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THE ETIOLOGY OF INTERSTATE WAR: A NATURAL HISTORY APPROACH

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INTRODUCTION

There may be some disagreement on this, but I subscribe to the proposition that a field of study suffers when there is little awareness of its historical past. Somewhere between obeisance to Thucydides and Machiavelli on the one hand, and reference only to the hot button arguments that surfaced while you were in graduate school, there would seem to be a prudent attention to some "relevant" historical background. One virtue, of course, is to reduce the frequency with which the social science discipline re-invents the wheel, wasting many student-hours and research-hours working through a line of reasoning that has already been nicely and clearly articulated by one or more of our predecessors. Another is to help avoid repeating errors that have been made – and often rectified – in the past, and this can range from matters methodological to those of an empirical or conceptual sort. Yet a third virtue is to permit taking advantage of some painstaking and incremental gains that have been made and then all too often forgotten. A fourth and frequently needed virtue is to be able to recall how a certain concept was understood at its inception or earlier use. While there is little that we can do to prevent slippage, if not corruption, in the natural language of everyday practice, the scientific community might want to attend more diligently to matters of semantic clarity, continuity, and precision.

One way to take advantage of our intellectual legacies and to avoid the above-mentioned problems is to pay closer attention to the education of our graduate

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students, many of whom will be academic colleagues in less than a decade. They should be spared the presence of very recent PhDs in gateway and other graduate level seminars, especially since the odds are good that the latter's own graduate education was also excessively at the hands of new and immature young scholars. It is, of course, desirable that our graduate students be exposed to the latest ideas, methods, and findings, but it is equally desirable that they learn more about the field's intellectual origins and turning points. My advice would be to give the first and second year graduate students – in world politics and elsewhere – *some* exposure to very junior professors, but 50 to 75% of their courses might well be taught by those of greater experience in their first two years.

A second and equally effective way to avoid the loss-of-memory problem is to see to it that the courses we teach, panels and conferences that we organize, articles that we write, and books that we publish and edit take sufficient efforts to preserve and build on the more helpful work that has gone before. This anthology serves as an excellent example of how we may benefit from a rich mix of scientists who span and represent a goodly range of cohort groups. In this connection, with such founding fathers as Lewis Richardson and Quincy Wright, and such powerful reference figures as Kenneth Boulding and Karl Deutsch no longer alive, it looks as if I now find myself one of the more senior perpetrators of the scientific study of world politics. Thus it is an honor and a responsibility to contribute the opening chapter to this exciting and valuable collection.

STAGES IN THE EVOLUTION OF OUR INVESTIGATION

In light of these remarks, it might be useful to begin by suggesting one possible way of differentiating the stages that mark the development of a particular field of study – in our case, the search for an explanation of war between territorial actors, usually states. The first and longest stage is that of intelligent speculation – ranging perhaps from Thucydides to Morgenthau or Aron – in which we observe contemporary wars and near-wars, invent some plausible explanations, and then ransack history for those cases that seem to support our hypotheses. The opposite side of the same coin has us form certain generalizations from our recollection of the salient past and then try to apply them to more recent or contemporary events.

The Second Stage: Demonstrating Possibilities

The second stage – a very long time in coming – is nicely marked by Lewis Richardson's studies following his grim experience as a conscientious objector and ambulance driver with the 16th French Infantry Division during World War I; as the cartoonist Bill Mauldin quipped "If this is the war to end all war, why did they give it a number?" Deeply moved by the mindless slaughter in Flanders and other killing fields, on returning home to England he began to read the military and diplomatic historians and concluded that he could do considerably better. While probably unaware of these names, he followed the urgings of Buckle (1885), Quetelet (1828), Condorcet (1785), Durkheim (1895), and other apostles of quantitative social analysis who had gone largely unnoticed in European intellectual circles. In any event, Richardson – who was by profession a meteorologist – began to work on the etiology of "deadly quarrels," invoking mathematical models, operational coding rules, and statistical analysis. As is well known, few of his many papers were accepted by the scholarly journals, and it was not until seven years after his death that a group of his admirers published *The Statistics of Deadly Quarrels* and *Arms and Insecurity* (both 1960).

I would add three other scholars to this second stage. Just as important as Richardson, in my judgment, was Quincy Wright, an American professor of international law whose father was a mathematician and whose older brother Sewell had been a pioneer in the development of mathematical biology. The story has it that the latter admired Quincy's scholarship and his preoccupation with the scourge of war, but lamented the lack of methodological rigor in his work and thus introduced him to scientific method; hence the fifteen-year project that culminated in the monumental *Study of War* (1942). Then there was Pitirim Sorokin whose volume III of *Social and Cultural Dynamics* (1937) contains a rigorous and remarkably prescient body of data and discussion on armed conflict going back to the Roman legions. Finally, I would include as the fourth horsemen of the second stage the Polish economist, Ivan Block, whose six volume *Future of War* (1899) is really a quantitative and highly operational history of warfare over several centuries. His extraordinary rich and detailed study led to three major conclusions. First, that the appearance of the machine gun would permit a small number of men in trenches behind barbed wire to hold a long defensive line indefinitely against numerically superior infantry and cavalry forces; written in the 1890's, this was an uncanny prediction of what Richardson observed in the battles at the Marne, Somme, and Verdun. Second, he predicted that the cost of preparing to fight and then conducting such a war would be monumental. Finally, being an economist with that discipline's touching faith in the rationality of those in power, Block believed that the European elites would thus turn to other means of interstate conflict resolution!

There are others who come to mind, but these four scholars mark that four decades or so period in which the basic ideas of scientific method were first brought to bear on the explanation of war. Unfortunately, World War II intervened not only to put a halt to this promising systematic research, but to demonstrate beyond a doubt how desperately we needed it. Worse yet, with the end of that tragic set of episodes, the victorious major powers quickly turned on one another, easily mobilizing what Halberstam (1969) might have called the "best and the brightest" on both sides of the cold war.

The Third Stage: Correlates of War and Natural History

As a result, the beginning of the third stage was delayed by more than a decade, until a "critical mass" of social scientists came together at the Stanford Center for Advanced Study in the Behavioral Sciences, and put together plans that culminated in the establishment of the Center for Research on Conflict Resolution at the University of Michigan in 1957. The group that came together was, in any intellectual sense, quite revolutionary. They were explicitly committed to the proposition that rigorous social science research could generate knowledge about individual and collective behavior that could not only tell the elites and the attentive publics what sorts of behavior were conducive to the onset and escalation of disputes, but also what sorts of decisions and behaviors might help to head off serious disputes between and among states (as well as in other social settings). Without getting too autobiographical (Singer, 1988), I found this focus irresistible, and after a postdoctoral year in the Harvard Department of Social Relations, eagerly accepted an offer to join the Department of Political Science at Michigan in 1958. Working alongside of Robert Angell and Kenneth Boulding and tremendously stimulated by them and the others around the center, I began to lay out the research strategy that would mark the third – correlates of war – stage. Despite our eagerness to come up with findings that could, as soon as possible, help to turn the Soviets and Americans away from the menacing road to Armageddon, our sense was that a more modest and incremental research strategy was the responsible way to go. We saw the writing and thinking of the first stage as heuristically valuable, brimming with possible insights, and the work of the second stage as evidence that the war question could indeed be researched in the scientific mode. The time seemed appropriate to blend the traditional with the behavioral, and rather than succumb to physics envy and suffer from premature explanation, my/our decision was to pursue systematic description and empirical generalization; in short, we would embark on the natural history stage in war-peace research, much as did Charles Darwin in biology. He never stopped observing and he never stopped thinking, and – despite occasional suggestions of barefoot or dustbin empiricism – our Correlates of War colleagues continue to think, observe, and count now, thirty-five years after the birth of the project.

As I look at the work of my former graduate students from the beginnings in the early 1960s through the late-1990s, as well as others in that social scientific cohort (Vasquez, 1993), it seems that our third stage is still with us. Quite clearly, the emphasis is to articulate interesting queries about war and peace, summarize earlier speculation and research findings, lay out a sensible research design, and put to the test a well-formulated model that embraces the central query. Further, it is neither desirable nor likely that we try to move too rapidly out of this stage and the research paradigm that it entails. We still know so little about "what goes with what", and even where we have seen a respectable body of research, it often shows more inconsistency than a cumulative process should tolerate. But as this natural history/correlates of war work goes on, we need to attend to two key issues. One is the distinction between additive and integrative cumulation; as all of us keep repeating, just piling up more and more findings on related questions may strengthen the case for one or another important generalization, but it may not carry us very far toward understanding how these generalizations mesh together. To tinker with a metaphor used by Guetzkow back in 1976, we are certainly moving ahead in producing atolls of knowledge (he called them prematurely, "islands of theory"), but are not doing as well in bridging them together into an integrated archipelago. As I see it, advances on that front will mark the gradual transition to our fourth stage; it will rest heavily on natural history, but the emphasis will be somewhat less on descriptive generalization and somewhat more on explanatory interpretation that rests on such generalization.

From a detached scientific perspective, we need not be in a hurry; the more solid and numerous our generalizations, the more likely we are to get our explanations right. On the other hand, most of us are not only committed to the generation of knowledge in the best scientific tradition; we are also committed to what I call, not an "applied science", but to an *applicable science*. To that extent, it seems perfectly reasonable to begin thinking ahead a bit toward the possibility of one or more explanatory models that offer sufficiently compelling stories about how inter-state disputes go from minor to militarized to all-out war. To effectively contribute to more adaptive policies and behaviors on the part of those whom we generously call statesmen, we must have such stories, and they need to be pretty credible to us before we offer them to those who decide the fate of nations.

The Fourth Stage: Beyond Covariation

What sorts of research can help us to move beyond the correlates of war/natural history stage, and how far along might we already be? As I see it, there are several convergent research paradigms worth considering in this context. Quite clearly, the leading candidates for shaping the next stage are – and ought to be – models that reflect the formal – deductive – mathematical mode. This orientation reflects the reasonable view that the sooner we can use formal and well-specified models that can both integrate recent findings and suggest the next steps in empirical investigation, the better. Whether expressed in game theory, structural equations, or computer simulations, this orientation reminds us that we need to be thinking of how we move from empirical descriptions of what goes with what to the question of "how come?" In more sophisticated terms, this is the impending transition from the third stage – the correlates of war and the natural history by which disputes escalated to war – to more aggressive efforts to *explain* these processes.

Quite clearly, the stages that I suggest are not quite operationally distinct at their borders nor are they now mutually exclusive. The overlap is impressive, given that different research programs are underway at different times and in different places. For example, the really valuable systematic natural history type of research is today very strong in the public universities of the mid-west and south of the U.S., and is just beginning to surface on the west and east coasts of North America and isolated locales in Scandinavia and Japan. And at the same time we find a handful of studies underway in these same places, as well as in continental Europe, that seem to leapfrog the data-based, empirical generalization program and move beyond that to the articulation of formal "theories" that purport to explain and account for the research results generated by those colleagues of a more incremental turn of mind.

Having suggested that the natural history/correlation stage must not be the final one, let me nevertheless urge that it is probably the most crucial one – providing a powerful link between the anecdotal and the explanatory – let me now turn to a few considerations that might be kept in mind as we seek to make this stage more productive than it has been over the last three decades or so.

SOME RELEVANT ASSUMPTIONS

In any scholarly endeavor, one begins with certain assumptions, but all too often these are unknown, unnoticed, and unarticulated. Those that will shape any investigation are of three sorts: *ethical*, or what the authors and researchers consider to be morally right, wrong, or in between; *epistemological*, or what criteria we use to evaluate knowledge claims; and *empirical*, or what we assume about the way that the world works, used to work, and is likely to work in the future.

Ethical Assumptions

If not for the awesome volume of hot air on the topic, we could pass this dimension with a genuflection or two; but a look at the book titles, fellowship themes, conference announcements, and the content of the journal of the same name (*Ethics and International Affairs*) reminds us of the confusion – and yes, the ethical confusion – surrounding the study of war and peace. From my perspective, the confusion emerges out of the distinction in the west between facts and values, or worse yet, between empirical or positive theory and "normative theory". Typically, we treat ethical questions as if the answers are, and always will be, matters of subjective personal opinion, beyond the ken of scientific inquiry, and thus in the realm of "normative theory." This ignores the consequential dimension of ethics and illuminates another of the ways in which economic and mathematical reasoning have muddied the political waters. That is, individuals and groups are rarely able to select and automatically get their preferred outcome; what we do is select one or another preferred outcome and *then go on to select a strategy* by which we hope to achieve that outcome. To poke fun at some of our game theory and economics colleagues, life is more than preferring vanilla to chocolate and strawberry to both, and the global system is not just another ice cream parlor. Put simply, we not only make ethical choices on the basis of – inter alia – the likely consequences of such choices, based on our understanding of that connection, but also in light of our understanding of their achievability. Both of these are a function of our *knowledge* and thus very much a scientific matter.

Having said this, we need to confront the link between our research and teaching on the one hand and our normative stance on war on the other hand. To come at the question indirectly, those of us in the US who were doing systematic research on interstate war during the half century or so of Soviet-American armed rivalry were viewed with suspicion from at least two sides. The more evident criticism came from the "left" – those who opposed for example, the US role in the Vietnam War, saw that role as a creature of the Pentagon's (and Rand's) quantitative "whiz kids," and often believed that any quantitative research was inspired and funded by DOD. Perhaps there was a germ of truth in their assertion, given the links to Project Camelot, the counter-insurgency work of some, the largesse of ARPA (the Advanced Research Project Agency), and the Office of Naval Research, but a list of those doing (and publishing) scientific work on international conflict as revealed in the abstract books of *Beyond Conjecture…*(Jones and Singer, 1972) and *Empirical Knowledge...* (Gibbs and Singer, 1993) would show only a handful of co-opted Cold Warriors.

More to the point – and without for a moment suggesting that scientific method will single-handedly save humanity from the scourge of war or any other evil – we need to note that if we merely rely on the forces of light to overcome the forces of darkness, we are doomed to repeat most of the evils of our past. Wealth, power, prestige, and arrogance will usually carry the day against innocence, altruism, and humaneness. The defenses of the weak, and the most powerful weapon of decency, rest on knowledge and evidence. Most human beings live in a state of partial thralldom, manipulated, cajoled, and conscripted by the politicians and the priests who legitimize the tribalisms and the superstitions. They and we are the victims of our collective ignorance, an ignorance shared by the governors and the governed, even when the former wrap themselves in the garb of patriotism, throw up a smokescreen of alleged expertise, and hide behind a veil of confidentiality, classified information, top secret double talk, and the mystique of "the national interest". Nor is more education of the sort we still dispense likely to do much good; those who decide for war or peace (and those who act and speak for them) are usually the better educated in any society, and our grief is no less for that. In sum, the ignorant of the reformers are seldom a match for the ignorant of the establishment, and until we know much more about the etiology of war and can make that knowledge accessible and credible to the national security establishments and their critics, our efforts will remain largely academic.

Less evident but equally naive and more destructive was the skepticism from the "right": those who saw us as questioning not only the wisdom but the morality of the national security establishments; more than one of them would ask: "don't you think that we know what we're doing?" That is, if we trusted the wisdom, competence, and values of those people, we'd have no need to do research into the questions of war and peace. Reconsidering those cold war experiences from the perspective of a moderately dovish and quantoid consultant with a "top secret" and a "Q" clearance from my Naval Intelligence days until about 1970, I can give voice to these ethical issues. Quite simply, the answer should be "no, we trust neither your competence nor your ethics." Some examples: the US using the atomic bomb on the cities of Hiroshima and Nagasaki, when the Japanese were ready to surrender (according to Hap Arnold, Curtis LeMay, Tooey Spatz, Douglas MacArthur, Chester Nimitz, William Leahy, William Halsey, Ernest King, and probably, Dwight Eisenhower, inter alia); carpet bombing in Vietnam; training and funding state terrorism in Central America; the Japanese using bacterial weapons on Chinese villages; the Iraqis using poison gas on the Kurds; the Russians assaulting Chechnya; the Serbs engaging in "ethnic cleansing"; and of course, the Germans and the holocaust. The list is tragically long, and we've not even mentioned the super-powers and their genocidal "deterrence" capabilities and doctrines during the cold war.

While the list of items on the ethical agenda can be quite lengthy, let me wrap up for the time being with one more item. One has to do with how we finance our more resource-dependent investigations, especially when the generation or acquisition of data is involved. Some of us have been criticized for relying on the National Science Foundation for support on the grounds that we thus become beholden to the U.S. government, if not to the Pentagon or CIA, and no longer free to arrive at our scientific conclusions. The critics need to know two things in this context. First, NSF is the least politicized of any of the major foundations, and consistently funds work in world politics that runs the gamut from left to right and from hawk to dove. Nor do they impose any restrictions when it comes to publishing our results.

By contrast, the large private foundations (Ford, Carnegie, Rockefeller, MacArthur, Social Science Research Council, and by extension, the U.S. Institute of Peace) all tend to recruit from the same pool, read the same magazines, go to the same conferences, and drink from the same fountain. Not only is it rare to find a bona fide social scientist on their staffs (despite many PhD's) but worse yet, they seldom support scientific work in world politics. Their board members, senior officers, and professional staff members *are*, or are alarmingly close to, the foreign policy establishment. Thus whether we seek and accept funding from NSF or CIA or DOD or AID or ACDA or ONR or the White House, or whether from the non-governmental sector, it does not really matter. My early conclusion was that there is no such thing as "dirty money" for the simple reason that there is no "clean money." The federal government gets its money through means that range from misinformation to extortion and from enticement to coercion, while the foundations rest largely on the fortunes of the robber barons of the 19th century and the buccaneers of today's "free market" jungle. If we raise it honestly and use it rigorously and openly, it should be clean enough for the most finicky among us.

Epistemological Assumptions

While I see the ethical dimensions of our research and teaching roles as relatively clear-cut, the same cannot be said of those of an epistemological sort. We have a range of interesting and important differences among ourselves, and the other papers will make this abundantly clear. Let me lay bare several of those premises that I would like to see in evidence as our enterprise continues to move forward.

I begin with the one that will cause little grief in these precincts, but could lead to howls of anguish within the Modern Language Association; reference is, of course, to the syndrome know as "deconstruction," and is usually associated with those schools of thought that could be called "the posties," embracing the post-modern, post-structural, post-behavioral, and most germane to us, the post-positivist outlook. I make no pretense of being on intimate terms with colleagues of those persuasions or familiar with the distinctions among them. As I understand this fairly recent orientation – and it may go back to Karl Mannheim – developing a science of human behavior just is not possible for several reasons: a) there may not even *be* any empirical social reality; it is nothing more than imaginings given credibility by the language we use to describe these fictitious happenings; b) even if there *is* a social reality, we can never apprehend it because every human being is the product of his/her cultural identity, gender, age, profession, education, and accumulated personal experiences; and c) the representations of reality that we generally accept are "socially constructed," created and imposed on societies by the elites as an instrument of control.

A milder – and less nihilistic – version of the "mission impossible" school doesn't deny the existence of an empirical reality, but argues that we positivists define it too narrowly. For example, Alker (1996) suggests that our "data" must include not only the alleged historical and empirical phenomenon that we attempt to describe and explain, but also the reports about alleged observations of such phenomena, sometimes referred to as a call for more hermeneutics and less exegesis, as in biblical studies. Coming closer to the perspective of our small but growing "epistemic community" (Haas, 1965), are those in the tradition of Bull (1968) who – with a touching faith in their own observational accuracy and perfect recall – question the need for all this scientific paraphernalia, and assure us that the classical methods of the historian will do just fine. These traditionalists might want to listen to Mannheim more attentively; a good scientist attends to rigorous methods of observation, measurement, analysis, and inference simply because we *do* take some of the latter's warnings seriously.

In order to rectify all these failings of the contemporary scientific scene, we are offered quite a variety of dubious epistemology. Already familiar to the quantitative world politics community is Dessler's effort to borrow from meteorology the idea that we need to, and can, "identify the mechanisms through which specified outcomes occur" or "the real structures that produce the observed phenomena" (1991, 343 and 345). But at the end of this thoughtful essay, we are left with no guidance toward this end beyond the strictures of "scientific realism" (Bhasker, 1986) and the belief that causality does indeed obtain in the referent world. This line of reasoning will of course lead to the articulation of increasingly ambitious "theories" that are in truth, little more than speculative models that rest on problematic and far from operational premises. In my view, causality is a chimera invented to help persuade us that we really can *know* how the world works, and it might best be replaced with the concept of *explanation.* That is, as we uncover more and more regularities and covariations – and thus generate an increasingly rich and credible description of the ways in which all sorts of events and conditions go together empirically, we will arrive at increasingly accepted *explanations*, and as these persuasive "stories" are combined with what we think we know about individual and group behavior, the closer we will come to legitimate theories – in the sense of codified knowledge that commands the assent of our most capable colleagues.

Considerably more unnerving, and further down the self-styled "realist" path is an approach that is called "evolutionary epistemology" intended to resolve the realist vs. relativist impasse that arose out of the alleged passing of positivist and empiricist methodologies (Azevedo, 1996). These recurrent debates over epistemology in the social sciences need not surprise us, given how poorly scientific method is taught and how dubious the status of the social sciences in the academic world. Further, when we read those philosophers who tend to support and illuminate the scientific approach to social phenomena, we find that many of them are as confused as they are inaccessible; I find Hempel (1966) and Popper (1959), not to mention Harre (1970) and Lakatos (1970) not very helpful. Like religion, philosophy of science should be kept out of reach until we are sufficiently mature and experienced to be adequately immune to their all-too-plausible blandishments!

Turning from the doubters to the doers, let me lay out in telegraphic form some of the more relevant empirical-epistemic assumptions about the "nature of" the universe and the social systems – sometimes called "artificial" for reasons that still baffle me (Simon, 1969) – of concern to us, and ways of comprehending these systems:

1. The global system and the social groups that comprise it evince highly regular and thus recurring patterns, but we have only begun to discover them and confirm their existence. There are law-like regularities, but we still have only a dim view of them.

2. Despite the existence of all these regularities, they are far from deterministic; while certain types of states respond over and over to the same stimuli in the same context in the same way, there are plenty of exceptions, giving us statistical distributions rather then deterministic and perfectly uniform laws.

3. Even when we find extraordinary regularities, it is worth noting that the "causal path" between background conditions and behavioral outcomes can vary considerably; there are several ways to go from a given set of initial conditions to a well-specified outcome.

4. Regularities can take several forms, of which the simplest is a trend line in the magnitude of a given attribute of a given system; inasmuch as every social system is an evolving one, rates of change in such an attribute are also subject to change over time.

5. Cyclical patterns are often found, usually not in fixed intervals of real time, but in the form of recurring sequences or the order in which events and conditions follow one another.

6. We are not likely to uncover fully deterministic regularities not only because all the systems and sub-systems that we study are evolving, and in sequences

that may well be irregular, but also because there is an *inherent randomness* in their behaviors and interactions vis-à-vis one another.

7. While the magnitude of such stochastic phenomena will diminish as we reduce (inter alia) measurement error, erroneous simplifying assumptions, mis-specifications of our models, and the factors subsumed under the error term, it will never be reduced to zero; as asserted above, this residual randomness is inherent in the universe.
8. Our search for the law-like regularities in matters of war and peace will advance erratically as a result of both empirical findings and theoretical insights; we will not get very far relying on only one or the other, despite pendulum-like fashions; systematic, data-based investigations are rarely "barefoot" or "dustbin" empiricism, nor is the construction of formal models merely "intellectual gymnastics."
9. In our search for an explanation of inter-state war, we may discover empirically that two, three, or more sub-types arise out of systematically different conditions and processes, but we can always increase the goodness of fit by adding more differentiating variables; if not careful, we end up with a different model for each of the eighty-odd inter-state wars since 1816, and we can leave case studies to colleagues of the "no two wars are alike!" school.

Empirical Assumptions

Turning now from my ethical and epistemological assumptions, I begin to close in on my assignment, but since the theoretical substance of our work is so strongly influenced by those two sets of considerations, there is little need to apologize, nor is this intended as a diversionary tactic! From my perspective, the single most crucial assumption we make in developing an explanation for war is not whether the interstate system is anarchic or hierarchical, constant or evolving, ordered or random, nor is it whether the states in conflict are democratic, equal in capabilities, or quarreling over territory for example. It is *how we conceive of the national security decision process*. I say this so bluntly to emphasize my rock-bottom commitment to a "reductionist" model of world politics, in which the human beings who make the decisions for war and peace are the central actors in the narrative that is supposed to describe how certain conditions and events at several levels of social aggregation combine to move one or more of the states toward or away from the precipice. The decision process can be thought of as the funnel through which must pass all those global, regional, and domestic factors that will affect the behavior of the state vis-à-vis its neighbors, allies, and adversaries; nothing happens in the way of a state's behavior until the stimuli have passed through the decision process. There are several interesting characteristics of this process, and I enumerate them here: 1) Which individuals and groups are involved, in the fairly direct sense of the word; 2) How discrete and distinct are their decisions; 3) How do they define the "national interest" if I may use one of the most ambiguous and self-serving phrases in the lexicon; 4) What are the general decision rules.

While appreciating that there will be some variation across cultures and nationalities, historical epochs, and issues on the agenda, and that crises will not be addressed in the same way as day-to-day routine matters, the similarities are quite overwhelming. Thus, my general model – applicable to most bureaucratized and politicized societies, ranging in size and modernity from Belgium to China, from Bolivia to India, and from Ghana to Russia – begins with the assumption that the decision process involves a lot of people from several ministries, numerous offices, and various parliamentary factions, not to mention domestic and foreign interest groups, each with somewhat different priorities. Coalition formation is ever present. Second, and partly for the above reason, the process is not only lengthy, but also continuous with all sorts of feedback loops, erratic change of pace, and frequent redefinition of the problem. Not only do the players and the agenda change, but even when it appears that a move has been agreed to, it can often be eroded or reversed; worth noting, too, is that such closure at the decisional level can be questioned, altered, and sabotaged in its execution.

From this set of assumptions, it readily follows that, no matter how we try (Clinton, 1996), there is no way to objectively define "the national interest" or specify the criteria by which we identify its role in the foreign policy and national security sectors. Each player brings his/her own factional, bureaucratic, and personal preferences, perceptions, and predictions to the process. Further, as I have urged on several occasions (Singer, 1972 & 1991), in the absence of a solid, shared, and credible body of knowledge as to which of types of states behave in which manner under which conditions and in response to which stimuli, there is no quickly recognized consensus as to which policies are most likely to produce which results, and thus no alternative to the loose and shifting coalitions as they (gradually or rapidly) move toward a decision. This, incidentally, is why almost every policy decision must be explained in terms of more than one consideration, as each contributing person or group signs on for somewhat different reasons; illustrative in US policy are dropping the atomic bombs on Japan in 1945 or attacking the Iraqis in 1991. My students recognize this as "Singer's First Law" in which it is pointed out that *nothing* can be explained by one variable alone.

All of this, in turn, suggests the difficulty of identifying the operational code or decision rules: it can be done only – whether in more autocratic or more democratic societies – via very careful inferences based on fairly detailed and disparate bodies of evidence. Again, by way of illustration in the US context, consider the reluctance to intervene militarily in foreign disputes, crises, and disasters in light of such alleged "doctrines" as those of Nixon, Weinberger, Powell, and in due course, others. This leads, in turn, to the need for extreme caution in our embracing any single model of national security decision making, even in the more restricted case of initiation or entering into interstate war. One thinks of such widely accepted explanations as the "realpolitik" of Morgenthau (1948) or Kaplan (1957) or Blainey (1973), or the more sophisticated expected utility model of Bueno de Mesquita and Lalman

(1992). We need usually to ask *whose* utilities, whose subjective probabilities, whose expected gains and losses, etc.

To be sure – and scholars as knowledgeable as Simon (1969) and Riker (1962) have made this point – we can adopt the "as if " strategy – also known as the teleonomic as distinguished from the teleological – approach; rather than claim that a small group of competent, highly informed, and unambiguously patriotic experts actually sit down and calculate expected utilities, we merely assume that the policy behaviors that emerge out of the decision process are remarkably close to what might have emerged *if* such a calculus *had* been followed. Worth noting is that some of our colleagues, notably Bueno de Mesquita (1996) have done a rather successful job in post-dicting as well as predicting on the basis of such a strategy; but the jury is still out.

One consequence of this point of view is that we need to return, but with greater rigor, to research on the decision process. On the one hand, it made perfect sense during the first three decades of systematic research on war and peace to "black box" the process. It gave us the time to better understand the input-output association: under what ecological and interactional conditions which types of states and societies showed which types of behavior. We know quite a bit more about the conditions of state behavior now than we did in the late sixties (Geller and Singer, 1998), and are thus in a better position to infer which sets of decision rules – throughput – are more, or less, likely to be at work. On the other hand, much of the work to date (whether by psychologists or political scientists) has tended to be fairly casual, not only quite speculative but perhaps too simplistic as well.

Of course, any serious effort to get at the decision process will call into question our assumptions regarding "human nature." What sorts of people do we think are involved in the separate foreign policy establishments, the public and private interest groups, the regional and global organizations, the opinion makers, and not to be neglected, the attentive and not-so-attentive publics? Whereas Morgenthau (1948) and his "realist" brethren buy into the rather unrealistic premise that the dominant drive in *Homo Sapiens* is not so much knowing as it is power, control, and dominance, a little introspection as well as a lot of research should tell us that we are far less single-minded than that. Those of us who are normal are capable of subordination as well as dominance, cooperation as well as conflict, altruism as well as selfishness, and of course ludic as well as purposive behavior. Or to put it in the language of motivational psychology (McClelland and Winter, 1981) we respond to the need for achievement and for affiliation as well as the need for power.

In addition to this diversity of drives, my psychological assumptions embrace lability and plasticity; we not only find different drives in differing circumstances and moods, but also find a marked – but not unlimited – malleability. We differ in this respect from culture to culture and epoch to epoch, and in the same vein are vulnerable to indoctrinability in the sense that propaganda, education, and acculturation are all at work. On the other hand, this is not to embrace too eagerly the "nurture over nature" perspective. For example, Somit and Peterson (1997) make a persuasive case for the biological and genetically induced preference for hierarchical as distinct from egalitarian social arrangements, and the same may probably be said for pugnacity vis-à-vis pacifism.

To put it simply, any alleged theory of social behavior, especially concerning matters of war and peace, that rests on some simple, constant, and universal human drive or propensity needs to be looked at askance. Whether the presumed single drive is for wealth, food, land, sex, progeny, or power, it neglects a vast body of evidence, and is thus likely to eventuate in foolish – and in our case, dangerous – explanations. Having said this, however, one can always retreat into the above-mentioned teleonomic position leading to "as if" predictions; these can be treated as *null* models, and the discrepancy between empirically observed regularities and those predicted by the null model can turn out to be heuristically useful.

Let us move now from some of the simpler empirical assumptions to the more complex questions of which types of meta-model might best guide us in pursuit of those specific queries that might bring us to a compelling and powerful explanation of interstate war.

SOME META-THEORETICAL CONSIDERATIONS

While the line between empirical assumptions and those of a meta-theoretical sort is far from clear and precise, the distinction is probably worth preserving. Mostly it is the difference between one well-defined cluster of phenomena such as decision-making, public opinion, or personality, as opposed to a more complex interplay of several sets of phenomena and how they might be assumed to interact with one another. Where one comes out on some of these meta-theoretical questions will rarely *determine* the sorts of models that we put forward for empirical investigation, but it will certainly bias our tendencies.

Deterministic or Stochastic

At this level of abstraction, considering the range of meta-models, we encounter some critical issues that have drawn insufficient attention from the scientific peace research community. Unless we are quite clear and specific on these meta-model issues, we are likely to flounder as we examine and evaluate the more persuasive alternative explanations of interstate war. Perhaps the most pertinent is the eternal issue in all of the sciences: what is the mix of the stochastic and the deterministic in the stories that we invoke to account for the patterns and regularities that we turn up – or *believe* that we turn up?

Let me begin with the assertion that war is hardly ever inevitable, no matter how far down the road the protagonists have come. Whereas some of the more popular models come close to the mono-causal end of the spectrum (Blainey, 1973) and some look like a laundry list (Snyder, Bruck, and Sapin, 1962), almost all of them seem to assume that war is over-determined, my inclination is to see it as under-determined. The simile that comes to mind is that of the log floating down the river and getting caught against a boulder, thus deflecting the current against the shore, and gradually producing a small channel that eventually joins another river and so forth. While each of these events and processes can be explained in terms of a fairly deterministic principle, the likelihood of the ultimate outcome was far from inevitable. It required an intricate co-incidence of fairly improbable events. The research implication is nicely captured by Bremer's (1995) metaphor that the lock that conceals the explanation for war is not the kind that opens with a single key; rather it is a complex combination lock that requires us to know which of several sets of numbers must be entered in which sequence and following which movements.

Poisson or Periodic

A second meta-theoretical question is whether it pays to assume – or to look for – periodicity or cycles or waves in the onset of interstate wars. Given the lively debates and the moderate successes in economics and demographics, political scientists have from time to time gone off in pursuit of one or another of these possible regularities. Semantically, it might be useful to differentiate between them such that we define waves or cycles as the repeated unfolding of certain conditions and events that typically culminate in some recurrent outcome such as war; were these culminations to occur at constant (or nearly so) real time intervals, we would speak of periodicity, and begin to apply such techniques as spectral or Fourier analysis to ascertain the length of the intervals.

At the other end of this spectrum we find the premise that while certain classes of events appear and re-appear from time to time, the intervals are rarely the same; their magnitudes are best generalized by the Poisson distribution (Singer and Cusack, 1981). In the cited study, we looked for the possibility that the intervals between major power wars, measured in several ways, might turn out to be fairly consistent; they are not, as demonstrated by their goodness of fit to the Poisson distribution. These and other findings lead to the conclusion that the interesting regularities in interstate conflict are best understood in the more varied recurrent sequence mode. For example, while the evidence is not yet in, we hypothesize a systematically repetitive sequence that might be called the "from war to war" cycle. That model postulates the termination of each major war as marking a new and well-defined hierarchy amongst the major powers. Usually, this settlement is unsatisfactory to one or another of the powers – not necessarily on the defeated side – and in order to redress the arrangement, the dissatisfied state begins to increase its military capabilities. This, in turn, induces a gradual shift in domestic influence toward the beneficiaries of the military buildup who – in conformity with the well-established cognitive dissonance model – press regularly for heavier reliance on the military as a foreign policy instrument. As a result, they get into militarized disputes (Jones, Bremer, and Singer, 1996) more frequently, and – given their increased clout at home plus their touching faith in the threat and use of force – find these disputes escalating to war more often than might be expected.

Similarities or Compatabilites

Yet a third and rather vexatious meta-theoretical issue is that of the connection between inter-group similarities and their friendliness, as well as inter-group differences and hostility. This presumed correlation seems to arise from time to time and place to place as a sort of folklore, with the current decade providing an especially pernicious example. Following the demise of the Soviet empire and the passing of the cold war, we have witnessed a remarkable upsurge in violent conflict between large groups marked by differences in nationality, religion, language, and especially, ethnic identity. Journalists, politicians, and even academics seem to be accepting the proposition that these bloody conflicts can not only be explained by such cultural dissimilarities, but also are a part of their inexorable histories.

It used to be Russian and Turk, German and French, Chinese and Malay, but today it is Bosnian and Serb, Hutu and Tutsi, and Armenian and Azeri. The mere mention of some of these legendary hostilities is enough to remind us that they are far from permanent and require a lot more than cultural dissimilarity to bring them to war or genocide. As a matter of fact, some preliminary studies in the Correlates of War project suggest that most such dissimilarities rarely go to bloodshed, and typically require conditions of a serious scarcity and the agitations of ruthless politicians. More than a century ago, we were treated to the racial simplicities of Gobineau (1874) and today we have the dire but dubious speculations of Huntington (1996), and while the former tended to magnify and make more permanent these inter-group differences, the latter sees them as the root of some approaching clash of civilization – yet another crusade of Christians against Moslems?

Another side of this question is illuminated by some emerging empirical generalizations. First, we typically find that formal alliances are more likely to be consummated between societies that are fairly dissimilar in terms of regime type, religion, language, and ethnicity; to some extent this tendency is driven by the fact that disputes are ordinarily between contiguous neighbors amplified by the dictum that the enemy of my enemy should be my friend – even though he is several boundaries and cultures away. Second, as already implied, is that geographical proximity tends to correlate with political, cultural, and economic similarity while at the same time that very proximity is a frequent source of conflict. Out of this conflict arises the proposition thatwhile immediate neighbtors are likely to be similar in political, cultural, and perhaps even economic terms, neighbtors of an indirect and interrupted sort are likely to be relatively dissimilar. As Richardson observed (1960a, p. 296) in regard to cultural factors, "no general pacifying effect was found for either common language or common religion", but he did conclude that Chinese speaking groups experienced less war than expected while Spanish speaking groups experienced mor war than expected.

Moving from the cultural to the political and economic, our field is now awash in generalizations to the effect that the democratic dyad tends to be quite – if not absolutely – combat-free, but an interesting and credible variation suggests that it is not as much that these regional neighbors both have democratic or market economy regimes as that – whether democratic or autocratic, market or planned economy – they merely enjoy highly similar systems.

Looking at similarities in industrial or military capabilities, however, the story seems to be quite different. In the long-running debate between peace through parity" and "peace through preponderance", the latter seems to be coming out to be more historically accurate. Further reinforcing this generalization at the inter-state level is the evidence for the capability transition hypothesis, suggesting that as two rivals or potential rivals move closer to that similarity known as "power parity", their likelihood of a militarized dispute or war itself appears to increase.

In sum, the evidence so far is that the myriad similarities and differences between states provide toto simple and non-differentiating a basis for helping to explain inter-state war.

The Roads to War

One way to illustrate this under-determined and complex process is to note that there are quite a few roads to interstate war, and all of them have fairly frequent exit ramps. On the other hand, some of these exits are not clearly marked, and even when they are, the protagonists fail to see them because: a) they are not interested in looking for them; b) they are moving too fast; c) they are anxious as to what lies beyond them; or d) they fear an ambush from their own countrymen as soon as they slow down. As social scientists pursuing an improved understanding of the etiology of war, you might say that our central mission is to discover the location of these exits, improve their visibility, find out how to make them safer, and then go on to map the roads to which they lead.

It might not be so crucial that we concentrate on exit research were it not for the seldom-noticed reality that just about every state in the system *is always* on one or another of the roads to war. What justifies this rash assertion? It sounds rash because we are all so habituated to those war-preparation activities that we no longer notice them. First, every country socializes its children to think in terms of "we" and "they"; the world is full of foreigners, and by and large we are better or stronger or richer or nobler or more virtuous than they, especially if they are adjacent neighbors. Second, almost every state has an army, if not a navy and air force, usually recruited through conscription. Third, to encourage us to salute the flag, sing the anthem, pay for the armed forces and their equipment, deliver our young men (and women, too) to the tender mercy of the military, tolerate a lot of secrecy, and accept lethal doses of toxicity in our air, water, and soil, we are often being reminded of the vices and threats of one or another foreign state, and thus pushed along into a hostile frame of mind. Thus all are moving, at different rates, down the road to war, and our self- appointed task is to find out how to get them off that road, if not demolish that road entirely.

A more modest and interim task emerges from a more general perspective on the etiology of war. This begins with the theoretical proposition that armed conflict can be reduced or perhaps even eliminated through the existence and interplay of three sets of conditions. One of the more obvious is the existence of *effective institutions* for peaceful settlement and enforcement of agreed legal norms. Not only is the United Nations system a far cry from these desiderata, but most of the auguries seem to proceed at a snail's pace, with almost as many steps backward as forward, especially in regard to the major powers. A second peace-inducing element might be a widely shared global outlook embracing *agreed views* on justice, reason, and non-violence. Despite some isolated oases in the international desert, and some occasional intimations of emotional and cognitive growth (Inglehart, 1997), trends in this direction are depressingly weak and erratic. The third ingredient – and the one that poses the more relevant short-run challenge to us – is that of *incentives and constraints* vis-à-vis the behavior of foreign policy and national security elites around the world. In the near-absence of the political institutions and ethical norms, can we come up with some pragmatic inducements for getting off the roads to interstate war – or at least paying more attention to the exit ramps?

Obviously, most of us would not be engaged in the sort of research that we do if we did not think so. Let me try to make the connection by adumbrating a few illustrative research (and even researchable) questions, only a few of which have received serious treatment by members of our still all-too-invisible college.

1. In a dispute that has not yet become militarized, what sorts of moves are likely to convey a willingness to make concessions while at the same time reducing the likelihood that the adversary will exploit this putative sign of weakness, and resisting the domestic hawks' (a breed whose life-expectancy seems unlimited) efforts to erode the credibility of the regime?

2. When the regime of an adjacent state begins to respond positively to a domestic irredentist group and expresses "our responsibility to protect our brothers and sisters across the border", what sorts of moves might prevent this familiar process from escalating?

3. When two states are involved in a militarized dispute over a piece of territory, are there any compromises, such as redefining the boundaries or sharing the same piece of turf by using it at different times of the day, week, month, etc...?

4. As interest in preventive diplomacy increases, what are the indicators of early warning and timely assurance (Singer and Wallace, 1979; Singer and Stoll, 1984) that might improve the ability of regional or global third parties to anticipate which types of conflict are most (and least) likely to escalate?

In a follow-up to this conference, we should try to expand this list of research questions, but it is worth noting that my brief examples suggest that domestic politics may be the frequent catalyst of dispute escalation, and this raises the question of whether this conflict-inducing consideration existed in the pre-democratic epoch when royalty of seventeenth and eighteenth century systems nevertheless found it difficult to extricate themselves from foreign conflicts even without the tormentors from within.

CONCLUSION

One of the assignments to participants in this enterprise was to spell out what we considered to be the correlates of war, and most of the chapters here do a fine job on that score. But I have decided to finesse that assignment, for two reasons. First, I am reluctant to privilege a small handful of our findings, while ignoring many others. Second, a reasonably solid and highly detailed answer will be found in our recently completed *Nations at War* (Geller and Singer, 1998). I hope that I have thus paid my dues and will not be cast out as a free rider!