S./British casualties were listed as being "several," or "a few," or "a number" – in each instance, this number is approximated with a coding of 5 (comprising less than 1% of total recorded U.S./British casualties). If members of an allied tribe were killed, captured, or mortally wounded while assisting U.S./British forces during an engagement, they count as U.S./British casualties.

Tribe casualties: The number of Native American warriors or civilians killed, captured, or mortally wounded in each engagement. There were 97 observations where phrases such as "several" or "a few" were interpolated as 5 (comprising roughly 1.5% of total recorded Native American casualties). In cases where multiple tribes were involved in an engagement (12% of observations), casualty infliction and sustainment were coded as being divided among the tribes evenly. In some cases Army records attempt to distinguish casualties inflicted on each group, but this was rare and presumably imprecise. Moreover, assigning recorded casualties in this way works against the empirical claim that this variable was distributed in a highly uneven manner.

Tribal classifications

The event reports contain information on engagements involving Native American groups with 175 different names. This does not mean that the data span 175 separate tribes. Some tribes

had multiple names (e.g., Chippewas/Ojibways, Atsinas/Gros Ventres); some reports refer to specific bands within a larger tribe (e.g., Sakonnets are a subunit of Wampanoags and Chickamaugas are a subset of Cherokees); and some reports refer to warriors by the name of a town or landmark where they lived (e.g., “Hillabee” and “Tehama” warriors presumably refer to Creek and Nomlacki towns, respectively). After relabeling and merging groups where appropriate, the data contain information on 114 groups, each of which was positively identified as being a separate tribe.⁵⁹

These coding decisions were based primarily on consulting the Smithsonian Institution’s 20-volume *Handbook of North American Indians* (Sturtevant 1978-), which contains well-regarded discussions of more than two hundred tribes, while paying close attention to enumerating the subgroups and synonymies that are important for accurate classification. Two other useful sources were Wissler (1966), which is one of the most prominent anthropological texts on the Native Americans, and Swanton (1952), a work specifically devoted to enumerating tribal distinctions. In cases where proper classification was unclear, the decision rule was to combine groups that were relatively nonviolent and to disaggregate groups that were relatively violent.⁶⁰ This decision rule pushes against the central empirical claim of this paper that violence in the American Indian Wars was skewed.

⁵⁹ See Friedman 2013a for a more complete enumeration of American Indian tribes which includes another 53 groups that did not engage in recorded fighting with U.S. or British forces.

⁶⁰ For example, Apaches and Sioux are disaggregated into their main components (Chiricahuas, Jicarillas, Lipans, Mescaleros, Western Apaches; Tetons, Yanktons, Santees), while some “tribelet” in California and the Pacific Northwest were considered as components of their broader linguistic families (e.g., Shastas, Yokuts).

Comprehensiveness

In total, the data comprise 2,958 military engagements, recording 33,207 Native American casualties and 18,044 U.S./British casualties. Of course, these totals are not comprehensive, and interpreting these data requires having some sense of how they compare to the actual totals. For this purpose, one would ideally be able to compare the figures in this paper to some other, credible estimates of how many people were killed during the American Indian Wars. Unfortunately, it is not clear that there are any credible estimates of this figure (perhaps because no other source has attempted to compile these data in such a comprehensive fashion). There are, however, several ways in which to view the data presented here.

For example, we can compare the data on violent deaths in the American Indian Wars to the tribes' populations. If we combine population estimates for the 114 tribes in the data set then the tribes comprise a total of roughly 400,000 people overall. Most treatments assume that roughly one-fifth to one-quarter of a tribe would have been military-age males. A total of 33,207 casualties sustained across these tribes would thus amount to roughly thirty to forty percent of their military-aged male population at any given time. This proportion does not appear to be implausibly low, although interpreting these estimates still requires making assumptions about what the true proportion "should" look like.

An alternative way to assess these data is to draw on previous studies showing that data on the severity of violent events often resembles a specific kind of distribution defined by the "power law."⁶¹ Roughly speaking, if data are distributed according to a power law, this means that when

⁶¹ See, for instance, Richardson 1948, Cederman 2003, Bohorquez et al. 2006, Clauset et al. 2007.

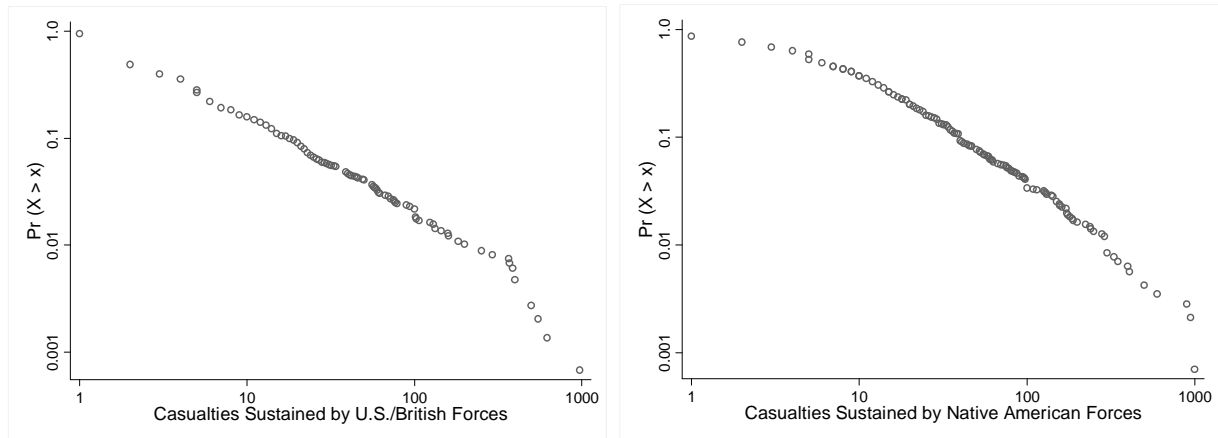


Figure 4.8. Log-Log Plots of the Data, Engagement Level

they are represented on a “log-log” plot (in which the logarithm of the event’s severity is given on the x-axis and the logarithm of the probability of an event being at least that severe is given on the y-axis), then data will form a straight line. Figure 4.8 shows how this is also true with data on the American Indian Wars when viewed at the level of the individual engagement. This provides additional support for thinking that the data are reasonably comprehensive, as their distribution largely conforms to theoretical expectations.⁶²

⁶² Friedman 2013b provides more detail on how power law relationships can be used to evaluate the comprehensiveness of event-level data on armed conflict. The main argument is that, since data on armed conflict are generally distributed according to power laws, it is possible to use the *distribution* of event-level data in order to draw inferences about the *comprehensiveness* of event-level data. Since larger-magnitude events are presumably well-recorded, it is possible to use the events we do see on the right side of the distribution in order to extrapolate the number of smaller-scale events we do *not* see on the left side of the distribution. The results of this analysis suggest that the data used in this chapter capture roughly half of total casualties sustained by tribal forces during the American Indian Wars, and perhaps ninety percent of casualties sustained by U.S. forces.

Chapter Five: Practical Implications

THE REFERENCE CLASS PROBLEM AND THE OCCUPATION OF IRAQ

After deposing Saddam Hussein during the 2003 invasion of Iraq, U.S. forces settled in for what many officials found to be a surprisingly long haul. Violence and political turmoil ensued quickly and grew for four years. Nevertheless, throughout much of this period the Bush administration and top military officers largely stuck to their initial strategy and maintained that the war was going well.

To much of the public, the administration's seeming inability to realize and correct its strategic mistakes indicated that it was in a "state of denial"¹ or that it was playing politics with the war. Critics have said that officials were determined "to win the political game at virtually any cost";² that "stubborn and misguided"³ commanders had their minds fogged by a "cloud of cognitive dissonance;"⁴ that decision makers "latched onto every positive event as a sign that better times were ahead,"⁵ and that they demonstrated a "determined refusal or inability to consider certain elements of the problem."⁶ Few cases in recent memory have led more people to

¹ Woodward 2006.

² McClellan 2007: 209.

³ Kaplan 2013: 190.

⁴ Ricks 2006: 168.

⁵ Hashim 2006: 31.

⁶ Allawi 2007: 8.

question why it is that military decision makers seem to struggle to evaluate their policies, or why they often stick to unsuccessful strategies for so long. These are the questions which motivate this dissertation, and this chapter examines them in the context of the U.S. occupation of Iraq.

One could justify selecting this case on grounds that it is another least-likely scenario for evaluating strategic assessment through a rationalist theoretical lens. And to some extent, the ideas developed in this dissertation do seem to shed light on some of the challenges of evaluating progress (or lack thereof) during the occupation's early years. Throughout this time, civilian and military leaders insisted that progress in Iraq would be cumulative: that it was only once Iraqis had reached a political consensus at the top that the country would consolidate and the insurgency would lose momentum. In this sense, even as critics pointed to unfavorable violence trends, there was a logic to U.S. officials' claims that they were meeting their benchmarks, and that just because the strategy had not worked yet this did not necessarily mean that it would not work soon. Section 5.1 describes these issues in more detail.

However, the main purpose of this chapter is not to evaluate U.S. military decision making empirically or to test the plausibility of this dissertation's theoretical framework, which were the intended functions of Chapter 4. The principal reason for focusing on the occupation of Iraq in this chapter is rather to use the case as a vehicle for developing arguments about how military decision makers can potentially deal with the analytic challenges that cumulative dynamics present for strategic assessment. Previous chapters of this dissertation have explained why military decision makers will not necessarily be able to learn, adapt, and realize their strategic mistakes simply by observing that their policies have previously been unsuccessful. If this kind

of information is not as meaningful as scholars generally believe, then it is important to think about other ways in which decision makers can revise their expectations and improve their strategic assessments, and that is the goal here.

Yet if the focus of this chapter is on practical implications, the following sections still revolve around theoretical development. In particular, Section 5.2 describes how existing scholarship on the “reference class problem” (a topic in the philosophy of statistics dating back to the English mathematician John Venn) provides a useful perspective on the difficulties of strategic assessment. Simply stated, the reference class problem deals with the importance of diagnostics before prescription: depending on the way that decision makers define the problems they confront and select other cases for comparison, this will often suggest very different policy responses.

This may sound truisitic, but Section 5.3 shows how decision makers, military doctrine, and public debate typically skirt around the reference class problem in both overt and subtle ways, rather than dealing with it directly. Section 5.4 goes on to explain why approaching the reference problem rigorously requires a depth of analysis that scholars largely neglect as well. Both military doctrine and empirical social science generally focus on identifying common patterns that hold across cases, all else being equal. These patterns are important, but this chapter argues that when it comes to strategic assessment and military decision making, it is often just as important to understand the ways in which cases differ, and how those differences should shape the prior assumptions which drive strategic assessment.

Section 5.1. Strategic assessment in Iraq, 2003-2007

Misleading prior assumptions

According to the U.S. Army's official history of the occupation, *On Point II*, "conditions in Iraq proved to be wildly out of synch with prewar assumptions."⁷ At the outset of the occupation, senior officials did not expect to be confronted with a prolonged insurgency. In their memoirs, the president, the secretary of defense, and the director of the Central Intelligence Agency all discussed how they were surprised when organized violence emerged after the fall of Saddam.⁸ Their misperceptions were widely shared throughout the government and most planning efforts for the war, including those conducted by civilian agencies. *On Point II* describes how "none of the organizations involved in [the planning] effort came to the conclusion that a serious insurgent resistance would emerge after a successful Coalition campaign against the Baathist regime."⁹

One of the common themes that emerges from the literature on pre-war planning for the occupation of Iraq is that U.S. officials misperceived not just the duration of the mission, but also the nature of the challenges it would confront. Instead of anticipating an organized insurgency, the administration formulated its prior assumptions based on previous experiences with "post-war reconstruction." For instance, President Bush reflected in his memoirs that "I had studied the

⁷ Wright and Reese 2008: 153, 79.

⁸ See Bush 2010: 258, Rumsfeld 2011: 520, 664, Feith 2008: 275-76, 415, and Tenet 2007: 318.

⁹ The plans surveyed in *On Point II* include the initial "Phase IV" plan for stabilization and reconstruction operations, the Coalition Forces Land Component Command's "ECLIPSE II" plan; the Department of State's *Future of Iraq* compendium; a 2002 National Defense University planning document; and a 2002 U.S. Army War College planning document. See, e.g., Wright and Reese 2008: 88-89.

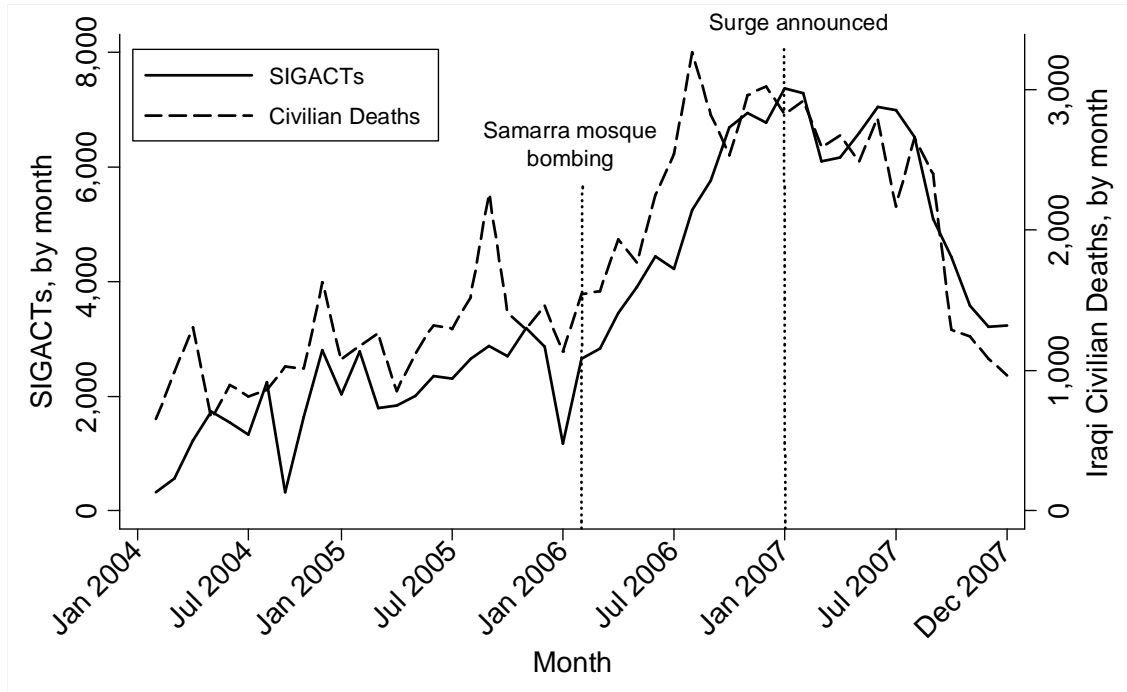


Figure 5.1. Violence Trends in Iraq, 2004-2007

Violence in Iraq continually escalated during the early years of the U.S. occupation, shown here by way of officially-recorded “Significant Activities” (SIGACTs) and Iraqi civilian deaths as measured by the independent group Iraq Body Count. Violence rose particularly during the summer of 2006, as a wave of sectarian violence followed the bombing of the Golden Dome mosque in Samarra. The Surge was announced in January 2007 and violence fell substantially in that year.

histories of postwar Germany, Japan, and South Korea” – all cases where U.S. forces had played a prominent role in helping to consolidate stability, but where they did not face substantial violent opposition.¹⁰ Similarly, when violence broke out in Baghdad during widespread looting after the invasion, Secretary of Defense Donald Rumsfeld viewed the problem through the lens of postwar Germany.¹¹ The historian’s office at the U.S. Department of State framed an analysis

¹⁰ Bush 2010: 356.

¹¹ Rumsfeld 2011: 476.

of Iraq in terms of the “success stories of post-World War II Germany and Japan.”¹² A widely-circulated RAND report on the lessons of *America’s Role in Nation-Building from Germany to Iraq* also framed its analysis of postwar planning around the challenges of “post-conflict reconstruction.”¹³ The report warned of some “episodic, guerilla-style violence” but also stated that “defeated or liberated populations are often more docile, cooperative, and malleable than usually anticipated.”¹⁴

Many U.S. military decision makers and analysts thus structured their prior assumptions for the occupation of Iraq around misleading analogies. These analogies, in turn, supported the mistaken view that the occupation would not be confronted with significant or protracted violence. And as we have seen so far in this dissertation – through both the theoretical analysis in Chapter 3 and the empirical analysis in Chapter 4 – once decision makers enter conflicts with misleading priors, they can often find it difficult to *revise* their expectations in a manner that indicates they should change course.

¹² Marc J. Susser, “Occupation and Postwar Government: Precedents and Options,” Memorandum to the Secretary of State, 28 February 2003. Similarly, a memorandum from Assistant Secretary of Defense Peter Rodman stated that “Historical analogies have been invoked, especially a comparison with experience in Afghanistan and the model of occupied Germany and Japan after WWII.” Rodman argues that these comparisons may be flawed, but in their stead he invokes a similar example: “A more interesting analogy is with postwar France.” Rodman, “Who Will Govern Iraq?” Memorandum to Secretary of Defense Rumsfeld, 15 August 2002.

¹³ The director of the Coalitional Provisional Authority, L. Paul Bremer III, recalled in his memoirs how that RAND report served as the basis for much of his thinking heading into the job (Bremer 2006: 9-10, 12, 14, 106). The report stated that “The cases of Germany and Japan set a standard for postconflict nation-building that has not since been matched” (Dobbins et al. 2003, xiii, 168).

¹⁴ Dobbins et al. 2003: 172, 195.

Counterinsurgency in Iraq as a cumulative process

This dissertation has explained why these analytic challenges result from the cumulative dynamics of armed conflict, and the Bush administration's strategy in Iraq was indeed largely framed in similar terms. The ultimate goal of the occupation was to stabilize the country long enough to buy space for political reconciliation, kill or capture enough insurgents to undermine their effectiveness, and progressively build the size and capabilities of the Iraqi Security Forces until they could defend the country on their own. In their recent history of the occupation, Michael Gordon and Bernard Trainor wrote that "At the White House, the hope was that the push toward sovereignty would soothe the Iraqis' grievances over the occupation and take the steam out of the insurgency. Politics, in effect, was to enable the military strategy."¹⁵ As with U.S. officials in Vietnam, decision makers in the Bush administration often characterized their strategy in terms of making cumulative progress towards making a major breakthrough. Secretary Rumsfeld wrote in 2005 that the "key question" was "when there will be a clearly discernible 'tipping point.' Eventually, more and more Iraqi people will decide that they will no longer side with the enemies of the legitimate Iraqi government and move to the middle. And the people in the middle, at some point, will decide that there is going to be a legitimate, free Iraqi government."¹⁶

Just as U.S. decision makers in Vietnam and the American Indian Wars struggled to assess their progress towards achieving similar breakthroughs, so too did Bush administration officials struggle to revise their views about how long it might take and how much it might cost in order

¹⁵ Gordon and Trainor 2012: 56.

¹⁶ Donald Rumsfeld, "Progress in Iraq," memorandum to President Bush, 29 November 2005.

to achieve these objectives in Iraq. Seen through this lens, rising violence did not necessarily indicate that the strategy was failing, because it was only once a legitimate political consensus had formed in Baghdad that the insurgency was expected to lose its momentum – politics was to enable the military strategy, and the not the other way around. In fact, opposition forces were very much expected to step up their efforts to block political consolidation the closer Iraqis came to achieving it.¹⁷

In light of these prior assumptions, President Bush recalled his thoughts at the initial instability in the aftermath of the invasion. “The chaos and violence we witnessed were alarming,” he wrote, “but it was still early.... I refused to give up on our plan before it had a chance to work.”¹⁸ Events that administration officials hoped would precipitate political consolidation included the capture of Saddam Hussein in 2003; the passage of Iraq’s Transitional Administrative Law in 2004; military operations in Fallujah that killed or captured several thousand insurgents in fall 2004; Iraq’s first national elections in January 2005; and the death of Al-Qaeda leader Abu Musab al-Zarqawi in June 2006.¹⁹ Of course, none of these events proved to be the turning points that Bush administration officials hoped for; but even if the occupation of Iraq was not panning out as expected, many top officials still believed that their strategy was on track.

¹⁷ Gordon and Trainor 2012: 97: “The assumption was that the period leading up to the January 2005 vote would be one of maximum vulnerability, but that if the election were held, the main danger would pass.”

¹⁸ See Bush 2010: 259, along with similar reflections by Rumsfeld 2011: 531 and Feith 2008: 124.

¹⁹ See Wright and Reese 2008: 37, 45, 228, 357, 475-76 and Cordesman 2008: 338.

If the occupation of Iraq had been a repeated process – if the odds of U.S. policy paying off stayed the same from round to round like a slot machine or a roulette wheel – then the behavior and perceptions of U.S. officials during this period would have been clearly irrational, consistent with behavior like the “gambler’s fallacy.” But because the war was a *cumulative* process, this is not an appropriate framework for evaluating the case. When rational decision makers are observing cumulative processes, they should not necessarily become more pessimistic over time as they implement their policies without achieving success.

Figure 5.2 helps to demonstrate this point by showing how the expected remaining duration of an insurgency does not necessarily increase as the war progresses. The top of Figure 5.2 presents data on the durations of 286 insurgencies.²⁰ As in previous figures, the shaded area represents probability density, which in this case is the proportion of insurgencies that terminate in a given year of the war. Based on this information, the solid line in the figure represents the expected *total* duration of an insurgency, conditional on lasting for one year, two years, three years, and so on. The dashed line then represents the expected *remaining* duration of the insurgency as it develops, and the important thing to note is that this stays relatively constant. (The bottom of Figure 5.2 reproduces the same pattern when limiting the sample to the subset of these cases which counterinsurgents won according to the data.) Thus as we saw with the analysis of U.S. commanders in the American Indian Wars, the fact that military leaders did not become markedly more pessimistic about their strategies even as they did not succeed is actually consistent with empirical patterns for how long insurgencies typically last.

²⁰ Lyall and Wilson 2009.

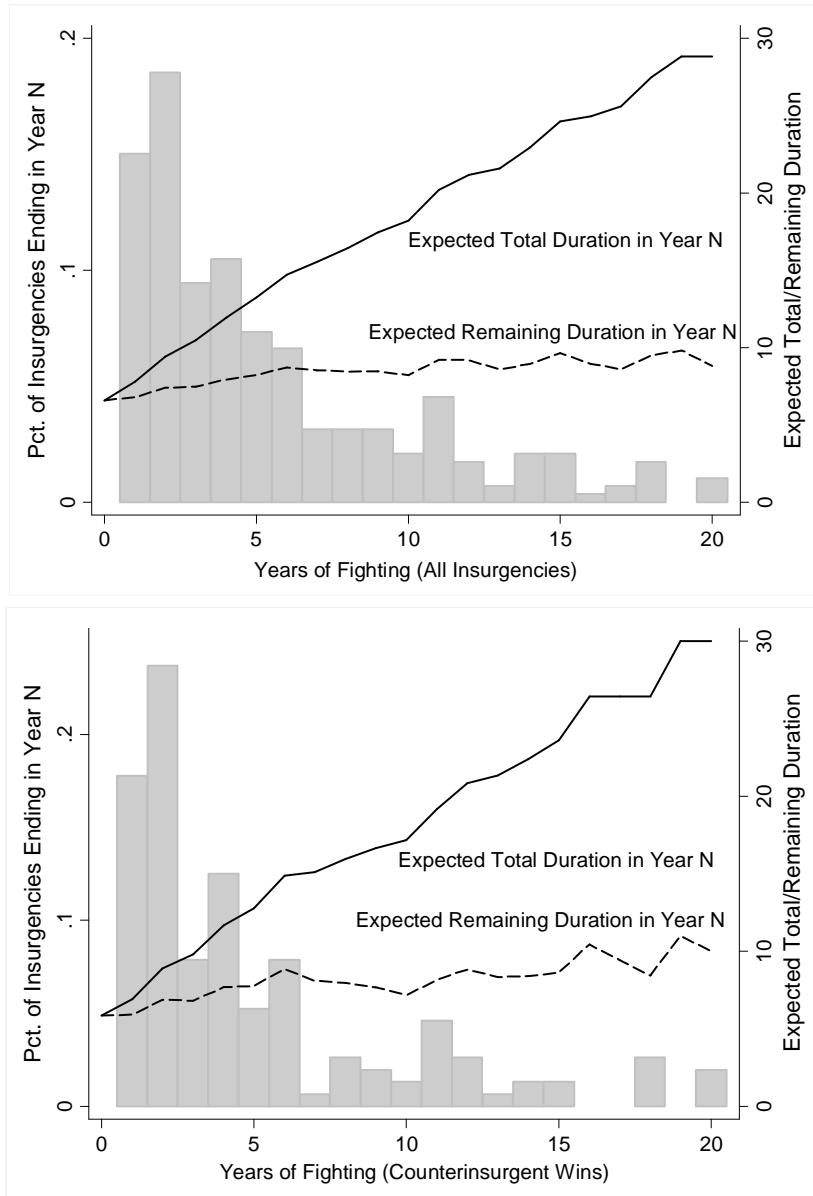


Figure 5.2. Expected Total/Remaining Duration of Insurgencies

Figure 5.2 plots data based on the duration of insurgencies from 1808-2002 based on Lyall and Wilson 2009. As in Chapters 3 and 4, the shaded area of the figure represents probability density, which in this case represents the probability that a randomly-selected insurgency will end in a given year of the fighting. The solid line represents the expected total duration of an insurgency, conditional on the insurgency lasting for a particular period of time. The dashed line represents the expected remaining duration of an insurgency, conditional on lasting for a particular period of time. Figure 5.2 shows how expected remaining duration stays relatively constant as an insurgency progresses. The top panel plots data for all recorded insurgencies (N=286), the bottom panel plots only data for wars that counterinsurgents won according to Lyall and Wilson (N=152).

And in fact, as the occupation dragged on, many top officials assumed that the problem was not that they were pursuing the wrong strategy, but rather than they were not pursuing their current strategy wholeheartedly *enough*. General George Casey, the top commander of U.S. forces in Iraq from June 2004 to February 2007, had titled his campaign plan “Transition to Self-Reliance.” Based on the assumption that the goal of U.S. policy should be to assist Iraq’s central government in developing the capacity and legitimacy to govern, this plan stated that “we will succeed by increasingly putting Iraqis in charge across all lines of operations.” To the extent that violence was not declining in Iraq, Casey thus argued that the solution was to expand the Iraqi Security Forces faster and reduce the U.S. military footprint further, so as to accelerate the consolidation of political power in Baghdad. This notion was captured in the oft-repeated slogan “As they stand up, we will stand down.” During his last year in Baghdad, General Casey often expressed his view that the principal problem with U.S. policy in Iraq was that this transfer of responsibility was not occurring quickly enough.²¹ As with many policy debates concerning cumulative processes, what many people saw to be a failure of the concept, others viewed mainly as flawed execution.

On this point, General Casey was actually in accordance with many critics of the occupation, who also believed that the central cause of rising violence was a U.S. military presence that provoked militant nationalism and prevented the Iraqi government from developing the kind of ruling authority that it would only earn by standing on its own. Debates about U.S. strategy in Iraq in large part converged in 2006 around a high-profile independent commission called the Iraq Study Group, chaired by former Republican Secretary of State James Baker and former Democratic Congressman Lee Hamilton. The Iraq Study Group’s final report began by stating

²¹ On Casey’s campaign plan, see Gordon and Trainor 2012: 200 and Kaplan 2013: ch. 12.

that “The situation in Iraq is grave and deteriorating.” Its two principal recommendations for dealing with this situation were to enhance “diplomatic and political efforts in Iraq and the region” and to change the mission of U.S. forces in a way that would “enable the United States to begin to move its combat forces out of Iraq responsibly” such that “the Iraqi government moves forward with national reconciliation.” The Iraq Study Group further warned that “Adding more American troops could conceivably worsen those aspects of the security problem that are fed by the view that the U.S. presence is intended to be a long-term ‘occupation.’”²² Gordon and Trainor place these statements in the following context:

For all the expectation that the group would come up with an alternative strategy on military matters, it largely favored the status quo: a gradual handover to the Iraqis and an American drawdown, with the hope that a strategy that had not worked in 2005 and most of 2006 would somehow bear fruit over the next eighteen months. Casey noted as much in an account of his Iraqi strategy, which he issued years after leaving. ‘I found the report a useful validation of what we were doing,’ he said of the [Iraq Study Group] assessment.²³

Well into 2006, debates about U.S. policy in Iraq were thus largely mired in a state of inertia. U.S. officials were attempting to transfer responsibility to Iraq’s central government. It was clear that this strategy had not achieved its goals, and yet decision makers and analysts still held out the prospect that continuing and perhaps accelerating this process could facilitate a legitimate

²² Baker and Hamilton 2006: xiii, 50. It is worth mentioning that the Iraq Study Group report then hedged its position on troop levels by stating that “We could, however, support a short-term redeployment or surge of American combat forces to stabilize Baghdad, or to speed up the training and equipping mission, if the U.S. commander in Iraq determines that such steps would be effective.”

²³ Gordon and Trainor 2012: 280.

and capable regime in Baghdad, and this political consolidation might, in turn, undermine the growing insurgency.

This dissertation has attempted to explain why debates about military strategy often get stuck in this fashion. Previous chapters have showed how the basic line of reasoning espoused by the Bush administration, General Casey, and the Iraq Study Group – that just because the strategy had not worked yet this does not necessarily mean that it would not work soon – has a genuine logic to which military decision makers often appeal, and that often makes sense for them to adopt. Though existing scholarship (and public opinion) generally hold that military decision makers should become increasingly pessimistic when their strategies fail to succeed, this dissertation has explained why this argument is not necessarily sound, and why rational decision makers might often conclude the very opposite.

This is not to claim that U.S. officials actually assessed Iraq in a rational manner. It is entirely possible that top officials expressed views at the time (which they then repeated later in reflecting on the experience) in strategic ways that were designed to hold up in public debates. But if that were true, then it would only reinforce the notion that these arguments possess a logic that can be genuinely difficult to disprove. If only from the standpoint of determining how to effectively critique policies like those that U.S. officials espoused in Iraq from 2003 through 2006, it may be necessary to engage the analytic issues developed here. In doing so, it is important to understand why many common expectations about the way that military decision makers can (and should) learn and adapt may often be misleading, and why arguments to that effect often fail to carry the day.

Changing assumptions, switching strategies

What then *did* eventually precipitate strategic change in Iraq? Even if many people believed that the United States stuck to its initial occupation strategy for too long, this strategy did not remain unchanged forever. In January 2007, President Bush announced his decision to “surge” 30,000 additional forces into Iraq, to replace George Casey with a new commander in General David Petraeus, and to adopt a substantially new military strategy for employing U.S. forces. Instead of the previous emphasis on maintaining a low profile and primarily acting as an enabler to the Iraqi government, U.S. forces would now play a much more direct and visible role in protecting the population. De-emphasizing the rapid expansion of the Iraqi Army and Police, the United States would now in fact support the creation of local security forces (the so-called Sons of Iraq units) which took over a large portion of responsibility for protecting their home areas. And while previous commanders largely saw their mission as being to work with and through Iraq leaders, General Petraeus took a much more aggressive stance in dealing with Iraq’s Prime Minister Nouri al-Maliki, insisting that U.S. forces be allowed to operate in areas that were previously off-limits because they belonged to the regime’s base of support.

There is already a substantial literature on what this change in strategy entailed, and how U.S. officials arrived at it.²⁴ Some aspects of this process remain unclear. Yet there is a general consensus that U.S. officials did not decide to change strategies simply because they ran out of patience with existing policy. Though standard theoretical frameworks generally argue that the number of “rounds of fighting” decision makers conduct without obtaining their objectives is a

²⁴ On the Bush administration’s deliberations over the surge, see Woodward 2008, Robinson 2008, Cloud and Jaffe 2009, Ricks 2009, Feaver 2011, Marsh 2011, Gordon and Trainor 2012, and Kaplan 2013.

primary mechanism by which they learn and adapt (and though the administration's initial refusal to change strategies despite the mounting violence in Iraq was a major source of consternation for critics), this is not what seems to have been the key mechanism precipitating the surge.

Instead, it appears as though the change in U.S. policy was precipitated by a fundamental shift in decision makers' views about what was really driving the violence in Iraq. As the occupation progressed into 2006, it became clear that military decision makers had underestimated the extent to which the war revolved around Sunni-Shiite sectarian fault lines in addition to pro-Baathist or anti-occupation sentiment. The sectarian nature of Iraq's violence became particularly clear after the Sunni group al-Qaeda bombed the Shiite Golden Dome Mosque in the city of Samarra in February 2006. As sectarian violence escalated in the months following this attack, it sparked a basic rethinking of the problem that the occupation of Iraq confronted.

A different diagnosis of the conflict naturally suggested a different prescription for what to do about it. President Bush recalled: "In the months after the Samarra bombing, I had started to question whether our approach matched the reality on the ground. The sectarian violence had not erupted because our footprint was too big. It had happened because al Qaeda had provoked it. And with the Iraqis struggling to stand up, it didn't seem possible for us to stand down." Bush also wrote that "Only after the sectarian violence erupted in 2006 did it become clear that more security was needed before political progress could continue."²⁵ Secretary of Defense Donald

²⁵ Bush 2010: 363, 393.

Rumsfeld recalled the situation similarly: “looking back, it is now clear that the effect of the bombing proved a game changer in Iraq.”²⁶

In comparison to its relative stasis over the previous three and a half years, U.S. policy shifted in relatively short order following this reconceptualization. In November 2006, National Security Adviser Stephen Hadley visited Iraq and reported back to the White House that instead of pushing for a broadly-inclusive and legitimate central government, Prime Minister Maliki was largely supporting “a campaign to consolidate Shia power in Baghdad.” Though it was unclear whether Maliki was a “witting participant” in this process, his government was making an “aggressive push to consolidate Shia power and influence.”²⁷ This made it implausible to think that violence in Iraq would decline as a result of transferring more responsibility to the central government – if the government was a party to the conflict then increasing its capabilities and freedom of action was probably making the problem worse.

Shortly afterward, the White House formally launched a review of its Iraq strategy, and then announced the surge in January 2007. When David Petraeus took command in Iraq, he assembled a “Joint Strategic Assessment Team” of military and civilian analysts to help critique Casey’s previous campaign plan and replace it with a new one. As Gordon and Trainor describe it, the “central point” of the team’s report “was that the war had been grievously misunderstood: the entire character of the conflict had changed, but U.S. military strategy had been unresponsive, increasingly irrelevant, and, at times, counterproductive to boot.” The increasing sectarian violence in Iraq had demonstrated that “The nation was in the middle of a ‘communal

²⁶ Rumsfeld 2011: 660.

²⁷ Hadley’s memo, dated November 8, was leaked and then published in the *New York Times* later that month. For a discussion of this memo, see Gordon and Trainor 2012: 291.

power struggle' among Shiite, Sunni, and Kurdish factions squabbling and killing each other... a development the American command had misdiagnosed and inadvertently abetted."²⁸ The principal changes in U.S. strategy described above – emphasizing population security, partnering with local security forces, and adopting a more aggressive posture in dealing with Maliki's government – were all measures designed to manage these intersectarian tensions in ways that the previous strategy had overlooked. By the end of 2007, violence in Iraq had declined by roughly ninety percent.

Key features of strategic assessment in Iraq

There is evidence to indicate that this change in U.S. strategy was indeed partly responsible for reducing Iraq's violence, but the issue remains disputed, and it is beyond the scope of the analysis here.²⁹ For the purposes of this dissertation, the key question is not what caused violence to decline in Iraq, nor whether the Surge was justified. The topic of this dissertation is strategic assessment and military decision making, and the point of this review is to examine why it is that U.S. officials changed their views so substantially in 2006 but not before. The review presented in this section suggests that this generally had little to do with how long the war had lasted and whether the previous strategy had been unable to reduce violence as quickly as planned (though this is the primary mechanism on which existing models of learning and adaptation in armed conflict tend to focus). Rather, the strategic shift in Iraq seems to have largely revolved around a reframing of the fundamental problem that U.S. policy was intended to address.

²⁸ Gordon and Trainor 2012: 356 cf. Kaplan 2013: 260-264.

²⁹ See Biddle, Friedman, and Shapiro 2012 on this issue.

Given the theoretical argument in Chapter 3, this should not be surprising. One of the central takeaways from Chapter 3 was that decision makers' prior assumptions crucially shape the way that they perform strategic assessment. This theoretical framework emphasized that the simple fact that a strategy has not been successful is largely indeterminate for determining how rational decision makers should think or behave moving forward. That argument helped to explain why debates about strategic assessment in Iraq, Vietnam, the American Indian Wars, and elsewhere often seem to obtain such little resolution, even after strategies are implemented for substantial periods of time without achieving their desired goals.

This theoretical framework, along with the review of Iraq policy provided in this section, thus suggest that a more effective way to approach strategic assessment is to focus on refining decision makers' views of what kind of challenge they are facing – something that essentially amounts to examining decision makers' prior assumptions in a way that is more fundamental than the kinds of gradual updating and revision that are the primary focus of existing scholarship. This is a form of learning and adaptation that the theoretical framework in Chapter 3 indicates *should* make a difference for approaching strategic assessment, and the Iraq experience suggests that this is something that *does* make a difference in salient cases. Yet as the remainder of the chapter will explain, this is nevertheless a subject that scholars and decision makers often underemphasize.

Section 5.2. The significance of the reference class problem in forming prior assumptions

As previous chapters have discussed, military decision makers often face substantial uncertainty about the type of opponents or challenges that they face. We have seen that existing scholarship

generally conceives of “type” in a quantitative way, defined as the probability of defeating an opponent in each battle or “round of fighting” and thus, by extension, determining how long it might take or how much it might cost for decision makers to achieve their intended goals. Before characterizing this kind of uncertainty, however, decision makers face a more fundamental question in determining what kind of conflict they are dealing with, and thus which potential “types” of opponent are relevant to the analysis at all. This problem was clearly evident in the run-up to the Iraq war, where U.S. officials structured their thinking around past cases of post-war reconstruction, rather than looking to examples of past insurgencies (let alone insurgencies that specifically revolved around communal divisions like those that drove violence in Iraq).

A bumper-sticker way to characterize this logic might be “diagnostics before prescription,”³⁰ and there is a substantial literature on this subject in the field of statistical philosophy. This literature describes what scholars call the *reference class problem*, and the origins of this subject are typically attributed the 19th century English mathematician John Venn. (His “Venn diagrams” are a way of representing reference classes visually.) Since then, the reference class problem has been applied to a wide range of topics in the social and natural sciences.³¹

The root of the reference class problem is that in order to make predictions about some event, analysts typically draw on information from other, similar cases. Venn used the example of trying to predict whether a hypothetical John Smith, aged 50, would live to the age of 51. In order to predict that probability, we could think of John Smith as a typical 50-year old man; then

³⁰ This phrase was the title of a 2010 essay by development economist Dani Rodrik, who argued that scholars and policymakers often pay insufficient attention to ensuring that they choose conceptual models that are appropriate for the context to which they are applying those models.

³¹ See Hajek 2007 for a review.

we could estimate his probability of surviving the year by looking at the percentage of *all* 50-year old men who do so.

But Smith no doubt has other salient characteristics, too. For instance, in Venn's example Smith lived in northern England and he has tuberculosis. So when we think about his chances of living another year, we could limit our "reference class" to the population of 50-year old northern English male consumptives, a group whose life expectancy is probably different from Smith's age cohort on the whole. Of course, this line of reasoning could extend forever and in numerous directions. "It must be remembered," Venn explains, that "every individual thing or event has an indefinite number of properties or attributes observable in it, and might therefore be considered as belonging to an indefinite number of different classes of things."³² Many of John Smith's "properties or attributes" will presumably affect our estimates of whether he will live to age 51. The crux of the reference class problem is that there is no way we can take them all into account, and yet we will have to find some way of making a prediction anyway.

Similarly, when thinking about military strategy, there are any number of ways that decision makers can stratify past experience in order to structure their thinking. Even if U.S. officials had studied insurgencies rather than post-war reconstruction efforts in planning for the occupation of Iraq, they could have studied all insurgencies, just those insurgencies that took place in the Middle East, just those insurgencies that involved external occupiers, just those insurgencies that occurred in "recent years" (however defined), or reference classes involving any combination of these and other attributes. This decision about which reference class to examine comes logically prior to determining the possible distribution of opponent "types" – it is not possible to

³² Venn 1888: 213.

determine how long it might take or how much it might cost to meet some challenge without first defining what that challenge entails.

So what would have been the proper reference class for thinking about the occupation of Iraq? One of the most important aspects of the reference class problem – and the reason that this literature falls within the subfield of statistical *philosophy* – is that there is usually no clear, objective way to answer that question. Venn writes: “When therefore John Smith presents himself to our notice without, so to say, a particular label attached to him informing us under which of his various aspects he is to be viewed, the process of thus referring him to a class becomes to a great extent arbitrary.... [T]here are no logical grounds of decision; the selection must be determined by some extraneous considerations.”³³ Venn is not being agnostic here – he is not saying that any reference class is as good as another. Rather, his point is that it is often possible for analysts to define reference classes in different, but equally well-reasoned ways.

One should therefore expect that when evaluating military strategy (or policy problems of most kinds) there will be debates about defining the proper reference class. Even after careful discussion, some uncertainty will probably remain about which reference class is most appropriate for structuring expectations *ex ante*. And thus as decision makers begin dealing with the problem, it may be important for them to in turn revisit their prior assumptions about the kind of case they are dealing with.

In a sense, this also involves learning about an opponent’s “type,” but in a way that is much more fundamental than how existing theoretical scholarship on armed conflict approaches the subject. Current frameworks generally focus on the way that battlefield gains and losses should

³³ Venn 1888: 214 cf. Reichenbach 1949: 440, 443, 460.

allow decision makers to gradually learn about how resilient their opponents are, and thus to progressively revise their inferences about how long it might take or how much it might cost for decision makers to achieve their desired goals.

The reference class problem, by contrast, suggests that different ways of framing the problem can suggest entirely different sets of relevant past experience on which to draw, along with entirely different ways of defining the key problems that military strategy should be structured to address. The U.S. experience in Iraq is a prime example of how revising these kinds of prior assumptions may often play a more decisive role in strategic assessment than the kinds of gradual learning about an opponent's capabilities and resolve that tend to be the primary focus of existing theoretical frameworks. As Section 5.1 described, reframing the "reference class" through which decision makers viewed the occupation from a problem of nationalist sentiment to one of communal conflict led officials to reverse key aspects of their military strategy: from keeping U.S. forces low-profile to making them the centerpiece of protecting the population; from building up the Iraqi security forces to cooperating with decentralized "Sons of Iraq" units; from bolstering the Maliki government to restraining its sectarian agenda. In each of these ways, new diagnostics drove new prescriptions.

Section 5.3. Obstacles to managing the reference class problem

It is not just existing theoretical frameworks that tend to marginalize the role of the reference class problem in strategic assessment, as decision makers, military doctrine, and public debate also tend to give this issue short shrift. Some ways of resisting the reference class problem are overt, others less so, and this section describes some of these issues in more detail.

Overt constraints on dealing with the reference class problem

The occupation of Iraq provides several examples of how military decision makers can overtly constrain discussions of the reference class problem, as some U.S. officials essentially forbid debate about whether they had misdiagnosed the nature of the war. Paul Bremer, for instance – the top U.S. civilian official in Iraq from May 2003 to June 2004 – reportedly did not allow subordinates to draw analogies between Iraq (which he saw as a case of post-war reconstruction) and Vietnam (a case of counterinsurgency that would require a very different response).³⁴ Defense Secretary Rumsfeld similarly forbade his staff from referring to Iraq’s violence as constituting an “insurgency” or a “civil war,” resulting in a “strange semantic fight”³⁵ in White House and Defense Department press conferences over what exactly to call the situation in Iraq.

Some people saw these word-choice debates as being a side-show from discussing substantive issues,³⁶ but this view masks the importance that these kinds of labels can have for framing the problem at hand. For instance, in searching for an acceptable term for violent elements in Iraq, U.S. officials generally employed labels such as “dead-enders,” “former regime elements,” “ex-Baathists,” and “Sunni rejectionists,” names implying that to the extent the resistance had any

³⁴ See Kaplan 2013: 82.

³⁵ Fearon 2007b: 2. Fearon continues: “It is not hard to understand why the administration strongly resists [calling Iraq a ‘civil war’]. For one thing, the U.S. media would interpret a change in the White House’s position on this question as a major concession, an open acknowledgment of dashed hopes and a failed policy. For another, the administration worries that if the U.S. public comes to see the violence in Iraq as a civil war, it will be even less willing to tolerate continued U.S. military engagement. ‘If it’s a civil war, what are we doing there, mixed up in someone else’s fight?’ Americans may ask.”

³⁶ E.g., Gordon and Trainor 2012: 195: “The terminological debate over whether it was a civil war, ‘ethno-sectarian conflict,’ or simply ‘communal violence’ distracted from the larger point: the problem had been going on for months before February 2006, and as the spring wore on it worsened.”

political motivation, it was driven by deposed members of the Saddam-era elite who viewed the new government as being illegitimate. This was, at the very least, a substantially incomplete characterization of the sources of violence in Iraq, neglecting the notion that in some places Sunnis, Shiites, and Kurds viewed each other as being direct threats to their physical survival, planting groups in the middle of local sectarian security dilemmas that largely stood apart from political battles in Baghdad.³⁷

Thus even if U.S. officials had admitted earlier that they had a potentially severe civil conflict on their hands, it would still have remained for them to determine just what kind of conflict it was. For pundits and the general public, the natural analogy was to the War in Vietnam. But as Stephen Biddle wrote in an article published just prior to the Samarra mosque bombing in 2006, it was important to make sure that the emerging debate about the nature of Iraq's violence did not coalesce around the wrong historical model:

If the debate in Washington is Vietnam redux, the war in Iraq is not. The current struggle is not a Maoist 'people's war' of national liberation; it is a communal civil war fought with very different dynamics.... Unfortunately, many of the policies dominating the debate are ill adapted to the war being fought.

...In a people's war, handing the fighting off to local forces makes sense because it undermines the nationalist component of insurgent resistance, improves the quality of local intelligence, and boosts troop strength. But in a communal civil war, it throws gasoline on the fire. Iraq's Sunnis perceive the 'national' army and police force as a Shiite-Kurdish militia on steroids. And they have a point: in a communal conflict, the only effective units are the ones that do not intermingle communal enemies.³⁸

³⁷ In a different context, see Khong 1992: 233-240, who discusses how the Johnson administration also resisted calling the War in Vietnam a "civil war" (and explains how this shaped policy debates in important ways).

³⁸ Biddle 2006: 2, 8.

Here, Biddle was making an argument about the dangers of faulty diagnostics driving faulty prescriptions. Even if U.S. officials revised their assessments of the conflict's "type" in a quantitative sense – accepting that the occupation would be far more violent and protracted than they initially believed – it was still critical to define the qualitative *nature* of that conflict in order to devise a military strategy that dealt with the key problems in a way that U.S. policy failed to do prior to 2007.

It would be mistaken to imply that no one in the government worried about these problems or questioned the assumptions driving U.S. strategy in Iraq. From the beginning, there were dissenters whose perceptions of the violence in Iraq appear more or less vindicated by hindsight.³⁹ These voices, however, were generally marginalized from debates. For example, when President Bush called a retreat at Camp David in April 2006 to assess the problem of escalating violence, debates about the nature of that violence were not on the agenda, and officials with access to these kinds of discussions were generally unwilling to air those kinds of dissenting views in front of the secretary of defense, senior military commanders, or the president.⁴⁰ These dynamics constituted a fairly direct barrier to debating the reference class problem during the occupation of Iraq.

³⁹ For example, Defense Intelligence Agency analyst Derek Harvey began reporting in early 2004 that violence was not simply a function of nationalist resistance to the U.S. occupation, but rather that it revolved around Iraqis prosecuting sectarian agendas in battling for political control. A Central Intelligence Agency "Red Team" warned later that year that Iraq's sectarian fault lines could plunge the country into civil war, a view later shared by Central Command military analyst Joel Rayburn. When the Joint Chiefs of Staff convened a "Council of Colonels" to analyze Iraq's violence in 2006, it too argued that "properly characterizing the war was critical" and that senior commanders had misdiagnosed the problem by not taking intersectarian tensions sufficiently into account. On these and related views, see Gordon and Trainor 2012: 20-23, 132-33, 160, 285-286 and Kaplan 2013: 181-182.

⁴⁰ Gordon and Trainor 2012: 210.

Limitations of military doctrine

Along with these relatively overt ways of marginalizing debates about the sources and nature of violence in Iraq, there are subtler (and broader) indications of how military decision makers may often underemphasize the importance of defining reference classes carefully.

Military doctrine, for instance, largely skirts the issue. Almost all military activity is guided by doctrine, much of which is conveyed in field manuals which provide a “body of thought on how Army forces intend to operate.”⁴¹ In this sense, military doctrine essentially formulates prior assumptions that help decision makers to ground their strategic assessments and prescriptions.⁴²

This doctrine, however, places most of its emphasis on defining patterns that hold across cases, rather than identifying ways in which different scenarios might require different approaches. This focus on *ceteris paribus* reasoning marginalizes the reference class problem rather than offering commanders structured guidance for how to deal with it. On this issue, it is useful to examine the U.S. Army’s field manual on *Counterinsurgency* (FM 3-24) which was released in December 2006.⁴³ As this manual was written at a time of especially intense debate

⁴¹ This quote is from U.S. Army Field Manual 3-0, *Operations* (2008: pars. D-1, D-4). Kretchik 2011 offers a comprehensive history of the Army’s “keystone” doctrine. Related works include Ney 1966, Doughty 1979, and Chapman 2009. Chapman’s book includes three entire chapters devoted to literature review.

⁴² On viewing military doctrine as Bayesian priors, see Friedman 2011, 2012.

⁴³ The manual was co-authored by the U.S. Marine Corps, for which it also serves as doctrine under the label of Marine Corps Warfighting Publication 3-33.5. Each military service retains its own corpus of doctrine.

about U.S. policy, it was widely viewed (and in many ways intended) to serve as a focal point for driving discussion about how the military could improve its performance in Iraq.⁴⁴

As the authors explain in the manual's forward, FM 3-24 articulates "fundamental principles" that form a "solid foundation for understanding and addressing specific insurgencies." The manual asserts that its purpose is to present "basic historical trends" that are "applicable worldwide," though this is not to say that the manual provides a rigid blueprint for military action: indeed the authors of FM 3-24 stress that "every insurgency is contextual and presents its own set of challenges" and that "users should assess information from other sources to help them decide how to apply the doctrine in this publication to the specific circumstances facing them." It is possible to read these words as being analogous to the core logic of Bayesian decision theory, in which doctrine offers prior assumptions, and decision makers then revise those priors based on information they receive in particular contingencies.

The problem with this model, however, is that it is unclear that a single set of prior assumptions can provide strategic guidance that is both actionable and "applicable worldwide." This chapter has aimed to demonstrate that all cases are *not* necessarily equal when it comes to defining key challenges and devising proper strategies for dealing with them, but here, FM 3-24 offers little guidance. The manual briefly discusses how insurgent methods often fall into one of six different categories,⁴⁵ how they vary in their purposes and objectives,⁴⁶ and how

⁴⁴ This is one of the central themes in Kaplan 2013.

⁴⁵ FM 3-24, pars. 1-25 to 1-30, 3-116 to 3-119.

⁴⁶ Ibid, pars. 1-15 to 1-23.

“contemporary insurgents may use different approaches at different times.”⁴⁷ But though the manual mentions distinctions among insurgencies and makes clear that decision makers should be sensitive to them, there is very little discussion of just how commanders should actually adjust their behavior from one case to another. In total, FM 3-24 contains 63 figures and tables summarizing key points. Nearly all of them establish general principles for counterinsurgency without providing commanders with explicit guidance on how to adapt to particular circumstances. The one exception⁴⁸ distinguishes among different ideal-type opponents without saying how these diagnostics should affect a commander’s prescriptions. As political scientist Stathis Kalyvas argues, FM 3-24 thus “espouses a definitively *nomothetic* posture, adopting a unified framework that sees these differences as variations on a single theme. It thus focuses on the common or universal characteristics of all insurgencies.”⁴⁹

To emphasize this point, it is worth mentioning that even though General Petraeus was one of the two military commanders who oversaw the development of FM 3-24, many of the most important changes he made to U.S. strategy in Iraq in 2007 worked against (or at least outside) the field manual’s recommendations. As Biddle describes it, “the manual assumes that insurgencies represent a contest for the loyalty of a mostly uncommitted general public that could side with either the government or the insurgents.”⁵⁰ In this way, FM 3-24 adopts the perspective that counterinsurgency generally constitutes an ideologically-driven problem like the

⁴⁷ Ibid, pars. 1-39 and 1-25. Those categories are: conspiratorial, military-focused, urban, protracted popular war, identity-focused, and composite and coalition.

⁴⁸ Table 3-6, “Potential Indicators of Insurgent Approaches.”

⁴⁹ Kalyvas 2008: 352.

⁵⁰ Biddle 2008: 348.

War in Vietnam, and its guidance is thus much more in accordance with the way that General Casey assessed the conflict from 2004-2006 than the way that General Petraeus approached the problem in 2007.

While the manual describes the importance of using counterinsurgent forces to protect the population, for instance, it generally describes the role of these forces as being to support the host nation and to reinforce its legitimacy – but in Iraq, where one of the central problems was that the central government was not an impartial ruler seeking to exercise legitimate authority over the whole country, it was a mistake to assume a common interest. FM 3-24 emphasizes the importance of rapidly developing host nation security forces – but in Iraq, the Army and Police were often enablers or direct participants in sectarian conflict, and thus one of the most important innovations of U.S. policy in 2007 was supporting widespread decentralization of security provision by way of the Sons of Iraq units which lay outside of formal security structures. This kind of measure, though widely seen as being critical to the reduction of violence in Iraq, is not described in FM 3-24 at all.⁵¹

“Perhaps ironically,” Biddle thus wrote in his review, “Iraq is precisely the kind of nonideological communal war of identity that the manual is least suited for,” even if many of its recommendations (on matters such as employing unified command, limiting the use of force, and emphasizing human intelligence) were nevertheless relevant to that case.⁵² These tensions help to demonstrate how when it comes to laying out prior assumptions like those in FM 3-24, one of the key challenges is sorting through which assumptions apply in certain cases, which are irrelevant,

⁵¹ On the role of the Sons of Iraq movement in reducing violence, see Lynch 2007, Long 2008, Rosen 2008, Simon 2008, Green 2010, and Biddle, Friedman, and Shapiro 2012.

⁵² Biddle 2008: 349.

and which may in fact be counterproductive. This is essentially a matter of dealing with the reference class problem, and it is a subject which existing doctrine tends to underemphasize.

Limitations of public debate

If military doctrine did not provide structured guidance for managing the reference class problem in Iraq, public debate about the occupation generally skirted the issue as well. A full review of this debate is beyond the scope of the analysis here, but without making too many broad generalizations about the nature of that debate on the whole, it is possible to identify several ways in which even critics of U.S. policy did not necessarily engage with the war's cumulative dynamics, or deal with the importance of diagnostics before prescription.

For example, critics of the occupation focused a large part of their attention on establishing the extent to which U.S. officials had underestimated the extent of political violence following the invasion of Iraq. Central to this debate were efforts to convince the Bush administration to drop its insistence that the violence fell short of a "civil war" or an "insurgency"; to ridicule of the president's claim of "mission accomplished" following the close of major combat operations; and to publicize the rising costs of soldiers and civilians killed as a result of the war. Debates about the number of civilian casualties in Iraq were a topic of special attention as violence mounted in the summer of 2006. While the U.S. government claimed that roughly 50,000 civilians had died as a result of the violence, a widely-reported study published in the British

journal *The Lancet* used survey methods to argue that this estimate should instead be more like 600,000.⁵³

These casualty estimates, which thus ranged by an order of magnitude, took center stage in debates about whether to “stay the course,” switch strategies, or withdraw from Iraq. Questions about the costs of the conflict (and the extent to which U.S. officials had underestimated them) were valid and important in their own right. But as this dissertation has explained, just because a military strategy has not worked yet, this does not necessarily imply that it will not work soon, and there are many cases where rational decision makers should *not* become more pessimistic about their policies, even when they have invested substantial resources in them without succeeding. Logically speaking, focusing on the costs of the occupation of Iraq in hindsight did not say much about the prospective costs of fighting moving forward. And from a practical standpoint, those arguments did not gain much traction with U.S. officials.

One of the ironies of debates about U.S. policy in Iraq was thus that both the administration’s supporters *and* its critics often staked their claims on the fallacy of sunk costs. For advocates of continuing the occupation, this illogic was borne out in arguments about how it was important to finish the job in Iraq so as to justify the costs of the war, and to ensure that soldiers and civilians had not died in vain. There were political reasons to adopt this stance, of course, but from a purely rational standpoint, that argument is unsound – past costs should have no influence on thinking about the prospective costs and benefits of adopting some course of action moving forward. But by the same token, it is also unsound to use prior costs as an argument *against* continuing a given policy. As Chapter 3 made clear, all that matters for rational strategic

⁵³ Burnham et al. 2006. On this article, the controversy surrounding it, and alternative estimates of civilian casualties at the time, see Badkhen 2006, Fischer 2010 and Spagat 2010.

assessment is the expected *remaining* costs of fighting, and when it comes to dealing with cumulative processes, previous effort should not necessarily make decision makers more pessimistic about their policies. This dissertation has aimed to demonstrate that is genuinely difficult to determine how to revise expectations during the course of conflict about how much longer it might take and how much more it might cost in order to achieve strategic goals; backward-looking debates about the prior costs of the conflict are not well-suited to resolving this uncertainty.

The previous sections of this chapter have also indicated that when public debates about U.S. policy in Iraq focused on the nature of the conflict, they generally did so in the context of drawing parallels to the War in Vietnam – an analogy that was not particularly relevant, and often actively misleading, when it came to dealing with the sources of the violence confronting U.S. forces.

As a rough heuristic for establishing the widespread use of the Vietnam analogy, Figure 5.3 plots the number of times that Iraq and Vietnam were mentioned in the same newspaper reports from 2003 through 2006.⁵⁴ For comparison, Figure 5.3 also plots the number of times that Iraq was mentioned in the same article as Bosnia or Kosovo, two recent cases where U.S. forces were employed to stop communal violence that was much more similar to the dynamics of the War in Iraq than the ideological nature of counterinsurgency in Vietnam.⁵⁵ Throughout the timeline, references to Vietnam significantly outnumber those to the wars in the Balkans, generally by a

⁵⁴ These tabulations were created from a LexisNexis search of major U.S. newspapers and wires, with media mentions aggregated by quarter.

⁵⁵ See Biddle 2006 on the comparative relevance of the Vietnam and Balkans analogies.

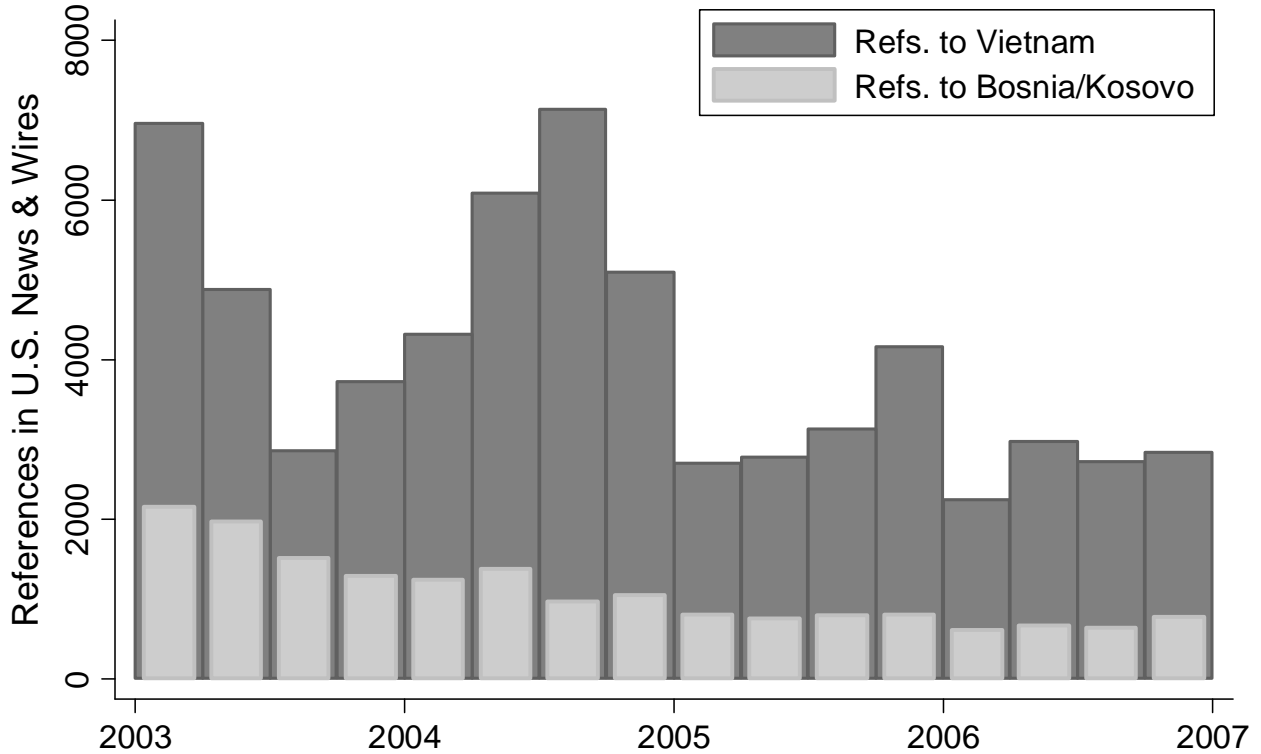


Figure 5.3. Media Mentions of Iraq and Vietnam or Bosnia/Kosovo, 2003-2006

Figure 5.3 displays the number of articles in U.S. newspapers and wires, aggregated by quarter from 2003 through 2006, mentioning Iraq and Vietnam, or Iraq and either Bosnia or Kosovo. This is a rough way of capturing how often these cases were associated in the U.S. public debate about the occupation of Iraq. The figure shows that Iraq and Vietnam were associated far more often than Iraq and Bosnia/Kosovo during the first four years of the war. There is no change in the pattern even throughout 2006, as intersectarian violence in Iraq was becoming more pronounced and the Vietnam analogy was becoming less tenable.

factor of four or greater. Moreover, the imbalance does not noticeably change during the course of 2006, even as sectarian violence spiraled after the bombing of the Samarra mosque.

Perhaps as a result of this “Vietnam redux” (to quote a passage cited above), even critics of the Bush administration’s policies like the Iraq Study Group did not significantly challenge the basic premises of U.S. military strategy which was designed to deal with problems of political ideology and nationalist resentment rather than the intersectarian drivers of communal conflict. People certainly disagreed on issues of feasibility, with administration supporters arguing that the strategy had a worthwhile chance of succeeding if given more time and detractors arguing that it was not worth staying the course with a policy that had yet to demonstrate results. But by and large, debates about U.S. policy took place within these parameters, without major alternatives views about the fundamental origins of Iraq’s civil violence.

There are several reasons why it is not necessarily surprising that the public debate did not do more to engage the reference class problem in Iraq. First, the Vietnam War was such a salient and formative experience that it would have naturally occurred to many people as being the analogy of first resort.⁵⁶ Second, the Vietnam analogy held obvious rhetorical appeal for critics of U.S. policy – while more recent experiences with peacebuilding in the Balkans might have been better comparisons in terms of dealing with the kinds of communal violence the United States confronted in Iraq, those operations had been much more successful than the Vietnam War, and this could have detracted from the political message critics were attempting to convey. Third, arguing that the Bush administration had misdiagnosed the conflict in Iraq may have

⁵⁶ See Hoffmann 1968, May 1973, Jervis 1976, Khong 1992, Reiter 1996, Rosen 2005, and Kahneman and Renshon 2007, among others, for literature on how people often view foreign policy through the lenses of analogies that, while suggested by salient surface similarities, may nevertheless impede careful analysis of fundamental problems.

opened the logical possibility that switching strategies might well achieve markedly better results, potentially justifying the notion that it would be worth deferring withdrawal in favor of experimenting with a different approach.

Perhaps for these and other reasons, the idea that Iraq's violence was being driven by intersectorian divisions quite separate from ideological and national resistance to the U.S. occupation remained a minority view among the general public through at least the summer of 2006. By focusing discussion on the prior cost of the war and by not challenging the prevailing "theory of the case" about the nature of violence in Iraq, both U.S. officials and their critics were unable to engage with the analytic challenges of strategic assessment in a convincing manner, and these debates failed to converge towards consensus. The theoretical framework in this dissertation has explained why that lack of convergence is not necessarily surprising given the cumulative dynamics of armed conflict, and suggested that improving strategic assessment largely depends on honing conceptions of the proper reference class for viewing a given conflict. But while the Iraq case indicates that changing policy makers' views of the nature of the conflict was indeed a critical component of their decision to switch strategies, this issue was not a major aspect of public debates at the time.

Section 5.4. The role for scholarship

In principle, scholarship can play an important role in providing rigorous, evidence-based ways of dealing with the reference class problem in strategic assessment. The reference class problem is in large part a matter of empirical analysis, examining open questions about the extent to which hypothesized distinctions among cases actually seems to correlate with meaningfully

different dynamics. What *are* the most important ways in which conflicts tend to differ? How *do* these dynamics impact the efficacy of different strategies for stabilization or conflict management?

Answering these questions forces analysts to deal with issues of defining the proper case universe, operationalizing relevant variables and gathering appropriate data, and then analyzing that evidence in structured ways that are sensitive to common confounds such as omitted variables bias and strategic selection. There is little inherent difference between forming the kinds of prior assumptions that drive strategic assessment and conducting theoretically-informed, empirically-grounded social science – these enterprises essentially involve the same analytic issues, with the traditional distinction being largely a matter of how rigorously analysts approach those issues, and how they intend for their findings to be used.

Dealing with the reference class problem in strategic assessment is thus a place where there is an unusual amount of overlap between the kinds of information that decision makers need in order to structure their decisions effectively, and the kinds of information that the social scientist's toolkit is suited to provide. As while few social scientists have access to the kinds of classified information that drive the final outputs of military decision making, the basic doctrinal assumptions which guide that process are – in the words of FM 3-24 – a matter of “broad historical trends” that are the traditional focus of cross-sectional scholarship on armed conflict.⁵⁷

This does not mean that analyzing these trends is actually easy to do. In fact, one of the reasons that it is important to approach the reference problem carefully is that so many

⁵⁷ For a broader discussion of the interrelationships between military doctrine and social science, see Friedman 2012.

seemingly obvious ways to divide conflicts into different subsets do *not* in fact lead to the observable distinctions one might expect. To demonstrate this point, Table 5.1 presents data on 286 insurgencies between 1815 and 2006.⁵⁸ These data are parsed into 15 different reference classes that could be potentially relevant for structuring expectations about the typical durations of insurgencies. For each of these reference classes, Table 5.1 reports three statistics: the mean duration of insurgencies within the reference class; the median duration of insurgencies within the reference class; and if we considered the war in Iraq to be a member of that reference class what its percentile rank would be in terms of duration.⁵⁹

Across all 286 insurgencies examined here, the average conflict lasts 6.8 years, the median insurgency lasts only 3.4 years, and thus the insurgency in Iraq is one of the longer insurgencies on record (it falls into the 77th percentile). Table 5.1 then demonstrates – perhaps surprisingly – that several ways of making the reference class more relevant to the characteristics of Iraq could have made it even harder to predict the extended nature of the insurgency.

⁵⁸ These data are drawn from Lyall and Wilson 2009: 70, who define an insurgency as a movement that “(1) uses small, mobile groups to inflict punishment on the incumbent through hit-and-run strikes while avoiding direct battle when possible and (2) seeks to win the allegiance of at least some portion of the noncombatant population. An insurgency is therefore not synonymous with “civil war” since civil wars can be fought conventionally (that is, with direct battles between opposing armies), with guerrilla tactics, or through nonviolence.”

⁵⁹ For the purposes of drawing these estimates, the “duration” of the insurgency in Iraq is considered to be eight and one-third years: the time between the end of major combat operations in April 2003 and the official conclusion of all U.S. combat operations in August 2010. It is important to note that this is probably an *underestimate* of the duration of the insurgency in Iraq because that insurgency continued after the official conclusions of U.S. combat operations (and U.S. troops stayed in Iraq past this date to continue assisting with the counterinsurgency effort). Needless to say, underestimating the duration of the insurgency in Iraq will make that conflict seem like *less* of an outlier, bringing the case closer to the mean of each reference class in Table 5.1.

Table 5.1. Comparing the Duration of Insurgencies across Different Reference Classes

<i>Reference Class</i>	<i>N</i>	<i>Mean</i>	<i>Median</i>	<i>Iraq pct.</i>
All insurgencies	286	6.8	3.4	77 th
All insurgencies since 1914	171	7.1	4.4	70 th
All insurgencies since 1945	129	8.0	5.2	65 th
All insurgencies since 1975	59	6.9	4.4	70 th
Insurgencies involving external occupation	112	4.5	2.6	85 th
Ethnic or religious insurgencies	171	6.3	3.5	75 th
Ethnic insurgencies only	151	5.9	3.4	77 th
Religious insurgencies only	75	6.9	2.5	72 nd
Religious insurgencies w/ Muslim insurgents	37	4.7	2.3	84 th
Insurgencies involving great powers	108	4.4	2.4	86 th
Insurgencies in the Middle East	53	4.9	2.4	81 st
Insurgencies in Iraq	6	2.5	0.8	100 th
Ethnic/religious insurgencies in the Mid. East	42	5.2	2.5	79 th
Ethnic/religious insurgencies in the Mid. East, with occupiers	21	3.8	2.0	91 st
Ethnic insurgencies in the Middle East, with occupiers, since 1945	6	6.4	5.1	86 th

For each reference class of insurgencies, Table 5.1 gives: the number of relevant insurgencies recorded in Lyall and Wilson (2009); the mean duration of those insurgencies, in years; the median duration of those insurgencies, in years; and what percentile rank in the data the occupation in Iraq would be if we considered its duration to be 8.33 years. Note that in all reference classes – and especially in the more precise reference classes at the bottom of the list – the occupation of Iraq had an unusually long duration.

For instance, it is typically assumed that the insurgency in Iraq was fueled by animosity against an external occupier. But insurgencies fought against external occupiers are on average *shorter* than the norm: they last only 4.5 years at the mean and 2.6 years at the median (a finding

that may be driven by the notion that occupiers tend to be great powers who bring powerful military capabilities which host nations often lack by themselves). Iraq's insurgency has both ethnic and religious components. Yet these factors do not correspond to significantly longer insurgencies on average, either. In fact, the average Muslim insurgency lasts only 4.7 years and the median Muslim insurgency lasts only 2.3 years. While the Middle East is typically thought to be a particularly challenging region in which to fight, insurgencies there also tend to be relatively short (4.9 years at the mean and 2.4 years at the median). Insurgencies in Iraq itself have been particularly brief, with the occupation of Iraq being longer than any of the other six Iraqi insurgencies in Lyall and Wilson's data.⁶⁰

The bottom of Table 5.1 examines the way that reference classes overlap: the categories here are ethnic/religious insurgencies in the Middle East, ethnic/religious insurgencies in the Middle East with external occupiers, and ethnic/religious insurgencies in the Middle East with external occupiers that have occurred since World War II. As these reference classes narrow, they become more conceptually precise. But they do not come any closer to being able to predict the protracted nature of the insurgency in Iraq, whose duration would fall at or above the 79th percentile in each of these reference classes. Ironically, narrowing the reference class make misperceptions about the prospective duration of the insurgency in Iraq seem *more* reasonable. It may very well be true that "most senior civilian and military leaders failed to review the historical records of military occupations and of Middle Eastern or Iraqi history"⁶¹ and it may be

⁶⁰ The six insurgencies in Iraq listed in Lyall and Wilson 2009 are the Iraqi-British campaign (1920-21), four Kurdish Rebellions (1961-66, 1974-75, 1980-88, and 1991) along with the Shia Rebellion in 1991. Note, however, that even when looking solely at insurgencies in Iraq, the members of this reference class have borderline relevance for thinking about the invasion in 2003.

⁶¹ Wright and Reese 2008: 569.

true that key members of the Bush administration were irrationally overconfident.⁶² But it is not entirely clear how an objective overview of that history would have necessarily demonstrated that certain assumptions were flawed.⁶³

Table 5.1 also helps to raise a second important challenge in dealing with reference classes, which is that as these groupings become more precise, this constrains the amount of data that are available for drawing inferences. The last reference class in Table 5.1 – ethnic/religious insurgencies in the Middle East with an external occupier that was fought since World War II – is still a very broad-brush way to think about the nature of the occupation of Iraq. But there are only six examples of previous insurgencies (at least, according to Lyall and Wilson’s well-regarded data) that fit these criteria. As we have progressively less previous experience to work with, it becomes increasingly difficult to draw useful lessons.

Ideally, we would like to think that when we make reference classes more *conceptually* precise, then their predictions would also become more *statistically* precise. In reality, these characteristics generally trade off. As Wesley Salmon explains, “since increasing the reliability of statistics generally tends to broaden the class and since narrowing the class often tends to

⁶² Johnson 2004: ch. 8.

⁶³ Perhaps the most important historical warning sign of a protracted insurgency in Iraq was the fact that Saddam Hussein needed to resort to widespread, repressive measures in order to contain Shiite resistance to his regime. In this interpretation, U.S. officials could have based their expectations not on the insurgencies that *did* occur in Iraq, but rather those that *might* have occurred if Saddam had not employed such brutal tactics (which the United States was not willing to reprise). There is nothing in this chapter which suggests that this line of reasoning is invalid. However, this argument relies on the interpretation of historical *counterfactuals*, which is very different from interpreting historical *evidence*. Historical counterfactuals play an important role in scholarship and policy analysis, but they are notoriously difficult to define or evaluate with precision. It is unclear how a rational decision maker should draw on this kind of information in structuring expectations, though again, this is not to say that counterfactual reasoning should not have played a role in planning for the occupation of Iraq.

reduce the reliability of the statistics, the principle involves two desiderata which pull in opposite directions.”⁶⁴ Especially when dealing with relatively small data sets – which is usually the case in cross-sectional analysis within strategic studies – narrowing the reference class can quickly lead to vanishingly small sample sizes and expanding amounts of predictive uncertainty.

Figure 5.4 demonstrates this problem by progressively narrowing the reference class based on the following elements of the occupation of Iraq: (1) it is an insurgency; (2) it takes place in the Middle East; (3) it revolves in part around ethnic/religious issues; (4) the counterinsurgent is a foreign occupier; (5) the conflict takes place since 1918; (6) the conflict takes place since 1945; (7) the counterinsurgent is a great power. Each time we nest one of these characteristics within the others, the reference class gets smaller. The mean duration of conflicts within these reference classes does not change very much across each grouping, but the uncertainty surrounding these estimates (shown in Figure 5.4 as the 95% confidence interval of how long the average conflict within each reference class should last) continues to widen. Even though the reference class becomes more conceptually precise as we continue to narrow it, the resulting estimates become less useful. In the final category, the uncertainty is so large that the confidence interval is literally meaningless: it includes the possibility that the average insurgency might have a negative duration.

In determining just what kinds of factors make a difference when it comes to estimating how long it might take or how much it might cost to defeat an insurgency, therefore, there are real constraints on performing rigorous analysis and the answers are not necessarily obvious. And if the last section showed how military doctrine tends to focus on *ceteris paribus* patterns rather

⁶⁴ Salmon 1970: 41.

Table 5.2. Predicted duration of insurgencies, by nested reference classes

<i>Reference Class</i>	<i>N</i>	<i>Average Duration, Low Bound</i>	<i>Average Duration, High Bound</i>
Insurgencies	286	5.1	6.9
Insurgencies in the Middle East	53	3.3	6.4
Ethnic insurgencies in the Middle East	39	3.4	7.3
Ethnic insurgencies in the Middle East, with foreign occupiers	21	1.8	5.8
Ethnic insurgencies in the Middle East, with foreign occupiers, since 1918	15	1.8	7.1
Ethnic insurgencies in the Middle East, with foreign occupiers, since 1945	6	0.1	12.7
Ethnic insurgencies in the Middle East, with great power foreign occupiers, since 1945	3	-1.2	11.3

Table 5.2 provides the 95% confidence interval for the mean duration of insurgencies that fall into increasingly specific reference classes. As the reference classes become more specific, there are fewer observations within them. As a result, the confidence intervals grow. In the final reference class, the confidence interval is so large that it conveys information that is literally meaningless: it includes the possibility that the average insurgency might have a negative duration.

than ways in which cases differ, the same might generally be said for scholarship on armed conflict as well. As with work in the social sciences more broadly, empirical work on armed conflict often aims to capture “average treatment effects”: the way that independent variables relate to outcomes, all else being equal.⁶⁵ The significance of those patterns is typically judged by how consistent they are and how little they vary. These findings are important in their own right, but as this chapter has shown, this information is by definition ill-suited to giving decision makers the information that they need in order to perform strategic assessment and to understand actionable links between diagnostics and prescription.

Together, the theoretical framework developed in this dissertation along with the U.S. experience in Iraq suggest that effective strategic assessment revolves around a set of conceptual issues and empirical information that tend to fall outside the scope of policy discussions, doctrinal manuals, theoretical frameworks, and public debates. The following chapter will conclude by summarizing these arguments and drawing out some of their lessons and broader implications.

⁶⁵ For example, Sambanis 2001: 259 provides the following characterization of the literature on civil war initiation: “A wave of theoretical and empirical research has recently helped identify important economic and political determinants of civil war onset and prevalence. However, common to all these studies is the practice of aggregating civil wars in a single category.”

Chapter Six: Conclusion

LESSONS AND BROADER IMPLICATIONS

For roughly forty years, one of the most prominent debates in physics revolved around a particle called the Higgs boson. The Higgs boson plays a key role in explaining how matter in the universe comes to be endowed with mass; it was once famously dubbed the “God particle,” and governments around the world have spent billions of dollars trying to find it. Particle accelerators like the Tevatron in Illinois or the Large Hadron Collider in Switzerland smash matter together at high energies in a way that dislodges subatomic fragments. Using this method, physicists have made many important discoveries, but decades of searching for the Higgs boson initially came up empty.

The problem with hunting for the God particle was that scientists were unsure of how to find it. Standard theoretical models predict that the particle will emerge if accelerators operate at high energies – but they cannot say exactly *how much* energy is needed in order to obtain this result. For that reason, physicists had to search for the particle across a wide range of experimental conditions. Careful measurement could indicate that the particle does *not* appear at a given energy level, but this said little about whether the next setting might be the one that produced the major breakthrough. Thus while some physicists often predicted that the discovery was just around the corner, there were always others who were not so sure: some experts hypothesized that we might not have the technical capacity to find the Higgs boson for decades; some argued

that it is impossible to detect or that physicists were looking for it in the wrong way; and of course, it was always possible that the particle simply did not exist at all.¹

The bottom line was that, at any given point in time, scientists had little idea of how much longer it might take or how much more it might cost to find the Higgs boson. And this led to heated debates because particle accelerators are so expensive. The Tevatron, for instance, had an operating budget of roughly \$100 million per year. Fermilabs continually needed to make the argument that its lack of success in the past did not preclude finding the Higgs particle in the future, that its experiments were always becoming more efficient and precise, and that it was closing in on one of the most important discoveries in modern science. When an economic recession forced the U.S. Department of Energy to cut more than a billion dollars in spending, it finally closed the Tevatron in October 2011. Many physicists howled that the government was pulling the plug just as the project hung on the cusp of success. And sure enough, when the Large Hadron Collider went into operation the following summer, its scientists announced that they had found the Higgs boson (or, at least, evidence strongly consistent with the notion it exists), an event that constituted one of the most high-profile discoveries in modern science.

The previous chapters have shown how debates about military strategy often play out in similar ways, as decision makers struggle to form and revise their expectations of how long it might take or how much it might cost to achieve their desired goals, and debate whether their past failures preclude the hope of subsequent success. This dissertation has explored the analytic challenges in doing so, and argued that understanding these challenges can shed new light on

¹ On the hunt for the Higgs particle in general, see Folger 2011, Lederman 1993, and Sample 2010. On physicists' forecasts, see Brumfiel 2011. Various views about the discovery potential of recent experiments include Jakobs and Schumacher 2008, Stancato and Terning 2009, and Hawking 1996.

theory, history, and public policy debates. Chapter 6 concludes that effort by summarizing main points. It then extends the analysis in additional directions, describing why understanding cumulative dynamics has significant implications for empirical research design, as well as how the theoretical framework advanced in this dissertation applies to fields beyond national security, including the search for scientific breakthroughs.

Section 6.1. Summary of main arguments

This dissertation has examined the conceptual foundations of strategic assessment, explaining how decision makers form prior assumptions about how long it might take and how much it might cost to achieve desired goals, and how they revise those expectations in the course of implementing their policies. In almost all forms of armed conflict and in many other kinds of national security policy, one of the central questions scholars ask is why decision makers find it so difficult to evaluate their policies or why they often stick to unsuccessful strategies for so long. This behavior is often assumed to represent nonrational influences such as organizational constraints, domestic politics, or psychology, but the previous chapters have developed a theoretical framework for explaining how even rational actors may often display these tendencies.

This theoretical framework begins with the problem of assessing uncertainty. Before hostilities begin, decision makers must account for the notion that opponents can vary a great deal in terms of their capabilities and resolve. Through a combination of inductive and deductive reasoning, decision makers must make subjective judgments about the expected costs of fighting. But even when these judgments are formed in ways that are deductively reasonable and

empirically grounded, there will often be cases in which decision makers' prior assumptions substantially understate the challenges that they face: all distributions have outliers, and those outliers are by definition difficult to predict *ex ante*.

The second component of strategic assessment is revising prior expectations as an armed conflict unfolds. Here, this dissertation made an especially sharp break from existing literature by explaining how once decision makers have formed initial misperceptions of how long it might take and how much it might cost in order to achieve their goals, then even perfectly rational learning may not allow them to realize the extent of their initial errors. When war is seen to be a cumulative process, then even if the expected total costs of fighting will continuously increase as a war progresses, the expected *remaining* costs of fighting can remain constant or even decline. Altogether, this framework explains why decision makers will often be unable to avoid, realize, and correct their strategic mistakes.

The previous chapters have demonstrated not just that this theory can be logically constructed in the abstract, but that it also serves as an empirically plausible way to reinterpret salient historical experience. In the American Indian Wars, for instance, one of the central challenges facing U.S. commanders was to identify the outliers like the Sioux and Seminoles, groups who could sustain and inflict exceptional levels of punishment in relation to the bulk of historical experience. Similarly in the Vietnam War, U.S. decision makers struggled to understand that the insurgents would be able to withstand levels of attrition that were all but unprecedented. A half century later, U.S. officials entered the occupation of Iraq with prior assumptions that were grounded in historical experience – but it was a misleading set of historical experience on which to draw, and it took nearly four years for civilian and military leaders to understand not just the extent of instability in Iraq, but also the *nature* of the violence that confronted them.

The difficulties of strategic assessment do not condemn decision makers to failure. Ultimately the United States defeated its opponents in almost every one of the American Indian Wars.² The tribes which commanders most severely underestimated also tend to be the ones that stand out most prominently in historical memory, but Chapter 4 made clear how they were far from being the norm. The opening stages of the occupation of Iraq may have been mishandled, but the change in strategy in 2007 preceded a drop of violence of roughly ninety percent that seemed to offer at least temporary promise. And in some cases, worries that set in amidst initially unsuccessful military operations simply end up seeming misplaced. When initial foreign military support to Libyan rebels did not topple Muammar Qaddafi's regime in spring 2011, for instance, criticisms that the mission had been seriously botched were at one point a matter of high concern for the United States and its NATO allies – but the Libyan rebels regrouped and ousted Qaddafi by the fall, and the pessimism that once swirled around this campaign will probably end up being a historical footnote at best to what will likely be remembered as a relatively smooth instance of international intervention.

Yet these examples only serve to emphasize that even in cases where military decision makers achieve the objectives they were looking for, they still tend to find it difficult to assess strategic progress in clear and rigorous ways. The principal aim of this dissertation has been to explain why this task involves genuine analytic challenges which contemporary scholarship typically does not take into account. Here are some of the main takeaways from this analysis.

² This is not to endorse U.S. military decision making, but only to show that the assumptions behind strategic decisions were often much more in line with objective, empirical evidence than the conventional wisdom entails.

1. War is not a slot machine

The central argument of this dissertation is that war involves cumulative dynamics that clash with many prominent theoretical frameworks (and often people's basic intuitions), which typically revolve around analogies to repeated processes like gambling. Repeated processes form the basis of a wide range of scholarly literature on learning and adaptation. In this literature, the notion that decision makers are observing outcomes that are independent and identically distributed is essentially the assumption of first resort. Comparisons to slot machines, roulette wheels, and card tables appear regularly in descriptions of relevant models, while people often use terms like "the gambler's fallacy" or "doubling down" to describe decision makers' behavior more broadly. Armed conflict, however, involves cumulative processes that have very different dynamics, along with a different logic for how rational actors should form and revise their expectations. These cumulative dynamics affect strategic assessment and decision making in ways that scholars typically overlook. Understanding these dynamics helps to explain why many debates about military strategy – much like the debate about hunting the Higgs boson – do not converge over time, why military decision makers often find it so difficult to evaluate their policies via trial-and-error, and why they often stick to unsuccessful strategies for so long.

2. Just because a strategy has not worked yet, this does not imply it will not work soon

This is not to justify "staying the course" when military strategies are unsuccessful, or to argue that this will always be the case, but to point out how existing theoretical frameworks provide an incomplete and potentially misleading foundation for understanding learning and adaptation in armed conflict. For instance, Chapter 2 showed how prominent international

relations literature on the bargaining model of war explicitly relies on the notion that armed conflict is a repeated process. This literature predicts that the longer decision makers go without achieving their intended objectives, the more pessimistic they should become about their ability to do so, and the more likely they should be to change course. One of the principal implications of this literature is that warfare is thus a self-terminating process, automatically providing information that gradually eliminates the bargaining problems that can cause war in the first place.

Understanding how war is a *cumulative* process, however, provides an opportunity to re-evaluate this theoretical framework and to question the assumptions that drive its main findings. The simple act of fighting without achieving strategic goals offers indeterminate implications for strategic assessment, and can cause combatants' perceptions to diverge. It can be perfectly logical for decision makers to become more optimistic about their strategies even when they continually fail to achieve their intended goals. The notion that this kind of behavior is irrational depends either on the notion that war is a repeated process, or on the notion that decision makers have certain kinds of prior assumptions about the kinds of conflicts they are waging and what the distribution of potential opponents types might be. Contemporary models of learning and adaptation in war generally *do* treat war as a repeated process, and they generally do *not* see strategic assessment as being conditional on prior assumptions. This dissertation argued that these are both important oversights with significant implications for the theory and practice of military decision making.

3. The value of establishing a rational baseline

One reason that it is important to question these theoretical frameworks is that they serve as benchmarks for assessing historical experience and policy debates. It is difficult to evaluate the rationality of any particular decision makers' behavior unless we first define what rational action would actually entail.

For instance, this dissertation examined U.S. military behavior during the American Indian Wars, the War in Vietnam, and the occupation of Iraq. In all of these cases, decision makers who struggled to realize and correct their mistakes are typically seen to have acted in a manner largely influenced by organizational inertia, political agendas, psychological constraints, or personal hubris. Yet this dissertation provided reasons to question those judgments. In some cases, inertia may be inevitable when it comes to strategic assessment and military decision making. In some cases, the relevant question might not be why decision makers failed to understand the nature of the challenges they were facing, but rather how they *could* have understood them – especially in cases like the World Wars or Vietnam, which are extremely salient experiences, but largely on account of how much they differed from historical norms.³

At the very least, the previous chapters have aimed to show that decision makers in these kinds of cases confront genuine analytic challenges which even perfectly rational actors would struggle to resolve, and that even these decision makers' sharpest critics tend not to engage

³ As Chapter 1 pointed out, theories of rational action can thus play an important role in framing empirical analysis, even without assuming that rational actor theories are descriptive. Anthony Downs's economic theory of democracy, for instance, substantially restructured debates about political participation – whereas previously the empirical puzzle had been why voter turnout was so low, Downs explained why it might be more relevant to ask why anyone voted at all given the infinitesimal probability that their ballot would make a difference in an election's outcome. See Downs 1957 and Monroe 1991.

directly. Understanding how cumulative dynamics affect strategic assessment thus offers a new rational baseline on which historical judgments, empirical predictions, and policy implications depend.

4. Diagnostics before prescription

Defining the analytic challenges of assessing cumulative processes is also, of course, the first step in determining how to mitigate them. This was the central subject of Chapter 5, which explained how decision makers and military doctrine tend to place insufficient emphasis on defining the proper reference class for viewing specific armed conflicts. Scholars, too, tend to place a great deal of emphasis on identifying common patterns that hold across cases, rather than identifying ways in which circumstances differ and thus require different responses. Developing rigorous taxonomies for matching diagnostics and prescription in this way is a difficult challenge that is typically underappreciated. This is especially the case given that (as Chapter 3 demonstrated) describing decision makers' prior assumptions is a logical requirement of predicting whether they should become more optimistic or more pessimistic as rounds of fighting proceed.

5. Focus on the theory of the case, not rounds of fighting

One of the more pessimistic implications of this analysis is that in many circumstances, there may simply be no feasible way to analyze a policy's prospects based on the extent of time and resources that decision makers have already sunk into it. The American Indian Wars were such a useful set of cases to examine here because they offer scholars an unusual opportunity to sidestep

the challenges of dealing with the reference class problem in forming inductively-oriented assumptions about the typical cost and duration of armed conflict. Because the American Indian Wars offer a relatively large cross-section of comparable cases, it was possible to evaluate decision makers' expectations in light of objective empirical evidence. But for other cases – the wars in Iraq, Afghanistan, and Vietnam, for instance – it is much less clear what the relevant historical comparisons should be. Without an objective base of evidence on which to draw, there are clear limits on scholars' ability to specify the kinds of prior assumptions that would have driven rational strategic assessment. It is even harder to imagine that kind of analysis making headway in contentious political debates about ongoing military operations.

Perhaps the most effective way to improve (or to challenge) existing strategic assessments is thus to focus on the basic assumptions policy makers put forth about the kind of challenge they are facing, and how they define the reference class of previous experience for structuring their policies and expectations. As Chapter 5 demonstrated with respect to debates about the occupation of Iraq, changing the “theory of the case” played a substantial role in causing U.S. officials to abandon an unsuccessful strategy – but both the administration and its critics tended to avoid discussing these assumptions directly in the war's early years. Much of the debate about the war in Iraq revolved around how long it had already taken and how much it had already cost. This dissertation has explained why those arguments will often be inconclusive, and why it may be best to focus on other factors when analyzing military strategy.

Section 6.2. Implications for empirical research design

In addition to reframing basic expectations of strategic assessment and military decision making, cumulative dynamics have important implications for empirical research design when it comes to identifying patterns of military cause-and-effect. To draw out this issue, consider the question of whether killing or capturing insurgents tends to increase or reduce the probability that counterinsurgents will be successful. This is one of the most fundamental questions in the study of modern military strategy, and it is hard to imagine that scholars can develop a convincing answer to this question without taking the cumulative dynamics of conflict into account.⁴

The intuitive way to investigate this question would be to use a standard regression framework. We could plot attrition on the x-axis and outcomes on the y-axis; a regression model would estimate the slope of this relationship, and this would produce an estimated “average treatment effect” for attriting insurgents. Empirical scholars would presumably go on to refine these estimates by accounting for potential confounds such as measurement error, sampling bias, and selection effects. These are important issues that regularly impede causal inference. But even if these issues were resolved, cumulative dynamics would still raise fundamental confounds to drawing causal inferences.

The problem stems from the fact that different insurgencies will presumably respond to attrition in different ways. The data will thus constitute a *mixed population*. Of course, almost all populations are mixed in some respects, and typically this does not block statistical inference. This is why the output of statistical analysis is generally interpreted as an “average” treatment

⁴ For overviews of the debate about the use of force in counterinsurgency, see Smith 2005, Ucko 2009, Kilcullen 2010, and Kaplan 2013, among others.

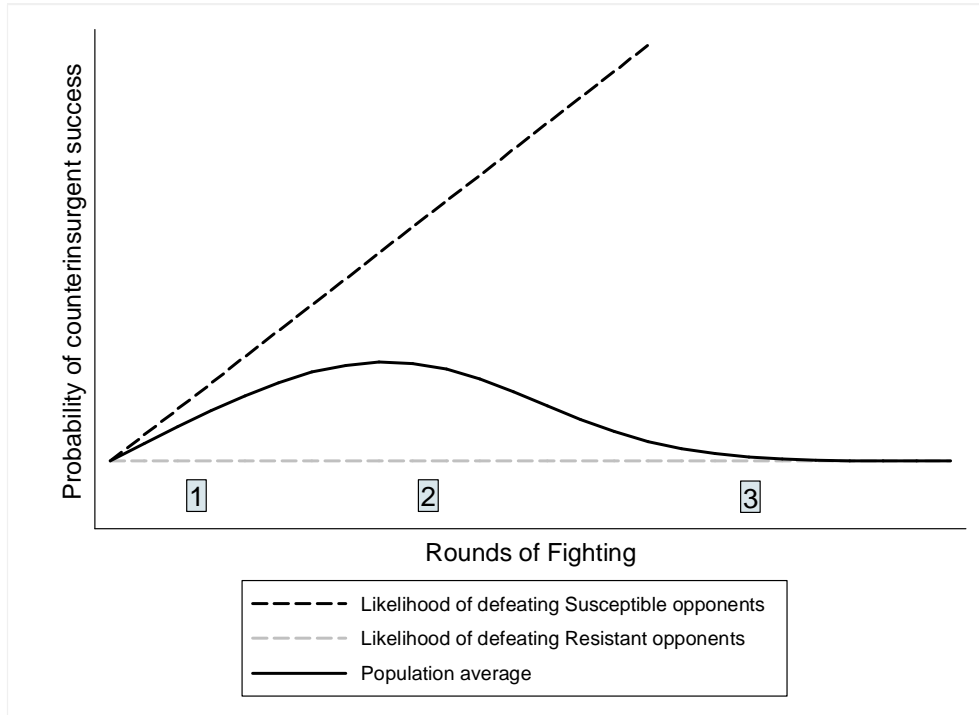


Figure 6.1. Cumulative Dynamics and Mixed Populations (Theoretical Schematic)

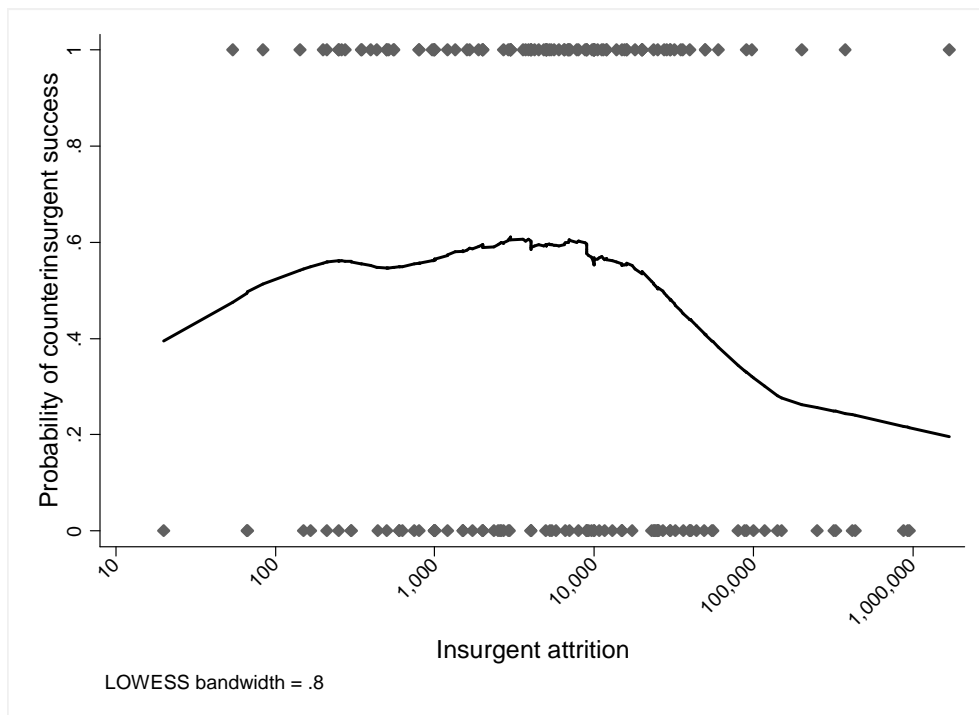


Figure 6.2. Insurgent Attrition and Counterinsurgency Outcomes (Actual Data)

effect: though individual cases presumably deviate from this average in any number of ways, it still often represents a reasonable estimate for whether some policy “works,” all else being equal.

But when dealing with cumulative dynamics, regression models are not only imprecise tools of capturing policy effectiveness – their output can also be fundamentally misleading. Figure 6.1 helps to explain why this is so. Figure 6.1 assumes that insurgencies constitute a very simple kind of mixed population, in which each member belongs to one of two groups. To be consistent with the contemporary theoretical literature, we can characterize these groups by how they respond to multiple “rounds of fighting.” The first group is not affected by the mounting costs of fighting, and so it is labeled “Resistant”: no matter how many rounds of fighting they conduct, their odds of being defeated always remain a constant probability, α . By contrast, the second group of opponents becomes increasingly brittle as their losses mount, and opponents in this “Susceptible” group are thus more likely to be defeated in each round of fighting. For simplicity, define the probability of defeating a susceptible opponent in the x^{th} round of fighting as $p(x) = \alpha + \beta x$.⁵ Figure 6.1 plots rounds of fighting on the x-axis, and the chances of defeating each kind of opponent on the y-axis.

Given the way we have designed this thought experiment, the average treatment effect of fighting is positive. We assumed that there is no case in which fighting is counterproductive. The chances of defeating an opponent in each round of fighting are never less than α , and when facing Susceptible opponents, the odds of strategic success continually increase.

⁵ Of course, the probability of defeating susceptible opponents cannot exceed 1. The pattern would not be much different if we modeled the Susceptibles’ response to casualties as a probit or logit function; nor would it be much different if we randomized β – see the appendix for a more thorough and formal discussion.

Yet this is not what a standard regression framework will capture, because regression models examine population averages, and these averages will tell a different story. The population average in Figure 6.1 is represented by the solid curve, which captures the proportion of insurgents who are expected to concede in each round of fighting. Note how this curve rises initially, but then it peaks and continuously declines.

An appendix to this chapter provides a more formal discussion of this phenomenon, but to see why it occurs, consider what takes place in the first few rounds of fighting, in the region of the figure labeled with a '1.' Moving rightward from the origin, the proportion of opponents defeated in each round of fighting rises as Susceptibles surrender at an increasing rate. However, as more of the Susceptible opponents are defeated, they will comprise a progressively smaller fraction of the population that remains. The population of opponents that we observe in each round of fighting will thus continually "improve" itself, as the Susceptible opponents are culled out.⁶ By the time we get to the latest rounds of fighting (the region in the figure denoted with a '3') there are almost no Susceptible opponents left. For this reason, the observed relationship between fighting and strategic success converges to the constant, α . This means that somewhere

⁶ In discussing the ability of mixed populations to "improve themselves" over time, one can draw an analogy to a large literature on survival rates in the medical sciences. Timo Hakulinen (1977), for instance, studied patients being treated for colon cancer: he found that the risk of death dropped sharply across this population following an intervention, but explained how it was difficult to determine how much of this improvement was due to people recovering from surgery, and how much was due to the way that heterogeneous populations naturally improve themselves as their least resilient members die off. Donald Shepard and Richard Zeckhauser (1980) extended a similar logic to evaluating the effectiveness of hernia repair, and took a significant step forward in generalizing the logic to a wide range of topics including students dropping out of school, rates of machine malfunctions, and patterns of criminal recidivism. Nancy Tuma and Michael Hannan (1984: 165-174) have since discussed these dynamics in the context of marital longevity; Mary Jo Bane and David Ellwood (1986) used a similar approach to modeling the length of time that households spend in poverty; Peter Leahy, Terry Buss, and James Quane (1995) built a related model to capture the amount of time that different households spend on welfare.

in the middle of the distribution (the region of the figure denoted with a ‘2’) the population average for the percentage of insurgencies who concede in each round of fighting must reach a maximum and begin to decline – even though the actual impact of fighting is positive, by definition, throughout.⁷

The observed population average captured by the solid curve in Figure 6.1 is not just a noisy approximation of real causal relationship. It is totally misleading: in many places, it has the wrong sign, and throughout it has the wrong functional form. And while we know that these results are misleading because they come from a thought experiment, it is unclear what we should make of similar patterns when we see them in actual data.

To demonstrate this point, Figure 6.2 *does* present actual data about the observed relationship between the amount of attrition inflicted on more than 200 insurgent movements, and whether those insurgencies were defeated.⁸ The population average of these data is represented with locally-weighted scatterplot smoothing. The result is exactly the kind of curve that we saw in the thought experiment: it rises, peaks, then falls. (The data even match the theoretical model in terms of second-derivatives: prior to the peak the curve is generally concave, and after the peak it is generally convex.)

⁷ The axes in this figure are not labeled so as to emphasize the generality of the dynamics it represents. The precise parameter values are: $\alpha = .05$ and $\beta = .025$; Susceptible opponents comprise two-thirds of the initial population. Those parameters were chosen in order to make the misleading nature of the population average clear. The chapter’s appendix provides a more precise discussion of how these dynamics play out more generally.

⁸ This dependent variable is drawn from Lyall and Wilson’s 2009 study: they define a “defeat” for insurgents (and thus a win for counterinsurgents) as a situation where “the insurgency is militarily defeated and its organization destroyed or the war end without any political concessions granted to insurgent forces.” The independent variable on attrition represents the same data that were introduced in Chapter 3.

Because Figure 6.2 is *not* the result of a thought experiment, we have little basis for judging what these patterns really mean. For example, it is possible that the actual relationship between attrition and success rises and falls: perhaps there is some middle ground where a judicious use of force will degrade the insurgency without enflaming the opposition further. This interpretation would be consistent with a wide range of theoretical literature; there is certainly no reason to rule this interpretation out, and if this interpretation were true, it would have important implications about the necessity of rationing the use of force in counterinsurgency.

But as this section has shown, the pattern in Figure 6.2 is *also* consistent with a model in which the effect of attrition is strictly positive, just not uniformly so. It is entirely possible that if we separated the data on insurgencies into relevant subgroups – that is, if we divided cases into different reference classes – then we would find that counterinsurgent success rates strictly increased as a function of attrition. This is also consistent with a wide range of theoretical literature, and it would have very different implications for thinking about the wars in Iraq and Afghanistan and for structuring expectations about military strategy more broadly.

This discussion shows how cumulative dynamics make it difficult to interpret even the most seemingly basic properties of the data, such as whether a downward sloping coefficient means that a policy is counterproductive. (In fact, the more effective the policy is over a subset of the population, the more steeply we should expect the population average to *fall* as the susceptible opponents drop out of the sample more quickly.⁹)

It is also worth reiterating that the problem here is largely separate from the standard confounds to causal inference that empirical scholars often wrestle with. The problem this

⁹ Again, see the appendix for a more formal discussion of this point.

section explained has little to do with issues like measurement error, missing data, or reverse causality. Nor is it related to selection effects: the confound does not originate with the way that policies are *chosen* – it is in the way that opponents *respond* to those policies as their impact builds over time. Some insurgencies are bound to be more sensitive to attrition. As this pressure mounts, these opponents are more likely to concede, at which point the war will stop, and the “treatment” cannot extend any further. In effect, this means that the amount of pressure that militaries can place on their opponents is right-censored. This problem would exist even if the relevant policies were randomized.¹⁰

Thus while empirical methodology is not the main focus of this dissertation, this section indicates how a theoretical understanding of the cumulative dynamics of armed conflict has basic implications for thinking about empirical research design. Moreover, one of the key challenges in performing this kind of analysis is once again dealing with the reference class problem, as this section has shown how understanding the way that different cases will respond differently to a given strategy is an important prerequisite for evaluating the effectiveness of that strategy on the whole. The next section will switch gears to talk about the ways in which this theoretical

¹⁰ Put differently: techniques like instrumental variables, Heckman selection models, regression discontinuity, and matching all attempt to screen out selection biases in how policies are applied. The assumption is that policymakers recognize the degree of difficulty in each case as they are making policy decisions, but that they do this imperfectly. Thus if we could control for the decisionmaking process (through propensity scores, instruments, exogenous shocks, natural discontinuities, etc.) then any remaining variation might be plausibly random: in some cases the policy would be applied too much and in some cases it would be applied too little. But because military actions generally terminate once they succeed (or fail), it is difficult to imagine a case where they are ever applied too much. (For example, if governments continue to kill or capture their opponents after the war is over, then this is generally thought to be something very different from battlefield attrition, and there are entire data sets devoted to capturing these kinds of “massacres” or “one-sided killings” as distinct phenomena worth studying in their own right.) Artificial randomization will not change this property of the data, and thus it cannot remove the obstacle to causal inference

framework can be extended in another direction, by discussing what it implies for public policy and decision making in fields outside of strategic studies.

Section 6.3. Cumulative dynamics and decision making beyond national security

When Lord Simon (Britain's Chancellor of the Exchequer in the late 1930s) described his country's arms race with Germany, he compared Britain to a "runner in a race who wants to reserve his spurt for the right time, but does not know where the finishing tape is."¹¹ This metaphor nicely captures the challenge decision makers face in dealing with cumulative dynamics. Even when they believe that it is possible to achieve a desired goal, and even when they believe they are making progress towards doing so, it is often difficult to determine how long it will take and how much it will cost to get there: the location of the "finishing tape," however that goal is defined, can be hard to predict.

Armed conflict and national security policy offer many salient examples of decision makers wrestling with these issues, but similar analytic challenges recur in many other areas of public policy. It is thus worth closing this dissertation by emphasizing that while military decision making has been the primary subject matter discussed in this work, the underlying theoretical framework is one that can generalize much more widely than this.

¹¹ The quote is from Cabinet minutes cited by Walt 1992.

Scientific programs and the HIV vaccine

For instance, just as with the pursuit of the Higgs boson discussed at the beginning of this chapter, a wide range of scientific research requires patiently pursuing major breakthroughs. One of the most salient examples from the last several decades has been the global attempt to develop a vaccine for the human immunodeficiency virus (HIV). In most respects, there is no similarity between this initiative and a military campaign like the war in Afghanistan. But seen through the theoretical lens of cumulative dynamics and decision making, these topics in fact share fundamental analytic features.¹²

More than 30 million people are currently living with HIV. Every year roughly two million people die from HIV-related causes, and about three million people will be newly infected. About six hundred thousand Americans have died as a result of contracting the virus. There are more than a dozen countries in Sub-Saharan Africa where infection rates are more than five percent of the adult population, and a handful where that fraction is more than one-quarter. Shortly after the virus was discovered in the early 1980s, developing an effective vaccine became one of the most high-profile goals in the biomedical sciences. In 1984, the U.S. Secretary of Health and Human Services Margaret Heckler predicted that this could be achieved in two years.

¹² This section is based on summaries of HIV vaccine development that were published in scientific journals, including Graham's review in the *Annual Review of Medicine* (2002), Desrosiers's review in *Nature Medicine* (2004), Berkeley and Koff's review in the *Lancet* (2007), Dieffenbach and Fauci's review in the *Annals of Internal Medicine* (2011), and Maurice's review in *Lancet World Report* (2011). Statistics are reported from the Joint United Nations Programme on HIV/AIDS (UNAIDS), whose most recent data is from 2009.

Of course, this prediction was wildly off the mark, as nearly three decades later scientists are still not particularly close to developing a functional vaccine. At the same time, it is not clear how anyone could have understood, *ex ante*, just how difficult a challenge HIV would present. For instance, a recent study chronicled more than three hundred vaccines that were developed for preventing twenty-eight different diseases during the twentieth century.¹³ Almost by definition, each successful instance of vaccine development involved achieving important scientific breakthroughs that helped to address major health policy problems. Some of these vaccines took decades to discover; but others were “low-hanging fruit” that were obtained rather quickly – and in any case, science today is far more advanced than it was just decades ago. In forming expectations about how long it might take and how much it might cost in order to develop an HIV vaccine, there are thus a wide ranges of reference classes to choose from, and plenty of evidence to suggest that biomedical research can clear difficult hurdles.

One of the main difficulties in making any predictions about HIV research is that the virus is in many ways idiosyncratic, which makes it hard to know how to benchmark current efforts in light of past experience – that is to say, it is extremely hard to determine the proper reference class for structuring predictions about HIV-related research. For example, there were initially no known examples of humans possessing immunity to the virus. This makes it difficult to develop candidate vaccines through the standard method of examining potential correlates of protection.¹⁴ Since animals do not contract HIV, this makes it difficult to gain headway through animal

¹³ Hoyt 2012.

¹⁴ Perhaps the best known example is the way that Edward Jenner and his colleagues developed the smallpox vaccine after noticing that milk maids were resistant to the disease once they had been exposed to cowpox. By observing a protected population (the milk maids) Jenner inferred that their immunity had something to do with being exposed to cows, and this suggested cowpox as a potential vaccine.

research.¹⁵ HIV has many known variants, and each of them mutates rapidly – developing a comprehensive vaccine thus requires finding some “Achilles heel” that holds across the virus’s multiple forms.

Without any of these rungs to stand on – observed immunity, satisfactory animal models, or a common weakness across viral strains – there was almost no chance of developing a functional vaccine for HIV in short order. But this is a far cry from saying that the challenge was insurmountable. To use the terminology from Chapter 2, this simply means that scientists began developing the HIV vaccine in a Buy-In Phase, where a great deal of basic research was clearly needed before there would be any chance of achieving the ultimate goal.

Chapters 2 and 3 discussed the challenges of forming and revising expectations about the how long it might take and how much it might cost in order to complete this kind of Buy-In Phase. These chapters also described how (at least in the military context) scholars generally assume that rational decision makers should become more pessimistic as they invest increasing amounts of resources in a policy that does not succeed. Similarly in the case of the HIV vaccine, many prominent scientists concluded after decades of work that their objective might be infeasible. Richard Horton (editor of the British medical journal *The Lancet*) wrote in 2004 that

Contrary to the predictions and promises of most AIDS experts, the signs are that a vaccine to prevent HIV infection will not be found for, at the very least, several decades to come – if at all.... The sum total of our knowledge about the genetics, biology, and geographical distribution of HIV indicates that vaccine scientists may have met their match in this adaptable foe. The reality seems to be that a vaccine against AIDS is becoming little more than a pipe dream.

¹⁵ Primates do contract “simian immunodeficiency viruses” (SIVs), and scientists do study them; but strains of SIV and HIV tend to differ in significant ways, such that most advances in protecting primates against SIV have not shown much utility in developing a vaccine for humans.

Robert Desrosiers (director of the New England Primate Research Center) gave a similarly pessimistic assessment in that same year, as he characterized ongoing efforts as little more than “empirical trial-and-error” or the “continued testing of feeble long-shots.” Scientists were at the time testing at least thirty different candidates for vaccine development, and all trials to that point had essentially failed. But one of the key arguments in this dissertation is that when decision makers are assessing cumulative dynamics, it is important to keep in mind that when some measures fail, this does not necessarily have any bearing on whether other, independent efforts will be any more or less successful. Though this dissertation has primarily dealt with contemporary models of “rounds of fighting,” it is not too much of a stretch to apply this same logic to rounds of clinical trials. And indeed, in recent years there have been several notable advances indicating that scientists may in fact be gaining the kind of traction they need in order to develop functioning defenses against HIV.

In September 2009, a trial conducted across 16,000 subjects in Thailand produced an estimated 31 percent efficacy in preventing HIV infection. Though the effect was modest, many scientists believe it nevertheless indicates major progress as researchers can now examine distinctions between populations who respond differently to the virus. In that same month, other scientists discovered two antibodies that were capable of neutralizing multiple strains of HIV, indicating potential movement towards identifying the long-sought “Achilles heel.” Meanwhile, researchers have significantly refined animal models, and in May 2011, some of them identified a partially effective (but permanent) vaccine that works in monkeys.

These developments are all grounds for optimism because they show how some of the main barriers to HIV vaccine research are gradually being worn down. It is probably appropriate to think that the Buy-In Phase of this research is ending, as scientists now have findings to use in

designing clinical trials that have positive probabilities of achieving impactful results. As Seth Berkley (former head of the International Aids Vaccine Initiative) recently stated, “We’ve seen more progress than ever over the past decade. There is no doubt in my mind that we are now at a critical turning point and the pace of progress is clearly accelerating.... I can’t predict when the problem will be solved. But I know it will be.”¹⁶

At the same time, it is important not to be too sanguine about this progress. Previous chapters have provided many examples of decision makers who similarly envisioned themselves on the cusp of success and later proved to be wrong. And if research on the HIV vaccine continues without success then this will entail genuine costs – both in terms of the time and effort invested in this research program *and* because these investments could have been directed towards other research that might have produced a better rate of return. Scientific resources for global health are limited, and there are many pressing concerns besides HIV: for example, efforts to develop vaccines for malaria, anthrax, and the avian flu have all received substantial public attention in recent years as well.

So how should governmental organizations like the U.S. National Institutes of Health (or non-profit grantmakers like the Bill and Melinda Gates Foundation) allocate their resources among these programs? These questions raise exactly the kinds of analytic challenges that occupy military decision makers who are deciding how to allocate their forces or assessing the benefits of pursuing certain objectives at all. Most people do not intuitively think of biomedical research and military planning in the same terms. But the similarities in strategic assessment across these fields should not be surprising, since both fundamentally revolve around estimating how long it

¹⁶ Quoted in Maurice 2011: 213.

might take or how much it might cost to achieve various goals. In this sense, these fields provide different examples of the same basic dynamics of decision making.

Development assistance

Just as scholars and policymakers discuss the prospects for breaking “conflict traps” in warfare, a great deal of research on economic development discusses the difficulties of helping low-income countries climb out of “poverty traps.”¹⁷ The international community provides about a hundred billion dollars in official development assistance each year. Much of it is intended to help recipients break out of poverty traps and begin a cycle of self-sustaining growth.

There have been some significant success stories: Botswana, for instance, is often cited as a country that made smart use of foreign aid and natural resources to maintain one of the world’s highest economic growth rates for the last four decades. In general, developing countries in Sub-Saharan Africa experienced an average growth in per capita GDP of roughly 10 percent per year from 2000-10. But there are also a number of prominent development failures, countries like the Democratic Republic of Congo and Ethiopia, where critics argue that foreign assistance has had little positive effect, especially for these countries’ poorest citizens. And much of Africa’s recent economic growth has been driven by countries like Angola, Equatorial Guinea, Nigeria, and Sudan, whose advancement has substantially depended on indigenous oil wealth. Many developing countries that lack large industries for exporting natural resources, such as Burundi,

¹⁷ See Bowles, Durlauf, and Hoff 2006 and Collier 2007 for reviews.

Haiti, Liberia, Tanzania, and Zimbabwe, have not even kept pace with the expansion of the world economy as a whole.¹⁸

How much longer will it take and how much more will it cost to help these states develop healthy governments and economies? And is this goal even feasible at all? This is an additional set of prominent policy questions that runs up against the same basic analytic challenges that were discussed throughout the previous chapters. Thus on the one hand there are some economists (most prominently William Easterly and Dambisa Moyo) who argue that many aid programs are misguided and even counterproductive, facilitating corrupt governments that are dependent on external funding rather than the kinds of self-sufficient systems that donors intend to facilitate. And on the other hand there are economists (most prominently Jeffrey Sachs) who argue that the aid programs are basically well-designed, and that the main problem is that the international community has not provided *enough* aid.

The contours of this debate resemble the kinds of disagreements about military strategy that have recurred throughout this dissertation. When dealing with cumulative processes, policy disagreements often become mired in seemingly intractable arguments about whether past failures are more a matter of the concept or the execution – whether the problem is that policymakers are pursuing the wrong strategies or that they have not implemented the right strategies extensively *enough*. And the reason that these kinds of debates recur so widely is that they are only partly about military strategy or economic assistance per se. More generally, these disagreements reflect the difficulty of analyzing cumulative dynamics.

¹⁸ These figures are from the World Bank's data unit. The GDP per capita of developing countries in sub-Saharan Africa grew by an average of 9.6 percent per year from 2000 through 2010. The global average for annual GDP growth per capita in these data is 5.7 percent per year.

Section 6.4. Conclusion

There are many other examples one could choose in order to demonstrate how cumulative dynamics play into broader scholarship and policy debates. As of this writing, related questions include whether economic sanctions will cause Iran to disable its nuclear weapons program before developing a functional bomb; how long it will take in order to develop political consensus (both nationally and internationally) to stop climate change; and how much fiscal stimulus it will take in order to jumpstart growth in the U.S. economy. These dynamics appear in other aspects of politics, too, such as competition among political campaigns. As columnist David Brooks wrote during the run-up to the 2012 election between President Barack Obama and the Republican challenger, former Governor Mitt Romney,

Both campaigns fervently believe that more spending leads to more votes. They also believe that if they can carpet bomb swing voters with enough negative ads, then eventually the sheer weight of the barrage will produce movement in their direction. There's little evidence that these prejudices are true. But the campaigns are like World War I generals. If something isn't working, the answer must be to try more of it.

Though this passage was written ironically, it captures a real connection between decision makers in armed conflict and many other fields who share common struggles in evaluating cumulative processes. All of the debates reviewed in chapter (and many others like them) involve supporters and detractors who either argue that previous experience has demonstrated that some policy will not work, or who argue that previous policies have not succeeded in large part because they have not been implemented extensively enough. This dissertation has explained why it can be genuinely difficult to evaluate the validity of these arguments. In almost any field, scholars and practitioners wrestle with questions about how long it might take or how

much it might cost to achieve a particular goal. Viewing these efforts as related attempts to assess similar dynamics opens up opportunities to take established ideas across disciplinary lines, and to bring new ideas to bear on diverse subjects.

In closing, there is another reason why it is important to view the study of cumulative dynamics and policy assessment as a fundamentally interdisciplinary matter. This is because investing resources almost always involves managing difficult tradeoffs with the pursuit of other goals. The U.S. government makes significant investments in all of the subjects discussed in this chapter: fighting the war in Afghanistan, finding the Higgs boson, developing an HIV vaccine, and providing economic assistance. These priorities are necessarily competing because they consume scarce resources, both in terms of taxpayer dollars and policymakers' attention. Any rational scheme for dividing scarce resources among these priorities must be based, at least to some degree, on a sense of how likely these efforts are to pay off, and the extent to which additional investments (or cutbacks) will affect these programs at the margin.

For example, many readers may be uneasy with the notion that the U.S. government allocates about \$1 billion each year to developing a vaccine for HIV (a virus that leads to the annual death of roughly two million people worldwide, and that has killed more than half a million Americans in total), while it spends more than \$600 billion each year on its military. Many readers would probably support a proposal for shifting a billion dollars from the latter to the former, believing that it would do more good in expectation if the U.S. government doubled annual funding for HIV vaccine research in exchange for cutting the annual defense budget by less than one-sixth of one percent.

But how do you *prove* that? How would you make an analytically rigorous argument to shed some light on this issue? And if you cannot make an analytically rigorous argument, then how confident can you be in advocating these sorts of policy shifts? How *convincing* can you be in advocating these sorts of policy shifts?

This is not to say that policymaking is entirely driven by the kind of rational analysis that has been the subject of this dissertation. But analysis has to enter the process somewhere, because we cannot expect government agencies to propose their own budgets in disinterested ways. The U.S. military will almost always demand more resources for promoting national security. The National Institutes of Health will almost always demand more resources for protecting Americans in a different fashion. Determining “how much is enough” in supporting these efforts is an inherently relative question that requires assessing competing priorities in comparable terms. The previous chapters have shown that current scholarship offers limited tools when it comes to estimating how long it might take or how much it might cost to achieve important objectives. The study of military strategy is just one of many fields that will benefit from continued attempts to examine and rebuild these conceptual foundations.

 Appendix. Formal supplement to Section 6.2

Assume that we are studying a population that can be stratified into two homogeneous groups. These groups respond differently to a particular policy. One population of cases is *Susceptible* to the policy, such that as decision makers invest more effort in this policy they become more likely to achieve strategic success. The second population of cases is *Resistant* to the policy, so that investing in these measures does not make it more likely that decision makers will achieve their intended goal. The size of these respective populations are proportional to s and r such that $s + r = 1$. We can now examine how this kind of heterogeneity affects the way we might perceive the effectiveness of the policy we are studying.

The discussion will begin by studying a case where the probability of achieving strategic success is determined by a linear function of how much decision makers have invested in the policy; the second part of the appendix will then generalize more broadly. To help make this discussion more concrete (and more consistent with the formal theoretical literature on armed conflict) it will be framed in terms of studying the impact of multiple “rounds of fighting.” There is nothing in this discussion which is particular to military strategy however, and this discussion avoids making any assumptions that should not be relevant to a wide range of subjects.

Linear case

Assume that the probability of defeating susceptible opponents within x rounds of fighting is $P_S(x) = \alpha + \beta x$, where β is assumed to be a positive number. The probability of defeating the resistant group does not change based on the number of rounds fought, so that $P_R(x) = \alpha$. Since the probability of defeating an opponent must be between 0 and 1, we can restrict the analysis to

values of α that are greater than zero, and values of x such that $\alpha + \beta x \leq 1$; beyond this point, there is no possibility of seeing any more susceptible opponents, the population can thus no longer be mixed, and so there would be no reason to be concerned about a mixed population confounding empirical results.

The term P is capitalized here to emphasize that it effectively represents a cumulative distribution function, much like the ones used to represent decision makers' prior expectations in Chapter 3. Just as a cumulative distribution function $F(x)$ represents the probability that a draw from a random variable will be less than or equal to x , the function $P_i(x)$ represents the probability that a randomly-chosen opponent will concede in or before the x^{th} round of fighting. In other contexts, we could think of x as representing levels of investment more generally, such as dollars spent, time invested, or any other index that can be expressed in terms of consistent units.

Within this framework, the probability of defeating a randomly-chosen opponent in or before the x^{th} round of fighting is $\theta(x) = P_S(x) \cdot s + P_R(x) \cdot r$. To see that the overall probability of defeating an opponent is increasing in x , take the first-order condition, $\theta'(x) = s\beta$. We could call $\theta'(x)$ the *unconditional treatment effect* for conducting a single round of fighting. Needless to say, the unconditional treatment effect is both positive and constant.

The word “unconditional” is important here, because it emphasizes that $\theta'(x)$ is only a good approximation of the policy as applied to an opponent that is selected at random from the entire initial population. As the policy is implemented, however, the population of cases will not remain the same: as rounds of fighting mount and Susceptible opponents are defeated, they should comprise a progressively smaller proportion of the opponents remaining. To see this,

define $s(x)$ and $r(x)$ as the proportion of Susceptible and Resistant opponents that we would expect to see remaining after round x . We can express these proportions in terms of P_S and P_R , while reweighting the results so that they still sum to 1.¹⁹ By doing this, we find that

$$s(x) = \frac{s(1 - \beta x)}{s(1 - \beta x) + r}$$

$$r(x) = \frac{r}{s(1 - \beta x) + r}$$

Given that $s + r = 1$, we can also express these functions as

$$s(x) = \frac{s(1 - \beta x)}{1 - \beta x s}$$

$$r(x) = \frac{r}{1 - \beta x + \beta x r}$$

These expressions show how the proportion of Resistant opponents, $r(\cdot)$, should be an increasing function of both x and β . (By definition, this means that the proportion of Susceptible opponents, $s(\cdot)$, should be a *decreasing* function of both x and β .) This is an intuitive result, reflecting the notion that as more of the Susceptible opponents are removed from the population (either because they have been exposed to more rounds of fighting or because they are defeated at a faster rate), they will comprise a decreasing proportion of the opponents that remain.

¹⁹ Note that in doing this we can ignore the term α ; this term is common to both populations, and so even though it reduces the aggregate *number* of opponents, it does not affect their relative proportions. If we eliminate opponents from both groups with probability α , then the remaining population will be proportional to $(1 - \alpha)s + (1 - \alpha)r$. In order to have this expression sum to 1, we would simply divide by $(1 - \alpha)$, which would return the proportions to s and r .

We can use this information to calculate a new expectation for the probability of defeating a randomly-chosen opponent in the x^{th} round of fighting, conditional on the fact that the opponent was able to reach the x^{th} round of fighting in the first place. We could call this the *conditional treatment effect*, denoted $\lambda(x)$. We can express $\lambda(x)$ as the proportion of opponents in round x who should be Susceptible, multiplied by the probability that these Susceptible opponents will in fact concede in the x^{th} round:

$$\lambda(x) = s(x) \cdot P'_S(x)$$

The conditional treatment effect for each round of fighting is once again strictly greater than or equal to zero.²⁰ This result is not surprising, as we have assumed from the start that each round of fighting either raises the probability of defeating a Susceptible opponent or else has no impact on the probability of defeating a Resistant opponent. It is only logical that, under these assumptions, an additional round of fighting will never lower the probability of succeeding.

This is *not* what we might infer, however, if we examined the data using a standard regression framework. Consider what happens if we were to study a cross-sectional data set of different counterinsurgency campaigns. If we were to plot the amount of attrition inflicted on each insurgency on the x-axis, and then the success or failure of each counterinsurgency campaign on the y-axis, then our data points would only reflect the subset of cases that terminated at each level of attrition. And when the regression models this, what it will be estimating is the way that these subsets *change* from one level of attrition to the next. This captures something that is very different from the policy's actual impact.

²⁰ This is because in the current setup, $P'_S(x) = \beta x$, and the proportion $s(x)$ cannot be negative.

What the regression framework is modeling here is how the conditional treatment effect changes as a function of x . We can express this function directly with the term $\Delta(x)$, and this demonstrates how misleading the regression model can be:

$$\Delta(x) = \frac{\partial}{\partial x} \lambda(x) = \frac{\partial}{\partial x} \cdot \frac{\beta s(1 - \beta x)}{1 - \beta x s}$$

$$\therefore \Delta(x) = \frac{\beta^2 s(s - 1)}{(1 - \beta x s)^2} \quad [1]$$

Since $s < 1$, then the expression here shows that $\Delta(x)$ will always be less than or equal to zero. This means that regression frameworks will always produce a negative slope coefficient given the assumptions described above. It would be very misleading to interpret this as indicating that the policy's impact is negative, as we have in fact shown that this will never be the case.

Moreover, note that as β increases, then the numerator in expression [1] will become a larger negative number, the denominator in expression [1] will become a smaller positive number, and thus $\partial\Delta(x)/\partial\beta \leq 0$. This means that as the policy becomes more effective, the slope of the regression coefficient will actually go *down*. This helps to demonstrate the basis for the main arguments in Section 4.3 about how standard regression models provide a misleading basis for analyzing cumulative dynamics in mixed populations.

General case

The discussion thus far has revolved around the assumption that the probability of defeating a Susceptible opponent is a linear function of the number of rounds of fighting. But there is of

course no reason that this need be the case, and it is worth examining what can happen when the probability of strategic success as a function of investment is either concave or convex.

When $P_S(x)$ is *concave*, then this will only cause estimated regression coefficients to decline at an accelerating rate. Recall that the conditional treatment effect is expressed as $\lambda(x) = s(x) \cdot P_S'(x)$, and that the regression model fits a slope to changes in $\lambda(x)$, which we can express by way of the product rule as:

$$\Delta(x) = \frac{\partial}{\partial x} \lambda(x) = s'(x) \cdot P_S'(x) + s(x) \cdot P_S''(x)$$

By definition, $s(x)$ is a positive number, and we have already shown that its first derivative is always less than or equal to zero; if $P_S(x)$ is concave, this means that $P_S''(x) \leq 0$. This means that whenever $P_S'(x)$ is positive, so that additional rounds of fighting make it more likely that Susceptible opponents will concede, then $\Delta(x)$ is again guaranteed to decline. This makes sense given that a concave success function would indicate that each round of fighting eliminates fewer opponents at the margin.

When $P_S(x)$ is *convex*, then $\Delta(x)$ can take on a wide range of values, including the possibility that the conditional treatment effect will rise in some periods of fighting and then fall in others – this is the pattern demonstrated in Figure 6.1.²¹

²¹ It may seem confusing to say that Figure 6.1 represents a convex success function given that the population averages for both Susceptible and Resistant opponents are represented as straight lines. However, keep in mind that in Figure 6.1, the y-axis represents the proportion of opponents that will be defeated in each round of fighting, whereas in the discussion in the appendix, $P_S(x)$ represents the cumulative probability that opponents will be defeated *in or before* each round of fighting. This change in notation helps to make the formal discussion in the appendix more tractable, and means that it is slightly different from the presentation in Section 6.2.

To begin, note that when $P_S(x)$ is increasing and convex, then $\Delta(x)$ will be expressed as the sum of a strictly negative term, $s'(x) \cdot P_S'(x)$, and a strictly positive term $s(x) \cdot P_S''(x)$. $\Delta(x)$ will thus be positive so long as $(-1) \cdot s'(x) \cdot P_S'(x) < s(x) \cdot P_S''(x)$, and it will be negative when the reverse is true. Since we can express $s'(x)$ as a function of s and P_S , then we can see that $\Delta(x)$ is only positive when:

$$\frac{sP_S(x)P_S'(x)(1-s)}{[1-sP_S(x)]^2} \cdot P_S'(x) < \frac{s[1-P_S(x)]}{1-sP_S(x)} \cdot P_S''(x)$$

$$\therefore P_S'(x)^2 \frac{P_S(x)(1-s)}{[1-sP_S(x)][1-P_S(x)]} < P_S''(x) \quad [2]$$

Expression [2] shows how $\Delta(x)$ will be positive so long as $P_S(x)$ is “convex enough” in relation to several factors. The expression therefore provides the following comparative statics. Keep in mind that all of the individual terms on the left-hand side of expression [2] are positive and less than or equal to 1.²²

- When $P_S'(x)$ is smaller in relation to $P_S''(x)$, then $\Delta(x)$ is more likely to be positive.²³ This makes sense because $P_S'(x)$ represents the proportion of Susceptible insurgents eliminated in the x^{th} round of fighting while $P_S''(x)$ represents the way this proportion *grows* from one round of fighting to the next. If this proportion is relatively small in one round but relatively large in the next, then this makes it more likely that we will see a larger number of insurgents defeated in round $x + 1$.

²² This shows again why $P_S(x)$ must be convex in order for $\Delta(x)$ to be positive.

²³ This is because the term $P_S'(x)$ only appears in the numerator of the left-hand side of expression [2].

- When s is larger, then $\Delta(x)$ is more likely to be positive.²⁴ This reflects the idea that when Susceptible opponents comprise a larger fraction of the initial population, then we are likely to see more of them in later rounds, where the probability of defeating them is larger (and thus the probability of defeating a randomly-chosen opponent who remains in the population would be larger as well).

²⁴ The derivative of the left-hand side of expression [2] with respect to s is $P_S(x)P_S'(x)^2(p-1) \cdot (1-p)^{-1} \cdot (1-sp)^{-2}$. Note that one of these terms, $(p-1)$, is strictly less than or equal to zero, while all of the other terms are positive. Thus, the derivative of the left-hand side of expression [2] with respect to s is strictly less than or equal to zero, and the inequality is more likely to hold.

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