INTRODUCTION

Much has been written on the causes of war; little has been learned about the subject. There are two reasons for this. The first is that the theoretical assumptions used to study the phenomenon are flawed and often erroneous. The second is that individual scholars have tried to do too much too soon. Typically, a single scholar working alone has tried to review a number of wars, reflect on their commonalities, and reach a conclusion. The end result has been some insightful suggestions, but little real evidence or documented generalizations. In the last twenty-five years, this has begun to change. Building on the pioneering efforts of Lewis Richardson and Quincy Wright, a community of peace researchers has emerged, with scholars testing very specific hypotheses and trying to document in a rigorous fashion the patterns of behavior associated with war.

What distinguishes this book on war from previous ones is that it will employ the large number of empirical findings generated in the last twenty-five years as the basis of its theorizing. Although this research has added important pieces of evidence that have moved the field beyond the imprecise and often contradictory explanations of the past, no clear theoretical explanation seems to be emerging from this process, although there is research that suggests such explanations. Because of inconsistencies and anomalies in the findings as well as differences in measurement and research design, the meaning and significance of these findings are hardly self-evident. Rather, they exist as a set of clues or pieces of a puzzle that need to be put together.

A scientific explanation will not just emerge from the research process, but must be constructed carefully from the evidence. While the empirical work on delineating various factors associated with war and specifying models of the war process can continue by testing various hunches, it has failed to date to provide a coherent explanation of war. One of the reasons for this may be that the dominant realist perspective that should be providing such an explanation has simply not been up to the task. It has not been able to explain inconsistencies

in a satisfactory manner, and an entirely new theoretical approach may be needed, one that will put both existing findings and unresolved questions into a perspective that makes sense of both (see Vasquez, 1983a; Banks, 1985; Mansbach and Vasquez, 1981).

What needs to be done is to stand back from the findings and see what they are trying to tell us. Rather than treat the scientific process in a conservative deductive manner as suggested by philosophers of science as varied as Hempel (1966), Popper (1959), and Lakatos (1970), we might be better served by being more radically inductive, for at least the moment, and treating existing evidence as a good detective would treat clues. We would then try to piece the clues together as we would a puzzle, hoping that as we did so we would come across a clue that would suggest new hypotheses. These new hypotheses would then tell us where to find the missing pieces of the puzzle and in doing so would provide a way of deductively testing the theoretical explanation we had constructed so far. Since a number of research efforts using existing data on alliances, polarization, capability, arms races, bargaining, decision makers' perception, status, and crisis escalation (among others) have been completed or are approaching completion, this is an ideal time to implement this strategy and try to synthesize a theoretical explanation that can guide the next stage in data collection and hypothesis testing.

The scientific research on war and peace in the last twenty-five years has demonstrated that induction can bear important fruit. That research now constitutes a sufficiently critical mass of evidence to provide a real turning point in the long human effort to discover the causes of war. If the turning point comes, it will support J. David Singer's inductive notion that in attempting to understand war, emphasis must be placed on systematic data collection and description so as to produce a body of empirical generalizations. Once the patterns or correlates associated with war are known, then it will be possible to explain them. What is significant about the scientific study of war from the perspective of the philosophy of inquiry is that progress and cumulation have not come from deriving a hypothesis, testing it, and reformulating it in light of the evidence. If one takes that positivist approach, then the findings seem much more inconsistent, ambiguous, and farther away from cumulation than they in fact are. If, however, one treats the findings as an aid to discover inductively what patterns precede war, then there is greater reason for optimism.

The debate on induction versus deduction has often been confused because there has been a failure to distinguish the logic of discovery from the logic of confirmation (Nagel 1961; Scheffler, 1967), as well as a

tendency to ignore that in practice inductive and deductive procedures do not oppose each other but go hand in hand. Many of the logical arguments against induction hold only on the question of how to validly test theories (the logic of confirmation). This book is concerned primarily with the logic of discovery. I review existing findings not to see if they confirm a particular explanation, but to see if in the absence of any *confirmed* explanation a new explanation consistent with the evidence can be *discovered*.[1] This new explanation must then be tested before it can be accepted.

Methodologically, this book does not follow the typical positivist approach that specifies a proposition, operationalizes its concepts, collects data and constructs a research design that adequately tests the proposition. Instead, what this book does is employ a synoptic review of all relevant evidence to see what has and what can be learned about the onset of war – what in some disciplines is called a meta-analysis (see Hunter, *et al.*, 1982). Such efforts always raise two questions: First, is it possible to compare studies that have different statistical analyses or measures, or are designed at different levels of analysis? Second, is there not a danger that such an effort will treat findings as more definitive than they are? Both of these are important questions, but in practice they turn out not to pose insurmountable obstacles. In terms of comparing studies, this is more of a statistical dilemma than a philosophy of science problem. On the statistical level, a Pearson's r of 0.15 and a Yule's Q of a 0.15 are not equivalent and tell us different things about a relationship. Philosophically, however, they are comparable in that they both tell us that the proposition has produced a "weak" association (see Vasquez, 1983a: 179–80). Statistical findings can be compared to make philosophical assessments about the empirical adequacy of various explanations. If this could not be done, then what would be the point of doing research in the first place? More importantly, in terms of the logic of discovery, differences in research design and measurement, even flaws and measurement errors turn out to be very useful because they provide clues about what might really be going on across a series of studies, particularly those that get different results using basically the same data set.[2]

This brings us to the second question, the danger of treating findings as more definitive than they are. This, of course, can be a problem with simplistic analyses that categorically assert what "science" has found, but it is not a problem if one is careful in assessing the evidence and explicit about one's judgments. In this analysis, I have taken pains to present the reader with all the various pieces of evidence I have examined to reach the conclusions I have drawn. Often I repeat in the

text or footnotes the actual findings. If a study has been criticized or followed by studies that have inconsistent findings, I give these equal attention. In this process, I have tried to act like a judge rather than a prosecuting attorney or defense counsel.

It is important to take this perspective if one wants to get at the truth or at least learn something from research. Unfortunately, some analysts wish to pursue skepticism's agenda and seek to use scientific studies to show that nothing can be known. Ironically, it is often the anti-quantitative and anti-scientific who take this tack. They then become "super positivists" using positivist criteria to show that a research design is flawed, a measure invalid, or a finding trivial. Having satisfied themselves that scientific research cannot produce knowledge, they then proceed to ignore it and study international politics in a considerably less rigorous and even speculative manner. I hope this book will show readers who have been seduced by this attitude what they have been missing.

I have approached the literature neither in a naive nor overly skeptical way, but as a detective looking for clues. In the end, of course, I have had to make judgments about measurement validity, research designs, and how much weight to place on a particular finding. Evaluation of empirical research requires that such judgments be made. To think otherwise is to misunderstand the nature of scientific inquiry. Nevertheless, this does not mean that judgments need be arbitrary. Whenever an important question is at stake, I trace for the reader the thinking process I went through in making a particular interpretation. Although it would be tiresome to do this for each judgment, I have done it enough so that the reader can make a judgment about how much confidence to place on my evaluation of a particular body of research.

These questions are important because, in this book, I try to uncover the dynamics of war and peace in the modern global system by examining the patterns of behavior delineated by existing research. These patterns, rather than a set of axioms, will be the foundation of my explanation. Instead of assuming that people either as individuals or as collectivities act in certain ways (as rational actors or utility maximizers, for example), I will try to base my explanation on what we empirically know about how people actually behave in certain situations. In other words, I will tend to explain how one action leads to another by saying that in those kinds of circumstances, what we know about people tells us that they will act in that way for these reasons or because of these factors, rather than explaining the action by a model based on an untested axiom. What this means is that my propositions will often be linked not by mathematical or logical deduction, but by historical contingency.

War is a very complex subject, in part because war does not result from a single set of causes. There are many paths to war, and in this analysis I try to delineate the modal (typical) path by which relatively equal states have become embroiled in wars with one another in the modern state system. I had initially hoped that a single explanation of war over all of history could be constructed. Instead I have come to the conclusion that there are different types of war and that each type can be preceded by different causal sequences. To explain war requires identifying the various paths that lead to war. What makes this even more complicated is that these paths may vary over long periods of history. In this book, I believe I have identified one path, for one type of war, in one historical era, the modern global system (1495 to the present).

In trying to identify the causal sequences that precede wars, I distinguish between underlying and proximate causes. Underlying causes are fundamental causes that set off a train of events (the proximate causes) that end in war. Of all the various issues over which wars can arise, I have found territorial disputes between neighbors to be the main source of conflict that can give rise to a sequence of actions that ends in war. Since all neighbors usually must, at some point in their history, contend with this issue, and because this issue is an issue over which most neighbors are apt to fight if they are involved in a war with one another, I see territorial disputes as an underlying cause of war. Whether or not it will give rise to war, however, will depend on how the issue is treated (the proximate causes). Since how states treat each other varies according to a number of characteristics, the proximate causes of war are much more varied than the underlying causes. Thus, while territorial disputes can be the origin for all types of wars, each of the different types of wars has its own proximate causes.

In this analysis, I have tried to identify these proximate causes by looking at the foreign policy practices that lead to war. Among equals, I have found that, within the modern global system, war is likely if the practices of power politics are used to try to resolve territorial disputes. Power politics behavior, rather than preventing war, actually increases the probability that it will break out. This is because the main practices of power politics – alliances, military buildups, and the use of *realpolitik* tactics – increase insecurity and hostility motivating each side to take harder lines. Coercion fails to produce compliance or compromise because the nature of the issue at stake is such that giving in (especially to an equal) is unthinkable. Under such conditions, the use of power politics produces a set of interactions and domestic political environ-

ments that make war increasingly likely. Between equals, war is brought about by each side taking a series of steps that increase hostility and make the issue at hand more intractable. This involves the disputants in a series of crises, one of which escalates to war. Evidence on which steps increase the probability of war and which characteristics of crises make them prone to escalation has been provided by empirical research.

The use of the foreign policy practices of power politics to handle certain territorial disputes will increase the probability of war, but whether power politics will be used depends, in part, on the nature of the global political system in operation. The global institutional context, in particular whether it provides norms and "rules of the game" for resolving issues, has a major impact on whether states will resort to power politics. Preventing war and creating peace involves learning how to build structures that provide mechanisms for resolving issues through diplomacy rather than armed force.

To summarize: In the modern state system one of the main sets of factors that brings about war among equals is the rise of territorial disputes, particularly between neighbors, that in the absence or failure of a global institutional network to resolve the issue politically makes actors resort to the unilateral solutions provided by power politics. Through elaborating this skeletal outline, I will explain why and how wars occur, why some wars expand, and why some historical periods and interstate relationships are more peaceful than others.

In trying to construct these explanations from the various pieces of research, I have found it useful to think in terms of causes and consequences. Many scholars, including the leading peace researcher in the field, J. David Singer, eschew causal language. Many share Hume's reservation that the notion of "cause" inheres within the human brain and not in nature.[3] In addition, there are a host of problems in making causal inferences. Despite these concerns, it is very difficult to construct an explanation of *why* wars occur or expand (especially if one is proceeding inductively) without thinking in causal terms at critical points in the analysis (see Dessler, 1991). Thus, I have found it important to distinguish whether some factor is really a correlate or a "cause." I have tried to see if a factor is really something that brings about war or is a consequence of war. I have thought it important in interpreting a study to see if its explanations and findings identify sufficient or necessary conditions of war. I have found it useful in determining the relative potency of variables to speak in terms of underlying and proximate causes. Without prejudicing the deeper philosophical issues, I have retained causal *language* and *think-*

ing at critical points in the analysis. When I have done this, I have tried to make it clear exactly what I mean by the language and to what empirical referents I am alluding.

Having said that, let me note some areas where I have found causal language misleading and have found the need to correct some of its mechanistic connotations. I have found it misleading to think of war as being brought about because a certain set of conditions or variables are in place. Such Newtonian conceptions and research based on them have not been very fruitful. Rather than seeing war as caused in this mechanical sense, I have seen war as an outcome, i.e. as something that flows out of a set of actions. Rather than seeing war as being *produced* by a set of conditions, I have found it more enlightening to speak of the probability of war increasing as certain actions are taken. To correct these misleading connotations, I have done the following. To emphasize that war comes out of a set of actions, I have spoken in terms of causal sequences and paths to war. To emphasize the probabilistic nature of war, I have spoken in terms of factors that promote or increase the probability of the onset and expansion of war, rather than of sufficient conditions – although I will use the latter phrase from time to time to distinguish these factors from necessary conditions.

My concern that war and peace have been conceptualized in an overly mechanistic manner in the scientific and traditional realist literature reflects a deeper concern that various criticisms that have been made of positivism need to be taken more seriously by those pursuing scientific inquiry. This book was written during much of the debate over positivism (see Ashley, 1987; Shapiro, 1981; Kratochwil 1989; Lapid, 1989; Hollis and Smith, 1991; Der Derian and Shapiro, 1989), and the analysis herein has not been unaffected by that debate. In this book, the importance of history, cultural variation and the role of beliefs and social constructions of reality are emphasized over the role of single factors, like power, or rationalistic explanations. My views are considerably less positivist than even traditional scholars like Gilpin and Morgenthau who see themselves as uncovering timeless laws of politics. More fundamentally, the debate over positivism has affected how I conceive of international relations theory and has provided an opening for reflection, which I have found more congenial to serious theory construction than the strict positivism of the recent past.

These various philosophical issues are pursued in Part I of the book in which I explore the conceptual questions that need to be resolved before constructing an explanation of war and peace. In Chapter 1, I address the question of how the phenomenon of war *should* be concep-

tualized in order to understand and explain it. Here, I outline the theoretical assumptions about war that I have found useful to make and which are employed in the subsequent analysis. I learned early on that not all wars were alike and that different explanations would be required for different types of war. In Chapter 2, I present and justify a typology of war and argue that each type has its own causes. I then limit myself to explaining wars of rivalry, wars that are fought between relative equals. The concept of rivalry is defined and its dynamics outlined. In Chapter 3, I assess realist contributions to our understanding of war and its failure to provide an adequate understanding of the dynamics of peace and war. I argue that power politics theory, rather than providing an explanation of war and peace, actually reflects an image of the world that decision makers sometimes hold and a set of foreign policy practices that once implemented increase the probability of war. I discuss how the institution of war evolves and the role learning plays in the onset of war by creating and institutionalizing a culture of war at the global level. In a more general sense, this chapter is concerned with how and why violence is used by some collectivities in some periods, and not by all collectivities in all periods.

Part II is the heart of the book. It is devoted to constructing a scientific explanation of the onset and expansion of war and the nature of peace. In each of these chapters, the main scientific findings are put together as pieces of a puzzle to come up with a explanation of war and peace. I begin, in Chapter 4, by examining one of the main underlying causes of interstate wars – territorial disputes. I argue that territorial contiguity is the source of conflict that most frequently leads to wars, and I provide evidence to show that this is the case. The reason why human collectivities will fight over territorial issues more readily than other issues is not known, but I speculate that it may have something to do with an inherited tendency toward territoriality. A focus on territoriality can explain a number of patterns that other perspectives have not explained. Nevertheless, territoriality should not be conceived of as a drive or instinct that makes war inevitable. Territoriality makes humans very sensitive to threats to their territory, but how they deal with these issues is the main factor determining whether they will go to war. Chapter 5 specifies some of the proximate causes of war by outlining how war comes about between relative equals when they treat highly salient issues in a power politics fashion. In this chapter, I provide a detailed analysis of the empirical literature to outline the typical steps to war that rivals follow. Delineating the steps to war provides a way of explaining why some rivalries end in war while others do not. In Chapter 5, the focus is on why interactions

between rivals encourage them to take certain steps that lead to war. However, domestic political factors are also important in explaining the steps to war, and these are delineated in Chapter 6, which focuses on the linkage between global and domestic factors. Chapters 5 and 6 identify the main causal sequence that leads relative equals to war. Chapter 7 identifies the causal sequence that leads some wars to expand. In that chapter, I examine the research findings on the scope, severity, and duration of war in order to explain how some wars expand to become world wars.

These three chapters specify proximate causes, but it is important to remember that structural factors have a major impact on whether the interactions that produce the steps to war are likely to be taken. Why rivals initiate the steps to war in the first place cannot be fully understood without reference to the global institutional context. This is done in Chapter 8, which examines peace structures and the role of peace in the onset of war. A full explanation of war must explain how and why a peace breaks down, encouraging states to resort to the practices of power politics. This chapter delineates the main factors associated with interstate relationships and historical periods that are comparatively peaceful. This demonstrates that world politics need not always be a struggle for power, that war is not inevitable, and that peace is possible.

The analysis presented in Part II explains war by: (1) looking at how certain issues become prone to violence if they are handled in a certain way; (2) identifying the ways in which issues are treated that are most likely to result in war between equals; and (3) examining how the global institutional structure permits or discourages political actors from handling issues in a way that will result in war. In Chapter 9, I integrate the various analyses of each of the previous chapters to construct an overall theoretical explanation of the onset and expansion of wars of rivalry. In this chapter, I also discuss the implications of the analysis for the cumulation of scientific knowledge on war and on peace. The appendix, which the reader should not ignore, provides a more detailed specification of the theoretical argument in a propositional format. These propositions, which are keyed to each chapter, provide the best summary of the book. They also constitute a research agenda for the future.

Before turning to the body of the work, let me make some final points about the way in which this book is written and about the terminology I employ. Although it is common to review literature and summarize various approaches in theoretical works, I have avoided that in this book. I am not so interested in writing a book about what

various scholars have said about war as I am about writing a book that tells us something about war. For this reason, I draw upon the literature and use it to synthesize empirical findings, rather than spend a great deal of time summarizing it. Likewise, while on occasion I will criticize other explanations, as in Chapter 3, my purpose in this book is not so much to demonstrate that one "theory" is better than another, as it is to create a new explanation. Whether it is attractive is up to the reader to decide. Whether it is true will be a matter for future research to decide.

The general reader, therefore, should be cautioned that the aspects of the analysis that are based on research can be accepted with more confidence than those aspects that break new ground, even though the latter may sometimes prove more interesting. Technically, from a scientific point of view, the explanations provided herein must be tested systematically before any of them can be accepted.

Throughout this book, I have tried to write in a style that will make it accessible to the non-specialist. Nevertheless, I do periodically use technical terms employed in the research that I have found most useful, because progress in part depends on the use of a common language. However, since much of this research has been guided by the realist paradigm, scholars often use concepts that I find to be fundamentally flawed. These include such basic concepts as power, the balance of power, hegemony, influence, deterrence, and labels such as major *power*, minor *power*, or employing shorthands, like "France," to personify societies. My main concern with each of these is whether the things these words refer to have actually existed in the way the concept or label implies they do. What, for example, are the theoretical preconceptions involved in referring to a state or some other political actor as a "power"? Have states really been deterred, or do we just assume that? What does it mean to say that France took a particular action? In this context to what does the term "France" really refer, and can we avoid the theoretical dangers inherent in using such an anthropomorphizing label?

Generally, I have avoided using theoretical terms I view as hopelessly flawed. The one area in which this proved awkward was with the terms *major power*, *minor power*, and *great power*. Since I do not think that countries can be treated as billiard balls that are simply a bundle of capabilities, and because I believe the realist paradigm to be an inadequate guide to scientific inquiry (see Vasquez, 1983a), I have abandoned the conceptually distorted habit of referring to countries as "powers." The capability of countries may not always be their most important characteristic; indeed the fact that countries are "states" is

probably more important. For these reasons, I refer to them as "states" rather than "powers." I also avoid the term "great" because it is normatively biased in favor of the elites who ran those states.

Unfortunately, most conceptual problems are not so easily solved. Let me begin the analysis by turning to a concept that has received surprisingly little scrutiny – the idea of war. Its meaning and scientific definition are a proper subject for the first chapter.

1 CONCEPTUALIZING WAR

> In defining a word, one may be doing a lot more than one suspects.

DEFINITION

It seems only fitting that a scholarly work should begin with a definition of the subject at hand. Yet defining the subject of an inquiry is no light task. Although lilliputian scholars often get so bogged down in definition that they never get to the inquiry itself, poor definitions not only lead to confusion but often end up telling you more about the person who stipulated the definition than about the subject. Because the subject of definition and meaning is more complicated than appears, it needs to be analyzed before even attempting a definition of war. For this reason, I begin this chapter with a discussion of the nature of definition – how definitions are employed in and affect inquiry, the relationship between definitions and conceptualizations, and how adequate definitions and concepts can be distinguished from "flawed" ones. With this philosophical framework in mind, I argue that at the beginning of a scientific inquiry, it is best to begin with a simple working definition and to delineate the more interesting insights of complicated definitions and concepts of war as a set of theoretical assumptions separate from the definition itself.

In the second section, I select a working definition for the inquiry and elucidate some of its latent assumptions. Next, I identify the operational definition used by empirical researchers as the working definition of war in quantitative world politics discourse and analyze its latent assumptions.[1] In the third section I turn to a review and assessment of various conceptions of war by analyzing some of the more important theoretical definitions that can be found in the history of international relations thinking and in other disciplines' study of war. From these I draw out a set of insights that form an inchoate conceptualization of war. In the final section, I derive from this conceptualization the main theoretical assumptions about war I have

found most useful in my own attempt to understand war and which will serve as a foundation and a perspective for this analysis.

Surprisingly, not much attention has been focused on how war *should* be defined or even on the broader question of its conceptualization. Most scholars seem content to work with modified everyday or ordinary definitions, since "we all know what a war is," a sure sign that we may not know anything theoretically significant about war.[2] Nevertheless, ordinary definitions provide, with slight modifications and stipulations, three useful functions. First, they can delimit the empirical domain of an inquiry by providing the "defining criteria" (Frohock, 1974: 56–60) that detemine what phenomena will be included or excluded. Second, they try to provide at least some consistency of usage so that the word refers to the same thing. Of course, since everyday language is robust and living, most words have more than one meaning, and this can lead to ambiguity and a lack of conceptual rigor if an analyst fails to stipulate in what sense a concept is being used. Third, ordinary definitions help us get started; they allow us to talk to each other about the subject, to investigate and research it, to see it.

It is the latter function that makes words and concepts so important and, hence, scholarly decisions about how a word *should* be defined are much more complicated than was first understood. As a number of analytical philosophers have pointed out, if we do not have a word for something, it is difficult for us to even see the phenomenon and thus be aware of its importance and its connection to other phenomena. Indeed, without a word, we have trouble understanding whether the thing at hand is a single phenomenon or several (Hanson, 1965: ch. 1). One of the major contributions an intellectual or a scientist can make is the invention of a new word that permits us to see things we never saw before. Darwin's notion of evolution and natural selection, Freud's conception of the unconscious and of repression, Marx's idea of exploitation were all words that these seminal thinkers invented to communicate and see what no one else had really seen before. Once these words were uttered and applied we began to understand what before we only darkly perceived. Once having done so, our world (the phenomena we recognize) changed.

In this way, concepts have a dramatic impact on the world we perceive. The hope of science is that it can formulate concepts that illuminate what is causally significant and obfuscate the unimportant, rather than the other way around. The history of science is the history of rejected conceptions that distorted relationships more than they clarified them. Nevertheless, even when concepts appear to work

scientifically, one can assume that there are aspects of a concept that are more illuminating than others.

Since science and knowledge advance, in part, by conceptual changes, the greatest task of definition is to produce a conceptualization that will result in a major breakthrough. However, because everyday definitions are derived from cultural experience rather than scientific analysis, it is highly unlikely that they will be up to this task. This makes for a predicament, because, in order to formulate a really useful scientific concept, something must be known about the subject, but in order to know something significant about the subject there must be an adequate conceptualization of it. The practices of scientific inquiry deal with this predicament by changing concepts as inquiry unfolds. The most adequate scientific concepts are the products of a fully developed theory and research program, which usually are not the concepts with which the inquiry began.

The history of science demonstrates that, while conceptualization should be taken seriously, any particular concept of definition should not be taken too seriously too early. Since the most important aspect of concept formation is to develop a conceptualization that will illuminate what is theoretically and causally significant, and since that will only be known if the concept produces hypotheses that pass scientific tests, the most important criterion for accepting an initial definition of war is that it permit research to begin. For the point of any empirical study of war, as Fukui and Turton (1979: 3, quoted in Ferguson 1984: 4) point out, is to focus on "inspecting the phenomena and not defining the word." Once we have adopted a working definition it should lead research in a direction that will produce suggestive findings. If such findings are not produced, this may be an indication that the definition is flawed, since it is not fulfilling the purpose for which it was chosen. When a research program is producing null findings, then there is a good chance that the fault lies in poor conceptualization. Very poor concepts can produce failure and frustration.

This discussion assumes that there are no *real* definitions, definitions like those Plato sought for Justice, definitions that are absolute and reflect metaphysical verities. Instead, we remain in Plato's cave and can only develop *stipulative* definitions, working definitions that arbitrarily delimit what we are trying to study. Since science is primarily a process, it does not expect that any one stipulative definition will become a real definition that fully captures for all time the truth of a subject. Nevertheless, it should be clear that while "modern" science does not expect its definitions to reflect metaphysical verities, it strives to make its most useful concepts reflect "causal verities."

Scientists can never know if their concepts are doing this, and some question whether there are any "causal verities" in nature other than those we impose through our conceptualization. Despite this caveat, science and knowledge seem to progress by assuming that concepts that produce significant findings are capturing something about phenomena (if not nature) and not simply reflecting mental processes. The only ultimate test that science has had that it is, in fact, understanding the world is that its practices, which in a certain period of history won control over a large area of inquiry, have succeeded in providing answers that have permitted people to control and manipulate their physical worlds. Clearly, part of the promise of the scientific study of war has been the prospect of this kind of manipulation and control.

How does one get a conceptualization that will produce fruitful research that will be policy relevant? There is no easy answer to this question, but it is clear that even inductively oriented researchers do not test every possible claim. Rather, they mentally sift through ideas to see which make sense and to reformulate and make more rigorous those concepts that seem promising. Since all definitions focus on only some factors, one way of determining whether a concept is likely to produce interesting research is to uncover the latent theoretical assumptions it makes and examine the extent to which these assumptions seem plausible.

The problem with this portrayal, as critics of "positivist" interpretations of science maintain, is that it makes the process of selecting definitions appear much less complicated and fraught with pitfalls than it is. The portrayal ignores the fact that definitions and language play a variety of roles within a culture and not just the pursuit of scientific truth. In particular, it must be recognized that definitions do not simply provide a way of discussing phenomena, but help produce phenomena (see Shapiro, 1981: 5, 20–21). It is important to understand how the constituting function of language affects social inquiry and how intellectual discourse affects language.

Definitions that provide a constituting function are usually associated with an institution and its practices and for this reason can be called *institutional definitions*. Such definitions create a social institution (a set of practices) by delimiting what practices constitute the institution and what do not. In this way, they help make a certain activity come into existence and attempt to control it once in existence. Rules of games (chess, baseball) or definitions of ethical behavior and customs (like promising) are institutional definitions in that, by defining what it means to play chess or to make a promise, they provide a

kind of constitutional structure for the activity.[3] This defining structure also serves to keep the institution "pure" by keeping its practices distinct from those of other activities or institutions. The "constituting" function of language is often performed by intellectual analyses, which can be scholarly, religious, political, magical, legal, and/or scientific depending on the dominant intellectual modes of a culture.

The presence of influential *institutional definitions* of a phenomenon under inquiry may not only hamper "objective" analysis, but "frame" and push inquiry in directions in which the investigator is not fully aware. For example, Grotius defined war as a legal condition between juridical equals that is declared and which regulates the way those contending by armed force may behave (see Wright, 1965: 9–10). The declaration of war is important because it sets aside normal international law and announces that a special set of laws on warfare will now govern relations. Grotius did not simply define war as it appeared historically in his day, but defined the institution of war; he defined war as an institutional fact within the existing system (and global culture) of early-seventeenth-century international law.

His definition demonstrates that definitions do not just uncover phenomena but create them. However, it should also be clear that Grotius does not start *de novo*; he inherits an ongoing activity and tries to shape and push this raw material to fit an ideal. The ideal type, if sufficiently influential (as was Grotius' analysis) can take on a life of its own. The ideal type of the activity, while rarely achieved in practice, acts as a demi-urge constantly trying to shape the events of history to conform to a definitional recipe. Since this is the case, there is always the danger or the prospect (depending on one's point of view) that a new definition or concept of war can change the practice of war; that it can shape the institution of war the way in which both the just war tradition and Machiavelli, in different ways, attempted to do. The fact that this can occur is most obvious with legalistic definitions like Grotius', because the institution of international law that he employed has become outdated. It has been replaced by less obvious and more informal institutions that nevertheless satisfy the definitional function, as can be seen by the fact that war is easily distinguished from riots, revolution, and ubiquitous violence.

From the perspective of the constituting function of language, what is considered war is a product of history – a product of the beliefs, formal and informal laws, and customs of a particular period. This emphasizes the notion that war is a social invention, a fact created by an institution that takes certain actions and makes them a thing. The definition of war reflects the process by which the verbs to fight and to

kill become nouns, the process by which the actions become an institutional fact. In the process, that action is changed and controlled. The raw action is controlled by the dominant ideas and thoughts about the action (themselves a product of learning from engagement). These ideas try to add to and delete characteristics from the raw action, giving the action, in this case war, a goal and purpose, a strategy and a set of rituals (like declaration, surrender, treatment of prisoners, diplomatic negotiating etiquettes). However, these institutional ideas must constantly confront the raw action itself and any ideas of individual participants that may not reflect the ideals.

For the phenomenon of war, the main source of deviation from ideals is likely to come from attempts to win a war. "Innovations" of this sort are often seen as immoral and illegal. They involve things like unrestricted submarine warfare and civilian bombing. If they are successful, then they become selected out and the institution accommodates itself to them. At times, some forms of warfare are so different from prevailing customs that the term *war* is not applied to them. These forms of fighting are beyond the conceptual pale of the definition of war. When this occurs, we have a good illustration of the fact that definitions not only reflect "reality," but help create it and give it a degree of legitimacy. This is most evident today in the concept of terrorism, which is seen as something separate from war, although the case for its separation is primarily political and appears motivated by questions of legitimacy and conformity to recent historical practices, rather than a scientific delimitation of the empirical domain.

From this analysis it should be clear that the definitions of most terms do not emerge from a scientific pursuit of the truth. The history of a definition is a history of a culture's or discourse's view of the world it has created given the raw material at hand. Because we make our own world and how we have made it affects our behavior, it is always difficult to have a concept capture the world as it is. Concepts do not just reflect or capture reality, but help make reality. In the process, they distort or illuminate different aspects of the world they are shaping. In this way, a definition's history can provide the traces of those aspects of the raw action that any given age or culture saw as most important, but cultural importance is not necessarily the key to scientific understanding.

How a phenomenon should be conceptualized can become a matter of contention between science and other discourses. Why a particular concept or definition may come to dominate an age or inquiry poses a jurisdictional problem for scientific inquiry. Principles of contemporary scientific concept formation (see Hempel 1952) try to evaluate the utility of definition and conceptualization solely on their basis for

explaining empirical phenomena. Concepts and definitions, however, have purposes and consequences other than their ability to explain. Concepts may have a religious, metaphysical, ethical, or cultural utility. Statements may be believed not because they are "scientifically true," but because they help one get along in the world; live the proper way; capture the metaphysical essence of why we are here; or satisfy a group's or a "society's" political and economic interests.

One of the problems of discussing politics is that not everyone's primary purpose may be the search for "scientific truth," which is another way of saying that what is meant by the Truth is a question of debate and contention. Science wants to argue that whether we really know something is true must depend, at least in part, on the criteria people use to believe things. If some people believe things because it is in their personal or political interests or because they desire it, this does not make them "true" (nor necessarily false). However, even while those who are opposed to scientific thinking might agree with this view, this framing of the question ignores the more fundamental question, which is why should scientific (empirical) truth be the only or primary basis for accepting statements or beliefs? Once this question is asked, it becomes clearer that science itself must be seen as an institutional discourse that competes with other institutions and their discourses for the control of language and belief in certain domains.

This is the major insight suggested by Foucault's (1972, 1980: 112–14, 131–33) analysis of modern science. "Post-modern" critics, like Foucault (1980), and Shapiro (1981), have gone a long way in refuting naive positivist explanations of science as "objective." However, just because scientific discourse can be seen as competing with other discourses for control of beliefs does not mean that there are not good reasons (both epistemological and practical) for choosing scientific criteria of truth over others in questions of empirical observation (i.e. concept formation) and explanation.

Whether a scientific concept should be permitted to exercise a constituting function, thereby affecting the practice of war as an institution, is a question that must be held in abeyance for now. Suffice it to say that until the empirical study of war and peace is able to provide an adequate scientific concept, the question is moot. In the meantime, the realization that different concepts of war may affect the practice of war suggests that an analysis of different conceptions of war may provide insights about the nature of war and how it changes.

Although this discussion of the nature of definition has not answered all questions posed by the act of defining, it has made it clear that there are non-theoretical assumptions and consequences involved

in accepting definitions which can make the task of defining an important social phenomenon, like war, subject to a number of pitfalls. In particular, the use of previously accepted definitions that appear highly theoretical may carry with it the unconscious acceptance of historical institutional legacies that may lead a research program in directions that would not be warranted from a purely scientific perspective. This problem can be avoided by not incorporating too many untested assumptions in a definition at an early stage of inquiry because it could produce a too narrow or distorted empirical domain for fruitful research. Rather, many useful assumptions can be taken as auxiliary hypotheticals to form a broader conceptualization of the phenomenon that both guides research and theory construction and is changed by it. In this way, the initial working definition remains fairly stable in its identification of the empirical domain and does not have to carry the weight of the entire theoretical enterprise. I will begin by selecting a working definition of war that can accommodate a number of conceptualizations and at the same time satisfy two of the main functions of ordinary definitions – demarcation of the empirical domain and consistency of use.

DEFINITION OF WAR

Probably the best way to move from an everyday definition of war to a working scholarly definition is to try to think of what phenomena it would be most useful to study to learn about war and what phenomena would make the effort too diffuse and divert it from its main focus. One immediately thinks of the two world wars, extended and major fights that shaped history, and a host of other more limited interstate wars, like the Franco-Prussian war. Certainly, any study should include these kinds of wars, and it is what Webster's Dictionary means by war when it defines it as "a state of usually open and declared armed hostile conflict between states or nations."

What about armed clashes between "primitive" tribes or border conflicts? Are these wars? They are organized and fought under the authority of a leader, characteristics associated with state warfare, but for some investigators these clashes are too small (in terms of the number of casualties) or too short in duration to study. Others might not want to study such clashes because they might see them as a fundamentally different human phenomena that would tell us little about (interstate) war.

What about acts of violence like revolutions and civil wars, which take place within the same society rather than between societies? Is the

distinction between what is *within* a society or a state and what is *without* the critical distinction that should be made to determine the proper domain of what is war? If so, then some of the longest and bloodiest violent political acts of history will be excluded. What is the theoretical justification for doing this and what are the implications for research?

What of interpersonal violence? Is war fundamentally different from these acts or just a special case of human violence? What about organized acts of human violence like dueling and vendetta, are these wars? And what of violence itself? Is human violence similar to animal aggression? Are the wars fought by ants for territory and the food associated with it that different from human wars? Does war have to result in death or can it still be war if no one is killed? How important are attitudes in war? Was the Cold War a war because of the extreme hostility? Are two states with a protracted border conflict that have only periodic clashes with few fatalities, at war with each other?

Such questions can be irritating, especially when they are raised by the sophist. Answering such questions, however, is much simpler than such an extended list would imply. Two things must be kept in mind. First and most important, what is or is not a war or special class of war can be determined by whether one thinks this phenomenon is caused by the same set of factors. I once met a historian who argued, perhaps facetiously, that there were as many causes of war as there were wars. For him, war simply did not exist as a general social phenomenon. Gang fights, wars among ants, and World War II can be treated as wars if we believe that they are all caused by the same factors. Since we usually do not, or at least think it not very useful to begin an inquiry with that assumption, they are not usually included as part of the class of events that we choose to call war. This is not the case however, with civil wars, border clashes or "primitive wars" which may, in fact, have the same causes as interstate wars. Conversely, revolutions are seen as a distinct phenomenon because theories of revolution, especially those that emphasize relative deprivation (Gurr, 1970), are seen as inapplicable to interstate war. This underlines the point that what is or is not part of a phenomenon in science is determined by whether a separate theory is needed to account for it.

Second, even though we may not treat something as war, this does not mean that studying it may not give us valuable insights about the causes of war. Studying interpersonal violence, animal aggression, or even labor strikes may tell us something about the respective psychological, biological, and political dynamics associated with war. Studying phenomena that seem related to war may elucidate different

aspects of war. For example, studying animal aggression may help us understand territoriality and other ecological factors associated with war. Studying family violence may add insights about the role hostility and frustration play in the onset of war. Studying gang wars and labor strikes may tell us something about war as an instrument of force. Studying interpersonal violence may elucidate psychological factors and the role of motivation and emotion in war. Studying "primitive" warfare helps us understand factors associated with war that precede modern complex societies and that may be basic to the human condition regardless of history and culture.

Eventually, science may be able to explain what makes certain acts of violence fundamentally different from each other, as well as understand how they are related and connected, if they are. It can not be known in advance which perspective is correct. The scientific utility of these different perspectives can only be assessed in light of their ability to generate a fruitful research program that culminates in an accurate and explanatorily powerful theory. In this sense, the disciplinary division that has given rise to different approaches to war in political science, sociology, psychology, social psychology, anthropology, animal behavior, history, and geography is a useful way for social science to spread its research bets.

Given this diversity and interest from various disciplines, it is difficult to find a common working definition and nearly impossible to reach a consensus on a theoretical definition. Nevertheless, a review of the more conventional definitions shows that, while no one definition can accommodate all disciplines, there are some that can accommodate more than one. These, as would be expected, are the more straightforward and less theoretical definitions. The one that will be used for the purpose of this inquiry is that offered by Hedley Bull (1977: 184): "War is organized violence carried on by political units against each other."

The only immediate reservation that I have about this definition is the use of the term *violence*, which remains undefined. Even a narrow definition of violence as direct bodily harm through physical action (rather than broader definitions such as destruction of property, psychological domination, or harm brought about by structural conditions (see Galtung and Hoivik, 1971) seems too broad for what Hedley Bull has in mind. It would seem that war must involve organized violence that aims to kill members of another group, not simply to do them harm, otherwise war become too much like *force*. This reservation, however, is not so complicated that I am prompted to add to the semantics of war by giving my personal peculiar definition.

The overall advantages of Bull's definition can be readily seen by

examining one of its competitors. For example, Malinowski's (1968: 247, also cited in Levy, 1983: 50) widely used anthropological definition of war as "an armed contest between two independent political units, by means or organized military force, in the pursuit of a tribal or national policy," suffers from defects not present in Bull's definition. Malinowski's emphasis on *independent* political units suggests that anti-colonial wars of liberation would not be seen as wars, neither apparently would massacres of indigenous groups like the Turkish massacres of the Armenians. Likewise, objections can be raised to confining the goals of war to tribal or national policy, or even to the notion that war is pursued for policy reasons rather than some other (including unconscious) reason.

I present these objections as illustrative of the kinds of problems confronted by working definitions. Since not much can be gained by systematically examining various working definitions and indicating why I prefer Bull's to some other, I will not dwell on my reasons for selecting his. I find Bull's definition useful for three reasons. It does not confine war to interstate war, so it is sufficiently broad to accommodate the work in political science, history, anthropology, sociology, and aspects of social psychology and geography. Second, it lacks terms that are contentious or would impose too great a theoretical perspective. Third, the main theoretical term it does include, "organized," I find particularly useful.

Since it is analytically impossible for a definition not to impose any perspective on a subject matter, it is important to make Bull's assumptions explicit. The most obvious assumption made by his definition is that war involves collective violence; it is not simply *conflict*. Bull's focus on violence is important, because it makes it clear that war and conflict are not synonymous. Conflict is a very broad and somewhat ambiguous term. One could easily argue, as E. H. Carr (1939 [1964 ed.: 42–44]) does, that conflict is pervasive and inevitable, since it can not be assumed that there will ever be a permanent harmony of interests among political actors. Despite the reality of such conflict of interests, it is clear from the historical record that not all conflict ends in war. Indeed, war remains a relatively infrequent event in the history of most nation-states; Small and Singer (1982: 78) report that there were only 118 international wars from 1816 to 1980. Since this is the case, there must be something that distinguishes the vast majority of conflicts that do not end in war from the very few that do, which suggest that the causes of conflict are not the same as the causes of war.

Another important assumption made by the definition is that war is *organized* violence. This has three implications. First, it means that war

is an ordered activity with rules and customs. This encourages an examination of how conceptions of war affect the practices of war. Second, it means that war is not random violence, but focused and directed. It reflects, no matter how irrational its overall impact or the chaos of immediate battle, some rational purpose for which it was initiated. Third, it is organized in the sense that it is collective and social, not individual. This is not interpersonal violence between individuals who have a personal dispute. Indeed, one of the theoretically interesting things about war is that it is fought by people who usually do not know each other and have no personal dispute or animus toward one another other than that which has been defined by those controlling the war.

The next critical assumption made by Hedley Bull's definition is that war is fought by political units. Again this emphasizes the collective aspect of war, but it is not violence between just any collective actors, but primarily between political organizations. Economic organizations do not fight wars, they compete in other ways. Even robbers and their victims do not typically engage in war, but in something else, and the point is not usually to kill. There is something about politics, what it does and the functions it serves, that seems to make it more prone to violence than other activities. Exactly what that is, is left unspecified by this definition.[4]

Finally, unlike other major definitions, this definition does not indicate what is the aim or purpose of the violence except that it is directed toward the political units and only to the members of those units as a means to an end. Unlike Malinowski, Bull does not define war as aiming at a tribal or national policy. He does not tell us that war is a specific instrument to attain a particular goal that can not be otherwise attained. Whether war, in fact, does that becomes a matter of empirical investigation and not definition.

While it is useful to delineate the assumptions of the working definition of this inquiry, it is even more important to be aware of the working definition of war that has guided research within the field of international relations. For most of the research discussed in this book, the working definitions of war have been the operational definitions of those who have collected scientific data on war. The Correlates of War project has provided the most thorough and influential quantitative data set on war, and its operational definition is: "An international war is a military conflict waged between (or among) national entities, at least one of which is a *state*, which results in at least 1000 battle deaths of military personnel" (Bremer, *et al.* 1975: 23 [1992 ed.: 387]; see also Singer and Small, 1972: 37, 39).

The use of an operational definition to guide a research program without ever explicitly developing a conceptual definition has led some to raise questions about procedure, since from a philosophy of science perspective one first has a theory and then collects the data. This did not happen with the Correlates of War project, and the fact that it did not reflects, in part, the difference between scientific ideals and practice, as well as the early stage of the scientific study of war. Singer and Small (1972) did not employ a specific concept of war because they wanted to create a data set that could be employed by the entire field to test hypotheses from a variety of theoretical perspectives. In addition, although they did not have an explicit conceptual definition, their operational definition and their actual collection of data were informed by the conceptual definitions of their two main predecessors – Quincy Wright and Lewis F. Richardson.

The problem Singer and Small faced with the work of Wright and Richardson was that these scholars' respective definitions stemmed from very different theoretical perspectives and were at times contradictory and had, as a result, produced different sets of phenomena for investigation. Like Singer and Small, Quincy Wright (1965: 636) was concerned with determining the empirical domain of the concept and eventually settled on two defining criteria: "all hostilities involving members of the family of nations, whether international, civil, colonial, or imperial, which were recognized as states of war in the legal sense or involved 50,000 troops" (Wright, 1965: 636, also quoted in Small and Singer, 1982: 37).[5] Since Wright was conducting his study during the realist–idealist debate of the inter-war period, it should not be surprising that he should combine both the idealist legal emphasis with the realist emphasis on power and impact (See Vasquez, 1983a: 134–36).

Richardson (1960b), being outside the field, took a different perspective. He began by throwing out the very concept of war and replacing it with the much broader notion of "deadly quarrel." Influenced by psychology and having little respect for the scholarly study of politics (which he regarded, with reason, as unscientific) Richardson saw all killing, whether it be an individual act of crime or a war, as stemming from aggression. Therefore, for him the only thing that distinguished wars from other deadly quarrels was their magnitude, and Richardson (1960b: 6) ordered his data on the basis of the number of deaths grouped by various cut off points of the logarithm to the base ten.

Singer and Small decided to collect data on wars from 1816 on by combining lists of wars and deadly quarrels of Wright and of Richardson, since for them every instance of military combat is a potential datum (see Small and Singer, 1982: 37; their data were first published

in Singer and Small, 1972). However, since Richardson included incidents that did not involve *military* combat and Wright included incidents of military combat that Singer and Small found problematic, they went through the combined list and eliminated the cases they regarded as "non-wars." For them, an incident is a non-war either because of "the inadequate political status of their participants," or because there are fewer than 1,000 battle deaths (Small and Singer, 1982: 38, 54).

Although these criteria appear innocuous enough, closer inspection reveals how difficult it is to free research from dominant perspectives, both those of the present and of the past. The first criterion, on political status, leads Singer and Small to focus primarily on the use of violence by nation-states, either against other states or against entities that the system of nation-states did not recognize as nation-states. This criterion leads them to distinguish between interstate wars and extra-systemic (imperial and colonial) wars. For interstate wars, their data set is the most definitive to date. For the extra-systemic wars, the data is the most accurate to date, but woefully incomplete for non-national entities, which are usually the victims in this historical period.

The discrepancy in the quality of these two data sets may be seen as part of the historical legacy of Western imperialism and racism that simply did not regard non-Western groups as civilized or as human beings equal to whites. It is not unfair to assume that such attitudes played some role in accounting for the fact that Western nations did not bother to record in any systematic way the fatalities sustained by non-national groupings in imperial wars of conquest or pacification.

This historical legacy forced Small and Singer (1982: 56) to make certain "practical" data-collection decisions which resulted in the discrepancy in the two data sets. First, Singer and Small decided to count the battle deaths only of recognized nation-states to determine what is or is not a war for their data set. This establishes a double standard in that, for an interstate war, the minimum fatality threshold for inclusion is a combined total of 1,000 battle deaths from all the sides that fought in the war; whereas for extra-systemic wars (since non-national participants' deaths are not counted) the nation-state must sustain that level by itself in order for the incident to be recorded as a war. This eliminates from consideration an entire range of unequal contests, even if they amounted to massacres.

Second, Singer and Small require that 1,000 battle deaths must be sustained by nation-states participating in an extra-systemic war *each year* in order for the war to be considered as ongoing; whereas for interstate wars once the 1,000 battle death threshold is crossed it is a

war and the length of the war is determined by other factors. The reason for this research decision is that many "pacification" efforts drag on, and Singer and Small do not want these years to count as wars because it would inflate the frequency of war in the system in any given year or decade (see their discussion in Small and Singer, 1982: 56–57). Nevertheless, these decisions have the effect of making some people's deaths and wars not count, which is just the way the West viewed these conflicts in comparison to their own "real" wars.

This "coincidence" has led some (like Duvall, 1976) to charge that Singer and Small's operational definition is ideologically biased. In one sense, this is true, but, in another, this accusation is misleading since the reason the definition is biased has much more to do with previous history and governmental records than it does with Singer and Small. They are faced with the fact that others, less objective than they, have managed to shape and control the past so that part of the record is not easily reconstructed. The only alternative would have been to make crude estimates, and they were unwilling to do this because it would have greatly reduced the scientific reliability of their data.

What are the theoretical implications of this historical bias that has been handed down to researchers from the past? The clearest is that the data set on interstate wars is more complete and more valid than the data set on extra-systemic wars. This means that we can have more confidence about inferences based on the interstate data than on the extra-systemic. Since this is the case, analysis of the two data sets should be kept separate, which usually has been done. It must also be recognized that generalizations about interstate war may not apply to extra-systemic wars or to all war. The causes of interstate war may be fundamentally different from the causes of extra-systemic war, certainly the motivations (or reasons given for) these wars are different. Nor must researchers assume that the absence of interstate war means that there is no ongoing war and the system is at peace, a common mistake in data (as well as historical) analysis. If the absence of interstate war is taken as an indicator of peace when, in fact, there is ongoing extra-systemic war, then a correlation between a set of independent variables and "war/peace" may really be a correlation between independent variables and different types of war. Finally, it must also be noted that the elimination of military confrontations with fewer than 1,000 battle deaths, which means, in effect, the elimination of many border clashes, may be eliminating from observation an important set of wars and underestimating the effect of territorial considerations.

This discussion should make it clear that even the most scientific,

systematic, honest and fair-minded attempts to define war with a minimum of theoretical "distortion" not only produce a number of theoretical implications, but are easily subjected to prevailing ideological assumptions about what is *really* war and what is peace.[6] Nonetheless, the solution to such problems is not less science, but more science. Better data, painstakingly reconstructed if necessary, and rated for its reliability, could be collected for extra-systemic wars. Data on wars with fewer than 1,000 battle deaths has been collected by Kende (1978) and by Gantzel (1981). In addition, one should not conclude in the face of problems that nothing can be learned from Correlates of War data or any data because it will always be biased and its results suspect (a conclusion Duvall (1976) comes close to making). Instead, one must be aware of the domain to which the data and the findings are applicable, a point which reminds us that we must be detectives if we are to solve the war puzzle and not expect an incontrovertible answer to emerge miraculously from the logical rigor and objectivity of research designs.

THE CONCEPT OF WAR

Because even working and operational definitions make theoretical assumptions that may distort the world, I have avoided beginning this inquiry with an explicit theoretical definition. This keeps the empirical domain and research as open as possible. Nevertheless, since the ultimate point is to discover what is the best way of viewing the world so that we can understand the causes of war, a review of theoretical definitions provides an understanding of the various concepts of war that have existed and some insights about what may be the most useful theoretical assumptions to make in trying to explain war. Such a review also tells us something about what the various periods of history saw as the most important elements in war (or at least the wars of their time).

Quincy Wright's review of classical Western conceptions of war remains one of the most useful and will serve as a basis for beginning this review. According to Wright, one of the earliest important definitions of war comes from Cicero, who defined war as "contending by force," (quoted in Wright, 1965: 10). This suggests that war involves contention *over* something and that while war differs from other contentions in that it employs a special means, namely force, we should not lose sight of the fact that war is a form of contention. In politics, contention consists of disputes over objects of value (i.e. stakes). "Contention" is the general term applied to the various means

that political actors use to resolve a dispute. From this perspective, war may be considered a violent way of getting objects of value.

As Wright (1965: 10) notes, Grotius takes exception to Cicero's definition by maintaining that war is not simply a contest, but a condition, by which he means, "a legal condition." Although this may have been true in Grotius' time (and as was shown earlier, the impact of his work helped make it true), it is not necessarily true for all time. Put another way, it is possible to think of Grotius' exception as stating that war under conditions of accepted international law can become a kind of legal contest that regulates how and for what purposes armed forces may be employed.

Certainly, this is Wright's (1965: 8) view, who formally defines war as "the *legal condition* which *equally* permits two or more *hostile groups* to carry on a *conflict* by *armed* force" (italics in the original). Wright sees war as a condition or period of time in which special rules come into place which permit and regulate violence between governments. The purpose of this violence is to settle disputes. Wright also has behavioral concerns that make him wonder about the other characteristics of "the condition which prevails while groups are contending by arms" (Wright 1938 cited in Wright 1965: 8 note 1). Law and custom, according to Wright (1965: 698) recognize that "when war exists, particular types of behavior or attitudes are appropriate." From this perspective, any given culture (including a global culture) consists of a number of different conditions (situations), each with its own appropriate behavior pattern. War can be seen as simply one of those conditions with custom informing neophytes how to behave when faced with such a situation, and law institutionalizing and sometimes shaping existing custom. What this appropriate behavior pattern is, what brings it about, and how regular it is throughout history and different cultures provides a latent social science conception of war within Wright's more explicit legal focus.

Today this legal focus appears less relevant, and many see this Grotian idea of international law as unrealistic. Much of this thinking is part of the legacy of the two world wars. Yet despite that dual trauma, Hedley Bull (1977: 184–89) reminds contemporary analysts that war has an order to it. It has a purpose; it is fought on a certain basis and often according to norms and rules. A world order involves an understanding of when and for what reasons war may be initiated and of who can initiate it (in the modern system only sovereign states). International society does not permit states to go to war for just any reason; it identifies either through law or intellectual argumentation the *casus belli* and legitimate reasons for war (Bull, 1977: 188). The

presence of war does not mean that there is no order in the world, that all is a Hobbesian anarchy; rather, the strength of order in a global society is reflected in how it makes war. A true anarchy is not characterized by war, as Bull (1877: 185) notes, but by a "more ubiquitous violence". This, according to Bull, has been the historical alternative to war.

To understand how war differs from ubiquitous violence, we might return to Cicero's notion that war involves a contest. If, in the early seventeenth century, war had become, in part, a legal contest, then one wonders what kind of a contest it was before and what kind it has become since. Such a question makes it clearer that the word *contest* is a metaphor that implies that the "deep structure" of a contest (the elements that make a contest a contest) may also be the "deep structure" of war. In order to have a contest, in any period of time, according to the way we use the word, it is necessary for the parties to be aware of the formal and informal rules of the contest. There must be a prize, the parties must be aware that they are contending, that there are winners and losers, and that contenders have some sense of what it means to win and lose – although each may underestimate or not anticipate the most important consequences of their actions. A contest is an "institutional fact" whose meaning is only fully apprehended to the extent one understands how the practice of contests is created, sustained, and implemented. Because a contest is a human institution, it will vary with time and place, although the deep structure that is reflected in the word will provide a way of illuminating the similarities of different contests.

This analysis suggests that *legal* contests are only one kind of contest, which is another way of saying that how and why parties fight is determined by the tradition, customs, and thought of time, which set up the rules of the contest so that the contending parties have a shared understanding of what they are doing.[7] This view comes close to an anthropological conception of war which sees war as an institution that is invented by humans and persists because it satisfies certain functions.

Margaret Mead (1940: 402) argues that war is an invention, like writing, cooking, marriage, or trial by jury. It forms part of the knowledge which is derived from the cultural inheritance of a group. For Mead (1940: 403) war is a *social invention* that gives people the idea that war is the way certain situations are to be handled. From this conception, it is clear that war is learned behavior. Since Mead defines war as fighting and killing between groups as groups, the learning is collective learning. A society or tribe learns from its wisdom and folklore

that when confronted with certain situations (with characteristics X, Y, Z), war is the appropriate response. As these inherited lessons are put into practice, the invention is elaborated and changed depending on the consequences. If such a conception is accurate, it would go a long way in identifying and predicting when war will occur, although not necessarily the causes of war.[8]

Mead's conception of war as a social invention provides a theoretical advance over the conception of war as a violent contest in that it suggests an explanation about the origin of war as a human phenomenon; whereas the conception of war as a contest does not tell us why violent contests appear in the first place. Mead's analysis implies that war first came about because some people learned to handle certain situations in this way. Why they learned to handle the situation in a violent manner rather than some other way is not known. Mead does point out, however, that not all societies are aware of the invention, so she concludes it is not inherent in human nature. More importantly, to eliminate war, she maintains, it will be necessary to develop a different way of handling the situations that a society handles by going to war. While such functionalist explanations are fraught with logical problems (see Brown, 1963: ch. 9), particularly the tendency to treat consequences as causes, the search for functional equivalents to war is theoretically useful in that it encourages analysts to think about what role war is playing within global society, what its manifest and latent purposes are, and how these purposes could be attained in nonviolent ways.

Mead does not discuss why war became such a popular invention that it has persistently been utilized. Biologists, to the extent that they concern themselves with war, tend to explain its persistence in evolutionary terms. Biologists do not look at war, *per se*, but at animal aggression. Although only a few species exhibit behavior similar to war, a number are territorial and defend that territory by being aggressive. A biological definition of *territory* is "an area occupied more or less exclusively by an animal or group of animals by means of repulsion through overt defense or advertisement" (Wilson, 1975: 256). Animals that hold territory will fight a stranger of their own species that enters their territory. They will also fight over food, and males will fight over females, both of which can be associated with territory.

Wilson (1975: 247–48) argues that territoriality evolved because a limited food supply made the territory worth defending, and the risks associated with that defense were limited. Employing conventional biological assumptions, he states that animal behavior is guided by what is advantageous over time and evolves when it has a selective

advantage for certain individuals; this, in turn, increases the probability that the entire species will evolve toward that behavior. It may be that the human institution of war evolved out of this inherited tendency of vertebrates to be territorial.

For most biologists, it is inconceivable that any widespread animal characteristic, including human aggression, could not be advantageous for survival and reproduction (Wilson, 1975: 254). This has led some to argue that war persists because the clans and groups that employed war succeeded over those who did not and thereby passed on their more "aggressive tendencies." If this were correct, it may explain why people learned to handle certain situations by going to war. Such lessons might be passed on not only intellectually, but biologically, for as Wilson (1975: 255) states, "the capacity to learn certain behaviors is itself a genetically controlled and therefore evolved trait."[9] Because any evolutionary perspective is subject to teleological tendencies and overgeneralization, these arguments must be treated cautiously (see Shaw and Wong, 1987a, 1987b; Goldstein, 1987; and Kitcher, 1987). Nevertheless, a biological perspective is important, because it reminds us that we have a genetic inheritance, and that may make us fight over certain things, like territory, but not others, as well as make us more predisposed to learn certain behaviors.

Clearly, humans more than any other animal have learned to use war, but for what purpose? The conception of war that places the goal and purpose of war at the center of its emphasis is that of Clausewitz. Clausewitz provided two famous definitions of war: "War is a mere continuation of policy by other means" and "War therefore is an act of violence intended to compel our opponent to fulfil our will' (Clausewitz, 1832, Book I, ch. 1, sections 2, 24, J. J. Graham translation). The first delineates the purpose of war and the second elucidates the logic of its means. For Clausewitz, war is a *political* act of *force*. The political aspect for him is the most important, and much of his treatise lectures about the need to keep the political aim of war in the forefront and not permit the dynamics (and emotional) aspects of force from obscuring that political aim. This focus on the instrumentality of war makes Clausewitz emphasize the rationality of war.

Clausewitz sees war as an instrument that is required by a certain activity, namely politics, and by certain situations. Clausewitz puts forth the idea that war occurs when normal politics and diplomacy fail. When the existing diplomatic practices (whether they be the simple exchange of ambassadors, or elaborate mechanisms of conflict resolution and international law) are unable to satisfactorily produce an agreement among disputants, then war becomes a means by which

one side can compel its opponent to fulfil its will. War occurs when one side is not willing to lose or give up a political aim in light of the costs of fighting a war. This implies that wars are fought only over certain types of issues and that these issues may change depending on the particular historical needs, culture, or law of an era (see Luard, 1987: ch. 3; Howard, 1976).

It also suggests that, if a society has other ways of resolving disagreements in a binding or mutually satisfactory fashion, war will not be used. From this perspective, peace may be achieved by keeping certain issues off the agenda and/or providing binding alternative ways of making political decisions. Whether these are two crucial characteristics that distinguish peaceful systems from those that are war-prone is a question that has received insufficient attention. However, from the perspective of understanding the causes of war, two questions are raised by emphasizing the instrumentality of war: what is it about politics that causes certain issues to emerge and be defined in such a way that they are irresolvable, and what is it about violence that makes it such an attractive instrument?

Because politics has from earliest times involved the realm of rule and the ruled, power and authority, privilege and obligation, politics has entailed control – control of people and resources, activities and territory. Control raises at least three problems. First, who controls? Second, who benefits or is hurt by the particular set of controls? Third, how is control imposed, since control is naturally resisted and will be resisted even more by those who are hurt by it? To define the world in such terms is to create a set of issues that can in the right circumstances be viewed as life and death issues. By definition, for such issues a group is willing to fight and individuals willing to risk their lives either to assure that they do not lose, or that they will gain important *privileges.*

Clearly, war exists because there are fundamental conflicts of interest in the world, which can not be harmonized away, as the utopians had hoped (Carr, 1939 (1964 ed.: 49, 51–53, 87–88)). There are some issues that call for so much change and so harm prevailing interests that they can not be peacefully resolved. In Bismarck's words, "Not by speechifying and counting majorities are the great questions of the time to be solved ... but by iron and blood" (1862). It should come as no surprise that those issues often involve territory.

Given the fact that a certain aspect of politics consists of getting groups (not just isolated individuals, see Claude, 1962: ch. 7) to do things they do not want to do, then force, which Clausewitz would define as an act to compel our opponent to fulfil our will, is one means

to achieve that end. Why? Because force is the *ultima ratio* among sovereigns (Schuman 1933 [1958: 274]) and war is the ultimate form of force. Clausewitz (1832: Book I, ch. I: sect. 4) maintains that when all else fails to convince opponents, you can change their position by making them suffer or killing their people ("place him in a situation which is more oppressive to him than the sacrifice which we demand..."). Alternatively, in those cases where you do not need your opponents, you can gain your goal simply by removing them from the face of the earth, as Rome did to Carthage.

Historically, it can only be surmised that humans learned that force, and particularly violent force, would help establish political control and be a successful means to gain certain ends. Although the exact origins of war as a social invention are unknown, it seems that this particular use of war coincides with early agricultural civilizations, a time when human groups became more tied to particular pieces of land, and does not coincide with the more primitive hunting and gathering societies (Wright, 1965: 76; Mansfield, 1982). In addition, agricultural societies have a "higher" level of organization, and a certain level of organization is needed to marshal armies and get them to fight battles. Nevertheless, it is clear that some preneolithic societies did use violence to gain and/or defend access to water and food resources (Zur, 1987: 127). Although egalitarian societies generally have a low level of fighting, they can be expected to use violence to take over better land along a river, if faced with absolute scarcity, or some other form of circumscription that affects their material well-being (Ferguson, 1987: 9). Whatever its exact origin humans learned that collective armed violence was an important instrument in gaining one's end.

In more modern terms, violence becomes an attractive instrument because it provides a way of escaping interdependent decision-making. The resolution of political issues usually requires getting others to agree with one, if not to actually take certain actions. This entails an interdependence in the sense that one can not reach and implement a decision without others. When those who are involved in such interdependent decision-making become stalemated, violence, provides an escape by ending one's dependence either though physical domination or elimination of the other. Force, unlike other ways of making decisions, is able to provide such an escape because it is a *unilateral* means (see Mansbach and Vasquez, 1981: 283–84) that depends on one's own capability and not the goodwill of others. If, in addition to these "intrinsic" qualities, force and war have a history of success and are legitimated by custom and not overly restricted by law,

then, given the right issues, it can be expected to be a widely utilized instrument for dealing with stalemated situations. War becomes a useful invention because it provides a means of making unilateral, but binding, political decisions.

Clausewitz' emphasis on war as a form of force raises the question of whether the concept of *force* might not be a better scientific conceptualization of the phenomenon at hand. From this perspective, there is no need to make the causes of war a great mystery by looking for psychoanalytical factors or the roots of human aggression. War can be explained by explaining why force rather than some other means is employed to gain a certain stake or resolve a dispute. Explain the conditions under which force is the only means, the preferred means, or the most efficient means and you have explained war – for war is but a special use of force. Other inducements, like frustration, can be seen as simply additional reasons for using force.

While a persuasive case can be made for studying force rather than war *per se*, the main objection to this view is that there may be a great deal more to the phenomenon of war than its utility as a political instrument. Violence may have deeper, even primordial, aspects that are difficult to contain once unleased. The use of violence may not stem from its instrumentality, but from these deeper aspects of which we are not fully conscious and which we poorly understand. Clausewitz's own need to lecture about keeping the goal of war in the forefront suggests that war is something that is inherently not easy to control, that violence toward and domination of an opponent can become ends in themselves, making war irrational.

Psychological conceptions of war emphasize the violence of war and see the need or drive of humans to kill as the main cause of war. The Clausewitzian view of war as a rational instrument has had difficulty surviving Freud's seminal critique of rationality. For Freud, the reasons and conscious motivations for war, whether they be derived from the cool cost-benefit calculations of military strategists or the casuistry of moral theologians, are mere rationalizations for deeper drives. Although his views of violence and of war were never fully developed, Freud (1930) saw war as an explosive reaction of the id against the increasing repression of modern civilization. The more repressive a civilization, the more severe the explosion. War is a collective and violent slip of the tongue that permits an indiscriminate satisfaction of the desires of the id. At the same time, and more ominously, Freud sees war as a product of the death instinct trying to destroy what has been created and built up by Eros (the life instinct).[10]

While it is difficult to test Freud's view scientifically, it provides a

perspective for questioning political, sociological, and anthropological conceptions of war. Psychoanalysis, along with broader psychological approaches, affirms that war stems from an aspect of human nature and involves, fundamentally, not a cultural invention of a political instrument, but a *mental* state. "The state of mind" that is most associated with war is extreme hostility. This hostility often appears bizarre when it is exhibited by individuals who have not had any direct contact with people whom they consider their enemy. Nevertheless, this kind of hostility is considered part of the behavior that is typical (appropriate) to those in a condition of war. This view of war is so widespread that a number of definitions of war, including Wright's, make hostility a defining characteristic of war, and one of the dictionary definitions of war is "to be in a state of hostility." This emphasis on the mental state of war can be so great that societies are sometimes said to be "at war" even if there is no fighting (Wright 1965: 11–12) as in "the Cold War."

To see wars where there is no killing is something most analysts, even psychologists, are not prepared to do, because for them it is the violence and not the hostility of war that they are seeking to explain. Nevertheless, conceptions of war that emphasize hostility are saying that such emotional extremes can provide reasons in and of themselves for violent actions and make it difficult to use violence in the limited coercive manner that Clausewitz advocates. This raises the question of whether some wars are fought not for political aims, but in order to satisfy aggressive feelings stemming from hostility, frustration, crowding or some other psychological condition. Whether such psychological factors are the effects or causes of conflicts of interests that help produce wars, and whether they are spontaneous or artificially inspired by leaders, it is often the case that extreme hostility (frustration, etc.) appears to play an important part in decisions about war.

Not all conceptions of war as a mental state place emphasis on hostility. Hobbes, who lived before the era of modern psychology, declares:

> For Warre, consisteth not in Battell onely, or the act of fighting; but in a tract of time, wherein the Will to contend by Battell is sufficiently known ... War consisteth not in actual fighting, but in the known disposition thereto, during all the time there is no assurance to the contrary. All other time is PEACE. (Hobbes 1651: pt. 1, ch. 13)

For Hobbes, individuals are at war if they are predisposed to fight and there is no assurance that they will not fight. Hobbes (1651: 205) views war as a mental state that generates what today would be called

extreme insecurity, but interestingly he has little to say about hostility. He does not see hostility as a cause of war nor central to it; rather, war is a condition with "continual feare and danger of violent death." This image gets very close to capturing the immediate experience of battle, and Hobbes may have generalized from it to develop a conception of war. This apparently is where at least the English term, war, gets its meaning, since its etymology can be traced to *werra*, which means confusion, strife.

Although those who look at war in terms of mental states emphasize different experiences (hostility, insecurity, frustration), these conceptualizations generally share an assumption that violence and fighting are the most important aspect of war. It is not surprising that all of these approaches try to explain war by explaining violence, in some cases interpersonal violence, thereby making the concept of *violence* an alternative to the concept of war. Whether such an attempt is ultimately successful will depend, as stated earlier, on the extent to which violence between groups is not fundamentally different from violence between individuals.

Regardless of one's position on this question, the notion of violence as a key aspect of war is a conception that needs to be more fully explored in the literature. An emphasis on the fighting and killing in war makes it clear that war is an activity, not an object with its own ontological existence. The tendency of English (a tendency which is even worse in German) to describe activities by nouns rather than verbs fundamentally distorts the world we are trying to understand. Instead of seeing war as something mechanically caused by certain factors, it might be more illuminating to see war as an action to which states resort when faced with certain situations. In this sense, the older concept of "warfare," which Margaret Mead employs has a more authentic ring to it, as does Hobbes' notion of warre. The human tendency "to warre" is what we are trying to explain, and how one goes about explaining it will depend very much on whether it is conceived as a noun, whose existence must be accounted for, or a verb, whose action comes forth.

With such diverse conceptions of war, each with their own crucial assumptions, it is not wise to choose among them. Nonetheless, no serious inquiry on war and peace can proceed without addressing and making judgments on at least some of the issues that have been raised by these conceptions. From the above analysis, some basic insights about the nature of war can be gleaned to help identify what may be the most important characteristics of war, theoretically speaking. From Hedley Bull's definition, which is being used as the working definition

of this analysis, from Margaret Mead's notion of warfare, and from several other definitions, it can be stipulated that war is a *group* activity, fought between and directed at collectivities. In this manner, it is fundamentally different from individual interpersonal violence in at least two ways. First, the behavior of collectivities poses conceptual problems not present in the analysis of individuals. There is reason to expect that collectivities behave differently from individuals and cannot be regarded simply as individuals writ large. This is especially true of the state and of bureaucracy, which must be treated as special collectivities. Second, war seems to require a certain level of social organization and tends not to be fought in its absence. Thus, the practice of war emerges in history as societies become more organized.

From the analysis of Cicero's definition, it can be stipulated that war involves *contention* over objects of value. As Grotius, Wright, and Hedley Bull make clear, this contention is organized in that it follows certain rules and norms that give the contention an underlying *order* from which each side derives certain expectations about its own and its competitor's behavior. The degree to which the contention (and warfare) is ordered may vary, depending on whether it is based on tacit understanding, custom, or international law. This means that war is a special kind of *contest* with winners, losers, rules, and prizes.

The metaphor of a contest implies that war is an informal institution. Margaret Mead compares warfare to other human institutions like writing, marriage, trial by jury. From her work, it can be stipulated that war is a *social invention* that is *learned* in history and shapes history.

The work of Clausewitz suggests that the purpose of this invention is to conduct politics through the use of force when other means of conducting it fail. From this it can be stipulated that war is a *political instrument* of *force*. It is political in that it is more frequently (except for certain "primitive" wars) associated with political activity than any other human activity. It is an instrument in that those who wage war attempt, at some point, to use it as a calculated "rational" means to an end. It is force in that it is a means that attempts to compel opponents to do something they will not do freely.

While war is political, it is also clear from the work in anthropology and biology that war has a peculiar and unknown relation to territoriality. Wars are often fought to defend or expand territory, or to enhance the wealth and status of a territorial people. Even when such objectives are not readily found, wars are territorially based contests in that they are fought over and on territory with the victor controlling the people and land of the defeated. From this it can be stipulated that wars are *territorial* in spirit, regardless of their stated aims.

Above all, war consists of fighting and killing. It is one of the most salient features of war that killing, which is generally frowned upon, if not prohibited, within a group, is encouraged and honored in war. War involves violence, and acts of violence are usually associated with certain mental states, such as extreme *hostility* or frustration. In addition, the presence or anticipation of widespread violence produces *insecurity*. These emotions, perhaps acting in conjunction with deeper unconscious motives, make war a difficult thing to control and therefore reduce its rationality. This often leads "society" (in terms of its political leaders, military officers, intellectuals, and priests) to try to carefully control war in terms of when, how, and why it is fought. From this it can be stipulated that war is an institutionalization of *violence* and is associated with clearly identifiable mental states before, during, and after wars.

War may be seen as an institutionalization of violence in the sense that the act of violence, which is a natural individual tendency, is an act which can no longer be taken by an individual without the permission of the larger group. The larger group generally prohibits the act within the group, as best it can. At the same time, it attempts to marshal the psychic and physical energy needed to carry out such acts in a concentrated form and for its own purposes and control. Thus, war requires a certain level of social organization able to bend families, clans, and individuals to a more general will. Since war makes strangers (who do not by definition feel hostile or frustrated toward each other) kill each other, the institutionalization of violence requires the artificial insemination of the collective with the mental states associated with individual acts of violence. Religion and ideology (including foreign policy ideologies, like realism) become a way for the state to institutionalize violence and bend the will of individuals and clans to its own. The psychological effects of attempting to control and channel violent tendencies, to simultaneously legitimate and prohibit violent acts, are not known, but may play a role in the unexpected "post-war moods" that follow major conflagrations and the "war hysterias" that precede them. The notion of institutionalization suggests that war is more adequately conceived as process and action that comes forth (as a *verb*), rather than a thing or object (as a noun).

THEORETICAL ASSUMPTIONS ABOUT WAR

From this inchoate conceptualization of war, I make six theoretical assumptions: (1) war is learned; (2) war comes out of a long-term process; (3) war is a product of interaction and not simply systemic

conditions; (4) war is a way of making decisions; (5) war is multicausal; and (6) there are different types of war. These assumptions should not be taken as untested axioms from which I will logically or mathematically deduce propositions (cf. Waltz, 1979, Zinnes, 1976). What I have done instead is to derive insights from the above conceptual work and induce some empirical generalizations and propositions from existing research to develop a set of assumptions through which to view war. These assumptions guide my inquiry and thinking about war and allow me to view war from different angles, with each angle focusing on a different aspect of war. I then use these perspectives and assumptions to interpret and help explain existing evidence, thereby constructing an explanation of war and peace. This in turn will suggest new research and avenues of inquiry, which, when completed, will undoubtedly lead to a modification of some of the assumptions. I take this inductive approach to theory construction because I believe it is the most viable way of integrating the various pieces of evidence that have emerged in the last twenty-five years (cf. Guetzkow, 1950).

War is learned

War can be conceived as learned behavior in two senses. Groups learn to make war as a general practice that is available to them, and subsequently they learn in their dealings with others that war is an appropriate response to a particular situation. Taking Mead's perspective, the first thing we want to know is how and under what circumstances war was invented. What are the origins of war and how did clans, tribes, and societies learn to fight? What is it in a people's experience that makes them accept war as an institution and adopt beliefs that provide a rationalization for and legitimation of the use of force and violence? Anthropology and studies of animal behavior provide some suggestions for answers to these questions.

In general, it is fair to assume that people have probably learned to fight by fighting. From major wars they have developed a set of lessons that helps them identify situations that are best dealt with by going to war. On the bases of previous experience, lessons are derived that recommend certain practices as a successful (good, or proper) way for dealing with war-threatening situations. These lessons become embodied in the folklore and rituals of a society and become part of its customs. In this way, war is invented and reinvented; it becomes learned as part of a culture of behavior.

Once this invention has become part of a culture, it is still necessary to explain how and why a group comes to the conclusion that a war is

the best way to handle the situation facing it. Again, learning can be seen as playing a crucial role. Here a collectivity learns not how to invent a practice, but instead learns that, of the variety of existing and possible practices upon which it can draw, war is the most appropriate. If war is conceived as one of several foreign policy practices, then explaining war can, in part, be reduced to explaining why in general one foreign policy practice is chosen over another, and what in particular makes force and violence selected options.

It is assumed that any given political system, including a global political system, will have at least informal rules that influence, if not govern, what practices are appropriate to which problems. Historically, war, because of its costs and because of normative prohibitions, has never been the means of first choice, but has been resorted to only after other acceptable practices have failed. This suggests that war grows out of a sequence of interactions. Collectivities learn to go to war from the failure of "normal politics and diplomacy" to produce acceptable results.

It would be a mistake, as will be shown below, to think of this process as always culminating in a single calculated decision to go to war. Rather, what is more common is that different interactions evoke certain responses, and these responses become steps that lead one or both sides to an increasingly probable "decision" to go to war.

War results from a long-term process

While some wars, particularly between the very unequal, may emerge suddenly and from a single decision, more typically it appears that wars grow out of a long-term political relationship that has become increasing intractable, conflictive, and hostile. These three effects lead one side, and often both, to behave in certain ways and it is these behaviors (rather than a deliberative policy *per se*) that evoke a set of interrelated actions that result in war (cf. Singer, 1984: 6). From this it follows that the outbreak of war is usually not a sudden process, but evolves out of a series of steps.

This means that a Newtonian model of mechanics (in which one or more causes appear and then there is an effect) is not an appropriate model for explaining war. I assume that we will be misled if we confine our search solely to the "causes" of war and the "conditions" of peace. We must instead try to uncover the process by which war comes about; the process by which two or more states learn that the situation they are facing is best handled by going to war. Theodore Abel (1941: 853) affirmed something like this some time ago when he maintained that

war should be studied the way crime is, not by searching for the causes of crime, but by studying the process by which individuals become criminals. This is not to say (as apparently Abel would have said) that there are no correlates of crime, but only that such correlates may not be as revealing as the actual process by which individuals become criminals. To understand war, we may want to see if there is a "*pattern* according to which a war situation develops" and evolves (Abel, 1941: 853, italics in original). I assume that for many wars, especially wars between relative equals, a war situation grows out of a relationship between two political collectivities. This relationship has a history, present, and future that affects the nature of their interaction and the likelihood of war.

War is a product of interactions, not simply a result of systemic conditions

For this reason, I assume that a study of interactions (dyadic, triadic, and so forth) is going to be a more fruitful avenue of inquiry than the systemic analyses that have guided most inquiry in the past twenty-five years. What few dyadic-level studies we have support this claim (see Rummel 1972b; Vasquez and Mansbach, 1984: 415; Bueno de Mesquita and Lalman, 1988). I want to argue in favor of a position that sees war as the outcome of the foreign policy practices of states. Some of these practices set up a dynamic of interstate interactions that produces an external and domestic political situation in which decision makers and their policy influencers are so constrained that it becomes increasingly difficult for them *not* to decide in favor of war.

The external situation has to do with the political relationship that is generated by a particular sequence of interactions. The taking of some actions decreases the probability of certain kinds of actions while increasing the probability of others. War becomes likely when the sequence of diplomatic actions fails to resolve highly salient issues, resulting in an increase in the level of conflictive actions, which in turn increases psychological hostility. This produces the kind of relationship between two (or more) countries that is prone to a conflict spiral in which the intractability leads to the taking of more conflictive actions, which produces more hostility, which makes for an atmosphere that nourishes even greater intractability and conflict. In order to explain war, it is necessary to uncover the causal dynamics that make such a relationship emerge and to understand how such a relationship acts as a set of constraints that encourage the taking of foreign policy acts that are likely to result in war.

The domestic internal situation, i.e. the domestic political context in which leaders and policy influencers operate, is shaped not only by internal political battles and competition for power, but by the effects of external interaction, which make certain policies and actions more likely than others. The impact of interstate interactions on the domestic political context has been a greatly neglected area of research. In order to explain war, it is necessary to discover whether, and then delineate how, the actions of one's opponents create in one's own society a domestic environment that encourages the adoption of bellicose policies.

War may be seen as resulting, in part, from a series of interactions which produce a dual structure (a domestic political context and an ongoing political relationship which act as a set of constraints) that encourages leaders to take steps that lead to war. It is this short-term dual structure, which foreign policy interaction produces, that has been left out of so many behavioral analyses of war. The reason for this is that these sets of constraints must operate within the broader and more long-term system structure.

Of the various systemic constraints that have been seen as playing a role in the onset of war, I see the *global institutional context*, which can be defined as those sets of formal and informal institutions that allow the members of the system to make binding political decisions, as the most important. Other system characteristics are probably less important for explaining the onset of war than they are for explaining other characteristics of war. For example, the economic structure of the system (see Wallensteen, 1981: 80–83), I see as important primarily for affecting the goals of a war (slaves, markets), and hence coloring the issues over which war might be fought. The power structure or distribution of capability, I see as important primarily for determining who will fight, when they will fight, and the type of war that will be fought.

I believe the global institutional context to be the most significant systemic characteristic not only because I believe that this is consistent with existing evidence, but because I assume that war is fundamentally a political institution that serves crucial political functions. If those functions can be satisfied by other institutions, then there is a good chance that the frequency of war can be reduced, if not eliminated altogether, without changing the economic or power structure of the system. This means that war is neither the product of any given economic system, although economic concerns and greed can motivate wars, nor the result of a particular distribution of power, although relative capabilities might determine how certain types of wars come about. Such claims are not easily tested, but it is important to make

such assumptions explicit because whether one places emphasis on the political, economic, or power structure of a system will push research in very different directions. While these issues will be dealt with in a later chapter, here I will only give my reasons for emphasizing the political.

War is a way of making political decisions

What is most impressive about Clausewitz' analysis is the idea that war is a political phenomenon. Although war may grow out of psychological frustration and hostility, at the level of collectivities, these mental states themselves are the product of political stalemate – of the inability to resolve outstanding political questions. Thus, to understand war, one must first have some understanding of politics. To the extent that one focuses on or emphasizes the wrong things in one's conception of politics, then it can be expected that one will misunderstand war – why it has dominated the past and whether it will continue in the future.

I accept David Easton's (1965: 50) definition of politics as the "authoritative allocation of valued things." This conception is different from the dominant realist conceptions of world politics that define politics as "a struggle for power" (Morgenthau, 1960: 27) in that it sees politics as a process that is guided by an end and not as a process whereby the means (power) has become the only end. For Easton, or anyone who wants to seriously analyse politics within a stable relatively non-violent political system, the struggle for power is only half of the story. The other and in many ways the more important half of the story is what is done with that power once it is attained. For Easton, politics involves policy outcomes. Power is used to put certain issues on the agenda rather than others, and then to resolve those issues in one way rather than another. The outcome of this process is to take valued things and give (allocate) them to people or groups in a way that all those involved regard, at least in the short run, as legitimate and binding (authoritative).

In an earlier analysis (Mansbach and Vasquez 1981: 57–59, 68–69, 188–89) I adopted the Eastonian definition of politics to elaborate an alternative to the dominant realist paradigm of international politics. Instead of the almost exclusive focus on the struggle for power, world politics is seen in a broader perspective as the raising and resolving of issues. From this focus, actors become involved in politics in order to keep or obtain objects of value, which can be defined as *stakes*. Stakes are usually linked to form an issue, and it is contention over issues that

constitutes the substance and purpose of politics. By definition, political actors contend over issues by developing proposals for the disposition of specific stakes. Actors can then be said to agree or disagree with these proposals by taking an explicit or implicit *issue position* on each proposal.

An *issue* consists, by definition, of a set of differing proposals for the disposition of stakes among specific actors. Actors try to resolve an issue by changing each other's issue position, which is done through an interactive process of sending positive and/or negative acts. This process, however, instead of directly affecting the issue positions of an actor, determines actors' attitudes toward one another, i.e. their affect or level of friendship and hostility. From this perspective, the shape of political contention is seen as a function of three general factors: the characteristics of issues under contention, the dynamics of interaction, and the nature of the institutional context in which allocation decisions must be made.

In stable political systems, like the United States, the institution that authoritatively allocates valued things is government (Huntington, 1968). Since the current global political system lacks a government, we might ask what institutions authoritatively allocate valued things. When we do this, we find that there are numerous institutions, including some very formal ones, like the International Civil Aviation Organization (ICAO), that allocate very important things, like air traffic rules, or some informal ones, like the G-7 that plays a major role in decisions affecting the global political economy. We also find that, for certain pairs of states and in certain historical periods, war is a way of allocating some things that appear to have extremely high value, like territory, trading rights, and the form of domestic governments. This allocation can be seen as authoritative because only in rare instances does the loser of a war not regard it as the "right" of the winner to take what it had demanded during the war, even though it may regard this as unjust and reserves its own right to take it back after it recovers. War in world politics has had a certain legitimacy by custom, and this has been, at times, formally institutionalized by international law. It is the way by which independent groups resolve highly salient issues that do not appear to be resolvable by any other means.

In this sense, war serves as a kind of *allocation mechanism,* and a better sense of the political nature of war can be attained by examining other allocation mechanisms. An allocation mechanism may be defined as a set of formal and informal rules for making and implementing political decisions (see Mansbach and Vasquez, 1981: 283). An allocation

mechanism is a set of procedures, norms, and/or devices for answering such questions as: how shall we decide these issues? on the basis of what procedure or norm? Two or more actors could decide to resolve a question on the basis of flipping a coin, accepting the majority vote of a previously constituted body, binding arbitration, an auction, or arm wrestling. Each of these examples embodies specific allocation mechanisms.

There are numerous allocation mechanisms that could, in theory, be employed. This means that allocation or decision making always involves two choices: first, selection of one out of the range of allocation mechanisms as the final authoritative means of reaching a decision and, second, the actual disposition of stakes under contention. What distinguishes highly institutionalized and hierarchical political systems that have stable governments from looser ones, like the current global political system, is that in the former actors have little choice over which allocation mechanism will be selected to resolve a given issue, whereas in the latter, which allocation mechanism will be the final authoritative mechanism can itself become a matter of contention, even though there may be some rules governing which allocation mechanisms must be tried first. In loosely organized political systems, allocation mechanisms that permit the unilateral resolution of an issue tend to evolve as the ultimate mechanism, because through practice they are the only ones able to make a final, authoritative (non-challengeable in the short term) and hence binding decision. These mechanisms become customary, legitimate, and even legal in due time. In the economic realm, such unilateral mechanisms have involved the exercise of wealth. In the political realm, such unilateral mechanisms have involved the exercise of force.

Despite the wide variety of specific means that can be used to make a decision, allocation mechanisms can be reduced to one of four basic types – force, bargains, votes, and games of principle – each of which is distinguished by the criterion that is used to make a decision (Mansbach and Vasquez 1981: 283; see also Young 1978). *Force* resolves issues on the basis of the strength of contending actors. *Bargains* aim to dispose of stakes through some sort of exchange or trade among the contending actors. *Votes* employ a consent mechanism (that specifies who may participate, whether votes will be weighted, and how the votes will be counted) to dispose of stakes. *Games of principle* resolves issues on the basis of some norm. Such norms can involve basic principles, like equality or equity (distributive justice), or more complicated normative systems, like utilitarianism, the Bible, Marxism–Leninism, or liberal capitalism (see Mansbach and Vasquez, 1981: 283–85).

This suggests that war as an institution is a form of force that has evolved within the global political system as a unilateral allocation mechanism that can make authoritative decisions. War is not simply an act of violence, but an allocation mechanism which is resorted to in the face of stalemate and the failure of normal politics to resolve fundamental issues. Since political systems that do not have functioning governments are more prone to failure, war is often seen as *a way of making authoritative decisions in the absence of government*. A more comprehensive view is to see war as emerging from a breakdown of normal politics, whether that normal politics be government, traditional diplomacy, or a negotiated regime.[11]

War as a multicausal phenomenon

Since normal politics can break down for a variety of reasons, this makes it clear that there are a number of different paths that can lead to war. In a more fundamental sense, war may be caused by other factors than just the breakdown of normal politics. Our review of the different conceptions of war revealed that there are many aspects of war, and its use as a political instrument is only one. The different aspects of war indicate that war can be fought for different reasons and stem from different motivations, and therefore is probably multicausal. For these reasons, I assume that there are a variety of factors and causes that can bring about war. This means that the hope that there are a few necessary conditions that must always be present in order for war to occur is probably not going to be fulfilled. Instead, the variety of factors associated with war and the changing nature of war through history suggests that war can be brought about by any number of sufficient conditions.

This raises a general question about causality and how we should think about the causes of war. War can be brought about by several distinct causal paths or causal sequences (see Levy, 1989: 227, 279, 281). Relations between states that follow one of these paths and are unable to take any of the exits will end in war. I assume that at this stage in the systematic study of war and peace, one of the most useful intellectual enterprises that can advance our knowledge is the delineation and documentation of such paths to war. In this book, I hope to uncover one causal sequence to war – that associated with following the foreign policy practices of power politics. I believe the sequence I have delineated is the main process by which *major* states become involved in wars with *each other* in the modern global system. In this sense, this book looks at one causal sequence to a certain type of war that is

produced in a particular historical era by following the practices of power politics. While that is a theoretically very limited set, let me hasten to add that within that set are the most important wars of the last 500 years.

At this point, our knowledge is not precise enough to distinguish clearly between correlates and causes, so I will be involved primarily in trying to document through an examination of quantitative research findings the overall pattern by which such war situations develop and intensify. Although I believe the delineated pattern will include the causal sequence, it inevitably will include a number of other correlates that future research may show to be of little causal significance or even epiphenomenal. Nevertheless, I cannot be more precise than the current research permits. I have sought to err in the direction of complexity and inclusion rather than exclusion, since I hope this analysis will spur research that will identify which factors in the path to war are the most potent.

The assumption of multicausality, or equifinality as it is technically called, means that, even if I am partially successful, one will still be able to point to wars that occur even in the absence of the identified causal sequence. All this means is that the causal sequence specified here did not produce that particular war. It does not mean that the analysis is inaccurate, because all a sufficient condition requires is that when it is present there is a high probability that war will follow; it does not require that this condition be present prior to every known war. This means that trying to delineate the causes of war by only studying wars in the hopes of finding a common pattern will not be successful. As will be seen later, alliances and arms races do not precede most wars, but this does not mean that they may not play a significant role in a particular causal sequence. The latter conclusion can only be made by identifying the role alliances and arms races play in a causal sequence and then seeing if every time that causal sequence unfolds the probability of war greatly increases. In terms of policy relevance, this means that even if some causal sequences of war are identified and eliminated, war may still be caused by other factors. This makes the task of bringing about permanent peace arduous and complicated, something which should come as no surprise.

If there appear to be many causes of a given phenomenon, then it may be the case that the phenomenon under question is not a single entity but a variety of phenomena that must be distinguished before any real progress will be made. This has certainly been the case in cancer research. Only as different diseases and conditions, like leukemia, lung cancer, breast cancer, etc. were distinguished, was any

progress made in identifying their causes and developing cures. This has led me to make a sixth assumption – that there are different types of war, each with different causes. I shall be confining my analysis, including the foregoing assumptions, to only certain types of war. However, since the identification of different types of war involves the construction of a theoretical typology, this last assumption requires a chapter of its own.

2 TYPES OF WAR

> The first step in scientific discovery is classification, but how to classify correctly?

If war is a single phenomenon that has an identifiable set of causes, then there should not be any need for a typology of war. Wars, regardless of time or place, should be associated with the same set of variables, give or take a few qualifications to account for idiosyncratic factors. On the other hand, if war is an amorphous concept that includes seemingly related but in fact very different phenomena, then there will be no clear pattern of correlates associated with war. How can one tell whether war is better conceived as a single phenomenon or several? And, if one thinks war involves several phenomena, how can one disentangle the phenomena so that the factors associated with each are elucidated?

Questions of this sort are standard in the philosophy of science, but to answer them productively requires that we become good scientific detectives – developing hypotheses in light of incomplete evidence, and then using those hypotheses to uncover new clues that will either confirm or falsify our hunches. We should think of war as several phenomena, if extensive research that conceptualizes war as a single phenomenon seems to lead nowhere. The failure of research is often a sign that conceptualization is fundamentally flawed – illuminating the unimportant and obfuscating the significant. An even more significant piece of evidence that a concept may be combining several phenomena is provided when different pieces of research inadvertently produce contradictory results – finding that sometimes war is associated positively with one set of variables and at another time negatively. Such anomalies are important "breaks," since they provide clues about how to disentangle the phenomena so as to capture the theoretical elements that will distinguish the different types.

Although even a cursory review of the historical record suggests that wars like the Second Schleswig-Holstein War, the Crimean War,

and World War I are quite different, most political scientists have held on to the belief that the underlying causes of war have not changed since the Peloponnesian War. This is a widely shared view, maintained not only by quantitatively oriented scholars, like Singer, but also by traditionalists, like Morgenthau (1960), Waltz (1979), and Gilpin (1981). Yet empirical research has produced findings, including the uncovering of one crucial anomaly, that undercut important aspects of this belief and support the more commonsense notion that wars differ.

I will begin this chapter by reviewing the evidence that led me to conclude that there are different kinds of wars and that a theoretical typology of wars is necessary. Then, after a brief look at the few efforts to differentiate war, I will present a formal typology and discuss why I believe that the three theoretical dimensions I have selected for constructing the typology are the most significant aspects distinguishing wars. I will conclude the chapter by indicating which kinds of wars will be the focus of this inquiry.

IS THERE A NEED FOR A TYPOLOGY OF WAR?

The most direct way to see if it makes sense to differentiate wars is to empirically examine whether different wars have different causes and effects (see the symposium in Midlarsky, 1990). There is not much evidence of this sort, because few have attempted to classify wars on the basis of some theoretical characteristic. The major exception has been Rasler and Thompson (1985a, 1985b), who separate out "global wars" from all other interstate wars. They argue, on the basis of Modelski's (1978) analysis, that not all wars are the same, that a global war – a war fought among major states for the leadership of the system – is different, because of the purpose of the war and its tremendous impact on the global political system. A global war is also likely to be quite severe in terms of casualties. For these reasons, they and Modelski (1978) argue that global war is a unique type and should be differentiated from other interstate wars (see also Thompson 1985; 1990).

Rasler and Thompson's (1983, 1985a, 1985b, 1989) research to date provides some support for their position. They find that global wars produce a long-term increase in governmental expenditures and in the tax burden, whereas other interstate wars involving major states have less of an impact (Rasler and Thompson 1985b; 1989: ch. 5). In addition, they find that while both global and interstate wars permanently increase the level of public debt in Great Britain and the United States, interstate wars do so to a much lesser extent (Rasler and Thompson,

1983: 506–12; 1989: 104–14). Finally, they demonstrate that global wars have an abrupt but short impact on the GNP of major states, but that other interstate wars do not have any effect on the economic growth of major states (Rasler and Thompson, 1985a; 1989: ch. 6).

This evidence lends support to their notion that global war – as a type of war – has played a major role in the growth and development of the state (Rasler and Thompson, 1985b: 491, 505; 1989: ch. 8; see also Hintze, 1902, 1906). However, since interstate wars sometimes can have the same impact (albeit of lesser magnitude) as global wars (Rasler and Thompson 1985b: 500, 503), this suggests that differences between global wars and other interstate wars might be one of degree rather than kind. This view is further supported by the fact that the US Civil War had the same kind of impact on expenditures and taxation as global wars (Rasler and Thompson, 1985b: 505 n. 16; 1989, 226, n. 17). Also, it must be pointed out that specific global and interstate wars in specific states do not always produce consistent results (Rasler and Thompson, 1985a: 530, 533; 1989: 172, 174, 192, 198, 202; Rasler, 1986: 930–31, 934). This suggests that a more theoretical classification of wars might be able to produce more consistent findings across global, interstate, and civil wars. For example, Rasler and Thompson's findings imply that classifying wars on the basis of the effort put into fighting the war and its subsequent severity may account for any war's domestic and global impact.

While Rasler and Thompson produce a body of evidence that suggests the need for a typology of wars, they provide only limited assistance in telling us on what basis to construct that typology. Neither Modelski nor Rasler and Thompson develop a full typology of wars that would tell us not only on what basis global wars are different, but also how the vast majority of wars can be categorized. In this regard, it is significant that Rasler and Thompson have provided some evidence to show that global wars have a different and greater impact than other wars, but that they have not shown that these wars have fundamentally different causes from other wars that involve major states.

Greater assistance and a more compelling reason for constructing a typology is provided by Singer, Bremer, and Stuckey's (1972) seminal study of the balance of power versus preponderance of power debate. In one of the first systematic comparative analyses of that debate, they uncover a major anomaly that can be used to support the notion that there are different types of wars. A detailed examination of this study provides important clues as to how wars might be classified.

Singer, Bremer, and Stuckey (1972) find that (among major states) in the nineteenth century, less war occurs when capability in the system

is dispersed, which is consistent with a parity or balance of power notion of peace; but that, in the twentieth century, less war occurs when capability in the system is concentrated, which is consistent with a preponderance of power notion. This finding is a major anomaly not only because it is unexpected, but also because the logic of realist theory can not account for it. There is no clear reason why a balance of power should produce peace in the nineteenth century, but war in the twentieth! A scientific explanation that captures the causes of war should be fairly generalizable and not shift so radically from one century to the next, particularly since the historical causes of World War I are often seen as originating in the late nineteenth century and not at the turn of the century.

Singer *et al.* (1972: 46–47) try to account for the irenic effect of relative equality in one century and the violent effect in the next by arguing that a fundamental change in the conduct of diplomacy occurred around the turn of the century. According to them, this change was brought about by the decline of aristocratic rules of the game and the democratization of diplomacy. The latter made domestic politics intrude more in the twentieth century and therefore made world politics more uncertain. According to Singer *et al.*, for both the preponderance and the balance models, uncertainty[1] is a factor in the onset of war. The difference between the nineteenth and twentieth centuries is that, in the nineteenth, a balance of power provides sufficient certainty for an international elite to know that war will not be profitable, but in the twentieth, only a preponderance of power will convince domestic policy influencers that a war can not be won.

There are several problems with this *ad hoc* explanation. There is little empirical evidence that the intrusion of domestic politics in democracies makes them less effective in their conduct of foreign policy (see Waltz 1967) or that they participate in war more (or less) frequently than other kinds of states (Small and Singer, 1976; Chan, 1984; Maoz and Abdolali, 1989). Even if they did, there is no reason to expect and little evidence to suggest that such an impact should be felt at the turn of the century. Certainly, there was little change in French or British democracy at the turn of the century, and the intrusion of domestic concerns in German foreign policy was not that different under Bismarck in comparison with Bethmann-Hollweg and Wilhelm II. Indeed, Mayer (1981) argues that the *ancien régime* persisted into the twentieth century, a hypothesis which, if correct, would undercut the very foundation of the *ad hoc* explanation.

More damaging is that additional empirical work has failed to sustain Singer *et al.*'s (1972) findings. Bruce Bueno de Mesquita (1981a)

was able to eliminate the intercentury difference. He does this by changing the dependent variable from nation-months of war, which is what Singer *et al.* employ, to the dichotomous war/no war. He argues (following Duvall, 1976) that the latter is a better measure of the onset of war and the presence of peace. Indeed, this is the case because the dichotomous variable simply describes whether, in any given period, there is war or no war; whereas nation-months measures the *amount* of war and thereby could erroneously lead to the inference that few nation-months of war was equivalent to peace. Singer *et al.*'s (1972) data analysis really tells us more about what produces wars of different sizes than it does about what is associated with the onset of war.

When Bueno de Mesquita (1981a: 564) examines the more relevant war/no war dependent variable, he finds that there is no statistically significant relationship between differences of capability (either between nations or coalitions) and the onset of war.[2] Bueno de Mesquita also elaborates the research design by examining coalitions, as well as individual major states, and finds that aggregating individual capability scores into coalitions, as one should do to properly capture balance of power thinking, does not improve the results.

Thompson (1983a: 153–54) is also able to eliminate the intercentury difference, but in his case by making changes in the measures of the independent (capability) variable. Thompson (1983a: 153–54) reduces all correlations, particularly the impressive 0.81 (for the nineteenth century), which is reduced to -0.38. When looking at only global wars, the correlation is reduced to -0.30. Both the Thompson and Bueno de Mesquita studies suggest that there is no difference between the nineteenth and twentieth centuries *with regard to* the role of capability concentration and the onset of war.

These findings should come as no surprise, since the logical contradictions between the balance of power and preponderance models make it likely that neither would prove to be correct. Balance of power thinking, like all realist approaches, sees world politics as a constant struggle for power. If one state succeeds in gaining a significant degree of power over another, then it will be in a position to successfully attack that other state in order to get what it wants. The only way the state being threatened can prevent the attack is by giving in, or by securing allies that would restore a balance of power. Although balance of power logic is clearest at the dyadic or interaction level, it has been applied to the systemic level, which is the level at which Singer *et al.* (1972) test it. At this level, it is assumed that a *system* of states where power is relatively dispersed and alliances are

flexible so that a balance can be maintained would be more peaceful and should protect the independence of states.

It seems that a balance of power will prevent the temptation of gaining an easy victory through coercive threats or wars of conquest. However, there is no rational reason why a mere balance of power or relative equality should *prevent* one side from attacking another. While a balance may mean no easy victory, it also means that both sides have at least a 50–50 chance of winning. Thus, as has been pointed out many times, real security lies not in a balance of power, but in a preponderance of power (Organski, 1958: 325). Only if the other side *knows* it will lose, will it be discouraged from attacking; but what then prevents the preponderant side from attacking? Sometimes the weak can save themselves by making diplomatic concessions, but the history of imperial wars shows that this is not always the case.

The difficulty in making either a balance or preponderance of power preserve the peace can be illustrated further by discussing the role of uncertainty in the decision-making process. The traditional rationale for the balance of power proposition is that states will not initiate a war unless they are pretty sure of winning it. In social science language, it is assumed that uncertainty makes states conservative and that leaders are unwilling to take risks. If you are not sure of winning, you will not attack. Since a balance of power increases uncertainty, it should decrease war. From this perspective, uncertainty produced by a relative equality of capability is seen as producing peace. According to Singer *et al.*'s (1972: 23–24) summary of the traditional wisdom, the parity (balance of power) model assumes that certainty is at the root of war, and they note that the major inhibitor to war is the lack of clarity in the power and alliance structure so that the outcome of a war is in doubt.

Conversely, the preponderance model assumes that uncertainty is at the root of war – if there is any chance you will win, then you will consider war an option. Only when the defender has a *preponderance* of power can a state be secure, for no one would initiate a war they were sure to lose. From the preponderance model perspective, Singer *et al.* (1972: 23) argue, uncertainty and a balance are associated with war because the lack of structural clarity and the ambiguity of alliance ties make for misjudgments and poor predictions.

Singer *et al.* assumed the contradiction would be resolved empirically (i.e. either certainty *or* uncertainty would be correlated with war). An alternate explanation is that the receptivity of leaders to one of these two logics may be correlated with their willingness to accept risks and tolerate ambiguity, so that any given capability distribution

at either the system or dyadic level will either prevent or encourage war depending on the characteristics of the respective leadership.

Bueno de Mesquita (1981a) uses the concept of risk to build a deductive argument against the traditional wisdom that is even more persuasive than his findings. He maintains that any given configuration of power (balance or preponderance) can produce either war or peace depending on whether leaders are risk-acceptant or risk-averse. Thus, Bueno de Mesquita (1981a: 546) concludes that any individual leader's potential for initiating war will depend on his or her particular risk-taking orientation rather than any distribution of power.

Although Bueno de Mesquita uses his analysis to move toward an expected utility model, the following criticisms of realist logic can be derived from his basic insight. It can be assumed on the basis of his argument that risk-averse leaders will be prevented from initiating war when there is a balance of power, whereas those that are risk-acceptant will only be prevented from considering war when the other side has a preponderance of power. Since it is unlikely that the leaders of major states will always have the same predisposition toward risk-taking, the success of any given configuration of power in preventing war is going to vary. Sometimes a balance of power will prevent war and sometimes it will not.

Whether it does or not will probably depend on a variety of personal characteristics, not just the disposition of leaders to take risks. Other personal traits, such as tolerance of ambiguity, cognitive complexity, rigidity, open–close mindedness, hard-line/soft-line tendencies, and dominance may play a role (see Guetzkow and Valadez, 1981a, 1981b; M. Hermann, 1980; and Etheredge, 1978). Such (utility) calculations are further complicated by the varying domestic political contexts of the contending states. Hard-liners and political fanatics, who are willing to assume greater costs in a war because they expect greater benefits than accommodationists and pragmatists, will be less likely to be discouraged by a balance of power. A domestic political environment dominated by hard-liners is apt to produce decisions and leaders that are less constrained by a balance of power than a domestic environment dominated by accommodationists or isolationists. Thus, the effectiveness of a given balance of power will change as leaders and domestic political environments change. This argument shows that there is no logical reason to expect either the balance of power or a preponderance of power to always prevent war.

How can these conclusions be resolved with Singer *et al.*'s (1972) findings? The primary difference between their analysis and that of Bueno de Mesquita is in the measurement of war. Since they examine

nation-months of war and not the presence or absence of war, their findings are not as germane to the question of peace as they are to the magnitude of war. Bueno de Mesquita's (1981a) analysis must be given greater weight on this question, because the balance of power and preponderance models are primarily analyses of how to maintain peace and avoid war. Although Singer *et al.* (1972) do not tell us anything about the onset of war, they have uncovered something significant about the role of capability and the magnitude of war. In accounting for their findings, they even say that the two centuries "are experiencing different types of war . . . " (Singer *et al.* 1972: 48), but they do not elaborate upon this in their *ad hoc* explanation. This suggests that *the distribution of capability will determine what form war will take, and not whether it will occur.* Clearly, since war is relatively rare and most dyads have either a balance or preponderance of power, there have to be other factors that cause war. If we look at how capability differentials affect the form war will take, then we have an empirical basis for constructing a typology of war, especially if it is discovered that the wars of the nineteenth century, which Singer *et al.* find associated with a preponderance of power, are fundamentally different from the wars of the twentieth century, which Singer *et al.* find associated with a balance of power.

If one "unpacks" Singer *et al.*'s (1972) correlations by looking at the cases that make up each century, then it becomes clear that the wars among major states in the nineteenth century are, in fact, very different from the wars among major states in the twentieth, and that this difference is accentuated by employing nation-months as the dependent variable rather than a dichotomous war/no war variable. Since the Correlates of War data set does not include the Napoleonic wars, their data on the nineteenth century lacks a "great power" general war (Levy, 1983: 52, 74–75). It is the absence of a general war, such as World War I and II, that is the primary reason that the period from 1815 to 1914 has been called the century of peace. Because there are no all-out world wars, wars involving major states in this period were considerably less destructive than in the twentieth century. Indeed, Levy (1983: 143) shows that the nineteenth century after 1815 is the least warlike period of the last five centuries.

Nevertheless, there are a number of wars involving major states in the nineteenth century data set, and based on the reasons for fighting, these can be grouped as: (1) state building wars, particularly those associated with the formation of Germany and Italy, and with the expansion of national territory at the expense of the Ottoman Empire; (2) imperial wars by the major states against considerably weaker

states or non-national entities outside the central system; and (3) limited wars between major states such as the Franco-Prussian War or the Crimean War. What these nineteenth-century wars have in common is that most of them tend to be fought between actors who are relatively unequal in power.[3] In other words, as a sample of cases, 1816–99 tends to consist of wars one would expect to be fought when one side has a clear advantage over the other. Conversely, in the twentieth century, the two world wars are the kind of wars that would be expected only when the two sides are relatively evenly matched. Because World Wars I and II (along with the Korean war) dominate the nation-months of war in the twentieth century, Singer *et al.* get a correlation associating relative equality with high war and preponderance with low war.[4] Neither of these findings means that a preponderance actually produced peace in the twentieth century or that a balance produced peace in the nineteenth century. Rather, it seems that a complex of factors, including the distribution of capability at both the dyadic and systemic levels, encouraged certain kinds of wars from 1816 to 1913 and other kinds from 1914 to 1945.

A closer inspection of the types of wars in each period suggests not only that the two periods are experiencing different forms of warfare, but that the wars of one period may have had an impact on the kinds of wars that were fought in the second period. The imperial wars of the nineteenth century along with the state-building wars of Germany and Italy and military successes of Japan eventually produced effects that led to a major power transition in the twentieth century and a set of diplomatic tensions that status quo states were unable to resolve in a manner that would assimilate new powerful states into the existing world order (see Kennedy, 1987: 203–4, 206–10; 212–15; Doran, 1989: 393; Doran, 1991). The ("small") wars of the nineteenth century created both the (relative) capabilities and the issues that were the foundation of the large wars of the twentieth century. It is not that the nineteenth century was different from the twentieth century; it is that the nineteenth century produced the twentieth century!

This analysis, along with the evidence marshaled by Rasler and Thompson (1985a, 1985b, 1989), supports the idea that wars should be differentiated and that constructing a typology of wars would be conceptually useful in explaining findings. The interpretation of the anomaly uncovered by Singer *et al.* suggests that capability differentials may be the most important theoretical dimension upon which to construct a typology.

A TYPOLOGY OF WAR

Previous efforts

Two different approaches may be taken to the construction of typologies. One is nomothetic and behavioral and strives to explain war regardless of time and place. The other is an approach that sees the specific time and place in which war occurs (i.e. the historical conditions) as the most critical factor in explaining war. A review of both approaches provides important insights and criticisms that must be taken into account before attempting to construct any new typology of wars.

One of the earliest theoretical attempts to classify wars is that of Lewis F. Richardson (1960b: 247–48), who differentiates wars according to the number of participants on each side. Richardson showed that the typical war was a dyadic war (one actor on each side) and that multiparty wars were fairly rare. Richardson was particularly interested in constructing a model that would explain the difference between these dyadic wars and more complex wars, like the world wars. His analysis is relevant because he makes it clear that the sheer number of participants, regardless of their power, can be an important factor in distinguishing different types of war.

Most behavioral approaches tend to base their typologies on the nature of the participants, especially their political power, rather than the number of participants. Typical of this approach is that of the Correlates of War project, which distinguishes between interstate and extra-systemic wars. The former must have at least one member of the international system (i.e. a nation-state) on each side, whereas the latter need have only one nation-state as a participant, which allows for the inclusion of imperial and colonial wars. This focus on nation-states stems from the realist belief that, in the modern system, nation-states are the most powerful and thus the most important actors (see Vasquez, 1983a: 134–36, 139–40). Small and Singer (1982: 46–52) make this emphasis more explicit by further breaking down interstate wars into central system wars and major power wars, depending on the political status of the belligerents. Similarly, Levy (1983: 51–52; 1990) classifies wars according to whether a great power participated on one side (wars involving great powers), both sides (great power wars), or involved most of the great powers (general wars).[5] Midlarsky (1986a, 1988) follows in this tradition by separating out "systemic" wars (e.g. World War I and World War II) as a special focus for his inquiry in order to identify the unique conditions associated with their occur-

rence (Midlarsky, 1986a: 109). Siverson and Sullivan (1983: 477) point out that critical aspects of balance of power theory apply only to wars that have major states on each side and not to other wars, even if they involve major states, which implies that wars with major states on each side are special in both their causes and dynamics.

Because these classifications have been constructed more for convenience and for eliminating wars than for theoretical comparison, they are of only limited aid in constructing a typology based on the capability differentials of the belligerents. More significantly, actor-based classifications can be criticized for assuming that war can be understood by looking at the power or foreign policy of a single state, rather than by looking at interactions and relationships between political actors. Research on foreign policy behavior suggests that an interaction and dyadic approach is more productive than a single-actor (or monad) approach (see Kegley and Skinner, 1976: 308–11; see also Singer, 1982: 37–38). Therefore, classifying wars on the basis of the distribution of capability across belligerents rather than the political status of the individual participants may improve analysis. One attempt to move in this direction is that of Stoll (1980), who distinguishes between *opportunity wars*, those arising from serious disputes between unequals, and *threat wars*, those arising from serious disputes between equals (see also Siverson and Tennefoss, 1982). This is an important insight that is consistent with the interpretation given of Singer *et al.*'s (1972) anomalous findings; and it will be employed later in constructing a typology of war.

The review of nomothetic approaches suggests two dimensions on which wars might be classified. The first, based on Richardson, is to distinguish dyadic wars from those involving three or more parties. The second, based on criticism of the monad approach, is to distinguish wars according to the relative capability of the opponents.

A major criticism of nomothetic approaches is that they are ahistorical. Like most positivist efforts, they can be faulted for assuming that wars, regardless of previous history, are more or less caused by the same set of variables. Marxists or students of history like Organski (1958) and Modelski (1978) would want a typology of wars that would recognize that the type of war a political unit could initiate would vary greatly depending on its own and the system's economic and historical development (see Thompson, 1983c). From this perspective, the purpose of the initiator determines the type of war that will be fought, and a search for the causes of war would start with the factors that made the initiator have that purpose in the first place and try to attain it through force of arms in the second place. Indeed, for some (e.g.

Organski and Kugler, 1980; Gilpin, 1981; Modelski, 1978), the historical perspective makes them see war as natural and periodic; unlike Richardson and other more behaviorally oriented peace scientists, who try to identify the conditions of war and of peace so that the latter may be encouraged and the former prevented. Consequently, the more historically oriented investigate the causes of war by trying to identify *when* war will occur, rather than asking *whether* it will occur.

A focus on *when* wars will occur tends to lead inevitably to explaining and differentiating wars on the basis of the reasons given for fighting them. Wars tend to be classified according to their goals, and, in the more theoretical analyses, an emphasis is placed on the historical conditions associated with these goals and the historical and economic prerequisites for the initiation of war (cf. Howard, 1976). By paying more attention to such questions, we may better understand how the policies that precede these wars produce them (see Gantzel 1981: 44). Eventually, this may make it possible to classify wars according to the policies that led to war, e.g. imperial wars, hegemonic wars. In the absence of such knowledge, classifications that have emerged from this approach have been very substantive, sometimes focusing on policy, sometimes on goals, and sometimes on the participants. Quincy Wright (1965: 638–40, Appendix XX), for example, classifies wars according to whether they are balance of power, defensive, imperial, or civil wars. Lider (1977: 195, cited in Gantzel, 1981: 48) distinguishes among wars fought between superpowers, among imperialists, between imperialists and liberation movements, and among third world states.

Some scholars wish to concentrate only on the wars that are the most important turning points in history, which they see as a special type. Modelski (1978) and Thompson (1985) view the wars that resolve the question of who will exercise leadership over the global political system, what they call *global wars*, in this manner. Those who emphasize the traditional realist struggle for power tend to speak of these wars as *hegemonic wars* (Farrar, 1977; Gilpin, 1981: 199–203; see also Toynbee, 1954: 234–87, 322–23 and Dehio, 1962, both cited in Levy 1985b: 346) in which dominant powers and challengers fight it out. Organski and Kugler (1980: 42–47) see many of these wars as involving power transitions (see also Organski, 1958: 325–38; Doran and Parsons, 1980). In response to this discussion, Levy argues that his own category of *general war* provides the least problematic way of identifying these sorts of wars and has offered a more precise conceptualization and operationalization of his original concept (cf. Levy, 1983: 51–52 and Levy, 1985b: 364–65). While there is disagreement among these schol-

ars over exactly which wars are the most important and why they are important, there is an emerging consensus that, of all the wars fought, only a few are influential in determining the historical structure of the world system (cf. Thompson 1985; Levy, 1985b: 372–73, Table 1; Thompson, 1988: 105, Table 5.4).

While a truly adequate substantive classification of wars must await the outcome of more theoretically oriented historical research, like that currently being done on the world system (e.g. Braudel 1966; Wallerstein 1974, 1980; Thompson 1988), it is possible to distinguish certain wars (e.g. world wars and imperial wars) that appear to be unique and historically important in their own way, and therefore worthy of studying as a type. Because the fights that lead to world wars appear sufficiently distinct to serve as a basis of differentiation and their historical impact is sufficiently great, they will be singled out in Chapter 7 for special investigation.

Big wars among major states have been the subject of considerable attention (see Midlarsky, 1990; Levy, 1990). What these wars should be called is a matter of disagreement. Some of the labels (like hegemonic wars or global wars) involve taking a theoretical stance about the causes of these wars. While it is clear that these wars have a set of causes that differentiate them from other wars, it is not clear that the specific causes (or factors) identified, respectively, by Gilpin (1981) (see also Farrar, 1977), Modelski (1978), or Organski and Kugler (1980) are the true causes or the only factors associated with these wars. This makes a more theoretically neutral label, like that of *world war*, more appropriate. Since Levy's (1983) label *general war* is not as obvious a term as *world war*, it will not be employed; however, his operational definition of these wars can provide a way of comparing and perhaps testing the different interpretations and will be incorporated in this analysis.

A *world war* may be defined as a large-scale severe war among major states that involves the leading states at some point in the war (see Levy, 1985b: 365) and most other major states in a struggle to resolve the most fundamental issues on the global political agenda (see Mansbach and Vasquez, 1981: 110–12). This definition places emphasis on who, how, and over what the war is fought, rather than on the foreign policy that led to the war, the reasons for which different parties entered the war, or the consequences of the war. The latter focus, particularly the notion that such wars could transform the structure of the system and produce a new hegemonic leader is *not* incorporated, even though Levy (1985b: 364) is willing to pay some deference to it in his conceptual definition (as opposed to his operationalization).

Instead of making such claims defining criteria, they will be treated as possible empirical outcomes of a war. I make this choice not only because it is methodologically less cumbersome, but also because I do not believe that such wars are necessarily and primarily caused by hegemonic or leadership struggles. I believe there are other factors that cause these wars and that are responsible for their scope and severity. Even though many of the wars of this type have the impact analysts see, some scholars have a tendency to discuss the causes of these wars in terms of their consequences, a logically invalid procedure.

Identifying wars of a special type, however, is still a far cry from a typology of war. Substantive classifications have not been able to serve as a complete typology of war for two reasons. First, most substantive typologies do not even pretend to be logically exhaustive (i.e. there may be a host of wars which do not easily fit any type). Second, historical classifications have to date been insufficiently theoretical. They do not identify the characteristics that are most significant for determining why the wars have different causes; they merely state that the differing goals and policies of actors (e.g. those involved in civil, imperial, and world wars) lead them to fight different kinds of wars. To overcome these problems, one must take a more nomothetic approach, but this raises the danger of creating a general typology that is ahistorical and ignores the possible changing reasons for and causes of war. Since this work is concentrating on explaining wars in the modern global system since 1495, with special emphasis on wars since 1815, that danger should be minimized.

Types of war

To construct a nomothetic typology, it is necessary to identify the theoretical characteristics of war that will most likely differentiate wars according to their causes and thereby help analysts uncover those causes. The typology offered here is based on three separate theoretical dimensions. The first and most important is the distribution of capability between the belligerents. Since the previous analysis of Singer *et al.* (1972) suggested that the distribution of capability is probably more related to the type of war than to the onset of wars, wars between equals should be separated from wars between unequals.

Wars between equals, I call *wars of rivalry*, and wars between unequals, I call *wars of inequality*. It is assumed that wars between equals are more oriented to the logic of the balance of power and prey

to its deficiencies – such as mutual fear, suspicion and insecurity; arms races; and the temptation of preventive war. Wars between unequals lack all of these characteristics. These wars are more oriented to either the logic of preponderance of power and of opportunity or to the logic of revolution, depending on who is the initiator. Unlike wars between equals, where both sides tend to employ the same logic because they perceive the situation from a similar foundation of capability, wars between unequals involve two parties who employ different logics and initiate wars for different reasons – the strong seeking dominance and the weak liberation.

I call wars between equals *wars of rivalry* because one of their most distinguishing characteristics is that they are usually preceded by long-term rivalries, whereas wars between unequals are not. The presence of a long-term rivalry makes the pre-war relationship more prone to mutual hostility, while the relative capability results in mutual frustration. This makes both sides willing to hurt and harm the other side as an end in itself, often with minimal regard to the costs. Conversely, wars of inequality are more likely to be governed by rationalistic cost-benefit considerations, especially when viewed from the perspective of the initiator. Here decisions by the stronger to initiate war are more apt to follow the kind of cool, calm, and collected calculations described by expected-utility theory (see Bueno de Mesquita, 1981b). The opportunity for victory is an important prerequisite for going to war. Emotions that do enter tend to be confined to the weak who react to domination with resentment (M. Singer, 1972) and rage (Fanon, 1968), and revolt out of an inability to continue to tolerate the situation (see Oglesby, 1967). Nevertheless, if rebellion is to become successful revolution and liberation, then the war must be initiated at an opportune moment – when the normally stronger is distracted, conquered by another party, or weaker than before (see Waterman, 1981). Since inequality is responsible for the use of rational calculation, the rage of the weak, and the way the war is fought, I call these wars, *wars of inequality*.

On the whole, wars of inequality are more susceptible to cost-benefit analysis because it is clear that differences in capability will have a major impact on the outcome of the war. Conversely, in wars of rivalry, the relative equality of capability makes it less clear that the difference in capability will make a difference, thereby making the outcome appear to turn on other factors – resolve, diplomacy, military tactics, courage and will, staying power, morale, luck, support from allies.[6] Because differences in capability affect the logics that will be employed by the participants, as well as their emotions, it can be

expected that the foreign policy interactions and practices preceding the two types of war will be different and that the wars will not only be fought for varying reasons but will probably be caused by different factors.

In order to differentiate wars on the basis of the relative equality of the belligerents, it is best to measure differences in capability as a dichotomous variable that would clearly demarcate dyads according to their relative capability. *Capability* may be defined as the resource base an actor can utilize to attain certain ends. It is assumed that over the long run, capability that is employed will lead to political successes or failures that give an actor a *reputation* and *status* in a global hierarchy.[7] Resource capability for individual major states has been adequately measured in the Correlates of War project in terms of its demographic, economic, and military components. Discriminant function analysis or several clustering techniques could be employed on these data to demarcate dyads as either relatively equal or unequal. Of course, in any given pairing, the differences in capability will vary, so that in some wars of rivalry, the differences will be so small that they will be difficult to measure validly; whereas, in others, like the Seven Weeks War, one side will have a clear advantage over the other, even though both sides are relatively equal. To capture this information, ordinal measures can be used to rank dyads *within* the dichotomous categories.[8]

The second dimension by which wars can be differentiated is the well-known distinction between limited and total war. This distinction is based on both the ends and means associated with war (Morgenthau, 1960: ch. 22; Kissinger, 1957: ch. 6; Osgood, 1957) and is derived from Clausewitz' (1832: book 1, ch. 1) warnings about war becoming absolute. The distinction based on means is easier to make because it refers to the extent to which an all out effort is made in fighting the war. Total wars involve a high mobilization of society, whereas limited wars command comparatively fewer resources and personnel. The degree of mobilization reflects the salience of the issue at stake in the war and, as such, affects the way in which the war is fought on the battlefield. Because total wars take on the characteristics of a fight for survival, they tend to mobilize all resources and means to wage battle with few restraints, even though there may be prohibitions on specific types of weapons (like gas warfare). Limited wars are much more restrained in the means that are employed and where and how the war is fought. They may also be explicitly coercive – using force to try to get an opponent to take a specific action, although they need not be.

In terms of goals, limited wars are wars that have specific and concrete aims and leave much of the territory and independence of the opponent intact. The goals in total wars are much more open-ended and often expand as the war progresses. Total wars often demand the complete overthrow of the leadership of the other side whether through demand of unconditional surrender, as in World War II, or complete annihilation, as in the Third Punic War. As Stedman (1990: 1–2) points out, wars become total because "one or both of the antagonists comes to believe (or believes from the start) that the cause of the conflict lies in the character of the opponent," which usually means the leadership or ruling elite of the society. As a result, a peace that concludes a total war often involves attempts to reconstruct the opponent's values and institutions by some sort of social engineering scheme (Stedman, 1990: 3) that purges the society of its flawed character and elements, which are seen as residing in the leadership, policy influencers and bureaucrats. The creation of Weimar Germany and the post-World War II reconstruction of Germany and Japan all reflect such schemes. At the domestic level, civil wars and revolutions that are totalist have similar consequences, as the reconstruction following the US Civil War and the revolutionary enactments of the French, Bolshevik, and Chinese revolutions illustrate.

What distinguishes limited from total wars is the ability of participants to confine themselves to goals that are subject to bargaining and do not raise questions of survival or the ire and hostility of one side to such an extent that they fight as if survival were at stake. Two factors influence the extent to which wars will be limited. The first is the nature of the issues and the stakes over which belligerents are fighting. Wars that are fought over marginal border questions and comparatively minor changes in the dispositions of stakes (such as economic, strategic, or colonial advantages) are not going to be fought in the same manner as wars that are fought over far-reaching territorial and ideological demands, hegemonic claims, or fundamental changes in the global order and rules of the game. Of these, challenges to territory may be the most fundamental. Kugler (1990: 202–3) notes that wars between major states that become total have the core territory of states at issue, whereas limited wars, like the Crimean War, do not. The second factor that influences the extent to which wars will be limited is whether the practice of war and its related discourse in any given period restrains, by custom, the goals and means employed in war. The practices of mercenary armies of the *condottieri* of Renaissance Italy, for example, aimed to keep the loss of military personnel to a minimum, while the practices of twentieth-century armies do not.

Likewise, the stylized military tactics of the eighteenth century made war limited, whereas the tactics of Napoleon encouraged total battles.

Despite the eloquence of Clausewitz, Morgenthau, and Kissinger in making the limited/total war distinction, it is the most difficult of the three dimensions to operationalize. This is because the concept itself is multidimensional and because some participants in a war can be engaged in a total struggle while others are not. This complexity bespeaks of the theoretical richness of the concept and need not be seen as an insurmountable obstacle to its use. Although this is not the place for a complete operational definition, showing how some of these problems can be overcome will make the various aspects of the concept clearer.

The distinction between limited and total is one of degree and not kind, so that it is best measured at the ordinal level by ranking wars. Since it is a comparative concept, rather than a natural dichotomy, many of the problematic cases can be taken care of by looking at the degree to which they are totalist or limited, e.g. limited wars that have totalist tendencies or total wars that are restrained in one aspect or another. Whether the means of a war are limited or total could be measured by the extent (percent) to which the population and economy of a society is mobilized to fight the war. Such indicators as the percent of production devoted to the war effort, government control of production, sharp increases in taxes and/or changes in budget allocations, the amount of credits sought and the extent of deficit spending related to the war effort are ways in which mobilization might be validly and reliably measured. A second possibility for measuring means would be to examine the military tactics employed with attention to restraints on the use of certain weapons or the avoidance of certain targets. Such a military-oriented approach would be more judgmental and not as precise as the domestic mobilization approach. In addition, the mobilization approach has the advantage of directly measuring the extent to which wars penetrate the domestic structure of a society. Nevertheless, the military approach might be useful for early wars where economic and social data are scarce.

Measuring the goals of a war is less straightforward. This is because classifying goals is judgmental and goals can change as the war evolves. Nevertheless, an examination of government documents along with public statements and memoirs should permit analysts to classify the goals of participants in a war as either limited or totalist. Limited goals can be operationalized as those confined to marginal changes in territorial, economic, or political rights. Totalist goals can be defined as those that are more hegemonic or destructionist in char-

acter and would be indicated by goals that insist on the overthrow of the head of government or ideology of one's opponent; demand large areas of the core territory of an opponent; attempt to fundamentally change the rules of the game of the global political, economic, or social systems; or seek to produce fundamental shifts in the global pecking order. Of course, what is typical of totalist goals is that they evolve in that direction, as is best illustrated in World War I, so that the most reliable classification is going to be made *ex post facto*. Nevertheless, it would be possible to code the different stages of a particular war to see how it changed – for example, before and after Hitler's invasion of the Soviet Union.

What makes measurement additionally problematic is that because goals and means refer to two different things, it is logically possible for participants to have a goal of one type and a means of another. Figure 2.1 presents four categories in which the war effort of an individual belligerent can be classified: limited means and goals, limited goals and totalist means, totalist goals and limited means, and totalist means and goals. This ordering can be used to rank a belligerent's war effort. It assumes that goals are more apt to determine means than vice versa. To distinguish the differences in war efforts, a point system can be employed so that each category is scored as 2, 4, 6, or 8 with pure limited war efforts receiving a 2 and pure totalist war efforts an 8, as indicated in Figure 2.1.

Employing a scoring system for war efforts of each belligerent provides a way of overcoming the other major problem – how to code a war when one side sees itself fighting a limited war and the other a total war. The simplest way to classify wars would be to aggregate the war effort scores of each participant. Dyadic wars would be ranked separately from complex wars. For dyadic wars, a pure limited war would be a 4 and a pure total war a 16. For complex wars, the advantages of the scheme are impressive. For example, take the Vietnam War, which as a civil war involves a total war between the Republic of Vietnam and the Democratic Republic of Vietnam, but how does one incorporate the war between North Vietnam and the US? It is a total struggle for survival by one side, but limited on the other. The scheme in Figure 2.1 would score it as an 18 (DRV-8, ROV-8, US-2) much larger than the Russo-Japanese war, but still far away from World War I and World War II. The latter wars would score over 60 with World War II scoring as more totalist because of the explicit conquest aims of Germany, Italy, and Japan and the unconditional surrender demands of the Allied states. When classifying wars, it is important to keep in mind that the limited/total dimension is

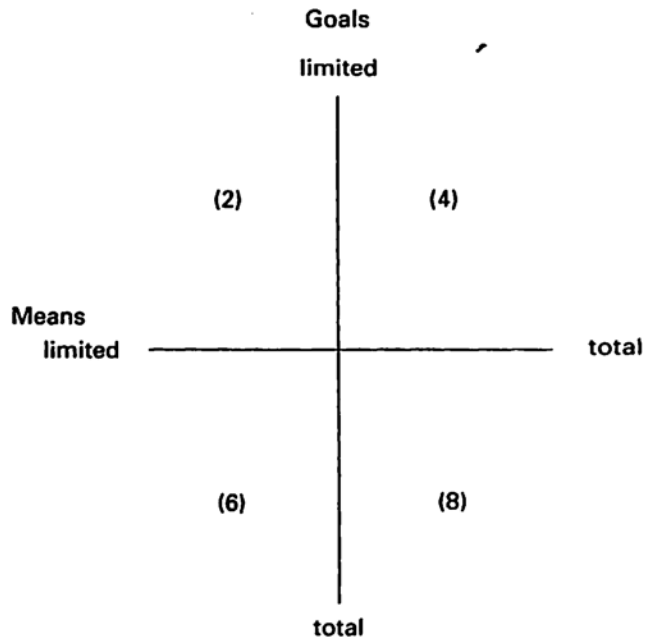

Figure 2.1
Classifying individual war efforts

comparative across wars within history. The point system outlined here is able to make such comparisons admirably. More importantly, for the purposes of this analysis, the point system demonstrates that the theoretically rich concept of limited versus total war can in principle be operationalized.

It is assumed that wars that are fought as differently as limited and total wars are must have different factors associated with them, if not different causes. The dynamics of contention that precede total wars should differ significantly from the dynamics that precede limited wars. While these are the basic reasons for including this dimension in the typology, the distinction between limited and total wars is also useful for distinguishing the impact of wars. Total wars are likely to have different and longer-lasting effects than limited wars. This is true not only for internal effects, like taxation and public debt (see Rasler and Thompson, 1985b, 1983), but also for global effects, like the changes in global leadership that concern Modelski (1978), Thompson (1985), and Gilpin (1981).

Combining the limited/total dimension with the previous one on differences in capability suggests some important insights. Because wars of rivalry are more prone to social psychological factors, it

becomes more difficult for decision makers to follow Clausewitz recommendations to keep war from becoming total and irrational in terms of its costs if the war drags on. In the absence of other states that can mediate a war that does not result in a quick victory, wars of rivalry are apt to become total wars.[9] Conversely, wars of inequality are less likely to become total, and if they do, they become total wars for different reasons. Rather than becoming unlimited because one side is unable to follow rational calculations, they become unlimited out of a cold calculation of what will be required to accomplish one's territorial goals. Usually this means that the stronger wants to occupy or colonize the territory of the weaker. Such wars of conquest become total when the conquered are not needed and are seen as an obstacle that must be removed, or the conquered are so culturally dissimilar that there is no desire to assimilate them. The lack of cultural similarity can serve to remove moral inhibitions about slaughtering these people and lead to genocidal acts. On the other hand, a policy of total assimilation can require that the defeated submit to the conqueror's civilization, which can mean adoption of a new religion and language, acceptance of foreign education as superior, name changes, and so forth.

The final dimension used in the typology classifies wars according to whether they are dyadic or complex (more than two participants). This distinction is derived from the work of Lewis F. Richardson (1960b: 250). In an analysis of Wright's (1942: Appendix XX) list of 200 wars (omitting civil wars) fought between 1480 and 1941 he found that 117 had only one participant on each side, 28 had two against one, 12 had three against one, etc. It is assumed that the modal dyadic wars will be easier to understand than the more complicated ones. Wars with more than three participants are called *complex*, because they may entail constructing a model of war expansion or diffusion. In addition, it can be assumed that the political dynamics of wars involving more than two participants will differ from the more straightforward dyadic wars.[10]

Theoretically, the distinction between dyadic and complex wars is important for understanding how the onset of world wars differs from that of other wars. World wars often grow out of wars that were initiated in the expectation that they would be limited, but for some reason were unable to be confined to the initial parties (see Sabrosky, 1985: 181; see also Sabrosky, 1975). This was the case in both World War I and II where the initiator and its allies hoped that allies of the target would not fight and made efforts to keep them neutral and face them with a *fait accompli*. Nevertheless, the initiator was willing to take the

risk that others might intervene. The case of the two twentieth-century wars suggests that initiators do not always employ a cost-benefit calculus in a sufficiently rational and synoptic manner, and that expected-utility theory (Bueno de Mesquita 1981b) based primarily on capability calculations will be less successful in analyzing complex wars than dyadic wars, particularly complex total wars.

The hope of mitigating war by keeping it more limited clearly lies in understanding why major states are unwilling or unable to confine the war to initial parties. Once a war involving major states on each side becomes complex, there is an increase in the probability that it will also become total. This is because each new party that enters the war adds its entire agenda of issues to the coalition's war aims, making the issues under contention even more intractable and less susceptible to bargaining. This not only makes a negotiated end to the war more difficult (thereby lengthening the war), but the sheer number of issues encourages a total war where the winners take all. Such questions are important to explore not only by examining world wars, but by examining those cases of war expansion, like the Crimean War, that could have become a total war, but did not.

Making a distinction between dyadic and complex wars also provides a way of seeing, more generally, how and why wars expand and how alliances and contagion may affect the war process and the severity and duration of wars. Also, treating complex wars as a type will help move research beyond the finding that alliances make wars expand, to a deeper understanding of why wars become contagious. Finally, of interest is determining just how and why the causes and consequences of dyadic wars are different from the causes and consequences of complex wars.

Figure 2.2 combines the three dimensions to construct a logically exhaustive and mutually exclusive typology of war. Since difference in capability is seen as the most important dimension, wars of rivalry are separated from wars of inequality, and then these two basic types are broken down further in terms of the scope of the war (limited–total) and the number of participants (dyadic–complex). This results in four kinds of wars of rivalry and four kinds of wars of inequality. Among wars of rivalry, a limited dyadic war would be the Russo-Japanese War or Sino-Indian War and a limited complex war would be the Crimean War or the Seven Weeks War. Total wars of rivalry would include the Punic War as a dyadic war and World War II as a complex war. Interestingly, most civil wars of any length can be classified as total wars of rivalry that are dyadic, if there is no external intervention, while internationalized civil wars are complex.[11]

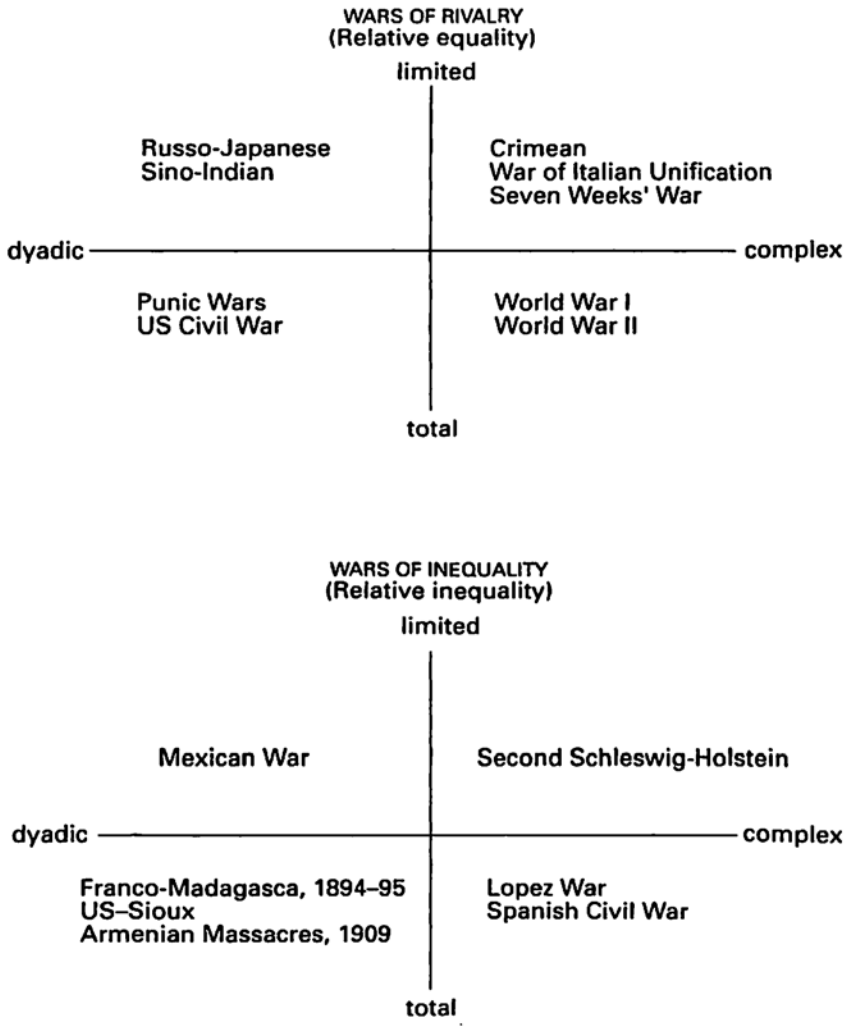

Figure 2.2
Typology of war

Among wars of inequality, a limited dyadic war would be the Mexican War, where a strong (and growing) state takes advantage of a weaker (and less populous) neighbor to increase its territory. A limited complex war would be the Second Schleswig-Holstein War, where two strong states, Prussia and Austria-Hungary, gang up on Denmark to gain territory. Dyadic total wars of inequality would include the many imperial wars of conquest that massacred entire sections of a population in an attempt to dominate them, and the mid-twentieth century reversal of that domination in wars of national liberation.[12] Complex

total wars of inequality are rarer and usually involve the outside intervention of one or more parties in an ongoing total war that clearly tips the scale in favor of one side, as in the Lopez War or the internationalized Spanish Civil War.

The typology can be used for a variety of purposes. Significant comparative inferences can be made without employing all eight categories simultaneously. Particular wars can be compared by placing them within the categories of the typology. Wars can also be analyzed quantitatively, employing two categories at a time with two more as controls, without reducing the cases to such a small number that there would be little basis for generalization. The full typology would still be useful for "unpacking" and interpreting aggregate correlations so that they could be theoretically related to historical analyses of specific wars.

In this analysis, the typology will be used to delimit the scope of the inquiry and aid in the interpretation of evidence. If different types of wars have different causes, then any attempt to construct a scientific explanation of war must indicate what kinds of wars are being explained and *why* these wars are different from others. The typology presented in Figure 2.2 can provide satisfactory answers to both these questions. In addition, it allows one to sift through existing evidence and reinterpret aggregate correlations by breaking down the cases upon which they are based into the typology's theoretical categories.

As I began this inquiry, it became clear to me that a single explanation of war would not fit all cases. This confirmed my earlier suspicion that some explanations, such as rational choice cost-benefit analysis, provided an adequate model for describing decisions in some kinds of wars, but an overly simplistic and misleading account of others. As I investigated the question further, it seemed to me that some wars, like the twentieth-century world wars, were preceded by long-term rivalries with elaborate foreign policy practices, like alliance making and arms races, whereas other wars lacked these policies and appeared to start from more straightforward instrumental calculations. The explanations with which I was working seemed to apply to the wars that were least susceptible to rational choice explanations that focus on costs and benefits and more intelligibly understood by social psychological explanations, learning models, and variables other than simply "power."

Put in another way, I suspect that wars of inequality tend to more easily conform to simple modernist ideas of rationalistic calculation of costs and benefits; whereas, wars of rivalry need to be explained by more complicated cognitive models of decision making.[13] The typo-

logy presented in Figure 2.2 is the result of the attempt to try to identify theoretically these wars and differentiate them from other wars. Since the explanations developed in this book apply only to wars of rivalry, it is important to understand the defining characteristics of these wars and how the dynamics of rivalry affect political conduct.

CONCEPTUALIZING RIVALRY

It is assumed that wars between equals, particularly wars among the strongest states in the system, do not break out unless there has been a long history of conflict and hostility between the disputants. Hence, these wars have been labeled wars of rivalry. The idea of rivalry is often used in historical description. Historians speak of the rivalry between Prussia and Austria for Germany, between France and England for colonies, and of Soviet–American rivalry in the Cold War. Despite this use, the concept has not been the subject of much analysis, even though more technical definitions exist in the study of animal behavior (Wilson, 1975).

Recently, within the Correlates of War project, disputes among rivals have begun to receive attention (see Diehl, 1985a, 1985b; Diehl and Goertz, 1991; Wayman, 1983; Wayman and Jones, 1991; Gochman and Maoz, 1984; 609–11; Huth and Russett, 1991). Rivals have been identified operationally by Diehl (1985a: 334) as any two states that have had at least three militarized disputes within the last fifteen years, and by Wayman (1983: 15, 18) as any two states that have at least two disputes every ten years. Wayman and Jones (1991: 5–6) identify a "long-term" enduring rivalry as one that has at least five reciprocated militarized disputes, with the disputes spread over a twenty-five-year period.[14] While these are useful operational definitions, they do not provide a conceptualization based on a theoretical understanding of the dynamics of rivalry. There has been little reflection and no research on how and why states become rivals. The analysis in this section attempts to provide such a conceptualization.

Rivalry is, above all, a term that characterizes a competitive relationship between two actors over an issue that is of the highest salience to them. In this sense, the term can easily refer to two male deer who buck over a mate, two individuals within a company who compete to become a vice-president, or to two states, like Prussia and Austria, who struggle to achieve hegemony over a geographical region. Nevertheless, while in each of these cases a rivalry is present, the behavior that characterizes the rivalry will be extremely different. This reflects the fact that behavior is a learned response that will vary, given the

prevailing customs and rules applied to a situation and the genetic tendencies of a given species, as well as the different structures of systems.

Rivalry between political collectivities is typically characterized by a sustained mutually contingent hostile interaction. Gamson and Modigliani (1971: 7–12), although they do not offer this as a definition of rivalry, characterize East–West relations in the Cold War in these terms. *Mutually contingent* can be taken to mean that each side's foreign policy actions are apprehended primarily in light of the foreign policy of the other side, and not simply as a result of internal factors or bureaucratic inertia. *Hostile interaction* is taken here to mean that a major motivation behind actions is psychological hostility, i.e. more emphasis is placed on hurting or denying something to the other side than on gaining something positive for oneself.

What distinguishes a rivalry from normal conflict is that issues are approached and ultimately defined not in terms of one's own value satisfaction, but in terms of what the gaining or loss of a stake will mean to one's competitor. An issue results from contention among actors over proposals for the disposition of stakes among them (Mansbach and Vasquez, 1981: 59). An issue, as Randle (1987: 1, 29) points out, is a disputed point or question that requires (as Coplin *et al.*, 1973: 75 argue) collective action on a proposed allocation of values.[15]

The number of disputed questions, or stakes, that are seen as part of the same issue can vary depending on how actors *link* stakes together. The underlying perceptual foundation of an issue or technically the *issue dimension* that links stakes together (see Mansbach and Vasquez, 1981: 60) can run the gamut from an extreme actor dimension, where the emphasis is on who is getting what with the focus on enemies and friends, to an extreme stake dimension, where the focus is on what is at stake and there are no permanent friends or enemies.[16]

Contenders that link stakes together on the basis of an *actor dimension* will have a large number of stakes in a few issues. Conversely, issues that are defined on the basis of a *stake dimension* will consist of a relatively small number of stakes that are brought together on the basis of some obvious substantive focus and/or geographical location (law of the sea, trade (or monetary) questions among OECD states). The hallmark of a rivalry is that issues are defined on the basis of an actor dimension. Indeed, as the rivalry intensifies, all issues may be collapsed and linked into a single grand issue – us versus them.

Whether an actor dimension or a stake dimension is in force has profound effects on how issue positions are determined, the types of stakes that are discussed, the kinds of proposals that are made for the

disposition of these stakes, and the general pattern of cooperation and conflict that emerges in contention. The issue position an actor takes can be said to be a function of the decision-making calculus the actor employs for determining whether it is for or against a given proposal. There are three such calculi: (1) a *cost-benefit* calculus, in which actors determine whether they are in favor of or opposed to a proposal on the basis of the costs and benefits they would accrue if the proposal were adopted; (2) an *affect* calculus, in which actors determine whether they are in favor of or opposed to a proposal on the basis of whether their friends or enemies are in favor of it; and (3) an *interdependence* calculus, in which issue positions are determined on the basis of what effect the position on this issue will have on *other* issues on the agenda (see Mansbach and Vasquez, 1981: 191–97 for elaboration). Leaders in a rivalry will adopt a negative affect calculus rather than a cost-benefit or interdependence calculus. Leaders who employ a negative affect calculus favor any position that will hurt their opponents and oppose any position that will help their opponents.

A relationship between two states that is characterized by a consistent use of the negative affect calculus over time will lead each actor to link more and more stakes into a single issue, since the issue is being defined simply as what hurts one's opponent. As a result, the type of stakes under contention changes, with concrete stakes becoming infused with symbolic and transcendent importance.[17] As actors become increasingly concerned with relative gain and loss, stakes which may have had comparatively minor value are now seen as having great importance because they represent a commitment to bigger stakes. Thus, West Berlin in the 1960s becomes worth risking a nuclear war, because it is symbolic of America's defense of Europe. Eventually, the contention between the two actors may take on the characteristics of a titanic struggle between two ways of life or even between good and evil, particularly if leaders find it necessary to rationalize their policies with a higher purpose in order to mobilize domestic actors to make sacrifices for the coming struggle. When such highly moralistic and/or ideologically sharp language is introduced, then the contention can be seen as involving transcendent stakes.

As one moves from concrete to symbolic to transcendent stakes, issues become more intangible and hence less divisible. What little research there has been on issues shows that the more tangible an issue, the greater the likelihood of eventual resolution, while the more intangible, the more contentious and conflict-prone an issue (Vasquez, 1983b; Henehan, 1981: 13). Because some types of stakes are inherently more resolvable, the type of stakes over which actors contend will

have an important effect on their interactions. Concrete stakes, because they are tangible and divisible, are more likely to permit compromise. Symbolic stakes make actors less flexible and more willing to stand firm because their symbolic nature leads to fears about losing a reputation for credibility or of establishing a bad precedent, if they entertain concessions necessary to bring about a compromise. Issues involving transcendent stakes are the most difficult to resolve, because they reflect fundamental differences over values, norms, and/or rules of the game (Vasquez and Mansbach, 1984: 426).

The issues that seem most prone to be infused with symbolic and transcendent qualities, at least in the modern global system, are territorial questions. For a variety of reasons states regard territory as the most salient stake over which contention can occur. It is not an accident then, that most rivalries originate over attempts to control the territory and space between equals. This is certainly true of the rivalry between the Valois and Hapsburg, France and England, and Germany and France. What is ironic about this is that territory, at least on the surface, appears as a very concrete stake that could be divisible. Except for minor border changes, political actors do not perceive it this way. Instead, for reasons that perhaps stem from our evolutionary past, human collectivities treat territory in symbolic and transcendent terms, infusing the concrete stake with all kinds of normative and ideological significance. Once they do that a prolonged hostile interaction is difficult to avoid. This does not mean that other issues can not give rise to rivalry, witness the Soviet–American and Anglo-German rivalries, but that relationships between equals that contend over contiguous territory are more prone to rivalry and to rivalry that ends in war.

Rivalry is born because contention that focuses primarily on symbolic and transcendent stakes tends to be irresolvable. A relationship that is dominated by these kinds of stakes will produce conflict that tends to fester and escalate.[18] In order to resolve issues, actors indicate how they would like the stakes under contention to be distributed. Proposals for the disposition of stakes can be analyzed in terms of the way in which they propose to distribute costs and benefits between the contending parties. Initially, in any contention, each side suggests that it get most, if not all, of the benefits and the opponent bear most of the costs. Actors that contend over symbolic and transcendent stakes will not move from their initial issue positions because of the feeling that "my claims are just and yours are not." Negotiation may be further hampered when proposals of both sides take on the characteristics of a zero-sum game.

Table 2.1. *Types of proposals for disposing of stakes*

		Distribution of costs	
		Equal	Unequal
Distribution of benefits	Equal	I	II
	Unequal	III	IV

Source: Vasquez and Mansbach, 1984: 427

The key to whether a proposal will produce agreement and a resolution of the issue or foster disagreement and stalemate is how it assigns costs and benefits. Table 2.1 shows that there are four ways in which costs and benefits can be distributed: each side shares equally in the costs and the benefits (cell I); each side gets equal benefits, but one side bears more of the costs (cell II); or the reverse, each side shares the costs equally, but one side gets more benefits (cell III); and, finally, one side gets most of the benefits and the other bears most of the costs (cell IV). Proposals that are most apt to give rise to protracted disagreement are those in cell IV, because they call for a severely disproportional distribution of costs and benefits, with zero-sum proposals being but the most extreme of this type.

Transcendent and symbolic stakes usually give rise to type IV proposals. Successful negotiation typically entails moving initial type IV proposals to one of the other types. Some kinds of issues are more amenable to such a move than others – depending on the salience (importance) of the issue, the type of stakes that compose it, and the variety and number of stakes linked in an issue. Those issues which link a large number of stakes that are infused with symbolic and transcendent significance are highly unlikely to be the subject of serious negotiation without being fundamentally redefined (i.e. defused of their emotional content and de-linked from each other). Since this is often inherently difficult and politically unfeasible, some issues simply cannot be negotiated, but must be fought out.[19]

The way in which an actor dimension affects the types of stakes under contention and the types of proposals made for disposing of those stakes clearly has an effect on the pattern of cooperation and conflict that emerges in contention. To understand why this is the case, it is necessary to have a better understanding of cooperation and conflict. *Conflict*, and its alleged opposite, *cooperation*, are two of the

most ambiguous concepts within world politics. Elsewhere I have argued that to treat cooperation–conflict as a single dimension is erroneous (Mansbach and Vasquez 1981: 234–40; Vasquez and Mansbach, 1984). The empirical research supports this conclusion in that findings consistently show that cooperation and conflict are separate and uncorrelated dimensions, and that different variables are associated with each (Rummel 1972b: 98–99; Ward, 1982: 91, 93, 111, 118–20; see also: Salmore and Munton, 1974 and Wilkenfeld *et al.* 1980: 183–84, 192–93, 217–29).

These findings imply that the dynamics of conflict and the dynamics of cooperation are fundamentally distinct and require different explanations. One way of moving in that direction is to analytically separate the various dimensions embodied in each of the terms. According to the way these terms are typically used in analysis, they refer to three distinct aspects of behavior: (1) agreement vs. disagreement (a similarity or difference in *opinion* on an issue); (2) positive vs. negative acts (*behavior* that is seen as either desirable or undesirable); (3) friendship vs. hostility (*attitudes* reflecting psychological affect).

How these three dimensions interact is crucial for deciphering how a *relationship* emerges out of contention. It is assumed that differences in opinion (agreement–disagreement) shape behavior (i.e. the pattern of positive and negative acts), and behavior determines psychological affect (friendship and hostility). One of the great ironies of human interaction, pointed out by Coplin and O'Leary (1971: 9), is that conflictive (i.e. negative) acts are intended to change issue positions, but instead change affect. Typically, if an actor tries to resolve a disagreement on a salient issue by punishing another actor, this will generate hostility rather than any shift in issue position.

Spirals of conflict or cooperation occur when all three dimensions reinforce each other over time. In a conflict spiral, disagreement leads to the use of negative acts, which in turn produces hostility. The presence of hostility encourages more disagreement, which, if it persists, leads to a vicious cycle of disagreement–negative acts–and hostility. One should not try to explain the pattern of negative and positive acts, but instead how "normal" interactions give rise to cooperation or conflict spirals and the effects these spirals have on determining the overall relationship between two actors. This underlines the point that *behavior* is something that comes out of and shapes a relationship. Actors do not simply behave conflictively or cooperatively; they *relate* to each other in conflictive or cooperative ways. A relationship involves all three dimensions – a pattern of agreement and disagreement, a pattern of exchanging positive and negative acts, and the

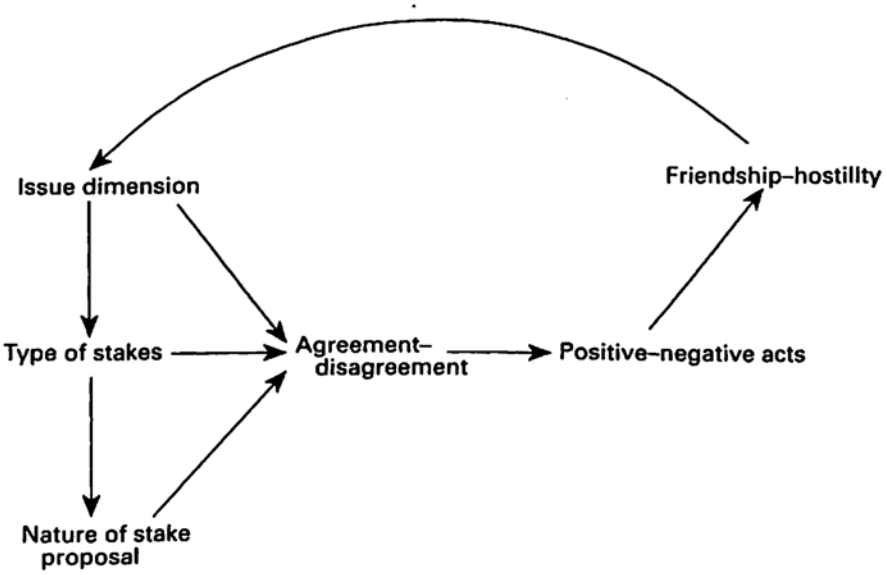

Figure 2.3
The role of issue variables and dimensions of cooperation and conflict
Source: Vasquez and Mansbach, 1984: 428

residual attitude of friendship or hostility generated by the previous two patterns.

Figure 2.3 depicts how the three separate dimensions are related to each other and the various issue characteristics. What is important for understanding rivalry is that the pattern of friendship and hostility that emerges out of the pattern of agreement/disagreement and positive/negative acts has a tremendous impact on whether the issues dividing the two are defined on the basis of an actor or stake dimension. The issue dimension affects the pattern of agreement directly, as well as indirectly through its effect on the type of stakes and the nature of the stake proposal. The issue characteristics determine the pattern of cooperation and conflict, with friendship and hostility feeding back to reinforce the predominant way issues are being defined.

When an actor dimension is in effect, conflict is more likely because there is a strong tendency to reduce all contention to a single issue. This encourages a pattern of persistent disagreement, since it reduces cross-cutting which moderates hostility (Dean and Vasquez, 1976). The presence of an actor dimension makes each side infuse concrete stakes with symbolic and eventually transcendent importance. This makes it likely that each side will offer proposals to dispose of the stakes on the basis of an unequal distribution of the costs and benefits. Such

proposals promote disagreement and, if this continues, stalemates and intransigence become the order of the day, as proposals become less flexible and more zero-sum. In such a situation, conflict spirals are likely, with negative acts becoming the predominant way in which actors try to resolve differences (Vasquez and Mansbach, 1984: 428–29).

We are now in a position to delineate the most important characteristics of a rivalry with the relevant technical terms. A *rivalry* is a competitive relationship among equals that links stakes into issues on the basis of an actor dimension. The actor dimension results from a persistent disagreement and the use of negative acts which build up negative affect (psychological hostility). Hostility reinforces the actor dimension which gradually reduces all issues to a single overarching issue. Simultaneously, concrete stakes are transformed into symbolic and transcendent ones, and proposals for the disposition of stakes and the resolution of an issue distribute costs and benefits on an unequal basis. This makes for more disagreement, greater use of negative acts, and an intensification of hostility, which in turn reinforces the actor dimension. An escalating conflict spiral results, which creates an atmosphere in which crises are likely to be born.

Rivalry becomes a way of life. The relationship is difficult to change because each side is involved in a vicious circle in which hostility makes actors define issues in ways that are intractable and threatening, and actors become hostile, in part, because of the way they have defined the issues that divide them. The relative equality makes for an interdependent decision-making situation from which neither side can escape, because neither side can overcome the other.[20] Stalemate results and the issue festers. Frustration ensues, and a cost-benefit calculus of normal politics gives way to the feeling that what is of primary importance is not one's own value satisfaction, but hurting the other side.

As the issues between two equals take on these characteristics, it can be expected that the normal relationship between actors will cross a threshold in which a particular set of events sets off a conflict spiral that results in a crisis. The first crisis acts as a kind of baptism of the rivalry, crystallizing the underlying processes (Vasquez and Mansbach, 1984: 429). Rather than defining a rivalry as engaging in three or more militarized disputes within fifteen years (Diehl 1985b: 1204) or as "enduring disputation" (Wayman, 1983: 18 note 6), the conception offered here can predict when a pattern of crises will be likely. Because the underlying processes that have produced one crisis (the actor dimension and the resulting pattern of disagreement, negative acts, and hostility) are difficult to change, they will produce others. In a

rivalry, the two sides expect their relationship to be punctuated by periodic crises.

What separates those few rivalries that have avoided war from the many that have not? Those that have avoided war have done so by breaking the pattern of crises and resolving the issues at hand. Those that have not, have adopted a set of foreign policy practices that have increased the likelihood of dangerous crises rather than reducing their occurrence. The actions they take bring them closer to war. They take these actions, in part, because the dominant global culture encourages them to do so and, in part, because some issues, namely territorial issues, are more prone to the use of coercion than other kinds of issues.

THE DOMAIN OF THE ANALYSIS

In this book, I will concentrate on constructing an explanation of wars of rivalry. I am concerned with explaining the onset of these wars, delineating what factors are associated with them, and stipulating which of these factors are of causal significance. The explanations I outline will also attempt to account for the different kinds of wars of rivalry, especially total complex wars, i.e. world wars. Why are some wars between rivals limited, while others are total? Why do some wars of rivalry remain dyadic, while others involve numerous pairs? How do these complex wars draw in the various participants? Does a war between the two major protagonists in the system infect all other states, or is it the case that wars involving a minor state(s) draw in the major rivals?

Questions of this sort will be addressed by trying to delineate the factors that makes wars limited or total on the one hand, and dyadic or complex, on the other. Although the answers that will be provided will apply only to wars of rivalry, it is hoped that understanding these dimensions will help in answering similar questions about wars of inequality. In this way, the typology fosters a cumulative body of knowledge of war, even though it requires separate explanations for different wars. Since the understanding of one kind of war will help us in understanding another, I will periodically allude to the types of wars I do not systematically examine when I think this analysis can shed some light on them.

Let me also note once again that the analysis presented in this book is based primarily upon studies of wars fought after 1815. While the wars after 1815 form a commonly demarcated period, there is reason to believe that all the wars fought in the modern global system (circa 1495 to the present) can be analyzed together. Because the wars from 1495

to 1815 and from 1816 to the present occur within the same historical structure, they should be caused by the same factors and therefore explicable by the same theory.

Since 1495, a number of critical events have occurred which may have had a fundamental effect on the causes, dynamics, and consequences of war that may make the wars after 1495 theoretically different from the wars that preceded them. First, two long-standing forms of political organization, the empire and the city-state, declined and were eventually replaced by a new form of organization, the nation-state (Braudel, 1966). This transformation was brought about by war, and, in turn, war seems to have been responsible for providing a competitive advantage to the nation-state as a form of organization (Tilly, 1975b, 1990; Zolberg, 1983: 280). Second, beginning in the late fifteenth and early sixteenth century, European states expanded and created a world economy (Wallerstein, 1974: 15; Wallerstein, 1983: 305–6), which set the basis for a global political economy of what Wallerstein labels "the modern world-system." The obvious need to regulate and rule the world economy, at least in some form, gives rise to certain states taking a leadership role. Modelski (1978: 214) considers this the emergence of "the global political system." Third, prior to and throughout this time, the long transition from feudalism to capitalism and the continuing advances in science and technology produced a distinct culture of modernity, which was accentuated by industrialism and the development of bureaucracy. These four elements (the nation-state as a form of organization, capitalism, science and technology, and the culture of modernity) shaped the historical structure of the current modern global system and undoubtedly had an impact on why and how wars occurred and the consequences wars would have within the system. When attempting to explain war, I will be limiting my explanation to wars in this particular historical system, even though, on occasion, I may make allusion to earlier wars, like the Peloponnesian War and the Punic Wars, to make a specific point.

Although the reasons for limiting the scope of this analysis are justifiable, they are primarily theoretical reasons and do not reflect hard empirical or historical evidence that the causes of war in the current system are in fact (or of necessity) fundamentally different from those of earlier systems. The question of how much one can generalize about war within the current system from 1495 to the present, or between the current system and other systems, must be held open until further research is completed. I choose to limit the domain of the analysis mostly to err on the side of caution (although some will find this period much too long), and not because I am

convinced that a general theory of war is impossible to construct. Since we have extensive data-based evidence on war only from 1815 to 1980 and limited data on war from 1495 to 1815, it is not realistic at this point to construct a general theory of war for all time.

I am concerned with wars of rivalry and particularly world war for several reasons. First, I focus on wars of rivalry because the empirical evidence is most plentiful on wars involving major states, with the most interesting findings dealing precisely with wars between major states that are relatively equal and with the two world wars. Second, the question of rivalry and whether and how it can be resolved without total war has been a major question in the twentieth century. Despite the ending of the Cold War, this question is still important at the regional level, where it is becoming increasing ominous as more rivals choose to acquire nuclear weapons. The viability and feasibility of many proposals for establishing global and regional peace turn on empirical questions related to the causes of war between rivals and/or on how wars can be limited. Third, I focus on wars of rivalry and world wars, specifically, because it is these wars that realist theorist have claimed as the kinds of wars they are most able to explain, and because I believe that the realist approach to explaining war is fundamentally flawed. If international relations inquiry is ever to move beyond realism, then it will be necessary not simply to critique the realist paradigm (Vasquez, 1983a) and develop an alternative perspective (Mansbach and Vasquez, 1981), but to show that even at the heart of the paradigm (on the topics that realism claims to provide the best answers) alternative explanations can provide better answers. Thus, I have turned to these wars because of the availability of scientific evidence, their policy relevance, and their paradigmatic implications.

Because realist approaches and, more recently, neorealist explanations have so influenced theorizing about war, it is necessary to come to grips with that tradition – to indicate which of their insights are crucial to understanding war and peace, to outline where the tradition has gone wrong and what it has failed to see, and to reformulate what it can contribute to a new and broader perspective on the causes of war and the nature of peace. This is the subject of the next chapter.