Introduction

War is a rare event in world politics, but it is always with us. How can we say this? On the one hand, if we note that the number of territorial states in the global system has ranged from fewer than 30 after the Napoleonic Wars to nearly 200 at the end of the twentieth century, that gives us about 400 nondirectional pairs of states in 1816 and about 18,000 pairs today. And even if we recognize that most wars are between neighbors, and thus reduce the possible pairs at war in a given year to the 40 bordering neighbors in 1816 and the 317 in 1993, the potential is never even approached. There were no wars under way in 81 of the 180 years since the modern interstate system came into being, and seldom more than one in any given year. On the other hand, 75 interstate wars led to a total of more than 30 million battle-connected fatalities among combatants, not to mention tens of millions of additional deaths among civilians in the context of these wars.

Another way to make this point is by looking at the frequency of war involvement by the members of the interstate system. During the time period under consideration here, we find that 150 states never experienced international war, 49 saw only one or two wars during their tenure in the system, 16 had three or four, and only France, Britain, Germany, Italy, Russia, Greece, Egypt, and Turkey were involved in more than 10 during the nearly two centuries since the Congress of Vienna in 1815.

The paradox is that most societies are in continuous preparation for a very rare event – but of course, if the event occurs, the stakes can be enormous. Not only does war usually bring extraordinary destruction to combatants and civilians, and to their homes and farms and roads and factories, but all too often the losers may find their lands occupied or annexed, and their wealth confiscated by the victors.
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Despite the costs of war, especially to the losers, the effort to avoid it nevertheless remains paradoxical. That is, the conventional wisdom says that “if you want peace, prepare for war,” on the premise that potentially aggressive neighbors will be deterred and intimidated rather than provoked and encouraged to follow suit. This is not the place to add up the myriad costs of these near-universal and time-honored preparedness programs — in terms of money, skills, opportunity costs, environmental damage, economic decline, and psychic and moral deterioration — but it is impossible to estimate the imbalance between costs incurred over the decades and costs avoided. And, of course, what gives the paradox a sinister twist is the frequency of the self-fulfilling prophecy — to which we will return in our discussion of the war-proneness of dyads.

In any event, the terrible destructiveness of interstate war has led a good many scholars to seek the causes of this deadly human practice. We fall readily into that group. But as those familiar with the Correlates of War (COW) Project might surmise, our ambition is somewhat more modest; the project’s title makes clear that social scientists need to know a fair amount about the correlates of war before we can speak with much authority about its causes. Until we begin to ascertain which conditions and events precede and co-vary with fluctuations in the incidence of war in world politics, it would be naive as well as presumptuous to claim any grasp of its causes.

This proposition might seem self-evident, but a moment’s reflection should remind us of how frequently it is violated; for every investigation devoted to the search for war’s correlates, there are thousands of studies that hope or claim to identify its causes or origins or roots. This need not surprise us, however. In addition to the powerful human drive to understand and/or eliminate war, there is the sad fact that until quite recently no one thought to bring scientific methods to bear on the study of war. As a matter of fact, it is only in the past century that the intellectual strategies and observational procedures of the physical and biological sciences have been understood as germane to any social phenomena. As a result, the literature on armed conflict is full of interesting, suggestive, plausible, and provocative guesses, speculations, and assertions — often labeled “theories” in the less precise academic precincts.

On the other hand, the pioneering efforts of Bloch (1898), Sorokin (1937), Richardson (1960), and Wright ([1942], 1964) have not been without result; each of these scholars helped us to make the transition

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(described in Singer 1990) from the promises of Condorcet (1794) and Buckle (1885) to the scientific peace research movement marked by the appearance of Quantitative International Politics (Singer 1968). Today, we have upwards of 600 data-based articles, largely found in five major journals – all in English – as well as two dozen or so book-length studies in which problems of world politics in general and armed conflict in particular are subjected to rigorous and systematic treatment. While much of this research follows from the Correlates of War Project that began at the University of Michigan in 1963–1964, it has by now expanded dramatically in theoretical orientation, substantive focus, and methodological predilection. It is from this far-from-homogeneous body of scholarship that the study at hand emerges; our aim is to summarize and synthesize a large proportion of these empirical findings and then to integrate them into as coherent an explanation of modern interstate war as possible.

Integrative problems

As anyone who has read – or better still, written – the sort of review article found in the physical or biological science journals can easily appreciate, this is no simple task. Despite the strong scientific norms in favor of reproducibility, the inducements away from reproducibility – and thus, comparability – are often powerful. That means that even though several studies are intended to test the same theoretical model, they often will not. Partly this is a data and measurement problem, with individual investigators measuring the same variables in different ways, observing different regions of the world or looking at different historical periods. Furthermore, we can use different research designs, postulating different time lags between predictor and outcome variables, computing moving averages over time spreads of differing lengths, using different transformations to cope with the historical outlier cases, and assuming the reciprocal effects of our predictor variables to be additive in some designs and multiplicative in others.

This lack of perfect – or even proximate – similarity from one study to the next means that even if they all point to the same conclusion, we cannot assume theoretical convergence, nor can we assume that they point to differing conclusions even if the statistical results are quite dissimilar. In disciplines that rely more on laboratory experiments (Campbell and Stanley 1963) rather than historical experiments
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(Singer 1977) we typically find a much higher level of comparability and thus a stronger basis for treating their findings as cumulative. As a matter of fact, a set of methods known as meta-analysis has evolved in the social sciences, along with an increasing sensitivity to its strengths and weaknesses (Wolf 1986). In our field, the best known effort to explicitly compare and combine the statistical results of a fairly well defined set of studies (Rummel 1985) met with mixed success. But as the scientific world politics movement continues to grow, and especially as we adhere more closely to the historical experiment type of research design, we can expect greater attention to the canons of reproducibility and comparability (Singer 1975). Until then, faced as we are with rather idiosyncratic approaches to the measurement of our variables, empirical domain, and research design, what do we do?

We make do. More precisely, we do what all good scientists do at every stage in a given investigation. We think hard, looking first at what we and others claim to know about the query at hand, paying close attention to the larger class of studies to which we assign this one, distinguishing between those claims that rest on the revealed wisdom, professional folklore, or armchair “theory” on the one hand, and those that rest to an appreciable extent on reproducible evidence on the other. We then go on as suggested above, to ascertain the similarities and differences that characterize the more systematic studies. And if, for example, the findings for cases covering one region, type of society, or historical period turn out to differ from those for another empirical domain – and this may not be obvious in their published articles – we take careful note.

Our point here is that the most rigorous devotee of scientific methods is called upon, time and again, to make intuitive judgments. There certainly is a checklist of factors to consider when we evaluate and try to integrate disparate results from the data-based literature, but in the final analysis it is little more than a judgment call. In making such calls, we essentially fall back on our theoretical biases, which rest of course on our reading, recollection, and reinterpretation of that vast body of literature with which we are familiar. To be sure, a very large portion of that literature is a vague mix of historical anecdote, policy-induced hunch, contemporary fashion, and vague recall of some long-ago graduate seminar. This is quite acceptable as long as we keep the source of our intuition in mind and then go on to evaluate it in a truly skeptical fashion, and as long as we lay it
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alongside the formal models in literature (not for empirical veridicality, but for logical consistency) and check it against the data-based generalities (many of which, so noted, will point in more than one direction).

Further confounding this hard-to-reproduce process is the personality or the mood or the immediate incentives that are at work. Statisticians like to distinguish between Type I and Type II errors and that distinction is germane here; in the Type II error, we overestimate the evidence that leads us to see a pattern when it may not exist in empirical reality, while the Type I tendency is more likely to infer randomness and to thus overlook a strong but nonobvious configuration. Both are, in principle, to be avoided, but those of us who assume that social phenomena are inherently systematic, patterned, and law-like – and who are therefore more positive about scientific method – normally will err in the Type II direction; a possible example might be the excellent and very flattering Vasquez article on “The Steps to War” (1987) in which the author carefully examines most of the Correlates of War findings on the alliance-to-war relationship and interprets them as more coherent and consistent than would those of us who carried out the studies. On the other hand, those who are excessively critical, or take a dim view of the social science enterprise, or see international history as little more than one unique event after another will tend to miss a potentially significant but less visible set of regularities. We dwell on this issue at some length because the scholarly worth of this volume is in part dependent on how rigorously, carefully, and creatively we handle this task.

Theoretical views

By now you have a rather clear sense of our methodological orientation, but little clue as to the theoretical predisposition. A decade ago, this would have been a relatively straightforward discussion, but due to conceptual carelessness, semantic insensitivity, and epistemological innocence, the theoretical waters are once again muddied beyond belief.

All of us have been exposed to quite a variety of normative and empirical orientations vis-à-vis the subject of world politics, and the dominant tendency has been toward agnosticism. That is, given the thin and scattered range of evidence-based findings, we have wisely
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taken the view that one or another of the theoretical schools may ultimately be vindicated, but that in the meantime our task should be to test the indicators of a variety of them against the historical record. Put differently, those of us with that orientation have urged considerable open-mindedness toward conflicting explanatory models of world politics, coupled with a strong commitment to systematically examining the evidence in support of them.

However, the field took a remarkably counter-productive direction in the early 1980s (Singer 1989b). The bootless and sterile debate that had absorbed some of us while in graduate school in the post-World War II decade – “realism” versus “idealism” – again surfaced as a topic of allegedly serious discussion. As we see it, the issue emerged out of the policy debate that marked the onset of the Soviet–American “cold war.” Such scholars as E.H. Carr (1939) in Britain and Hans Morgenthau ([1948], 1967) in the United States – sharply criticizing Western policies of the inter-war period as naïve and idealistic because of their alleged reliance on international law, international organization, and collective security – argued for a more hard-nosed and Machiavellian view of international politics. For these self-acclaimed “realists,” there were three basic premises: (1) the system is and would remain essentially anarchistic; (2) statesmen do and should think primarily in terms of their “national interests;” and (3) the dominant currency is and will remain that of military and industrial capabilities. Despite the telling critiques of Ernst Haas (1953, 1964) and others, not to mention the disastrous consequences – many of which are only now becoming apparent – of the superpowers’ eager embrace of policies rooted in “realism,” generations of students have been subjected to these arguments. This issue is still alive, with the contemporary version pitting neo-realism against neo-liberalism (Keohane 1986; Kegley 1995).

We express this view for several reasons, the first of which is semantic. There surely is a more honest label for this perspective, including power politics, realpolitik, machpolitik, balance of strength, etc. More substantively, even if one could call idealism or realism a theory, we would need to articulate and then put to the empirical test some of the behavioral consequences of their premises, and many of those who bandy about these terms have, to date, failed to do so. Others, including ourselves, have done so, and much of the data-based literature turns out to address a variety of the relevant consequences. In sum, it is time to get on with the articulation and testing of rival
models and hypotheses regarding armed conflict and to move beyond the literary debate.

Later in the book we present further evidence, but for the moment we can summarize the factors that make us question certain elements within the “realist” paradigm. First, it usually assumes the territorial state to be not only the single most dominant actor in the system, but also a unitary one driven by considerations of expected utilities for the state as an entity, rather than a coalition of varying cohesiveness each of whose constituents pursues its own parochial interests in the familiar stew of domestic and foreign policy interactions. Second, it assumes a degree of anarchy in the system that ignores the web of institutional, legal, and pragmatic constraints; these “regimes” while expanding rapidly toward the close of this century were by no means absent even as far back as the Congress of Vienna. Third, it downgrades or even ignores the role of ethical criteria in security decision-making; international history is full of cases in which moral prohibitions mitigated or even prevented moves that would violate, or appear to violate, domestic or international standards. And, fourth, it pays far too little attention to the importance of competence, rectitude, credibility, and legitimacy, all of which interact with material capabilities to make for a state’s power, understood here as the ability to both exercise influence and resist influence attempts by others.

Having said all of this, we have no intention of ignoring the theoretical implications of realpolitik and related orientations. As a matter of fact, we already have two excellent collections of rigorous and systematic articles that explicitly address such implications; the earlier is Correlates of War II: Testing Some Realpolitik Models (Singer 1980) and the more recent is Reconstructing Realpolitik (Wayman and Diehl 1994). More significantly, both the speculative and the more systematic literature using formal models and/or data-based investigations are full of alternative and more diverse theoretical points of departure. Nor can there be much ambiguity as to our position here: we are not going into the investigation committed to any one particular explanation of war and then subject it to careful scrutiny. Rather, we pursue here a search for consistent empirical patterns (Bremer and Cusack 1995), and do so despite the opposing view that theory should precede empirical investigation. An early critic of our project described it as “count first and think second,” but that phrase is no more helpful than its opposite of “fantasize first and examine second.”
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Merely putting these alternatives so crudely is to illuminate the fact that few of us ever follow one or the other of these extremes; most of us spend our research careers working both sides of the street and crossing back and forth quite frequently. We get general ideas from all sorts of stimuli and try to think of historical examples that support or question these ideas, and we encounter all sorts of facts and ask which generalizations they undergird or undercut. We doubt that our colleagues do it any other way. When we reconstruct our research for our readers, we may present it in extremis – from either the inductive or deductive side of the street – but we doubt if such reconstructions ever capture what we really do.

Thus, it comes as no surprise if we confess that on the one hand we want to be very responsive to the empirical evidence generated by the data-oriented community of world politics scholars, while at the same time coming in with our own theoretical predilections. What might these latter be? As you might surmise from the above, our bias is towards the importance of realpolitik variables – but with questions as to their consequences. In studies reported in Correlates of War II, we find repeated support for the significance of system structure, relative capabilities, and geopolitical considerations, but we also find that these factors turn out to move the states toward war in certain contexts and away from war in others. Further, as we lay out our understanding of the decision processes by which governments deal with the issues that culminate in war or peace – or stalemate, crisis, or compromise – it will become apparent that we cannot completely embrace one or another of the contending models that dominate the literature.

Ex post facto experiment, or international history as social science laboratory

In our discussions so far, you have no doubt observed frequent reference to the importance of testing models and hypotheses against the empirical or historical evidence, but the rationale is by no means self-evident. To be sure, the objective is to generalize about world politics and interstate armed conflict, and it thus makes sense to examine these phenomena as they unfolded in the past. On the other hand, the global system and its relevant attributes are in a state of continuous change, thus making the system of the mid-twentieth century or the early nineteenth century little more than a rough
analogy to today’s system, and far from identical. One implication is that we might be equally justified in testing our models in such other empirical domains as pre-industrial societies, labor-management conflicts, or campus politics, and another is that there is no appropriate domain; every empirical domain offers at best a rough analogy to the global system of the present or the impending future.

This is, then, yet another argument against the possibility of a science of world politics; not only are no two cases sufficiently similar, but the context for every case will also turn out to be different. On the other hand, if the world offered perfect similarity from one case to the next, life would indeed be less interesting, and if every interstate war arose out of identical circumstances, our research task would be simple. More seriously, it is the very mix of similarity and difference from case to case that makes it possible to compare them and then go on to search for generalizations. In any experimental design, the essential task is to “control for” several factors by holding them constant in quite a few cases while varying one or two at a time to see which combinations of factors lead to which specific outcomes. Such manipulation of our predictor variables is partly provided by “nature” in the sense of the configurations given by history and partly by the ways in which we set up our research design. And to reiterate our thesis here, the global/international system is an evolving one, with some of its properties changing slowly over time, others rapidly fluctuating, and still others remarkably constant over the decades and centuries. As long as we are aware of this reality, have figured out how to measure these properties over time and across regions, and control for them in our analyses, this is not a handicap at all. Conversely, such evolution offers an interesting scientific challenge and at the same time creates the opportunity to carry out the sort of ex post or historical experiment essential to the development of solid knowledge on matters of war and peace.

Organization of the volume

From what has been said so far, our objective is quite clear: to offer a coherent synthesis of research findings leading to an explanation of interstate war for the historical period since the Congress of Vienna; but our motivation is not merely one of scientific curiosity but of securing knowledge that might be applicable to the relevant future as well. Thus our attention to cross-temporal comparisons over the past
and our hope of coping with the developing system as it unfolds in the decades ahead. In pursuit of such an explanation, we reiterate that we build heavily on the results of prior data-based research – ours and that of our growing company of like-minded colleagues. But once again, a caveat. We will be strongly influenced by existing findings – some of which are abstracted in Jones and Singer (1972) and Gibbs and Singer (1993) – but, as noted earlier, these studies are far from perfectly convergent. And even if they were, we have lived through enough of this type of research to appreciate the ways in which it can go wrong. Thus, while taking second place to no one on the virtues of the data-based historical test of our theoretical models, there will be instances in which we come out disagreeing with some apparently solid and consistent findings; we need not apologize for this, but we will certainly try to explain. Sometimes the judgment will rest on reservations about certain aspects of the studies, sometimes we will conclude that the premises and reasoning of a formal model – or even a well-articulated essay – are more compelling, and with some frequency we will recognize the relative infancy of our science and go with the more intuitive judgment. We make these points explicit to counter the anti-scientific view that the scientific method is little more than rigid adherence to certain mechanistic procedures. Algorithmic computation is a virtue, but independent judgment of conflicting evidence is more virtuous still.

With these points spelled out, we turn to the rationale by which our material is organized and presented. In addition to the approach that claims to have already identified the major factors in the etiology of war, there seem to be three general ways of organizing one’s material. First is the listing of groups of variables – such as economic, geographic, cultural, technological, etc. – all of which need to be considered. Second is one or another version of the “process model,” in which we begin with an evaluation of the factors that are furthest from the moment at which war is chosen or rejected, as in Bremer (1993b). From the distal to the proximate, we typically examine historical background, the state of the regional system, the capabilities of the protagonists, the precipitating events, the decision context and personalities, on up to the immediate conditions at the “moment of truth.” However, only a modest effort has been made to link these background and ecological phenomena to the onset of war. The work of Russell Leng (1983, 1984, 1993) – focusing on recurrent crisis interaction – is a notable exception in this area.