Regions of War and Peace

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1 Introduction

Earth's billions of people reside in nearly two hundred countries characterized by varying levels of development, governed by numerous forms of political organization, and adhering to the traditions of widely disparate cultures. Could it possibly be that these obviously different peoples conform to common patterns in when, how and why they go to war against others? One might be tempted to immediately answer "no" or at least "doubtful." But others might answer "of course," perhaps citing similarities such as taboos against incest that are common in virtually every culture and society. In this book I ask whether general knowledge about when wars are likely to occur is possible. In an attempt to understand war onset generally, I consider patterns of war and peace among the great powers, as well as in four minor power regions of the globe.[1]

The research project culminating in this book began as a relatively straightforward effort to determine whether a well-established theory of great power interactions could be modified to help understand interactions among minor powers. The theory modified is power transition theory, which posits a hierarchical international system and emphasizes the importance of relative power relationships and the incentives and disincentives states face in their considerations of acting to change the formal and informal rules that govern their interactions. In order to extend the theory to minor powers, careful consideration must be paid to identifying the international sub-systems within which such states interact. This leads to a new operational definition of regional sub-systems. Armed with this notion of what constitutes a region, I press ahead with

[1] Throughout this book I use antiquated terms such as "great powers" or "minor powers" even though they have gone out of style, because there are no widely accepted replacements.

the application of power transition theory to analysis of minor power interactions and find that in spite of considerable similarity between what transpires within these regional sub-systems and within the overall international system, there are nevertheless some differences. Thus my extension of power transition theory to minor power regions requires the definition of regional sub-systems and then raises a follow-up question of why, despite some basic similarity, persistent differences distinguish some of those regions.

In writing the book I thus begin with one, but quickly come to have three tasks. First, modify power transition theory so that it applies to minor as well as great power interactions. Second, define minor power regional sub-systems and then analyze the modified version of power transition theory within those regional sub-systems. Third, attempt to account for the fact that, in spite of a reasonable amount of similarity across the regions, there still are some persistent differences. Although I list these three tasks sequentially, they are interrelated pieces of the puzzle of when and why wars break out between states. The conditions which my version of power transition theory suggests make war more likely do appear to affect when wars occur. A plausible case that these conditions say something about why the wars break out when they do can be made. But there remains an unanswered question of why these conditions are more important in some regions than others. I believe the question remains because the processes of political and economic development not only affect when and why wars break out between states, but also because the developmental process cannot be separated from the wars themselves. Thus, there are persistent differences across regions of the international system because the processes of political and economic development are not evenly achieved around the globe.

My conclusion is that we can understand a significant amount of the war and peace interactions of both great powers and minor powers by paying attention to the hypothesized causes of war suggested by my modification of power transition theory. However, the rest of the story about war and peace interactions remains hidden unless we allow for the cross-regional differences I uncover. I think these differences indicate that at later stages of development, states are more likely to wage war given the conditions central to power transition theory. The more developed states are, the more applicable power transition theory is to their behavior. In a sense, then, the usefulness of power transition theory and my extension increases as national development progresses. The fundamental, and I think fascinating, resulting question is what accounts for the poorer fit of the theory at early stages of development?[2] I begin to address this question toward the end of this book, but necessarily provide only a very crude first attempt. Even that crude first analysis, however, suggests how we might better proceed in the future as we try to address the question.

Issues at stake in this book

The overall task undertaken in this book is to extend knowledge about great power interaction so as to help understand and anticipate minor power interactions. In addressing this task a trio of intellectual issues is raised. The first has to do with the fact that almost all of what we "know" in world politics research is based on the historical experiences and intellectual culture of the West generally, and of the great powers more specifically. We have a great power bias not only in our empirical analyses of world politics, but also in our theorizing. This underlying bias could render the aggregate task of my book quite difficult. The second intellectual issue involves the epistemological question of whether we can aggregate the disparate experiences of different cultures, different time periods, different resource endowments, into one unified analysis. Some political scientists focus exclusively on the "parts" of the international system while others focus on the "whole." The two foci are largely antithetical, or at least are often presented as being so. I treat the epistemological debate as an empirical question, and discover support for those who espouse a "parts" epistemology *and* for those who subscribe to studying the "whole". Finally, a third intellectual issue is why we should care whether the Third World resembles the First? This may seem a rather dismissive reaction to my effort, but as summarized below, there is a growing body of literature within the field of realist security studies that specifically debates whether the Third World matters.

Western/great power bias in "what we know"

Much (maybe most) of the extant empirical and theoretical research on international conflict is informed by the history of great power

[2] I only provide evidence in this book that power transition theory's applicability seems to increase with development, and thus I limit my claims to it. However, I think the interrelationship between development and war makes it very likely that the applicability of other theories varies as development progresses. If this hunch of mine is correct, the key in the future will be to think about incorporating developmental processes into explanations of state behavior. Obviously this will be a harder task with some theories than with others.

(largely Western and European) interactions. Theories are created with this history in mind, and many statistical analyses draw heavily on data reflecting the interactions of the great powers. For example, the first quantitative investigation of the relationship between power distribution and war focused exclusively on the great powers (Singer, Bremer, and Stuckey 1972). More recently, empirical evaluations within the expected utility theory tradition conducted post-*The War Trap* focus on a set of strictly European dyadic observations (*inter alia*, Bueno de Mesquita 1985a; Bueno de Mesquita and Lalman 1992; and Bueno de Mesquita, Morrow, and Zorick 1997).[3] Various theories have been offered either based explicitly on, or otherwise created to explain, great power interactions. As evidence, consider that Gulick's (1955) definitive history of the balance of power is *Europe's Classical Balance of Power*; in his introduction to structural realism Waltz (1979: 73) claims: "A general theory of international politics is necessarily based on the great powers," and, according to its earlier proponents, Organski's power transition theory "can be tested fairly only if we locate conflicts whose outcomes will affect the very structure and operation of the international system" (Organski and Kugler 1980: 45). Most likely these would be conflicts among the great powers.

The centrality of great power interactions to research in world politics has not diminished. Recent titles include William R. Thompson's (1988) *On Global War*, Benjamin Miller's (1995) *When Opponents Cooperate: Great Power Conflict and Collaboration in World Politics*, and George Modelski and William R. Thompson's (1996) *Leading Sectors and World Powers*. Perhaps the clearest statement of this preoccupation is provided by the title of Jack Levy's (1983) *War in the Modern Great Power System*. There is no corresponding *War in the Modern Minor Power System*. Preoccupation with great power politics is understandable; most international relations researchers are from the Western world, if not specifically from one of the great power states. Thus, the history they analyze and explain is, in a very real sense, their own. At the same time, the great powers have existed as political units for a long period of

[3] I mention the expected utility theory research program because it is one of the most sophisticated bodies of theory and evidence currently available about world politics (see Bueno de Mesquita 1989 or Morrow 2000 for supporting arguments). In making the reference in the text, however, I appreciate that expected utility *theory* offers a general argument. It is only the *empirical evaluations* after *The War Trap* that are restricted to European interactions. For a recent global evaluation of expected utility theory, see Bennett and Stam (2000).

time and have been more highly developed than states from other parts of the world, and thus data about them are more readily accessible. Finally, interactions among these heavyweights have been enormously consequential for themselves and for the rest of the world. If scholarly attention has to be restricted to a sub-set of states, this clearly is an important one.

Hopefully, those who write books about great power politics understand that they are restricting themselves. A few explicitly recognize these restrictions. In his analysis of territorial disputes since World War II, Paul Huth (1996: 6) writes: "At present, the scholarly literature on the causes of war in the twentieth century is oriented toward the major powers, in general, and European international politics in particular . . . international conflict behavior outside of Europe has not been studied extensively." Kalevi Holsti, summarizing his critique of extant international relations research, agrees (1996: 205): "The world from which these theoretical devices and approaches have derived is the European experience of war since 1648 and the Cold War. They have also drawn heavily upon the experiences of the great powers . . ." Similarly, in the introduction to her edited volume, Stephanie Neuman (1998: 2) writes: "mainstream IR Theory – (classical) realism, neorealism, and neoliberalism – is essentially Eurocentric theory, originating largely in the United States and founded, almost exclusively, on what happens or happened in the West . . . Few look to the Third World to seek evidence for their arguments." I think it likely Neuman would agree that it is also true that few look to the Third World to develop their arguments in the first place. Finally, Jeffrey Herbst (2000: 23) complains about "the problem that almost the entire study of international relations is really an extended series of case studies of Europe."

So what? If the great powers have been especially consequential in their interactions, and if many of the above researchers consciously understand that their arguments and evidence are restricted only to great powers, what is the harm of a great power bias? There are a number of ways in which great power bias may be harmful. First, which states are designated great powers is somewhat subjective. Is Japan a great power after post-World War II economic recovery? Japan's economy has attained enormous size (third or second largest in the world depending on how GDPs are compared), but the Japanese military establishment is relatively small. Saddam Hussein's Iraq in 1990 had one of the largest armies in the world, but was not considered a great power. If Japan is excluded because of insufficient military resources, and Iraq is excluded (presumably) because of insufficient economic resources, then it must be the combination of economic size and military resources that defines the great powers. Most current lists of great powers continue to include France. This seems odd because France's military and economy are roughly comparable in size to those of a number of states definitely not included as great powers (such as Turkey). Perhaps once a state attains great power status it remains a great power forever. However, if this is true it is difficult to explain why Spain, Portugal, Holland, Austria, or Italy are no longer listed among the great powers. Arguably possession of nuclear weapons is a defining characteristic of great power status. If true, then India and Pakistan unambiguously established themselves as great powers in 1998. But, if true then it also becomes impossible to identify great powers before the first successful atomic explosion in 1945. A similar problem exists if we identify great power status with veto power on the UN Security Council. It is not so clear who the great powers really are. Thus any restriction to "the great powers" is an arbitrary restriction. We cannot be sure that all of the theorists and empiricists referred to above make the same arbitrary restrictions, and thus we cannot conclude that there is any cumulative progress in the corpus of great-power-specific research.

A second problem introduced by restricting analysis to the great powers (assuming a definitive list of great powers existed) is that there may be something odd about great powers compared to the rest of the world's states which we thereby exclude ourselves from knowing if we only study the great powers. Medical researchers at West Virginia University might restrict themselves to analyses of residents of the Mountaineer State. In so doing they would likely draw a sample of individuals heavily representative of coal-miners (relative to samples that might be drawn elsewhere). If they then found that smoking had no effect on whether West Virginians develop lung cancer (presumably because being a coal-miner is such a risk factor for lung cancer that miners who do not smoke suffer lung cancer as frequently as miners who do), such results would surely be of interest in various corporate boardrooms in North Carolina – and presumably would be "true" for the sample studied – but would not provide a revealing picture of the causes of lung cancer generally. The medical researchers at WVU might restrict themselves out of practical necessity (the West Virginian subjects are close at hand and thus easy, like the great powers, to study), but the consequence of doing so could be very misleading.

If, as seems often the case, those who conduct their research under the shadow of the great power bias do not take such considerations into account, the consequences can be very profound. With the availability of desktop computers of staggering computational capability, the most common case selection procedure in recent quantitative studies has become to include gargantuan quantities (over a million in some analyses) of annual observations of all dyads observed over some time period, such as 1816 to 1992 (or to include all "relevant dyads" similarly observed). Thus, most current quantitative research on world politics includes interactions among the non-great powers and between the great powers, as well as between the great and non-great. However, these analyses almost always evaluate hypotheses, generated from theories or from loosely theoretical arguments, about great power interactions without consideration that what makes the arguments "work" for the great powers might prevent them from "working" for minor powers. To quote Holsti (1996: 14) once again: "Are we to assume that the ideas and practices that drove interstate wars between Prussian, Saxon, Austrian, and French dynasts in the eighteenth century must repeat themselves in twenty-first-century Africa?" Apparently we are. In the introduction to her edited volume quoted above, Neuman (1998: 17) goes on to admit that the contributors to her book generally: "find the claim for universalism by mainstream IR theorists annoying . . . " What annoys the contributors is that little or no effort is made to address very basic questions such as Holsti's.

These complaints against traditional empirical conflict analysis might strike many as contrary to the spirit of the enterprise. For many practitioners, the discipline of international relations is designed to uncover general relationships between political phenomena around the world. Thus, the idea that one set of variables is associated with war in Region A but a very different set may be associated with war in Region B suggests general relationships do not exist. Consequently, it is common in international relations research to assume away this potential. Given the advances in knowledge that empirical international relations studies have offered in recent decades, this assumption might be warranted. However, there is a large group of scholars who might inform and thus improve international relations research above and beyond what has been achieved by assuming universality: area specialists. Area specialists explicitly focus attention on one part of the world. Each sub-set of area specialists with the same state or region of focus is as guilty of potential bias as are the great-power-centric analysts. However, the *field* of area studies, taken as a whole, suggests a strong caution against unexamined assumptions of universality.

Square pegs and round holes: can we combine great powers and minor powers?

As just mentioned, a currently prevailing tendency in international conflict research is to analyze the behavior of all dyads of states with respect to hypotheses drawn from models and/or theories heavily informed by great power behavior. This assumes implicitly that all of the dyads are similar enough to make such aggregation ("pooling," in the jargon of statistics) acceptable. Colloquially this is an assumption that the procedure does not cram square pegs into round holes. In terms of the wider discipline of political science, this assumes generalists are correct and area specialists are wrong.

The previous sentence caricatures both political science generalists (whom I refer to as "generalists") and area specialists (whom I refer to as "specialists") as holding polar-opposite epistemologies on whether political scientists should study the whole as the sum of its parts (the generalist position) or each individual part as disparate pieces of an inconsistent whole (the specialist position). I am not aware of any generalist or specialist who holds such polarised opinions about epistemology, but the caricatured distinction is often made. I repeat it here because it serves a useful heuristic purpose for introducing how and why I aggregate regional parts into a global whole.

According to Robert H. Bates (1997: 166) the distinction between the two epistemological camps is caricaturized as follows:

> Within political science, area specialists are multidisciplinary by inclination and training. In addition to knowing the politics of a region or nation, they seek also to master its history, literature, and languages. They not only absorb the work of humanists but also that of other social scientists. Area specialists invoke the standard employed by the ethnographer: serious scholarship, they believe, must be based upon field research . . . Those who consider themselves "social scientists" seek to identify lawful regularities, which, by implication, must not be context bound . . . social scientists strive to develop general theories and to identify, and test, hypotheses derived from them. Social scientists will attack with confidence political data extracted from South Africa in the same manner as that from the United States and eagerly address cross-national data sets, thereby manifesting their rejection of the presumption that political regularities are area-bound.

Chalmers Johnson (1997: 172) draws the distinction quite clearly by suggesting that genuine area studies require that:

> for a researcher to break free of his or her own culture, he or she must immerse oneself in one's subject, learning the language, living with the people, and getting to understand the society so thoroughly as a participant that it problematizes one's own place as an objective observer.

Since it is impossible to gain this sort of knowledge for more than a country or two, general knowledge across countries is impossible. Finally, Ian Lustick (1997: 175) summarizes the distinction thus:

> the nomothetic (generalist) side argues that knowledge of specific cases is possible only on the basis of general claims – "covering laws," as it were – whether derived in a process of logical inference or inspired on the basis of empirical observation. The idiographic (specialist) side responds that each case is unique and that knowledge of it can be acquired only through direct immersion in the subject matter.

Clearly these caricatured positions are polar opposites. Again, I admit I am unaware of any individual dogmatically arguing that general knowledge is or is not possible. However, there are convincing arguments to be made both ways. What's more, a number of scholars operating within the sub-field of international conflict studies offer arguments consistent with the specialist position staked out above.

Raymond Cohen (1994) offers a plausible argument that the well-known democratic peace applies only to the Western Europe/North Atlantic group of states by critiquing the other areas of alleged democratic pacificity. His argument thus suggests that something specific to this large and admittedly consequential region accounts for the observed pacificity of democratic dyads. Aggregating all the world's dyads and "pretending" the democratic peace phenomenon is general obscures the fact that the Western Europe/North Atlantic group of states accounts for the finding about democracies remaining at peace with one another, and thus prevents discovery of whatever it is about this specific area that causes the democratic peace.

John Mueller (1989) explains how World War I fundamentally changed attitudes toward warfare in the West, and that since then war among such states has been basically obsolete.[4] Kalevi Holsti (1996)

[4] Singer and Wildavsky (1993) also argue war has become obsolete within the developed West but is still common in the developing world.

argues that wars prior to 1945 were Clausewitzian in type, that they were consciously selected policy options designed to affect relations with other states. After 1945, however, war is fundamentally different. What Holsti calls "wars of the third kind" (wars fought within states or because of political weaknesses internal to states) have replaced the earlier types of warfare. According to both authors, cross-temporal aggregations of data are thus inappropriate unless they take into account how attitudes toward war or the nature of war have changed with the passage of time. Mueller's argument might suggest that aggregation of the West, where war is obsolete, with the non-West, where war is still an option, is as inappropriate as temporal aggregation.

There are also a number of quantitative international conflict researchers who explicitly reject aggregating observations of minor powers and great powers. Both Midlarsky (1990) and Thompson (1990) argue that great power wars must not be combined in analyses with minor power wars because great power wars have system-transforming consequences which make them fundamentally different from minor power wars. In so doing they are repeating Levy's (1983: 4) earlier claim that "Wars in which the great powers participate should be analyzed apart from wars in general because of the importance of the great powers and the distinctiveness of their behavior . . ."

At the other end of the spectrum are those who argue such regional or temporal distinctions are red herrings. In addition to the generalist position staked out by Bates (1997), and the general tendency within international conflict research to employ an all-dyads case selection procedure, a number of specific researchers have espoused the generalist argument. Przeworski and Teune (1970: 4) begin their primer on social science inquiry with the admission that: "The pivotal assumption of this analysis is that social science research, including comparative inquiry, should and can lead to general statements about social phenomena." Reacting to claims such as those of Levy, Midlarsky, and Thompson from the previous paragraph, Bueno de Mesquita (1990a) argues that focusing exclusively on great power wars is a use of *ex post facto* knowledge to select on the dependent variable. He concludes "There currently is no compelling basis for believing that big wars are qualitatively different in their causes from lesser disputes" (p. 169). In a separate article Bueno de Mesquita provides a detailed account of a relatively minor war that he argues had system-transforming consequences (Bueno de Mesquita 1990b).

My predisposition and training encourage me to lean in favor of the generalist's perspective rather than the specialist's. And yet, when one reads Ayoob's book (1995), Holsti's book (1996), or the essays contributed to the Neuman volume (1998), one is repeatedly struck by how plausibly expressed are the concerns that political relations are different in the developing world. The newness of Third World states, the incomplete control of Third World governments over their own people and territory, the pervasive problems of poverty and lack of physical and political infrastructure, all combine to make a rather convincing argument that the international situation confronting Third World leaders is, or at least appears to be, different from the one confronting leaders in great power states.

Those generalists who employ an "all-dyads" approach can point to their results and conclude "no noticeable difference" across great power and minor power dyads. But this conclusion is likely based on *not having looked for a difference* since the operating assumption in such research is that there is no difference. At a minimum, the "all-dyads" researchers should look at a sub-set of their data, that which excludes the great powers. Do the relationships between joint democracy and peace (for example) persist when one only considers Third World dyads? Few or none have bothered even to ask such questions.

In terms of hypothesis testing, to the extent that this is a mistake it is a conservative one. If over-aggregation of dyads into global analyses is inappropriate, then any results actually found in the "all-dyads" studies are likely to be *even stronger* in the appropriate sub-group. But theoretically, this conservative hypothesis-testing mistake could be a major error. Specifically, if we really only have a theory about what the great powers do, then the appropriate referent group for analysis is simply the great powers. It might be that the relationship hypothesized for the great powers is operative in the Far East too, but we do not understand why this should be so based on our theory. What we need to do is enrich our theories by building context into them. For example, if we hypothesize that great powers fight wars to preserve a balance within their international system, then the way to generalize this to minor powers is not to include every minor power dyad in a global analysis. Rather, the correct way to generalize this theory to the minor powers is to think about what minor power international systems are, and to include dyads from these minor power systems in a unified, albeit not necessarily global, analysis with the great powers. If support for this unified analysis were uncovered, it would mean states in the great power system and in minor power international systems fight wars to preserve balances.[5]

In spite of my training and intellectual predisposition, I am persuaded the distinction between generalists and specialists can be treated as an empirical question. In summarizing her edited volume's central critique, Neuman (1998: 17) writes: "The criticism leveled here is not meant to imply that the whole body of IR Theory is irredeemably flawed. Rather it holds that the question of relevance itself needs to be empirically tested." In this book I react to what I call the "square-pegs-in-round-holes" issue by taking up Neuman's challenge: I treat the epistemological debate as an empirical question.

I do this in two ways. First, consistent with the argument made two paragraphs above, I try to build context into my elaboration of power transition theory. I think systematically about minor power systems, and only include cases I think relevant to my revision of the theory. Next, the first step in my statistical analyses is to determine whether pooling minor and great power observations into a unified analysis is statistically appropriate. The likelihood ratio tests conducted in chapter 5 and in the appendix are thus empirical tests of whether or not the minor power dyads are "square pegs" with respect to the "round hole" my theory expects these pegs to fit. Finally, I also allow for the possibility there might be differences specific to a given region by including a set of variables representing each minor power region I study. The inclusion of these regional variables could improve the overall fit of the statistical model to the data on war onsets, and/or some or all of them could be statistically significant. Either of these outcomes would be interpreted as support for the specialists. Finding that the group of regional variables collectively does not improve the fit of the model, or that none of them individually is statistically significant would be interpreted as support for the generalists.

As it turns out, I find that the generalists and specialists are both partially right. There is a general similarity across great and minor powers (as generalists would expect) but there are also characteristics of regions

[5] Additionally, correct specification of the relevant domain of cases applicable to the theory being evaluated will have the benefit of facilitating comparison across theories. If we restrict analysis of a given theory to the correct set of cases about which the theory speaks, we know the empirical domain of the theory. We can then compare this empirical domain to that of competitor theories. One criterion by which we judge a theory superior to a competitor concerns its empirical domain. If theory X's domain subsumes theory Y's, X is a superior theory. However, only if we correctly specify the empirical domain of our theories is such progressive comparison possible.

that make them differ (as specialists would expect). Further exploration of what these region-specific differences might be is offered in a subsequent empirical chapter. This exploration is made possible by allowing for the possibility that there is something to the specialist epistemology. As is so often the case when intelligent people disagree, the "truth" appears to lie in between.

Does the Third World matter?

If the truth lies between the specialists and generalists, then there are some similarities and some differences across various regions in terms of when wars occur. This means we might partly address questions of how similar the Third World is to the great powers, as well as how and why it might differ. In so doing, though, we might be asked why any of this information is important. We might be asked whether the Third World matters.

This question is normatively offensive. Of course the Third World matters. Most of humanity lives in the Third World. Almost all of mankind's ancient civilizations arose in the Third World and thus our species' cultural heritage springs from what we now call the Third World. Most of the material resources that facilitate the easy life those in the developed world enjoy are delivered to the developed world from the Third World. Obviously the Third World matters.

And yet, this obviously true normative reaction belies the possibility of a dispassionate appraisal of how important, specifically to those not in the Third World, knowledge about the Third World might be. Such a dispassionate assessment is at the heart of a debate within the exclusively realist security studies literature about whether the Third World matters. The number of studies touching on this debate is large, but the handful of citations I think best includes David (1989, 1992/1993), Van Evera (1990), Hudson *et al.* (1991), and Desch (1996). The unifying question in this debate is the extent to which the United States (and other great powers) should concern themselves with affairs in the Third World. Van Evera argues the Third World is largely irrelevant to the great powers. At the opposite extreme, Hudson and her co-authors argue that the Third World is more important than Europe. A somewhat more constrained, but clearly pro-Third World view is offered by David, while Desch summarizes both sides and concludes that some areas in the Third World are very important to the great powers, but primarily as military bases.

This literature is relevant to my study because I believe the empirical analyses in subsequent chapters of this book suggest the variables central to my analysis indicate when wars in the Third World are more likely. This means clues exist which might be used to anticipate and possibly diminish the prospect for war in the developing world. If the Third World matters to the great powers either in military strategic terms or in economic terms, or even if only to scholars in terms of understanding what makes war more likely, this is useful information.

I think it reasonably easy to reject nearly out of hand the statement that the Third World does not matter to the First at all. Even Van Evera, the most strident of those skeptical of the Third World's importance, is more accurately represented as suggesting that, since America's security resources are limited, it must pick and choose where it exerts influence. In Van Evera's opinion the main threats to the United States do not arise in the Third World, and thus it should not squander resources there. This is, however, clearly a debatable position (and is vigorously challenged by David and by Hudson *et al.*). I would also stress that it is not based on any empirical analysis by Van Evera. Consequently I think the specific question of whether the Third World matters to American security interests is unanswered. If the answer to this specific question is "yes," the findings of this book are important.

More broadly, a wide range of scholars operating in other sub-fields of international relations research have suggested that the Third World is likely to remain the main locus of interstate conflict for the foreseeable future. This is certainly a conclusion common to Mueller (1989), Singer and Wildavsky (1993), and Holsti (1996). If this conclusion is correct, then the findings of this book are again important.

Finally, any scholar interested in understanding the causes of war should be interested in explanations of war that account for a larger proportion of the world's actors. The realist security studies authors summarized above are basically concerned with questions specific to what America's foreign policy should be. The rest of us, concerned with international conflict more broadly, must seek as wide an understanding of war as possible. Whether this means we develop knowledge of how war differs from place to place, or we develop knowledge about how similar war patterns are around the world, a broader understanding of war must be our goal. Upon this criterion the findings of this book are important.

The tasks of this book, the extension of power transition theory to include minor power interactions, the development of a new definition of what regional sub-systems of the overall international system are, and the investigation of persistent cross-regional differences in the onset of war, raise important intellectual issues about how biased our knowledge of world politics is by the experiences of the great powers, whether we can meaningfully speak about a "global whole" tying otherwise distinct regions together, and whether the outcome of these efforts is useful. I believe all three are addressed in ways that establish the importance of the project reported in this volume. At a minimum, however, those interested in these issues should care about how, and how well, I address my tasks.

Plan of the book

The various tasks comprising this book's subject matter are intricately linked. Nevertheless, the book considers them sequentially. The idea is first to build the argument, then to take the steps necessary to evaluate the argument, and finally to consider the subsidiary question of cross-regional differences. Chapters 2–7 thus move from the general to the specific and then back to the general (with chapter 8 offering a summary, some implications, and directions for future research).

Chapter 2 describes and summarizes the theoretical origins of my effort. I draw on power transition theory. This theory has been around long enough, and has been discussed by sufficient past writers, that there are a number of misunderstandings of it from which I wish to disencumber myself. Thus, chapter 2 lays out power transition theory as *I* understand it, and highlights the strengths, while admitting the weaknesses, which convince *me* it is sufficiently well established to justify elaboration.

Having presented the theoretical origins of my project, I turn in chapter 3 to my revision of power transition theory. The revision I propose, the multiple hierarchy model, suggests the international power hierarchy has nested within it localized power hierarchies operating within minor power regions of the overall international system. Within these local hierarchies, interactions parallel those among the great powers atop the overall international hierarchy. After presenting my multiple hierarchy model I describe past thinking about regional subsystems in order to demonstrate that many others have come to similar conceptualizations, and to indicate how my work differs from these predecessors.

Chapter 4 offers a technical discussion of what a local hierarchy is. My operational definition of local hierarchies focuses on the ability to interact militarily, and calculates how power degrades as states attempt to project it beyond their borders. At some point the costs of power projection become too high to justify efforts to exert military influence any further. Beyond that point, states are not reachable militarily. Basically, my definition of local hierarchies calculates the area of the surface of the globe within which states can exert military influence, and calls the overlapping areas local hierarchies.

In chapter 4 I also discuss how I measure the two explanatory variables central to power transition theory and the multiple hierarchy model: national power and status quo evaluations. Considerations of national power are reasonably straightforward on account of a great deal of previous research by many other scholars. In contrast, evaluations of the status quo have received much less attention. Consequently there is arguably much more room to disagree with the measure of dissatisfaction than with the measure of relative power. Realizing this, and quite frankly realizing how readers may be dissatisfied with my operational definition of local hierarchies, I include a great deal of justification, elaboration, and, as possible, validation of the operational decisions I make. Chapter 4 is a long chapter because I find questions of measurement absolutely central to how we know what we know in the study of war and peace. There are no obvious measures of any of our concepts,[6] and thus it is incumbent upon us to be thoughtful in observation and measurement. Chapter 4 is long because of my attempts to be thoughtful.

In chapter 5 I present statistical evaluation of the multiple hierarchy model's hypothesis about factors that make war more likely to occur. All the statistical models reported support the hypothesis to varying extents. I include a set of variables representing the four minor power regions in my statistical models. The region-specific variables are included in order to capture differences potentially existing across the regions or in comparisons of them with the great powers. What these region-specific variables allow me to do is represent in my statistical evaluation the debate over epistemology between area specialists and generalists caricatured above. If these variables improve the fit of the model and/or are statistically significant, there is evidence the area specialists are correct and the world is not composed of uniform parts. Some of the region-specific variables are always statistically significant. This offers some evidence that the area specialists are justified in highlighting the importance of local context. Perhaps more importantly, the existence of these statistically significant regional variables allows me to estimate

[6] Political scientists seem in strong agreement on this point. Bernstein *et al.* (2000) title their essay on the difficulty of predicting political phenomena "God Gave Physics the Easy Problems." Similarly, Buzan (1991: 200) aptly reminds us: "Politics has never been a tidy subject."

the impact of changes in power and dissatisfaction with the status quo on the conditional probability of war within each region. When I do this I find the substantive effect of these important explanatory variables diminishes sharply as consideration shifts from the great powers through the Middle and Far East to South America and Africa. Inclusion of a set of control variables that could logically attenuate the relationship between the explanatory variables and the probability of war does not diminish support for the multiple hierarchy model's hypothesis nor affect the importance of the region-specific variables.

In chapter 6 I reanalyze my central propositions with a major modification to the dataset. Specifically, the analyses in chapter 6 differ from those in chapter 5 by incorporating great powers as actors within the minor power local hierarchies. This is an important analysis of how sensitive the results are to whether I "allow" great powers to interfere in local hierarchies. This reanalysis also lets me investigate whether variation in the opportunities great powers have had to interfere with local hierarchies causes the regional variations uncovered in chapter 5.

In the chapter 5 analyses the regional variable most statistically and substantively important represents Africa. This variable is also the most negative, suggesting Africa is the most peaceful region of the five I study. I refer to this odd finding as the "African Peace," and structure my subsequent discussion of what might cause the regions to differ around it.

In chapter 7 I follow up the empirical findings in chapter 5 with a discussion of the African Peace and of the larger question of why the region-specific variables matter. In essence, I try to account for the finding of important regional differences. I investigate whether the finding may be coincidental, caused by systematic measurement error, or due to some more readily understandable omitted variables. I offer a new set of analyses attempting to capture the important conditions systematically present or absent in some regions with conceptual variables. I thus try to replace the statistical significance of the region variables with conceptual variables such as underdevelopment and political instability. When I include additional variables in my statistical estimations, the substantive significance of the Africa variable is reduced (i.e., Africa appears less different), but the statistical significance of the Africa variable remains (i.e., Africa still is different). I close chapter 7 with a somewhat more impressionistic consideration of why the regional differences are so persistent.

In summary, chapters 2 and 3 describe power transition and the conceptual modifications I make to it in order to render it applicable to minor power interactions. This is the first task of the book. The second task is undertaken in chapters 4 and 5 where I present what I mean by a local hierarchy or regional sub-system, and evaluate the multiple hierarchy model within these local hierarchies. In chapter 6 I undertake sensitivity analyses by reanalyzing the multiple hierarchy model with a larger set of cases constructed to allow for great power interference in minor power local hierarchies. Finally, in chapter 7 I address the question of persistent differences in how well the multiple hierarchy model accounts for interactions across the regions. This is the book's third task. Along the way I provide a great deal of commentary on and consideration of the three intellectual issues discussed in the first section of this chapter. I also try to anticipate the many objections I understand others might have against the many choices I make along the way. I am aware of the grounds for criticism from which an effort as broad as mine can be attacked. However, I am asking some big questions, and with big questions there is always a lot of room for disagreement. I trust the disagreement will be productive.

Conclusions

The commentary provided by this introductory chapter might lead some to believe that no past international conflict researcher has paid attention to the question of whether or not his or her argument applies globally *and* locally. The extent to which this question has been ignored is impressive, but imperfect. One of the many reasons Bueno de Mesquita's *The War Trap* continues to be read is that in it he addresses exactly this question. In his statistical evaluation of hypotheses drawn from his expected utility theory, Bueno de Mesquita investigates the "cultural objection" (1981: 137–140), "the belief that politics in one place differs in idiosyncratic ways from the politics in other areas" (p. 137). This is especially interesting in terms of Bueno de Mesquita's rational choice model because one of the specialist critiques often raised against generalists (and specifically so by Johnson 1997), is that in other cultures rational expected utility maximization does not occur. If these specialist critiques of generalist arguments are correct, Bueno de Mesquita should be especially unlikely to find support for his hypotheses, such as that positive expected utility for war is common among war initiators. Nevertheless, Bueno de Mesquita demonstrates that if one looks individually at Europe, the Middle East, the Americas, or Asia, the initiators overwhelmingly have positive expected utility for war, whilst the targets equally overwhelmingly have negative expected utility for war. In fact, the strongest relationship is not found in the Western, European region but rather in the Middle East (the relationship between expected utility and whether a state is the initiator or target is also stronger in Asia than in Europe). Bueno de Mesquita thus breaks down his sample into regional sub-sets and reinvestigates his hypotheses, specifically asking whether they apply in disparate regional contexts. One might quibble with some of the specific decisions he makes in doing this, such as which states are assigned to which regions, but he is nevertheless to be highlighted as unique in addressing the empirical question of regional applicability. I am unaware of anyone other than Bueno de Mesquita who has published research similarly breaking their sample into regional sub-sets and addressing this question.[7]

Throughout the chapters to follow I adapt and then apply a great power theory to analysis of minor power interactions. I make every effort to do so with sensitivity to the problems to which my application may fall victim. Critical readers might nevertheless find the application naïve, or at least ill-advised. Those strictly adhering to the specialist perspective might be especially prone to deny the value of my effort, or to conclude *a priori* that the application is doomed because even though statistical regularities are uncovered, they may be trivial or otherwise of very little substantive importance. I try to address such concerns in more detail in the chapters to follow, and especially in chapter 7, but would beg the forbearance of these readers for the following reason. Since I attempt to be so sensitive to the potential pitfalls possibly preventing the application from succeeding, one might view my effort to extend power transition theory as being more likely to succeed than other such efforts at extension (such as Bueno de Mesquita's). The test of whether my revised power transition model applies is thus a relatively easy test. If the application does not work, we have evidence of how difficult such minor power extensions of great power arguments are. We also, I think, would learn something about how our theories, our research designs, and even the organization of our datasets do not reflect reality in the underdeveloped world. This would then constitute a sort of negative knowledge, a knowledge that efforts like mine do not easily work. Imaginative researchers might be able to make very good use of this negative knowledge in subsequently explaining *why* my effort was

[7] Although, for examples of thoughtful treatments of great power–minor power considerations in empirical analyses, see Goldsmith (1987), Papadakis and Starr (1987) and Rasler and Thompson (1999).

less than successful. I do not believe my effort fails. But I will admit that the application of power transition theory to minor powers provides an incomplete picture of war onsets. Several interesting directions for future research are nevertheless identified.

At a minimum, I think research such as I undertake in this volume is very important for scientific progress in the study of war and peace. Many researchers within this sub-field accept Lakatosian standards for identification of what is progressive scientifically. Lakatos's methodology of scientific research programs was first used to evaluate IR theory in the 1980s (see Bueno de Mesquita 1985b, 1989; Kugler and Organski 1989) by scholars applying it to their own research. Recently John Vasquez (1997, 1998) has applied Lakatosian standards to realist theory, and found it sorely wanting. According to Lakatos a scientific research program is progressive, among other things, if it is extended and updated in a way which allows users of the theory to understand the occurrence of additional phenomena while still understanding the occurrence of previously explained phenomena. This is referred to as excess empirical content. Like many researchers within the sub-field of international war studies, I accept Lakatos's standards for scientific progress. I believe the effort reported here is evidence of scientific progress within the power transition research program.[8] In this book I begin with the resource of a widely supported, established theory of great power politics. I then enlarge this theory's empirical domain to offer an hypothesis about war and peace interactions within minor power regional subsystems. I evaluate whether this hypothesis is empirically corroborated. I find that it is, but that the corroboration itself suggests, if only by hints, clues and impressions, that much more theoretical elaboration may be available by linking the process of political and economic development to the occurrences and purposes of war. The intertwining of these issues and questions made the book fun to write; I hope it will prove not only interesting, but also fun to read.

[8] For an independent assessment that applies Lakatos's methodology to evaluating how progressive power transition theory research has been, see DiCicco and Levy (1999).