

THE RELATIONSHIP BETWEEN REGIME STRENGTH AND THE PROPENSITY
TO ENGAGE IN ARMED INTERSTATE CONFLICTS

DISSERTATION

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ABSTRACT

The Realist paradigm minimizes or excludes altogether regime attributes as a weighty and enduring factor in the international behavior of nations. However, research on the behavior of democracies, in particular, has strongly suggested that some regime attributes exert a great of influence over a state's behavior, particularly its propensity to become involved in militarized interstate disputes. One specific result of this line of research is that democracies do not war against other democracies, or rarely do.

Regime strength, the ability to hold on to political power, was of interest to political scientists in the recent past, but research in this area has lapsed. This research is intended to explore the role of regime strength in explaining state behavior, and, especially a state's propensity to engage in armed conflicts. To this end, this research investigates the following hypothesis: Weak regimes are more conflict prone than strong regimes, because weak regimes use, among other policies, armed conflict with other states to attempt to ameliorate their political weakness.

Five case studies are used to assess the evidence of a connection between regime weakness and the propensity to engage in armed interstate conflict. This is for two reasons. First, it is important to capture the way the leader and regime thought about politically weakness, why, what it contemplated as a remedy, and why. Second, all the

cases (and I suspect most, though not all, weak regimes), are autocratic. The indicators of regime weakness in such societies are likely to be subtle, internal to the regime's leadership circle. There are no data bases available adequate for this purpose.

This hypothesis, regime weakness is associated with an elevated propensity to engage in armed interstate conflict, is supported strongly by the case studies. The results have implications for political science research and U.S. strategic policy.

First, the next step is to explore the comparative weights of regime weight and regime type using, as much as possible, the data and methodology of the existing *DPprop* literature. This line of inquiry is to be designed to reveal whether some of the behavior currently attributed to regime type, and democracy specifically, might actually be better attributed to regime strength.

Second, U.S. national security strategy, especially the contribution of deterrence to it, is currently linked to a regime attribute, regime type. Specifically, the U.S. seeks to spread democracy in the belief that democratic regimes tend to act in ways consistent with U.S. interests. If regime strength is also a strong influence on state behavior, then U.S. strategy ought to concern itself also with spreading strong regimes. If regime weakness is associated with a propensity for engaging in armed interstate conflicts, then national security strategy must reflect that.

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FIELDS OF STUDY

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INTRODUCTION

In all the abstruse discussions of deterrence with the Soviet Union that was characteristic of the Cold War, there was seldom discussion of the ways in which deterrence strategy as conceived at that time was implicitly a product of the attributes of the two regimes. Instead, deterrence was defined and debated quantitatively in terms of crisis stability, survivable second strike capabilities, and the amounts of destruction each side could inflict on the other. But, it is an argument presented in this dissertation that far more important than the sterile numbers and scenarios of stereotypic strategic analysis (having been guilty of plenty of it myself), were the political characteristics of the two regimes. In particular, regime strength (defined and discussed extensively in the literature review), greatly relieved the enormous pressures to hold onto political power and thereby reducing the chances of the desperate decisions that characterize weak regimes seeking to cling to power. In the face of those pressures, weak regimes have been willing to undertake virtually suicidal courses of action (see Appendix A for an example). Of course, had those pressures afflicted the U.S. or Soviet regimes, the consequences could have been bilateral suicide.

Deterrence may become the centerpiece of U.S. regional defense strategy, despite the current U.S. national security stressing preventative action. But this is by no means certain. Much is different now that the Soviet Union has faded as the primary U.S.

adversary. Some of these differences may exert a great effect on the priority given to deterrence as an instrument of policy by the U.S. For example, most potential regional adversaries of the United States will not possess nuclear weapons, though that is changing more rapidly than foreseen. Of those that do possess nuclear weapons, virtually all of them would have trouble delivering them to the United States.¹ Regional targets are more likely. Since the United States has pledged not to threaten nonnuclear states with nuclear attack, U.S. regional deterrence will have to take on a largely conventional character. The relative invulnerability of the United States means that intervention and conventional warfighting is less perilous now for the United States than it was during the Cold War. In the post–Cold War era, potential U.S. adversaries will no longer be backed by a state (i.e., the former Soviet Union) posing a strategic threat to the U.S. homeland. Therefore, although conflict may be as distasteful as ever, it is not as dangerous to the United States overall. It follows that deterrence of regional adversaries is less critical to U.S. security than was deterrence of the former Soviet Union. In other words, deterrence is no longer a necessity; it is an *option* to be evaluated just like any other policy option. At the very least, the costs of mounting a credible deterrent threat have to be compared to the costs of fighting a regional adversary, and the results of that calculation are not clear.

This research does not contain a reformulation of deterrence theory *per se*. Deterrence theory is too generic to require reformulation. However, if deterrence is to be a pillar of U.S. national security strategy, the way deterrence theory is *applied* to strategic policy does need reformulation to reflect the many differences between the Cold War and post–Cold War periods.

1. “Bombs on freighters,” etc., are always possible. However, we believe (and discuss below) that the operational problems posed by these modes of delivery are quite significant.

Using statistical analysis and case studies, this research seeks to make reliable generalizations about the motivations of many of the regional adversaries the U.S. is likely to seek to deter and the conditions that appear to correlate with successful deterrence of regional adversaries in the current era.

Definition of the Problem

If exploring modern deterrence is the ultimate objective of this work, then understanding the connection that connection between the character of political regimes and their behavior, particularly their propensities for engaging in militarized interstate disputes, is its central focus. Since the purpose of deterrence is to raise risks in the face of a national leadership's strong motivations to take undesirable actions, knowledge of the motivations that are linked to the attributes of different kinds of political regimes is an important prerequisite. If there is a reliable and general connection between regime attributes and its violent propensities, there may then be a way to place modern deterrence on a more empirical basis, or, on the other hand, to exclude deterrence as feasible in this period of our history.

More specifically, the purpose of this research is to investigate the possible connection between two regime characteristics, regime type and regime strength, and a leadership's propensities for engaging in militarized interstate conflict, particularly for regimes in the developing world. The findings are then applied to the problem of deterring these sorts of adversaries. The logic of this research then runs as follows:

Deterrence was the centerpiece of U.S. national security strategy.

Its effectiveness was largely the result of the character and motivations of our chief adversary, the U.S.S.R.

The motivations of our likely adversaries today are markedly different.

Those motivations arise primarily from the character of the political regimes of those states.

The most researched attribute of political regimes is regime type, democracy or nondemocracy, and different patterns of behavior are associated with different regime types.

Another attribute of political regimes, *regime strength*, is much less researched, and the objective of this research is to understand more deeply the influence of that attribute on the behavior of states, particularly their propensities for engagement in violent, external conflicts.

Armed with the results of that research, we can revisit U.S. strategy and assess what changes, if any, are needed.

The document is organized as follows:

Chapter I discusses the important differences in the attributes of the Soviet regime versus those of current potential adversaries and spells out its implications for formulating a deterrence strategy.

Chapter II reviews the literatures on the role of regime type in affecting state behavior (primarily the *DPprop* literature), so-called diversionary wars, and the concepts of weak and autocratic political regimes, particularly in the developing world.

Chapter III contains the case studies.

Chapter IV applies the implications findings of Chapters II and III for political science research and for U.S. strategic policy.

CHAPTER 1

DETERRENCE IN U.S. STRATEGY: PAST AND PRESENT

Contrasting Regional Deterrence with Cold War Deterrence

There are substantial reasons to suspect, *a priori*, that the most effective strategies for deterring regional adversaries from threatening U.S. interests may be different from the U.S. deterrence strategy directed at the Soviet Union during the Cold War. This is because many fundamental assumptions about conflict with the Soviet Union, which underpinned U.S. deterrence, are not likely to hold when deterrence is applied for very different purposes against very different types of states or regimes.

The assumptions behind deterrence of the Soviet Union can be sorted into three categories: those arising from the magnitude of U.S. interests at stake in the Cold War, those arising from the military capabilities deemed important for deterrence of the Soviet Union, and those arising from the character and motivations of the Soviet regime. In each of these areas, regional adversaries are proving to be quite different from the former Soviet Union.

The U.S. Interests at Stake and Credibility

Classical deterrence theory has long stressed the importance of strength of interests as a means of making deterrence threats credible to an adversary. During the

Cold War, U.S. retaliatory threats to deter Soviet nuclear attack against the United States were deemed to be highly credible because the interests at stake could not have been greater for the United States. Even when extending deterrence to protect Western Europe from a Soviet conventional attack, the U.S. interests at stake made what to many appeared to be an irrational threat (“the willingness to risk the loss of New York to save Paris”) appear (at least) not incredible. From the beginning of the alliance, NATO grappled with this so-called coupling problem during those periods of apparent loosening between the United States and Western Europe. However, while there was disagreement over questions of degree, most analysts felt that NATO’s “threat [of U.S. nuclear first use] that left something to chance” could be less than completely credible and still be successful, because the Soviet leaders could never be sufficiently convinced that the United States would not honor the commitment given the stakes involved—despite the vulnerability of the U.S. homeland to Soviet retaliatory strikes.¹ This point dovetails with the Soviet Union’s own relative satisfaction with the *status quo* throughout most of the Cold War. U.S. credibility could be less than perfect, because Soviet risk-taking propensities were relatively low. Put another way, being a relatively satisfied power, Soviet leaders would have had to become virtually convinced the U.S. would not resort to nuclear weapons to make serious attack on NATO seem worth the risk. Of course, this calculus would have changed radically to the extent the USSR became seriously dissatisfied with the *status quo*. The risks previously deemed excessive would cease, too.

1. Though it is impossible to know what the Soviets believed, many U.S. and European analysts commented on the credibility problems inherent in NATO’s strategy of flexible response. The existence of independent French and British nuclear deterrent forces eased the U.S. extended deterrence credibility problem, because Soviet leaders then had to contend with two additional paths by which nuclear war could arise out of conventional conflict in Europe. See, for example, Schwartz (1983).

It is for that reason that we can say the United States passed through a period of considerable potential as the Soviet Union disintegrated, and we owe Gorbachev a considerable debt.

By contrast, in almost all regional crises, threats to U.S. national interests will not be of similar magnitude. This suggests that the United States will find it more difficult to use strength of interests to bolster its credibility of deterrence in the eyes of an adversary. If so, the character of the deterrence threats that are expected to be effective must change. For example, without the buttressing effect of the most vital interests at stake, the United States has to develop deterrent threats that are not terribly costly or risky to carry out. Cost is meant here in all its senses: time, resources, casualties, political support, and the like. In the same way, what the United States threatens to do likely requires change as well. The problem may be exacerbated by the fact that nuclear weapons will not be an acceptable alternative despite attempts to develop so-called usable nuclear weapons (e.g., those with low yields or deep earth penetrators). Conventional forces, though more credible, may not appear sufficiently threatening or credible to deter regional adversaries, since they require great exertion to deploy and since their use may be costly. This takes the discussion to the military requirements for effective regional deterrence.

Military Forces for Deterrence

The military balance vis à vis the former Soviet Union focused largely on nuclear weapons. Although the basis of military stability with the former Soviet Union was not exclusively nuclear, the nuclear component of any military crisis between the two

superpowers made the Soviets conservative and cautious to a degree that would not likely have been achieved had the balance been entirely conventional. Several consequences for deterrence flowed from this state of affairs.

First, nuclear weapons existed in such numbers and were so overwhelming in their effect that neither side had to be terribly concerned about being able to destroy only the correct targets or, indeed, knowing with much certainty what the correct targets were. Since the early 1970s, each side could attack virtually all important target sets, though not necessarily as effectively as each would have liked (e.g., ICBM silos). The point is that the enormous destructiveness of nuclear weapons compensated for uncertainty about what the exact requirements of deterrence were.²

Second, nuclear weapons required no special competence to use effectively. A successful attack was almost entirely a matter of technology functioning properly. Therefore, each adversary could pin little hope on the prospect of avoiding destruction because of the incompetence of the other side, inferior generalship, lack of unit cohesion, and all of the other myriad ways in which the employment of conventional forces can be unpredictable. Therefore, military balances based on nuclear weapons are reasonably calculable, and adversaries can realistically contemplate the consequences of their use.

For deterrence of regional adversaries, the United States will have to rely entirely on conventional weapons, at least in response to conventional threats. Conventional forces inherently lack some of the fearsomeness and certainty of nuclear weapons. They may require considerable skill and resources to deploy. They may not function as hoped; unit cohesion may be low; generalship may be poor; and so on. In sum, the outcome of

2. Of course, many arguments occurred throughout the 1970s and early 1980s about the requirements for nuclear deterrence.

using conventional forces is much less predictable than the outcome of using nuclear weapons. Therefore, the magnitude of a U.S. deterrence threat based on conventional weapons may be difficult for an adversary to determine. This, in turn, suggests that, under some circumstances, conventional deterrence is likely to be far less reliable versus regional adversaries than was nuclear deterrence versus the U.S.S.R.

The Character and Motivations of U.S. Adversaries

Though it may not have been fully understood by all involved, nuclear deterrence was well suited to the character of both the U.S. and Soviet regimes. First, the U.S. and Soviet leaderships understood the dangers and capabilities of modern war and weapons, especially nuclear weapons.³ This assumption was based on the experience of the Soviet Union in World War II and the fact that they too possessed modern nuclear weapons. The Soviets had tested them, were familiar with their power, and wrote extensively about their properties. Therefore, the United States had little concern that a deterrent strategy based on nuclear weapons would be psychologically minimized by the Soviet leadership. Indeed, on those occasions when the United States encountered opponents who did minimize the power of nuclear weapons, both the United States and the Soviets were alarmed, as with Mainland China in the 1950s and Cuba during the 1962 Missile Crisis.

By contrast, the leaders of many Third World states may not have a good understanding of modern military capabilities, especially the advanced conventional

3. Freedman, L., *The Evolution of Nuclear Strategy*, New York: St. Martin's Press, 1983, pp. 257–272.

weapons fielded in the last decade or so, despite the many occasions the United States has demonstrated these weapons' capabilities. Or, perhaps, because those demonstrations also revealed the weapons' weaknesses.⁴ Regional leaders do not possess such capabilities, for the most part, and they have had relatively little direct exposure to them. Indeed, the ultimate capabilities of conventional weapons are not clear even to many advanced militaries as well. We may be at the opening stages of the so-called Revolution in Military Affairs, and it is well to remember that U.S. estimates of casualties for the first war with Iraq numbered in the many thousands. In other words, the U.S. performance in that campaign proved a surprise to U.S. strategists, as well as to Saddam Hussein. Even with that lesson "under our belt," there was considerable disagreement over the shapes the campaigns in Afghanistan and again in Iraq would take. With the steady introduction of many new types of systems, it may well be that our understanding of their full capabilities always lags behind their use. This lag of understanding must be far greater with regional adversaries.

Second, both the U.S. and Soviet leaderships valued their respective populations and economies, though perhaps not to the same extent or for the same reasons.⁵ Certainly, the Soviet leadership appeared more ready to regard the value of their state in

4. This is not to say that all Third World leaders are ignorant of the capabilities of all modern military forces all the time. It is to say that less advanced military states are less likely to comprehend fully the capabilities of the most modern forces than the states that possess those forces. Certainly, Assad of Syria is likely to know a good deal, but his military lacks many of the capabilities that make U.S. forces so potent: advanced C³I, effective air-to-ground weapons, stealth, and the like. Similarly, Mao knew a great deal about the powers of certain operational concepts, such as "People's War." But they grossly underestimated the capabilities of modern firepower to offset quantitative and morale factors and tactical adroitness in the Korean War. Similarly, he was markedly ignorant of nuclear weapons. On this latter point, see Freedman *op. cit.* (1983), pp. 273–282.

5. For discussion of this point, see M. Mandelbaum, *The Nuclear Question*, New York: Cambridge University Press, 1979 and S. Kaplan, *Diplomacy of Power*, Washington, DC: Brookings Institution, 1981, pp. 667–677.

instrumental terms. But even so, Soviet power required a labor force and industrial base to produce and sustain itself, and it was understood by the Soviet leadership that each of these resources were important to protect. But the United States also assumed that the Soviet leadership felt some deeper responsibility for the welfare of the Soviet people and that the future of the Soviet Union as a model to be emulated mattered. In a word, the United States assumed that the Soviet leadership was in some sense “patriotic.” Therefore, the United States felt it would be effective to base its deterrent strategy, in part, on a threat to destroy large portions of the Soviet population and economy. Precisely because this threat was assumed to be catastrophic to the Soviet leadership, it was seen as the last resort of U.S. nuclear deterrence strategy.

For many Third World regimes, there may be little analogous sense of responsibility or duty toward the population and its welfare. At best, the population and civilian economy are instrumental goods for such regimes. They are valued and protected only insofar as they are important to accomplishing the goals of the regime or individual leaders. More often, the population and civilian economy are viewed with suspicion, a necessary evil unavoidable in the process of holding national power. Such leaders regard their states more as a private preserve than a personal trust. As a result, deterrence based on threats to these populations and economies may be without much coercive power. One must be skeptical that threats to destroy the civilian electric power grid would have been very effective against Papa Doc Duvalier or Idi Amin.⁶ Similarly, the economic measures imposed on Haiti, Serbia, and ultimately Iraq did not prove effective, because they

6. This is not to say that punishment as a tool of deterrence is without merit, only that traditional countervalue approaches are likely to be less effective. Attacks on targets of special interest to national leaders may have merit, as discussed in Chapter Five.

created pain for groups to which the leaderships were largely indifferent. On what threats then can deterrence be based in the current era? One possibility is the threat to create political instability for governing regimes, a matter of highest importance to these leaderships. Indeed, that presumably was the rationale for U.S. economic sanctions. While the popular welfare *per se* may not be a high priority to these leaders, retention of political power is. However, these regimes are very skilled in repressing domestic threats to themselves. This point is explored in depth in Chapter Three.

Third, throughout most of the Cold War, U.S. strategy was based on the assumption that the Soviet Union was satisfied enough with its *status quo* and its future prospects that it would not run great risks that might jeopardize them. This notion is captured in the description of the Soviets as “opportunistic,” “conservative,” or “risk averse.”⁷ It meant that the Soviets would exploit opportunities, but would be much less likely to embark deliberately on a course of action carrying a high risk of substantial loss.

Another indication of this view was the U.S. focus on inadvertent war, arising out of crises, as the most likely source of war rather than deliberate, premeditated aggression *à la* Nazi Germany. This meant that deterrence had to address crisis interactions, an emphasis that led to concerns about crisis stability and crisis communication. The focus on crisis decision making, as opposed to premeditated plans to attack, also led to the use of so-called signals as a means of communication.⁸ Signals connoted military actions that

7. The deterrence literature on communication, generally, and signaling, in particular, is quite large. Much of it is controversial, since, for many, it has become synonymous with “gradualism” or using military force indecisively. As is so often the case, the original ideas are much richer and more nuanced than are the later recollections of them. We will cite only a few of the contributions that deserve mention. See Schelling (1963); Kahn (1965); Halperin (1963), pp. 95–112; Freedman (1983), pp. 173–219.

8. Halperin, M., *Limited War in the Nuclear Age*, New York: John Wiley & Sons, Inc., 1963, pp. 95–112; L. Freedman, *op. cit.*, 1983, pp. 173–219.

themselves had little military effect on the adversary, but were meant to communicate an intention and/or demonstrate a capability. Against an adversary following a deliberate plan, which presumably would take intentions and capabilities into account, signals could be expected to have little effect, except to make clear that surprise had been lost.

However, in a crisis in which adversaries are attempting to communicate commitments to protect their interests, signals could convey useful information, especially nuclear signals.

Unlike the Soviet Union, many Third World states are chronically dissatisfied with the *status quo* and its future prospects. That dissatisfaction may arise from many sources, but primarily it is related directly or indirectly to the unequal distribution of power, status, and resources in the international system.

Fourth, both the U.S. and Soviet regimes were “strong.” Strong here denotes securely in power by virtue of each regime’s ability rewards and punish. Obviously the mix of those two strategies for staying in power differed between the U.S. and U.S.S.R, but, throughout the Cold War, the progression of regimes in both states never felt itself seriously threatened by deposition.

Why is this so important, and why is this matter of regime strength the focus of this research? It is because weak regimes, those which feel seriously threatened, are constantly and intensely active in trying to fend off these threats. The strategies used are, again, a mixture of rewards and punishments. And, most dangerously, weak regimes are especially prone to enter into international disputes. These serve the function of reinforcing national unity through a state of constant emergency, of fostering dependence on and trust of the leadership, and of diverting the population and elites from their discontent. International disputes can easily escalate to militarized conflict in these cases,

because the weak regime has sharply constrained its ability to negotiate a compromise. Further, weak regimes are especially prone to take immoderate risks, since the alternative is worse, losing power.

Imagine therefore, the risks of war between the U.S. and Soviet Union if either regime had felt itself especially weak. Would Khrushchev have been willing to withdraw missiles from Cuba, if he and his colleagues believed that concession would likely lead to the loss of personal and party power to lead? It was precisely because the Soviet leadership did not feel terminally imperiled that war was avoided in that case, despite the fact that the nuclear forces of both sides would have made war bilaterally suicidal.

In the post-Cold War era, many of the U.S.'s current and potential adversaries are led by chronically weak regimes. The literature review contains discussion of why this is so. Such regimes are in a never ending campaign to retain political power, and this, I believe, leads them to be a disproportionately large source of military conflict in the world, usually involving high risks and small benefit. Note the recent war between Eritrea and Ethiopia which cost tens of thousands of lives for an insignificant piece of disputed territory. If the imperatives of regime weakness and its consequences are as I have portrayed them here, then deterrence is likely to be quite difficult.

This research explores the connection between an attribute of political regimes, regime strength, and a regime's propensity to engage in armed interstate conflict. The literature review is structured around the following bodies of literature that bear on this subject:

1. The literature on the connection between regime type and international behavior. Most of this part of the literature is devoted to the research on the behavior of democratic regimes.
2. The literature on so-called diversionary wars, do they exist and, if so, why? As I discussed above, there is reason to believe that diversionary wars are attractive to weak regimes.
3. The small literatures on regime weakness, why such regimes are particularly prevalent in the developing world, and how those regimes behave.

CHAPTER 2

LITERATURE REVIEW

Regime Attributes as an Independent Variable

The proper starting place in this discussion is the current status of Realism as the dominant theory for explaining international relations. Most basically, Realism flows from the core assumption that a state's international behavior is dominated by calculations of power among other states, how that distribution is changing, and how to change one's own power in response. Since its rigorous reformulation by Waltz (1979), Realism has been criticized on methodological grounds: under or over specification, vague and ambiguous, untestable, or indistinct from other competing theories. The proponents of Realism have responded by developing a broad range of variants aimed at remedying these problems, and, in the process, creating others.

My aim is not to review those methodological claims and counterclaims, which, in my view, have reached sterility. It is rather to point out that, in nontrivial ways, states behave repeatedly in ways difficult for Realism, in whatever form, to explain. The fundamental problem of Realism is its persistent inability to adequately predict international behavior. This is not to say that Realism possesses no explanatory power at all. It is to say, however, that it falls short as an exclusive theory of international

relations, and, since its proponents make such a claim, Realism must be judged as insufficient and inadequate by its own metric. In Chapter 5, I suggest the occasions when states do act according to the predictions of Realism.

If Realism possesses *some* explanatory power, but not enough, then the likeliest explanation is that some important independent variable(s) has been omitted. One strong candidate proposed by a large number of scholars is the domestic contents hidden by Realism's posit of the state as a unitary actor. Certainly there is considerable anecdotal and scholarly evidence to support the view that one aspect of a state's internal structure, regime-type, carries considerable weight in influencing that state's international behavior.

Anecdotal observations about the relationship between domestic political structures and dynamics and a state's (city-state in the case of the Greek) decision making on peace and war¹ have a long history. And even in the last few centuries, Kant's discussion of the issue did not occur in a vacuum.² But it is customary to regard Immanuel Kant's scholarship as the first systematic examination of one particular domestic political factor, regime-type and international behavior. In brief, Kant argued that republican regimes, i.e., those governed by law with some mechanism of accountability to the citizenry, are significantly less likely to engage in interstate military conflicts than states with more autocratic regimes. His explanation was based on the norms of republican societies, on the constraints they impose on leaders, and on the bonds that form between republican states.

1. Thucydides, R. Strassler (ed.), R. Crawley, V. Hanson. (1996), *The Landmark Thucydides: A Comprehensive Guide to the Peloponnesian War*, New York: Free Press

2. Kant, I., (1962), *Perpetual Peace*, Hackett Press

More recently, it is indisputable that French and Russian international behavior changed radically in the wake of their respective revolutions.³ The observation surely can be made with respect to Iran's revolution and the replacement of the Shah with a theocratic regime. Precisely the same occurred in the wake of World War II with changes of regime in both Japan and Germany. It is exceedingly difficult to develop a plausible counterfactual that entails Nazi Germany's membership in NATO.

Certainly many governments behave as if that regime-type is associated with a state's international behavior, hence their long-standing interest in exporting their own regimes to other states. This policy was a "principle of certainty" for both the U.S. and Soviet Union during the Cold War, and it appears to remain a high priority U.S. post-Cold War foreign policy. Revolutionary France, Russia, and Iran undertook aggressive proselytizing immediately after accession to power of the new regimes. One is compelled to ask why, if Realism is correct in its posit that a state's international behavior is dominated by external matters, states have continued to devote energy and resources to interfering in the domestic politics of other states. I mention these points to illustrate the idea that, apparently, at an informal, commonsensical, or intuitive level, the domestic behavior-international behavior link, particularly with respect to republican regimes, has been an idea "waiting in the wings" for a long time. One would surely expect to find some systematic empirical evidence bearing on the question and, indeed that systematic evidence does exist in several literatures.

The focus of these literatures tends to be on regime-type, especially democracies. I mean by "regime-type" not the international regimes studied under the aegis of regime

3. See, for example, Walt, Stephen (1996), *Revolution and War*. Ithaca, N.Y.: Cornell University Press.

theory but rather the political system by which a state is governed. Gurr (1974)⁴ operationally defined regime-type as a function of several attributes: method of executive selection, type of political competition and opposition, the characteristics and independence of executive policy making and its decision-making latitude, the distribution of authority, type of political participation, and the scope of governmental functions. Regime-type is generally an independent variable with the dependent variables different sorts of international behaviors, usually related to peace and war. The following section of the literature review is organized by those various independent variables: general war proneness, proneness to enter into so-called diversionary wars, and performance in crises and war.

Every study in Political Science, no matter how rigorous, will be found to contain certain methodological vulnerabilities: the inherent difficulties of operationalizing fuzzy and complex concepts, coding procedures, the choice of data sets, the quantitative methods utilized, the time term studied, the adequacy of the theoretical basis, and the like. One reason modern Political Science has had difficulty in building a cumulative body of reliable findings is because of a certain “research life cycle” (for lack of a better expression): A solid study is produced on an important area, with strong results. Then follow-up studies are done replicating the original. Then criticisms are raised based on the matters I’ve just listed. The proponents of the original finding respond. But, most of the time, minds are not changed because the methodological vulnerabilities are inherent and inescapable; they cannot be exercised or even definitively settled one way or the other. After a few volleys and responses, no more can be said. Then someone proposes a new research method or new

4. Gurr, 1974.

paradigmatic view of the problem, and new results are produced, perhaps agreeing with the original, perhaps not. For a brief while, there exists a certain optimism that finally the line of research has found a solid foundation, but quickly it becomes clear that the weaknesses of the original work also bedevil the new approach. There ensues an exchange of criticisms and responses, and the cycle begins anew. Minds are seldom changed unless outright errors are exposed. But, in the vast majority of instances, even the question of whether an error has been made or not is not clear-cut, and never can be.

I have made this digression to explain that I see no point in a literature review of tiresomely repeating these inherent weaknesses of every study. It is a sterile exercise. The reader should simply assume that all the inherent weaknesses of this sort of research are present in every study. Therefore, I confine my points to what I believe are the more significant factual, substantive, logical, or theoretical issues raised by each study when such are raised. Also, I devote most attention to what I take to be the major studies by virtue of originality or creativity. I note more briefly the studies that elaborate a particular research line, but do not fundamentally break new ground.

Regime Type and General War Proneness

The observations of a sociologist, Dean Babst, that “no wars have been fought between independent nations with elective governments between 1789 and 1941,”⁵ are usually credited with being the first empirically based observation about regime-type, specifically democracy, and international behavior. But that piece was short (more of an op-ed) and poorly documented, which, presumably is the reason for the obscurity of the journal in which it appeared.

5. Babst, Dean V. (1972), “A Force for Peace.” *Industrial Research* 14 (April): pp. 55–58.

However, the piece did come to the attention of Melvin Small and David Singer, who were the first to investigate what has become known as the Democratic Peace Proposal (*DPprop*) with reasonably sophisticated quantitative methods.⁶ Indeed, the study was an early application of the Correlates of War (COW) events database. Small and Singer simply counted the number of wars in which democracies and nondemocracies had been involved between 1815 and 1965. They found that democracies had participated in no wars with each other and fewer wars *in toto* than nondemocracies.

One problem, which made the authors themselves cautious in their generalizations, especially with respect to whether democracies are less war-prone vis à vis nondemocracies, was the small number of democracies that existed during the period they studied, indeed during any historical period. It makes the definitions of democracy and war an especially sensitive matter, since small changes in either can skew the results drastically. In my view, this problem continues to be a matter for concern. Another problem identified by Small and Singer is the confounding effect of geography. Even today when mobility is so great, wars tend to occur between adversaries who are geographically close, if not contiguous. For the 19th century, approximately 50 percent of the period studied by Small and Singer, the requirement of contiguity was almost entirely true for any large-scale military operation, though smaller amounts of military power could be projected long distances. Therefore, the effect of the small number of democracies was amplified by its correlative effect that most democracies were not contiguous to other democracies.

6. Small, Melvin and J. David Singer (1976), "The War-Proneness of Democratic Regimes," *Jerusalem Journal of International Relations* 1 (4): pp. 50–69.

In 1983, Rummel⁷ published a piece intended to meet these weaknesses of Small and Singer. Unlike Small and Singer, Rummel's independent variable was "libertarian" regimes, by which he meant regimes governing states whose citizens enjoyed two freedoms:

- a. Political freedom (civil liberties and political rights)
- b. Economic freedom

Small and Singer defined their independent variable, democracies, dichotomously as states characterized by regularly scheduled elections with free and legitimate opposition parties and candidates and a parliament or legislature for which at least 10 percent of the adult population could vote, enjoying authority over or parity with the executive branch. Notice, however, that Small and Singer included no element of economic freedom in their operational definition of democracy. Rummel's definition is more demanding than Small and Singer's, thereby shrinking the number of states classified as democratic (Sweden and Israel fail the test due to lack of economic freedom), thereby heightening the problem of the small N.

Rummel tested two hypotheses:

- a. Joint Freedom Proposition: Libertarian systems mutually preclude violence. Violence will occur only if at least one state is nonlibertarian.
- b. Freedom Proposition: Freedom inhibits violence, so the more libertarian the state, the less it tends to be involved in violence.

Rummel assessed these two hypotheses using the set of both nonviolent and violent conflicts for all states between 1976 and 1980, and the set of all interstate wars

7. Rummel, Rudolph J. (1983), "Libertarianism and International Violence." *Journal of Conflict Resolution* 27: pp. 27–71.

between 1816 and 1965. He used the directed dyadic Dimensions of Nations events data set and found strong support for each of the hypotheses. He found statistically significant support for both his propositions.

Of course, there are problems with Rummel's study. First, the time term he used, 1976 to 1980, is simply too small, but even more of a problem is the way Rummel defends his choice. He says *all* interstate conflict for five years is hardly a "limited sample." The use of both "limited" and "sample" are revealing. With respect to the term "limited," the issue is not the absolute amount of interstate violence present between 1976 and 1980; it is the amount relative to what might be regarded as representative in the Cold War international system. Second, in what sense was Rummel using a "sample"? To the extent that a sample suggests a random sample, a subset structured to be likely to contain the salient characteristics of a larger set, we have absolutely no way to know whether 1976 to 1980 is a sample or not. 1976 to 1980 can be said to be an "arbitrary" sample, but that is hardly the same as a random or representative sample. Indeed, if the universe of international conflicts is something that can be properly sampled as we might sample public opinion or census data, then in all likelihood, the period 1976 to 1980 is not a representative sample. Therefore, we have no basis for assuming Rummel's results are representative of any block of time larger than 1976 to 1980. This would not be a problem, except that Rummel wishes to generalize about the behavior of libertarian states from that time period. Third, Rummel attempts to eliminate Small and Singer's contiguity problem, but cannot because his N is so small. But he says that "the lack of wars between democracies is unlikely to have occurred by chance" The "chance" to which he refers, presumably, is the chance of sharing or not sharing a

border. I place “chance” in quotation marks here, because true chance has nothing to do with the matter. The location of states at any given moment cannot be properly characterized as the outgrowth of random processes. In my view, Rummel made a category error. Translated, what Rummel should say is, “the lack of wars between democracies cannot be explained by the fact that so few share a common border.” But, when rephrased this way, the answer is obvious: “Why not?”

Chan’s 1984 study was aimed at remedying some of the weaknesses of Rummel’s study by broadening the time period assessed, 1816 to 1980, and by broadening the type of conflict included to colonial and imperialist wars, as well as interstate wars.⁸ Chan focuses exclusively on Rummel’s second proposition, the Freedom Proposition, taking Small and Singer (1976) and Rummel (1983) to have established that “democracies do indeed seldom go to war or engage in violent behavior against one another.” Note that Chan’s formulation of Rummel’s Joint Freedom Proposal contains an important change: the requirement that there be no violence between democracies (as put by Rummel) has been relaxed. This is an important step, a point I will return to.

With his expanded time frame and types of wars considered, Chan focused especially on Rummel’s Freedom Proposition: Are democracies also less war-prone with respect to nondemocracies? Chan found that (as he put it) democracies rarely or never go to war with each other, but are just as apt to go to war with nondemocracies as are nondemocracies. Once in war, the degree of violence or intensity of war is not sensitive to degree of democracy. The exception to these findings, interestingly enough, occurs precisely during the five-year block covered in Rummel’s 1983 study, 1976–1980, when

8. Chan, Steve, “Mirror, Mirror on the Wall. . . Are Freer Countries More Pacific?” *Journal of Conflict Resolution* Vol. 28, No. 4, December 1984: pp. 617–648.

democracies were less war-prone generally, and not just with respect to other democracies. Chan could see no reason why behavior during this five-year block signals anything more than the truism that some five-year blocks are different than others.

All states seldom go to war against one another. As Chan reported,⁹ “out of 176 members of the interstate system that existed at one time or another during the 1816–1980 period, 94 (53.4 percent) have never had any experience with international war. . . . Of the 91 countries that became members of the interstate system during the 1944 to 1980 period, 73 (80.2 percent) have had this record.” Further, “the four countries with the highest war-per-year scores are commonly considered politically free. They are Israel (0.152), India (0.147), France (0.135), and Britain (0.115, which ties this country with USSR . . .).” Note that Israel was excluded by Rummel from the category of libertarian state, thereby eliminating an especially war-prone democracy from his pool.

So it can be hard to know what “seldom” mean when the “background count” of international wars is so low per state? Therefore, the point is that by moving from “never” to “seldom,” Chan moved in a problematic direction. For example, the term “seldom” makes it difficult to come fully to grips with ambiguous cases in which democracies *might* have fought each other, the so-called near misses identified by Layne and others. Those cases (whether large or small in number again depends upon the operational definition of “seldom”) are crucial, because they hold the key to establishing whether democracies are truly exceptional. If close study of the near misses was to suggest that democracies do indeed fight democracies, then one can expose the route by

9. Chan, 1984, p. 626.

which the wars happened. More important, if that close study suggested that democracies avoided war for reasons specific to democracies, than the basis for arguing democratic exceptionalism can be made all the more strong.

The next major piece on the *DPprop* was again by Rummel in 1985.¹⁰ In this study, Rummel attempted an early form of what has come to be called a meta-analysis: summary analysis of a group of small N studies in the hopes that aggregation will produce stronger results. Rummel included what he deemed to be the important studies published in the *DPprop* roughly between 1970 and 1985. He established a scoring system for evaluating each study according to its importance, relevance, usability, and degree of support for four different formulations of the *DPprop*, including his own: the Joint Freedom Proposition and Freedom Proposition. He found strong positive support for the Joint Freedom Proposition, but “nonrobust” (Rummel’s term), though still positive, support for the Freedom Proposition.

Rummel chose to do a meta-analysis, because it is precisely the tool of choice where limited studies in a particular area are suggestive of an important phenomenon, but ambiguously or weakly so. The careful aggregation of studies with complementary strengths (and weaknesses) can result in one large “virtual” study with more decisive results. And, indeed, Rummel’s study did succeed in doing that. The problem is that Rummel, for whatever reasons, did not seem to see the results for what they were.

He found strong support for the Joint Freedom Proposition (1.6 on a -2 to 2 scale, where 2 is “strong support”). But the Joint Freedom Proposition is a dichotomous proposition: either *all* democracies refrain from war with each other or the proposition is

10. Rummel, Rudolph J., “Libertarian Propositions on Violence Within and Between Nations.” *Journal of Conflict Resolution* Vol. 29, No. 3, September 1985: pp. 419–455.

falsified. Therefore, a score of 1.6 *may not be good enough*, because it means, though rarely, democracies *have* made war on democracies. At the very least, it should be a cause for concern, because it indicates violations of the proposition, a finding ironically buttressed by the robustness of the methodology Rummel chose.

Second, turning to the reported positive but “nonrobust” support for the Freedom Proposition, 0.17 on the same -2 to 2 scale. In my view, given the error bounds of a study involving subjective coding, 0.17 is equivalent to zero, neutrality. In other words, 0.17 supports that democracies are neither more nor less war prone than nondemocracies when the adversary is a nondemocracy. Somehow, Rummel instead interprets the finding as supportive of the Freedom Proposition, though, as he says, “nonrobustly.”

At this point, in the late 1980s, it would be fair to describe the status of the *DPprop* research as follows: little, if any, dissent from the proposition that democracies do not enter into violent conflict with other democracies had been published. Indeed, on the basis of Small and Singer (1976) and Rummel (1983), Chan (1984) felt secure enough with the soundness of the dyadic proposition that he focused almost entirely on the so-called monadic proposition: democracies are less likely than nondemocracies to be involved in international violence whether against democracies or nondemocracies. Chan found support for Small and Singer’s caution. Indeed Chan found that the monadic proposition was false. Rummel, on the other hand, continued to take the view that the aggregate research supported the validity of the monadic proposition.

It may be useful to digress briefly as to the significance of this debate over the monadic hypothesis. The dyadic hypothesis suggests that there is something exceptional about democracies. But, logically, if that is true, one would think at least some of that

exceptionalism would be extended to relations between democracies and nondemocracies. If it does not, then the dyadic finding too is put in a different light. Put another way, how exceptional are democracies if their pacific tendencies are narrowly confined to others like them?

In 1989, Maoz and Abdolali published a study intended both to reexamine the dyadic preposition and reduce the uncertainty surrounding the monadic proposition.¹¹ To this end, Maoz and Abdolali sought to replicate the earlier studies and to extend them in three ways: They assessed the sensitivity of the existing findings to changes in the definitions of democracy, levels of hostility (as opposed to simply categories), and, most important, to discriminating among armed interstate conflicts on the basis of who initiated them, who was the target, and who simply joined the fight.

With respect to the dyadic preposition, the authors found “clear support” (p. 30) for the notion that democracies never “fight one another.” There is some ambiguity with this finding, since the same data show that democracies do resort to force against one another, but not force above the level defined as war. Strictly speaking, therefore, they do fight one another, “but fall short of making war on one another.”

Second, like Chan, the authors found no support for the monadic proposition. They observed no relationship between regime-type and war involvement with nondemocracies. As I mentioned earlier, that also is identical with Rummel’s 1985 result, though he did not interpret it as such.

Third, they detected an interestingly mixed picture with respect to whether the number of democratic states in the world is positively or negatively related to the total

11. Maoz, Zeev and N. Abdolali, “Regime types and International Conflict, 1816-1976.” *Journal of Conflict Resolution* Vol. 33, No. 1, March 1989: pp. 3–35.

global incidence of interstate conflict. They found that as the proportion of democratic–democratic interaction opportunities rose, so also did the number of international disputes short of war. But the same relationship obtained as the number of autocratic–autocratic interaction opportunities rose. It was only with respect to mixed dyads that they found that the likelihood of war increased with the number of interaction opportunities. All but one of these relationships were sensitive to the year blocks chosen to study. Put another way, the relationships exhibited considerable cross-time variability. The only relationship not sensitive to time was the negative effect of number of democratic–democratic interaction opportunities on the number of wars.

How should these results be interpreted? First, they added to the already strong evidence that democracies rarely, if ever, war against other democracies. It must be kept in mind that this finding was entirely descriptive. Second, they added also to the evidence that, for whatever reason, democracies do not grant the same clemency to nondemocracies. Third, they raised the intriguing possibility that democracy *per se* might not be the causal explanation for the democratic peace, so much as the question of whether potential adversaries had similar regime-types, democratic-democratic or autocratic-autocratic. Put another way, if similarity-dissimilarity was the mechanism at work here, then democracy would not be exceptional. Indeed democracy might simply be an epiphenomenon in understanding the relationship between regime-type and war-proneness.

The study that more or less closed (for a time) the question of whether the dyadic hypothesis is true was that of Maoz and Russett.¹² The authors' objective was to focus on

12. Maoz, Zeev and Bruce Russett (1992), "Alliance, Contiguity, Wealth, and Political Stability: Is the Lack of Conflict Among Democracies a Statistical Artifact?" *International Interactions* Vol. 17, No. 3, pp. 245–267.

discriminating between the effects of regime-type on the one hand, and possibly confounding variables on the other. The possibly confounding variables chosen by the authors to control for were wealth, economic growth, contiguity, common alliance, bonds, and political stability. As with the other studies of the dyadic hypothesis (with the exception of Small and Singer, 1976) Maoz and Russett elected to use the summed dyads approach. The 1946–1986 period was their time domain specifically because this was a period in which the number of independent states increased by approximately 60, thereby increasing the interaction opportunities. Also, the economic data needed in this study became much more available in the post–WWII period.

One point to note is that Moaz and Russett’s data set was composed exclusively of conflicting dyads, which prevented them from assessing the relative likelihood that a given dyad or regime-type will be in a conflict to begin with. Also, the authors used a stringent definition of democracy and assigned any states falling outside that definition to either the anodemocratic or the autocratic category. The effect of this move was to eliminate what other studies identified as marginal cases (e.g., Rummel, 1983), and, as an *a priori* matter, helped ensure a strong dyadic effect.

Indeed, the analysis produced results consistent with the body of research strongly supporting the dyadic proposition. Contiguity was a powerful predictor of dyadic conflict, but that effect was statistically dominated by regime-type when both members of the dyad were democracies. As had others,¹³ Moaz and Russett found that members in an alliance are more likely “than expected” to engage in conflict with one another. The point

13. E.g., Bueno de Mesquita, Bruce (1981), *The War Trap*, New Haven, CT: Yale University Press, and Erich Weede (1984), “Extended Deterrence by Superpower Alliance.” *Journal of Conflict Resolution* Vol. 27: pp. 213–254.

with this type of finding is to remind oneself that conflict is not synonymous with violence. In that light, the finding is neither paradoxical nor counterintuitive. Indeed, it is probably fair to say that members of any type of an association are more likely to engage in conflict with each other than with nonmembers, if only due to the effect of greater interaction opportunities. Political stability also was found to have a strongly inverse relationship with incidence of conflict. Neither wealth nor rate of economic growth was associated positively or negatively with conflict incidence.

I think it is fair to say that this strong study more or less concluded the first phase of broad studies of the *DPprop* and especially the issue of whether the *DPprop* is exclusively a dyadic phenomenon or both dyadic and monadic. In summary, the near-consensus view at this point was that democracies never or rarely make war on one another, though they do dispute with one another, sometimes up to a low level of violence. Less agreement existed on the questions of whether democratic–nondemocratic disputes tended to be rarer or less intense than disputes among other nondemocratic dyads and whether the presence of democracies in the international system made that system less conflictual.

The Monadic Proposition

Benoit observed that researchers seemingly regarded the monadic proposition as entirely unsupported, though the support for it actually was mixed.¹⁴ He argued that three reasons existed “not to praise [the democratic pacificism proposition], so much as to dig it up.” First, he argued that the same normative reasons for suspecting democratic

14. Benoit, Kenneth (December 1996), “Democracies Really Are More Pacific (in General): Reexamining Regime Type and War Involvement.” *Journal of Conflict Resolution* Vol. 40, No. 4: pp. 636–657.

exceptionalism in the first place still existed. Second, the expectation still existed in and out of academia that democracies were more pacific than other regimes. Third, he believed the earlier statistical methods were “clumsy” and that state-of-the-art quantitative methods provided new ways to delve into the question.

Benoit’s criticisms of the existing studies, most of which have been reviewed here, are familiar. He points out that the operationalized definitions of regime-type and conflict involved collapsing more fine-grained scales into aggregates, thereby introducing an artificially contrived “lumpiness” in the scales that masked as much as revealed. He also was the first to note that every previous study had relied on statistical tests of significance unsupported by any foundational knowledge “about the underlying stochastic processes . . . concerning the nature of war events.” As a consequence, in his view, studies producing evidence in support of the monadic hypothesis had been erroneously dismissed on the basis of failing bogus significance testing.

Benoit dismissed the LOGIT and PROBIT regressions used by the other researchers exploring the *DPprop* on the grounds that the distribution of war events was likely to violate severely those procedure’s data distribution requirements for more or less normally distributed data. Instead, Benoit settled on a negative binomial model, which, he believes, will accommodate what he says is the great “variance” of war events data. He applied this model to two war events data sets, used by Butterworth¹⁵ and Small and Singer (1976), and Benoit finds that nondemocracies were two to three times more likely to be involved in wars than democracies. On its face, this result constituted the strongest, least equivocal support of the monadic proposition to date.

15. Butterworth, R. L. (1976), *Managing Interstate Conflict, 1945-1924*. Pittsburgh University Center for International Studies.

One cautionary issue with this study is its limited time term, 1960–1980. Also, Benoit used two scoring schemes for measuring democracy: the index of political democracy (POLDEM) developed by Bollen¹⁶ and the Freedom House index.¹⁷ Unfortunately he used only a single score from each index (POLDEM 1965 and Freedom House 1973) to represent the entire 20 years he studied. One would like to know whether there were significant yearly fluctuations in the scores, and what differences they would have made in his results had they been included.

Second, Benoit is certainly correct about the variability of the data. His own standard error bounds on either side of his regression lines are very large indeed: 100 percent above and below each regression line. Unfortunately, Benoit reported his results as follows: “. . . the findings indicate that democracies fought fewer wars *on average* than less-free regimes during the period observed.” But what can a statistic like an average properly reveal when the variability is so large?

The second notable study in this period focused on the monadic proposition was carried out by Bremer.¹⁸ As did Benoit, Bremer took the dyadic question as essentially closed, but not so the monadic. Using primarily a Poisson model and secondarily a negative binomial model, he tested for the monadic effect while controlling for the following confounding variables: proximity, relative power, alliance membership, power status, economic development, military capability, and the presence or absence of a

16. Bollen, Kenneth (June 1980), “Issues in the Comparative Measurement of Democracy,” *American Sociological Review* 45: pp. 370–390.

17. Taylor, C. L. (1985), *World Handbook of Political and Social Indicators III, 1948-82*, Ann Arbor, MI: Interuniversity Consortium for Political and Social Research.

18. Bremer, Stuart A. (1993), “Democracy and Militarized Interstate Conflict, 1865-1965,” *International Interactions* Vol. 18, No. 3: pp. 231–249.

hegemonic power. He found that, though proximity, the presence or absence of a hegemony, membership in an alliance, and economic development all had “statistically significant” effects on the frequency of conflict and violence among the states considered, the presence or absence of democracy in a dyad was the most potent dampening influence on whether a conflict would escalate to war and an important, though, weaker inhibitor of all conflicts generally.

The third important paper focused on resolving the monadic question was that of Rousseau et al.¹⁹ The authors pointed out the mixed character of the monadic proposition research and designed their study to clarify this picture. To that end, they compared monadic and dyadic effects directly rather than in two separate regressions, as had been customary in the research to this point. Also, in particular, they attempted to tease out the issue of what states were prone to initiating crisis, versus escalating an existing one, versus simply responding to one. Put another way, since the existing research designs had been focused on participation in conflicts and wars, a democratic tendency not to initiate crisis in the first place may have been obscured. They hypothesized that democracies would be less likely to initiate conflicts, regardless of the adversary, so the monadic proposition might be still true even if democracies were participants in conflicts no less often than other states.

As seems to be consistently the case with the monadic proposition, the authors’ results were qualified. They found only weak support for the monadic proposition in that democracies were not significantly less likely than any other regimes to be involved in violent conflict (as opposed to nonmilitary conflict) against nondemocracies. At the same

19. Rousseau, David L. *et al.* (September 1996), “Assessing the Dyadic Nature of the Democratic Peace, 1918-88.” *American Political Science Review* Vol. 90, No. 3: pp. 512–533.

time, however, the authors found more tentative evidence that democracies were less likely to *initiate* these conflicts in the first place regardless of the adversary. Though they found some statistical evidence they deemed suggestive, the authors did not feel confident enough to report it as a principal finding of their work. Instead they commended the question of conflict initiation to future investigation.

At this point in the evolution of the *DPprop* research program, two divergent developments ensued. On the one hand, the research program became increasingly concerned with penetrating more deeply into the *DPprop* phenomenon by examining less the “what” and “whether” of the matter, and more the “why.” At the same time, the first concerted criticisms of the *DPprop* findings were published, attacking both the monadic and dyadic propositions. In order to preserve continuity of this review, I discuss the “why” studies first and reserve discussion of the criticisms and responses for the final part of this section on the *DPprop*.

Explanations for the Dyadic *DPprop*

Since the mid-1990s, the *DPprop* literature has turned increasingly to the question of *why* the dyadic phenomenon exists at all, and, in the case of the monadic phenomenon, why it might not exist. This explanation issue is at the nub of the matter, because it contains in it the answer to the still deeper question: Are the dyadic phenomenon, and perhaps the monadic, inherent in democracy *qua* democracy and thus relatively unchanging, or are they products of a particular time in history and, therefore, transient? The former is the optimistic diagnosis, the latter the pessimistic.

Broadly, the proposed explanations for the “why” of the *DPprop* fall into one of three categories. The first, which has been labeled the normative explanation, argues that

democracies depend upon a set of shared norms such as moderation, compromise, practicality, and reciprocity and that these norms are manifested in a democracy's external behavior, not merely in its domestic behavior. The second, which has been labeled the structural explanation, argues that democracies by definition contain a variety of constraints on the international aggressiveness of its leader, such as public opinion, election, separation of powers, and an independent judiciary.

A third explanation also has been proffered, which takes the view that it is merely traditional strategic interests, which democracies tend to share in common, rather than the inherent characteristics of the regime-type *per se* that explain the evidence for the *DPprop*. Obviously the normative and structural explanations, being inherent in democracies, can be put in the optimistic category. The interest-based explanation can be put in the pessimistic category, since it implies that the *DPprop* would cease to exist once the particular community of interests that characterized the Cold War era ceases to exist, just as Realism has predicted.

Maoz and Russett²⁰ attacked the problem by directly comparing the statistical evidence for each explanation. They used the COW and ICB databases²¹ so as to test the alternative explanations across different data sets, and Gurr's POLITY II data for scoring degree of democracy. They define degree of institutional constraints as an aggregation of degrees of one-man rule, executive constraint, centralization, and scope of government actions, all drawn from the POLITY II. As in their other studies of the *DPprop*, the authors use the LOGIT regression method.

20. Maoz, Zeev and Bruce Russett (September 1993), "Normative and Structural Causes of Democratic Peace, 1946-1986." *American Political Science Review* Vol. 87, No. 3: pp. 624-638.

21. Brecher, Michael and Jonathan Wilkenfeld (1989), *Crisis in the Twentieth Century*. New York: Pergamon.

The authors found significant support for both the normative and structural explanations of the dyadic form of the *DPprop*. The normative explanation, however, provided “a more robust and consistent fit to the data than the structural one”.²² In particular, the normative explanation seemed to be a more powerful inhibitor of both the outbreak *and* escalation of conflict, whereas the structural explanation seemed to be more narrowly an inhibitor of escalation only. Weart²³ obtained similar results in support of the normative explanation.

The relatively weaker support for the structural constraint explanation is plausible. After all, the very institutions and procedures that can constrain a leader seeking war—public opinion, divided powers—can and often do also constrain a leader seeking to avoid one. In other words, constraint *per se* has no necessary policy valence. One must point out, however, the difficulties of operationalizing the presence and strength both of democratic norms and constraints. The authors use the simple existence of democratic institutions as the evidence for the presence and strength of democratic constraints. But there are two problems. First, the presence of the institutions may say little about their weight. Second, defining constraints in this way is circular, since the definition of democracy itself is operationalized precisely in terms of the same set of constraints. Therefore, the authors are measuring the same thing twice and giving them different labels. It is hardly a surprise that democracies score high on the scale of war constraints.

The way democratic norms are operationalized is also problematic, in my view. The factors aggregated into the democratic norm’s score, longevity and lack of internal

22. Maoz and Russett, p. 636.

23. Weart, Spenser (1994), “Peace Among Democratic and Oligarchic Republics.” *Journal of Peace Research* 31: pp. 299–316.

violence, can also be strongly positive in some nondemocratic regimes. Indeed, these two variables, longevity and lack of internal violence, are probably positively correlated in all regimes regardless of type, and hence are too blunt an instrument for “teasing out” the presence or absence of democratic norms. I believe that other indirect indicators, such as degree of freedom of the press and the judiciary, would have made superior measures.

Dixon attempted to tackle this problem of sharpening what should be meant by democratic norms.²⁴ He pointed out that there are different types of democracies, i.e., social democracies and classical liberal democracies, and these may well feature different norms. Therefore, he searched for that subset of norms that are common across all democracies, and he found that norm to be “bounded competition.” Dixon argued that democracies are suffused with all sorts of competition, the political variety being only one example, and that rules and boundaries universally constrain the actions of the competitors in every area of endeavor. These rules are meant to prevent violence and usually contain an explicit or implicit mechanism for adjudicating the competition. Dixon hypothesized that democracies extend this norm into their international behavior, and, therefore, he predicted he would find that two democratic states in conflict will be apt to deploy this norm of bounded competition to avoid escalation. Indeed, he went further to hypothesize that democracies structure their disputes specifically to render them amenable to peaceful resolution.

24. Dixon, William (March 1994), “Democracy and the Peaceful Settlement of International Conflict.” *The American Political Science Review* Vol. 88, No. 1: pp. 14–32.

Precisely because of his sharp focus on the prewar phases of international conflict, Dixon used the Alker-Sherman data set,²⁵ which has fields corresponding to the details of international diplomatic and legal actions. He then used the PROBIT regression method to compare the performances of democracies and nondemocracies from 1945–1979. He found that, indeed, “democracy does carry the direct positive effect on settlement” anticipated by his hypotheses.

This study extends Maoz and Russett (1993), at least as it addresses the problem of operationalizing democratic norms. However, the supporters of the normative explanation still have to confront a problem: Why is the evidence for a general democratic pacific tendency (the monadic proposition) so much weaker than that for the dyadic proposition? If the policies of democracies are suffused with democratic norms, why are they seemingly applied so selectively to other democracies only? And, in view of the fact that, though democracies rarely make war on other democracies, they are quite willing to enter into conflicts with other democracies up to the edge of war, why don’t the democratic norms inhibit more than simply war making? At the very least, this puzzle and apparent incongruity suggests we need to look more closely at what we mean by norms. The sense of that term when applied to social or personal norms may be easily and mistakenly extended to the realm of the political.

Owen offered a solution based on the idea that democratic norms are contingent.²⁶ He based his argument on a different foundation than did Dixon (1994). Owen asserted that classical liberalism is the basis for democratic norms: free individuals pursuing their

25. Alker, Hayward R., and Frank L. Sherman (1986), “International Conflict Episodes, 1945-1979.” *Inter-University Consortium for Political and Social Research*, Ann Arbor.

26. Owen, John M. (Fall 1994), “How Liberalism Produces Democratic Peace.” *International Security* Vol. 19, No. 2: pp. 87–125.

own well-being and self-fulfillment. Protections against coercion and violence such as free speech and regular elections are essential to permit individuals the required freedom of action. Indeed, in such societies, coercion and violence from any source are generally inimical. Therefore, all citizens of democracies share an interest in peace, and regard war only as an instrument to protect or restore peace. Democracies see that other democracies share these views and are therefore pacific and trustworthy. On the other hand, they believe nondemocracies may not share these views, and that they may attempt to take advantage of democratic reluctance to reach for war, if necessary. Therefore, Owen argued, democracies can confidently rely on negotiation and compromise with other democracies, but must be far more self-protective and mistrustful when dealing with nondemocracies. Thus democracies are pacific toward one another and entirely different toward nondemocracies. His analysis supports this argument.

There may be a circularity in this argument. Democracies do not fight each other because they know that democracies do not fight each other. Put another way, on what basis do democracies feel safe in stringently avoiding war with other democracies? Because they know that democracies do not fight one another. But why? Because they feel safe relying on nonmilitary means to resolve their conflicts. Why? Because democracies do not fight one another, and so on. For this reason, it is not clear to me whether Owen has, in fact, explained what he set out to explain.

Even though Maoz and Russett (1993) found that support for the structural explanation of the *DPprop* was weaker than that for the normative, other researchers have not agreed. For example, Morgan and Campbell found that for major powers, higher

levels of constraints on national leaders are responsible for lowering the probability that conflicts will escalate to war.²⁷ They do not find a similar mechanism at work for minor powers.

The authors began by noting the continuing problem of explaining how it can be that democracies do not fight democracies, but are as war-prone as any other regime-type when the adversary is a nondemocracy. If democracies are exceptional, why is their exceptionalism so strongly dependent on the regime-type of the adversary? Morgan and Campbell acknowledged that, despite research to the contrary, democracies in truth may be less war-prone even with nondemocracies. This would be the most direct explanation for the puzzle, hence the continuing research conducted on the monadic proposition.

Morgan and Campbell, however, chose to accept the weight of the evidence that democratic exceptionalism does not extend to nondemocracies, and they proposed that this apparent puzzle could be explained by a better understanding of the specific mechanisms by which democratic leaders are constrained. Specifically, the authors focused on three types of domestic constraints on a leader's freedom of action: accountability to a constituency whether through election or not; permanent, highly institutionalized political competition; and divided or shared decision-making power. The authors made special note of the fact that none of these constraints need be uniquely the product of a democratic regime. Indeed, certain types of nondemocratic regimes may be equally constrained. Morgan and Campbell hypothesized that these constraints would operate by reducing the probability of escalation in a dispute rather than reducing the

27. Morgan, T. Clifton and Sally Howard Campbell (1991), "Domestic Structure, Decisional Constraints and War: Why Kant Democracies Fight?" *Journal of Conflict Resolution* Vol. 35, No. 2: pp. 87–211.

probability of *involvement* in disputes in the first place. Note that Maoz and Russett (1994) found that structural constraints work in exactly this way, although with lesser weight than democratic norms.

The authors tested their hypothesis using the COW data set for conflicts and the POLITY II data set to operationally define domestic constraints. Their expectation was that the more constrained participants in conflicts were, the less likely they would escalate those conflicts.

Initial LOGIT regressions produced no support for the hypothesis. However, upon controlling for each state's strategic power or capabilities, the authors found their hypothesis was supported for highly capable states and poorly supported for less capable states. Morgan and Campbell suggested this may be because major powers actually can choose whether or not to escalate a conflict, whereas minor states more often are *compelled* to act rather than independently *deciding* to act. It follows that decision constraints ought to be more apparent with major powers than with minor ones.

There is, I believe, an *ad hoc* quality of this study, and the fact that, in the end, it is hard to say the picture has been clarified. The authors' expectations originally did not include the apparent importance of a state's power status. That variable was reached for only after no support was found for the study's original hypothesis. When that variable was included, support for the hypothesis was found, albeit still at relatively weak levels. But, even if the authors' interpretation is true, it seems to me they have succeeded only in making their original puzzle more puzzling. What explains the apparent difference between major and minor states? Is it really the case that the latter do not have the freedom to make decisions, so decision constraints are irrelevant for explaining their

behavior? What about when minor states are in conflicts with other minor states? Of course, the authors' proposed explanation is always possible, but, in my view, it is unlikely because of the puzzles posed here.

Fearon posed a different sort of structural argument based on what he called "audience cost."²⁸ The author posits a model of international crisis as a "war of attrition in which state leaders choose at each moment whether to attack, back down, or escalate." The concept of audience cost refers to the ability of some domestic political constituency to punish a leader for the decisions he makes, thereby constraining him, in this case in conflicts. Fearon argued that this yields a prediction that states in which large audience costs can be generated will be less apt to escalate a crisis, but once escalated, also less apt back down. Democracies, he argued, are capable of generating higher audience costs than any other regime-type, and this is widely understood by both democratic and nondemocratic leaders. Fearon suggested this sort of domestic structural feature may be at the heart of the phenomenon that democracies do not fight democracies.

The problem here is that Fearon's hypothesis exists only as a formal, rational choice model. It has been tested by no data. Therefore, one can say it is interesting, and deserving of being put to the test. But, beyond that, it is hard to go further.

For that reason, Eyerman and Hart put it to an empirical test.²⁹ Their hypotheses were that, based on Fearon's model, nondemocracies should exhibit more activity in pushing conflicts up and down the intensity scale as they maneuver freely for advantage, but democracies, once in a conflict crisis, should be constrained by audience costs in their

28. Fearon, James D. (1994), "Domestic Political Audiences and the Escalation of International Disputes." *The American Political Science Review* Vol. 88, No. 3, September: pp. 577–592.

29. Eyerman, Joe and Robert A. Hart Jr. (December 1996), "An Empirical Test of the Audience Cost Proposition." *Journal of Conflict Resolution* Vol. 40, No. 4: pp. 597–616.

ability to escalate and deescalate fluidly. Therefore democracies will manifest less “activity per conflict per unit time” than nondemocracies. Once committed, democracies, because of their stronger audience costs, will be less inclined to engage in complex moves up and down the escalation ladder. Rather, they will move directly and without bluff. To explore this prediction, the authors used the SHERFACS data set,³⁰ which contains fields for six distinct phases of conflict behavior. In principle, participants in a crisis may iterate endlessly among these phases. The time period of the study was 1945–1984. A Poisson regression model was selected. The authors found statistically significant support for their hypotheses, and, therefore, Fearon’s audience cost model.

But, more substantively, I believe there is a problematic assumption at the heart of the audience cost hypothesis: Democracies generate stronger audience costs than nondemocracies. The unspoken rationale for this assumption seems to be that leaders in nondemocracies are not so accountable to their domestic audiences. But that is to confuse “audience” with the “public” or the “electorate.” Virtually all leaders, democratic or nondemocratic, depend upon the support of constituencies, and those comprise the audience. In democracies, the constituency may be a subset of the public or the electorate. In nondemocracies, that constituency may be the military, a segment of the elite, an ideological party, an ethnic group, and the like. *A priori*, it is hard to understand why a nondemocratic leader would be any less vulnerable to his audience than a democratic leader would be to his. Therefore, audience cost ought not to be a very good discriminator of democratic versus nondemocratic state behavior. Until this problem is grappled with, it is hard to see what more can be said about the audience cost hypothesis.

30. Sherman, Frank L. (1994), “SHERFACS: A Cross-Paradigm, Hierarchical, and Contextually Sensitive, Conflict Management Data Set.” *International Interactions* 20: pp. 79–100.

Criticisms of the *DPprop* Research

At this point, the discussion shifts to the dissenting voices in the *DPprop* debate. In the mid-1990s, several cogent criticisms were published directed at the heart of the *DPprop*: the dyadic proposition.

Spiro raised a potentially lethal question: Is “the statistic that democracies never (or rarely) fight wars with each other significant?”³¹ Significance here is meant in the statistical sense. As every researcher has pointed out, both democratic regimes and wars are rare. Therefore, should we be surprised that wars between democracies seem also to be rare or even nonexistent? Spiro pointed out that the apparent equal willingness of democracies and nondemocracies to enter into war with nondemocracies is revealing. If either structure or norms were the explanations of the *DPprop*, the author would have expected at least *some* disinclination of democracies to make war, yet the bulk of the evidence supports the opposite view. These points caused Spiro to look more deeply into the statistical significance of zero wars.

To that end, the author compared zero wars with what would be expected by random chance for the period 1816–1980. He found that in the 19th century, the complete absence of wars observed between democracies and random chance are essentially identical, largely because so few democracies existed. In the 20th century his results are more perplexing and difficult to understand, at least as he argued them. In the 20th century, there are many more democracies than in the 19th, though still not many in an absolute sense. Spiro calculated the probability of war by “chance” between democracies for each year, a procedure which showed, not surprisingly, that the chance of war

31. Spiro, David E. (Fall 1994), “The Insignificance of the Liberal Peace.” *International Security* Vol. 19, No. 2: pp. 50–86.

between democracies was zero or very close to zero for almost every year. The exceptions are during the two World Wars where the chances of war between democracies was calculated to be relatively high. During those periods, the absence of wars between democracies presumably would be difficult for Spiro to explain. But he argued that the World Wars were atypical and, therefore, should be excluded. Once that was done, he argued that the difference between the chance likelihood of wars between democracies and the observed number, none, was insignificant. As has been discussed earlier, much usually depends on the classification of the regime-type of crucial states like Wilhelmine Germany, but, of course, here that was obviated by simply excluding it from the time period under consideration.

Spiro's question, whether zero wars is impressive or not, gets right at the nub of the matter, but, unless I misunderstand exactly what he did, his study seems not to have made his case. If one assumes that each year is independent of the last, then the procedure for establishing an aggregate probability across a span of years is multiplicative. Across approximately 200 years, the combined probability of war between democracies by chance is quite large, which suggests the number observed, zero or few, is worthy of explanation. On the other hand, if the years are not independent, the procedures for calculating a conditional probability would be employed, although how exactly that could be done in this sort of case is not clear to me. The point is that Spiro seems not to have done either one of these procedures in reaching his conclusion.

If I do misunderstand Spiro's work, my mistake is shared by Russett who responded to Spiro's points along exactly the lines just discussed.³² On its face, the

32. Russett, Bruce (Spring 1995), "The Democratic Peace—And Yet It Moves," *International Security* Vol. 19, No. 4: pp. 164–175.

procedure for determining the aggregate probability of independent events (multiplicative) yields an exceedingly small likelihood that the absence of war among democracies is due to random chance. He also pointed out, correctly in my view, that Spiro had the same problems over definitions of war and democracy that Spiro ascribes to the *DPprop* proponents.

Oren pursued a different line of criticism than Spiro's.³³ He pointed out, convincingly in my view, that the *DPprop* proponents had been ethnocentric in their various coding schemes for defining democracy. He argued that in every definition of democracy used, American-style democracy received the highest ratings, and that these coding schemes were applied retroactively to eras where the definition of democracy was importantly different. His particular illustration was the treatment of Wilhelmine Germany. Oren demonstrated that Wilhelmine Germany was widely regarded both as a state with democratic or certainly republican characteristics and as model to be emulated by democracies. Presumably, if the *DPprop* is valid, democracies eschew wars with other democracies based on perceptions of one another's regime *at the time*. If Wilhelmine Germany was regarded as a progressive model by democracies, then WWI would constitute an overwhelming example of democracies fighting one another.

I think this line of criticism is quite telling, and, so far as I've been able to determine, no rebuttal to Oren's argument has been published. It also implicitly contains a research program, which is to focus more on how the states regard themselves and potential adversaries as opposed to relying on a third party viewpoint—the researcher—particularly when that viewpoint is anachronistic.

33. Oren, Ido (Fall 1995), "The Subjectivity of the 'Democratic' Peace." *International Security* Vol. 20, No. 2: pp. 147–184.

The third major criticism of this period was provided by Layne, who took a methodological approach similar to that of Oren.³⁴ Layne focused on four case studies of “near misses”; that is four incidents nearly resulting in wars between democracies. He used the case studies to examine the ways in which regime-type did or did not figure in the decision making of the national leadership, and, in particular, their decisions to avoid war. The cases were:

- a. U.S. and Great Britain in 1861: the Trent Affair
- b. U.S. and Great Britain in 1895-96: The Venezuela Crisis
- c. France and Great Britain in 1898: The Fashoda Crisis, and
- d. France and Germany in 1923: The Ruhr Crisis.

In all cases, Layne found that the decision not to resort to war by one side or the other was driven by the balance of military capabilities on the scene. He could detect no overt indication of the influence of democratic norms, and the democratic structural constraints usually constrained more accommodating decisions than more bellicose ones. He found the last case to be a particularly unvarnished case of a democracy making war on another.

Russett (1994) responded to Layne, though, in my view, rather tentatively. He began by pointing the difference between not finding discernable democratic influences in the decision making and their actually nonexistence. The difficulty of divining intent at such a distance in time is indeed extreme. One might literally have been a “fly on the wall” during Fashoda or the Ruhr Crisis and still not know the role of democratic norms or constraints. As a consequence, he argued that these crises are open to alternative

34. Layne, Christopher (Fall 1994), “Kant or Cant: The Myth of the Democratic Peace.” *International Security* Vol. 19, No. 2: pp. 5–49.

explanations, such as Owen (1994) provided in support of the *DPprop*. Finally, Russett asked, even if Layne's interpretations are essentially granted, "So what?" Democracy is not the *only* reason democracies avoid war with each other, only a reason and balance of power is another.

I believe Layne got the better of this exchange. Owen's (1994) case studies, when read closely, seemed not to contravene Layne's points. For example, he agreed with Layne that the U.S. conceded the Trent Affair to Great Britain, because the Union was already fully absorbed in the Civil War (Owen, 1994, p. 111). Owen argued that Great Britain elected not to intervene in the Civil War out of liberal sentiment. That may well be so, but that was not the case Layne provided. Similarly in the Venezuelan Crisis, evidence of the role of liberal sentiment is hard to find, and Owen, himself, points more to what he calls "Anglo-Saxon chauvinism," (1994, 115) than to shared democracy as a force driving the eventual peaceful resolution. The Ruhr Crisis seems incontestable, and Owen does not include it.

With respect to Russett's argument that shared democracy is a sufficient condition for avoiding war, but not the only condition capable of producing that outcome, I believe Russett seems to have moderated or qualified the claims made for the *DPprop*. The proponents of the *DPprop* wish to argue for democratic exceptionalism, that shared democracy is a powerful force driving a remarkable phenomenon: the absence of war between democracies. Therefore, it seems suddenly mild to then argue that, after all, shared democracy is merely one of other factors (perhaps many other factors) causing democracies to refrain from warring on each other. The exceptionalism argument seems diminished by Russett's line here. Also, several of the studies already discussed in this

literature review (i.e., Maoz and Russett, 1992; Maoz and Russett, 1993) included balance of power-related variables like military capabilities or wealth. According to those studies, no variable was remotely as powerful as shared democracy for explaining the absence of war between democracies.

The *DPprop* and Other Independent Variables Other Than Regime Type

There is another line of research that sometimes can be considered an extension of the *DPprop* and sometimes a criticism of it. Its focus is on what other independent variables other than regime type are weighty in influencing a state's international behavior. In some cases, as related below, this research is designed to assess the combined contributions of regime type and those other variables such as international trade or national interest. In other cases this research aims to *substitute* other variables for regime type and thereby explain away the results of the *DPprop* literature as simply the result of confounding democracy with other, more fundamental factors. Pollins laid the ground work for this line of inquiry when he explored the connection between trade relations and political relations using a public choice model.³⁵ He found a strong connection between these two variables.

Dixon and Moon took the next step for this by showing convincingly that similarity in states regime-types and foreign policy orientations is strongly associated with increased trade levels between those states.³⁶

Taken together, these studies provided the outlines of an alternative argument to the *DPprop*. Trade is associated with reduced conflict and war. States trade more with

35. Pollins, Michael (June 1989), "Does Trade Still Follow the Flag?" *Political Research Quarterly* Vol. 83, No. 2: pp. 466–480.

36. Dixon, William J. and Bruce E. Moon (March 1993), "Political Similarity and American Foreign Trade Patterns." *Political Research Quarterly* Vol. 46, No. 1: pp. 5–25.

states with which they have positive political relations. Therefore the questions arises as to the division of labor between democracy and economic interdependence as the explanation for the *DPprop*.

Oneal et al. suggested that trade might be a *reinforcing* factor in the *DPprop*, as oppoed to a competing explanation for why states refrain from warring with one another.³⁷ Using LOGIT regression, the authors found that trade and economic interdependence are strongly associated with the absence of war regardless of regime-type.

Oneal and Ray attempted to address the division of labor by employing some methodological improvements in their coding of degrees of democracy.³⁸ They reported support for a “democracy effect” separate and more powerful than economic interdependence in explaining the absence of war between democracies, and they also showed a weaker, more general, pacific tendency of democracies towards their trading partners, regardless of the regime.

Oneal and Russett significantly extended this line of research on the *DPprop* and the pacifying influence of international trade in three ways.³⁹ First, they sought to settle the conflicting findings with respect to the effects of international trade. Second, they sought to assess the roles of regime types and international trade as complimenting and amplifying factors versus alternative explanations of propensity for conflict. Third, they

37. Oneal, John R. and Francis H. Oneal, Zeev Maoz, and Bruce Russett (1996), “The Liberal Peace: Interdependence, Democracy, and International Conflict, 1950–1985.” *Journal of Peace Research* Vol. 33, No. 1: pp. 11–28.

38. Oneal, John R. and James Lee Ray (1997), “New Tests of the Democratic Peace Controlling for Economic Interdependence, 1950–85,” December (1997) *Political Research Quarterly* Vol. ?, No. ?: pp. 751–775.

39. Oneal, J.R. and B. Russett, 1997. “The Classical Liberals Were Right: Democracy, Interdependence, and Conflict, 1950–1985.” *International Studies Quarterly*; 41: 267–294.

revisited the still disputed question of whether democracies are more pacific than other regimes as moorads. Fourth, they reviewed the findings of Mansfield and Snyder (1995-1996) that new democracies and regimes transitioning from autocracy to democracy have a significantly elevated propensity for entering into militarized disputes.

The methodology Oneal and Russett elected to use was essentially the same as their earlier studies: a pooled cross-sectional and time series design employing the LOGIT procedure. The data covered the period, 1950-1985, and was arranged in “politically-relevant” dyads. As before, they used dyads, because they felt *a priori* that the *DPprop*, as essentially a dyadic phenomenon, permitted the easy creation of dyadic variables, such as joint democracy, shared alliance memberships, and, especially, dyadic trade.

The dependent variable was presence/absence of militarized conflict using the COW data set. The independent variables were level of democracy, extent of regime transition from autocracy to democracy, dyadic economic interdependence, alliance membership, contiguity, the ratio of dyadic national capabilities and rate of economic growth.

The authors’ results supported all of their predictions. In brief, they found that, as shown before, democracies do not fight each other and international trade is an unambiguously powerful pacifying variable. Indeed, its weight is a good deal larger than that for democracy, though the latter is statistically quite significant. They also found no support for the view that states in transition between autocracy and democracy are at an elevated risk for armed interstate conflicts. But, as is discussed below, a number of methodological questions, especially with the creation of variables, make the picture less clear, in my view.

Although used by virtually all researchers in this area, there are serious difficulties with the use of dyads as the unit of examination. It compels the creation of composite or dyadic variables reflecting the amalgamated scores of the two dyad partners. Sometimes the scores are simply added or averaged, sometimes they are combined more complexly. For example the dyadic score for membership in inter-governmental organizations has been simply the addition of the memberships of each partner. The dyadic democracy score has been a more complex addition of the two scores divided by the difference of the two scores. The problem with all these methods is that they can obscure the relative contributions of each dyad partner. That is, identical dyadic scores can be produced if one partner is high and the other low on a particular score or if both partners are middling. Yet, one would think those ought not be equivalent cases. In a similar way, zeros can be problematic for this method of variable creation, if they happen to fall in the denominator. A second problem with several of the dyadic variables is that they do not scale properly. For example, an older method of scoring democracy, used by Maoz and Russett, included a factor representing Concentration of Power, which was combined with the individual democracy scores of each dyad partner. That produced results so distorting as to compel Russett and Oneal to discard the variable altogether.

The strategy the authors used for coping with this problem is to adopt the “weak link hypothesis, first suggested by Dixon (1994.) This hypothesis posits that the less “constrained” a state is, the greater its propensity to become engaged in militarized disputes. Though not stated explicitly, it appears that Oneal and Russett equate constraint with the degree of freedom of action a state enjoys. On this basis, the authors assert that autocracies are less constrained politically than democracies and states for which trade

constitutes a less fraction of GDP are less constrained than those for which trade is a greater fraction of GDP. With this as their theoretical framework, the authors avoid *joint* dyadic measures of democracy and economic interdependence by using, instead, the lower democracy and economic interdependence score for each dyad.

The problem with this approach is that I do not understand why in principle and observation should we posit that democracies are more constrained in their international behavior than autocracies? Perhaps the theoretical explanation is that autocracies are unconstrained by public opinion and other branches of government. However, in my view, that misconstrues the internal politics of autocratic states. Stalin and Hitler may have felt few constraints, but such autocracies are rare. By far the rule is that autocracies depend on the maintenance of constituencies through rewards and punishment, and, depending upon the context, severe constraints can thereby be imposed on autocratic regimes. Consider the regime of Saudi Arabia. Its ability to associate with other states and pursue certain policies (e.g. the war on terrorism) is severely circumscribed by the need to placate internal groups (e.g. the Wahabi clergy). Consider Iran until recently. The power of the theocrats and revolutionary paramilitary organizations prevented Iranian secular officials from pursuing a pragmatic course (e.g. freeing the U.S. hostages). Consider the post-Stalinist leadership in the U.S.S.R. It is clear that Khrushchev and Brezhnev had to maintain constituencies within the politburo and the party apparatus a requirement that eliminated many possible courses of action.

Consider also the weight of constraints in democracies. By virtue of the circumstances and his political skills, U.S. presidents such as Lincoln, Theodore Roosevelt, F.D.R., L.B.J., and the current president, have enjoyed periods of

extraordinary lack of constraint. Finally, note too, that U.S. presidents also can labor under constraints that prevent them from *avoiding* or *ending* conflict, as opposed to entering them. Neither J.F.K. nor L.B.J. felt they were free to ignore the conflict in Vietnam, and L.B.J. felt enormous constraints on pursuing policies to end that conflict with a negotiated settlement with the North.

On the eve of the war with Spain in 1898, the U.S. public was intensely jingoistic. President McKinley would have risked his presidency and his party's future had he elected to avoid war, as he was very much disposed to do.

In sum, I do not believe there is evidence to believe that democracies are less war-like, because they are more constrained and that autocracies are more war-like because they are less. As the core of the author's case is based on this weak link hypothesis, I must question the basis for the authors' results.

The case for applying the weak link hypothesis to economic interdependency is probably stronger, though again, not ambiguously so. All else being equal, it is logical to argue that greater economic interdependency ought to equate to greater incentives to avoid war and greater opportunity costs if a states does not.

However, I believe there are problems in the way Oneal and Russett choose to operationalize economic interdependency. As mentioned earlier, their economic interdependency variable is composed of the sum of a state's exports and imports to a dyadic partner divided by the size of the state's GDP.

The first question to ask is whether a particular ratio of trade to GDP is sensitive to the size of that GDP? For example, are the following two states equally economically interdependent:

1. State A with \$10 of trade and a GDP of \$100
2. State B with \$100,000 of trade and a GDP of \$1,000,000.

The interdependence ratios are identical for each state, but it is hard to avoid the suspicion that State A will suffer the greater consequences for ceasing to trade than State B. Obviously much depends on exogenous factors like population. If State A has a population of one individual and State B a population of a million, then, on the basis impact per capita incomes, one would think that State B would suffer most of the loss of trade. Other population figures would reverse that conclusion. Put another way, the absolute size of the trade/GDP fraction is not the same as the utility of that fraction to any particular state.

The second question to be addressed is the availability of substitutes for the trade lost in case of war. Those substitutes are almost certain to be available, *at some price*. And that, of course, is the rub. The trade/GDP fraction may be less important to understanding the costs of war than the difference in the resources needed to replace the lost trade or offset it in some way as a fraction of GDP.

Similarly, the trade (GDP fraction in peacetime may not portray the opportunity costs of war properly, depending upon the extent that the economy of a state changes as it moves from peace to war. Suppose a particular trading relationship consists entirely of luxury goods. In peacetime, those goods are in demand and, over the long term, comparative advantage ensures that such a trade is beneficial to both parties. Now suppose the demand for those goods drops to zero, perhaps because of war, perhaps not. What exactly has been lost? Each state will require some time to adjust, but beyond that point, each state's resources will again be allocated more-or-less appropriately of its

aggregate priorities. In wartime, those priorities will be military. Wars assuredly impose costs; they also confer economic effects of going to war, the trade/GDP fraction may be a poor reflection of how a state may come to think about its gains and losses.

Here again, Oneal and Russett can point to their results. Putting aside the question of whether a high trade/GDP fraction is constraining, this research and that of others show a strong connection between trade and diminished conflict. I think these results are compelling, but they leave open the question of why. If my points are correct, then the explanation is not so straightforward as simply the effect of the opportunity cost of lost trade.

In *Triangular Peace*,⁴⁰ Oneal and Russett attempted the political science equivalent of a Grand Unified Theory in physics. They began by returning to Kant and noted that Kant's prescription for international peace has three elements:

1. Republics with constitutions,
2. International trade, and,
3. An international organization of republican states.

Substantial research supports the first premise at least dyadically, and the second. But the third remained to be assessed and verified. Therefore, the authors posed the question, "What pacifying effects (if any) are provided by inter-governmental organizations?" In doing so, the authors endeavored to integrate partially the Realist and Liberal paradigms by hypothesizing that both Realist and Liberal variables can contribute to peace. Specifically, they acknowledged the role of national power to deter

40. Russett, B and J. R. Oneal, 2001. *Triangulating Peace: Democracy, Interdependence and International Organizations*. New York: Norton

war, though only temporarily and tenuously. But to move beyond this state of affairs, the authors argued that the Kantian triad of republics, trade, and intergovernmental organizations are necessary.

The authors designed their research to include both these Realist and Liberal variables. As with their previous work, they used LOGIT regression to assess relationships among state dyads arrayed in a pooled cross-sectional time series. The years covered were 1885 to 1992.

The dependent variable was militarized interstate disputes drawn from the COW data set. The dyadic independent variables are as follows:

1. Democracy
2. Economic interdependence
3. Joint IGO memberships
4. National capabilities ratio
5. Alliance membership
6. Contiguity
7. Distance

They are identical to those used by Oneal and Russett (1997) plus an international organization variable. Russett and Oneal also, created several Kantian and Realist-oriented system-level independent variables. The Kantian variables are:

1. Average democracy each year,
2. Each state's standing relative to that average,
3. Average economic interdependence each year,
4. Each state's standing relative to that average,

5. Average number of intergovernmental organization, and
6. Each state's standing relative to that average.

The Realist systemic variables are:

1. Degree a state enjoys hegemonic capabilities to suppress conflict,
2. Satisfaction with the status quo, and
3. Degree hegemonic states feel threatened, thereby causing systemic tensions and greater conflict.

The author's first evaluated the Kantian peace triad using the weak-link hypothesis as described above in Oneal and Russett (1997). As before, they found strong and significant support for the dyadic *DPprop* and the pacifying effects of international trade, especially for the latter variable. However, they did not find a significant relationship between the incidence of militarize international disputes and membership in inter-governmental organizations.

On the other hand, they also found a highly significant inverse relationship between the ratio of national capabilities and the incidence of militarized interstate disputes. This supported earlier findings that preponderance of power rather than power balances is conducive to pacific behavior within dyads.

Russett and Oneal then estimated the increased or decreased probability of militarized interstate disputes caused by the increase or decrease of the Kantian and Realist variables scores. They found that increasing democracy and international trade scores by one standard deviation reduced the probability of militarized international disputes by approximately thirty five and fifty percent respectively. Increasing the national capability ratio by one standard deviation reduced the probability of international

disputes by approximately the same amount as increasing democracy. However, the contribution of membership in inter-governmental organizations remained lowest, between ten and fifteen percent reduction in the probability of militarized interstate disputes, though the authors argue that the contribution is increasing steadily across time.

Russett and Oneal then turned examining the Kantian and Realist systemic variables with similar results. Systemic levels of democracy and international trade were negatively related to the incidence of militarized international disputes to a highly significant level. Memberships in inter-governmental organizations, again, did not have a statistically significant relationship to the dependent variable. As before, preponderant power had a negative and significant relationship to the dependent variable of approximately the size of the democracy relationship.

With respect to the Realist systemic variables, in addition to the importance of the national capability ratio, the authors found that the degree of hegemonic power was negatively related to the systemic incidence of militarized interstate disputes. That is, the greater the extent of power concentration in one state, the lower incidence of militarized interstate disputes. Similarly, a hegemon's sense of its own security was negatively and significantly related to the dependent variable. That is, the less secure a hegemon feels (regardless of the reality of matter), the greater the likelihood of militarized interstate disputes. The remaining Realist systemic variable, satisfaction with the status quo, proved not to have a significant relationship with the dependent variable.

In their conclusion, the authors argued that they have made their case. The Kantian triad of democracy, trade, and inter-governmental organizations are the way forward to enduring positive peace with the Realist power-related factors providing

shorter-term- more tenuous *negative* peace simultaneously. Even better, in the authors' view, the Kantian-developments can be expected to continue and gather momentum driven by the confluence of common national interests, rather than by a new and implausible international selflessness that must be assumed will grip the world.

Much of *Triangulating Peace* is drawn from Oneal and Russett (1997) already reviewed, so I only summarize those remarks before discussing issues specific to *Triangulating Peace*.

As with Oneal and Russett (1997), Russett and Oneal (2001) used the weak-link hypothesis, which has the collateral benefit of permitting dyadic analysis without necessitating creation of aggregated dyadic variables. I discussed reservations about that hypothesis earlier. Equally important issues are raised with the way authors have operationalized some of their most important variables. As already discussed, I disagree with interpreting a state's trade/GDP fraction as its disincentive to go to war with its trading partner is deeply flawed as a matter of economics.

The authors' formulation of dyadic national capability ratio seems to me to be problematic. Russett and Oneal defined national capability as an aggregate of a state's population, industrial output, and military power. This may be a quite reasonable operational definition if one is trying to research the long-term patterns of how and why states rise and fall in power. But considerations of whether or not to engage in a specific militarized international dispute tend to be much more short term and more focused on the purely military dimensions of national power rather than population or industrial capacity. Obviously longer term considerations have to be thought through to the extent a

state's leadership anticipates a protracted total war. But, even in those cases, the record suggests leaderships tend to under-estimate radically the durations of war they enter.

The military power components of the national capability ratio are also mistaken, in my view. This is because the measures of military power they incorporate into their variable are static and focused entirely on quantity of weapons and forces. Quality is excluded. So also are the factors that shape what fraction of a state's military forces it can actually bring to bear. Consider, Arab-Israeli national capability ratio in light of these points, or that of the U.S. and U.S.S.R.

Next is the variable measuring membership in inter-governmental organizations. Each state's score is simply the sum of all the inter-governmental organizations of which it is a member. I think this variable construction illustrates the difficulties to which the dyadic approach is prone. There are many inter-governmental organizations, and they differ in terms of importance and effectiveness. But the dyadic score cannot discriminate between a state which belongs to the small number of most important and effective inter-governmental organizations and a state which belongs only to the much larger number of lesser inter-governmental organizations. Ironically, this problem in variable construction may well have defeated the authors' expectation of finding a stronger pacifying influence for inter-governmental organizations. Perhaps, if the variable had incorporated some consideration of efficacy and importance versus simply number, the significance of this variable might have proved greater. Finally, Russett and Oneal observed that the pacifying influence of inter-government organizations is growing across time. So far as I can tell, they did not control for the fact that the

number of inter-governmental organizations has grown enormously since the end of World War II, the meaning of the apparent growing pacifying influence of these is not clear.

Finally, I want to register disagreement with the way that Russett and Oneal operationalize the effects of insecurity felt by a hegemon. They represented insecurity as simply the percent of GDP allocated to national defense. I think there are significant problems with this formulation. The percentage of GDP states allocate to defense is driven by a complex interaction of forces. Some have to do with the international insecurity a state feels, many do not. For example, the domestic inputs to a state's defense budget are well-understood, though difficult to measure reliably. But, more important, the authors' measure has two degrees of freedom. The percentage of GDP allocated to defense is a function of insecurity *and* the absolute size of the state's GDP. For example, it is unclear what percentage of GDP the Soviets allocated to defense, but the current view is that the figure fell between fifteen and thirty percent depending upon the period. The U.S. percentage for most of the Cold War was well below ten percent. Is there any basis for saying the U.S. felt less threatened than the U.S.S.R., and, therefore, less driven to spend for defense? I do not believe so. The more convincing explanation, by far, is that America's GDP was far larger than that of the Soviets (not that their GDP could be accurately calculated), and that the U.S. was a more efficient producer of defense capability.

In sum, with the exception of the democracy score derived from the POLITY data set, I disagree with the way the authors constructed and operationalized the dyadic variables, international trade, membership in inter-governmental organizations, and national security ratios, for which the authors found significant relationships supporting

their hypothesis. So also do I disagree with the weak-link hypothesis which is central to the authors' research design. Yet, in the end, they do find strong and significant relationships consistent with their predictions. In my opinion, the following conclusions best fit the situation: The authors have confirmed again the dyadic *DPprop* finding. But, beyond that, the relationships they produced connecting international trade, inter-governmental organizations, and national capability with a state's propensity for militarized interstate disputes, while plausible and well-based theoretically, do not have the same clear weight of evidence supporting the *DPprop*.

Farber and Gowa focus on the Realist variable, traditional strategic behavior, as the variable most weighty in explaining international behavior. This led them to argue that the democratic peace is merely a proposition limited to the Cold War and the particular traditional strategic interests that characterized the anticommunist coalition.⁴¹

Their results were challenged on both theoretical and technical grounds by Thompson and Tucker, who argued that far from being restricted to the Cold War period, pre-1914 democratic dyads show even less of a tendency to engage in militarized conflict.⁴²

Despite some spirited give and take between Farber and Gowa on the one hand and Thompson and Tucker on the other, think either emerged as decisively better supported by the evidence. Farber and Gowa claimed the logic of their analysis led them to argue that prior to 1914, the likelihood of war between democracies actually was higher than for other types of dyads due to their conflicting interests. But no other researcher has ever reported a similar finding.

41. Farber, Henry S. and Joanne Gowa (May 1997), "Common Interests or Common Politics?" *The Journal of Politics* Vol. 59, No. 2: pp. 393–417.

42. Thompson, William R. and Richard Tucker (June 1997), "A Tale of Two Democratic Peace Critiques," *Journal of Conflict Resolution* Vol. 41, No. 3: pp. 428–454.

Democratic Performance in Crises and Wars

Reiter and Stam assessed the relationship between domestic political institutions, specifically in democracies, and war outcomes⁴³ to see whether democracies operate under some systemically related advantages or disadvantage. Obviously, there is a strong connection between these kinds of questions and structural explanations for the *DPprop*.

The authors used the COW data set for all interstate wars between 1816 and 1982, and they posed three hypotheses:

- a. War initiators are more likely to win than targets.
- b. Democratic targets are more likely to win than other kinds of targets.
- c. Democratic initiators are most likely to win, followed by dictatorships, followed by mixed regimes.

Reiter and Stam found statistically significant support for all three, though that support was weaker for the second than for the other two. The authors surmised that the explanation was a combination of greater regime legitimacy, which permitted more effective mobilization of resources, and greater leadership caution, founded in the fear of public disapproval over a lost war, within the audience cost hypothesis. As the authors noted, this result, being consistent with the structural explanation, has important ramifications for the debate over normative versus structural explanations for the *DPprop*.

Bennett and Stam took the Reiter and Stam research (1998) one step further: Does the democratic war-making advantage that Reiter and Stam's results indicate diminish, increase, or stay the same over time?⁴⁴ The authors developed a dynamic model to test

43. Reiter, Dan and Allan C. Stam III (June 1998), "Democracy, War Initiation, and Victory." *American Political Science Review* Vol. 92, No. 2: pp. 377–389.

44. Bennett, D. Scott and Allan C. Stam III (June 1998), "The Declining Advantages of Democracy." *Journal of Conflict Resolution* Vol. 42, No. 3: pp. 344–366.

this issue for all wars from 1816–1990. They found again the Reiter and Stam democratic advantage, but they also found this advantage diminished over time until, after about a year at war, the greater chances of prevailing shifted to the nondemocratic side. The authors attributed this phenomenon to the decrease of democratic public approval that sets in when wars are protracted. This again provided support for the structural explanations of the *DPprop* and specifically Fearon’s audience cost hypothesis.

Gelpi and Griesdorf expanded the Reiter and Stam findings by asking whether democracies also possess an advantage over other regime-types in resolving crises in their favor.⁴⁵ The authors used the ICB data set for international crises from 1918–1990. They hypothesized that, according to Fearon’s audience cost hypothesis, democratic initiators would be more likely than nondemocratic leaders to prevail in crises and that democratic defenders would be less likely. This logic is based on the notion that democratic leaders will not initiate crises without large and robust public support as a precondition. That is exactly what their results showed.

Regime Transition and War Proneness

Mansfield and Snyder posed a question arising out of both the normative and structural explanations for the *DPprop*: Is the war proneness of democracies related to how recently they became democracies?⁴⁶ The authors proposed that during the transitional phase of nondemocracy to democracy, newly democratic states become more war prone than nondemocracies, not less, and that, in that early phase, they do fight wars against other democratic states. The authors found strong support for this hypothesis. On average, newly

45. Gelpi, Christopher F. and Michael Griesdorf (September 2001), “Winners or Losers: Democracies in International Crisis, 1918-1994.” *American Political Science Review* Vol. 95, No. 3: pp. 633–647.

46. Mansfield, Edward D. and Jack Snyder (Summer 1995), “Democratization and the Danger of War.” *International Security* Vol. 20, No. 1: pp. 5–38.

democratized states were about 60 percent more likely to go to war than nondemocratic states. This period of elevated war proneness begins about one year after the new regime is established and can last five to ten years. Further, the authors found that newly democratized states are even somewhat more likely to engage in war than states in transition to a new nondemocratic regime, although the war proneness of both types of regimes is elevated during its early years.

The suggested cause of this phenomenon is interesting. The authors attributed this tendency to the *regime weakness* arising from threatened traditional elites at risk of displacement and to the need of the new regimes to recruit new elites and mass supporters. They supported this argument with four case studies: Victorian Britain, the France of Napoléon III, Bismarckian and Wilhelmine Germany, and Taisho Japan. So far as I can discover, this is the first time the weak regime literature was brought to bear in the *DPprop* research. It is especially interesting, because the strong-weak dimension poses another explanation for the *DPprop* that could threaten to confound considerably the arguments for democratic exceptionalism. I have developed the weak regime theme more extensively below.

Thompson and Tucker attempted to rebut these findings.⁴⁷ First, they pointed out that, by Mansfield and Snyder's own evidence, both democratizing and "autocratizing" regimes are more war prone than established regimes. Second, they identified apparent methodological mistakes made by Mansfield and Snyder that bring their findings into doubt.

The exchange ended with Mansfield and Snyder's reply⁴⁸ in which the authors used a revised statistical model to restudy the effects of newness. The effect of the model

47. Thompson, William R. and Richard Tucker (June 1997), "A Tale of Two Democratic Peace Critiques." *Journal of Conflict Resolution* Vol. 41, No. 3: pp. 428–454.

48. Mansfield, Edward D. and Jack Snyder (June 1997), "A Reply to Thompson and Tucker." *Journal of Conflict Resolution* Vol. 41, No. 3: 457–461.

was to make even more similar the comparative war proneness of both new democracies and nondemocracies, though again the transitioning democracies apparently manifested the tendency more strongly.

This argument, like so many others of its type, cannot be settled definitively, but it seems to me it masks the more interesting question. The issue is not so much whether the tendency is stronger or equal to new nondemocracies, but rather why the tendency is raised at all in both regime-types. This points to a third variable hitherto omitted from the discussion, the weak state dimension to which all new regimes may be vulnerable, and which I address at greater length later in this chapter.

Diversionsary War and Regime Type

The connection between domestic conflict and the decision to initiate war has become elevated to the status of folk wisdom. Shakespeare instructed the statesmen of his day to “busy giddy minds with foreign quarrels.”⁴⁹ Jean Bodin argued in the same vein when he said that “the best way of preserving a state, and guaranteeing it against sedition, rebellion, and civil war is to. . . find an enemy against whom (the subjects) can make common cause.”⁵⁰ Shakespeare and Bodin reveal themselves to be adherents of what today is called the diversionary theory of war⁵¹—although “theory” here is used in a loose sense. The diversionary theory suggests that a major cause of war lies in regimes’ efforts to divert their populations’ attention away from internal discontents by creating an

49. Shakespeare, W., *King Henry IV*, London: Shakespeare Society, 1845.

50. Bodin, J., *Six Books of the Commonwealth*, Oxford: Basil Blackwell, 1955.

51. Note that both of these admonitions are aimed at the problem of bolstering *popular* support for the national leadership. This is the first indication that, as is so often the case, this bit of folk wisdom misses an important part of the puzzle. For in many types of regimes, popular support is only secondary to elite support.

external conflict. The expectation is that even a population fragmented by internal divisions will unite to face a common enemy from outside. Put another way, the diversionary theory assumes a hierarchy of values and loyalties in which the bond of common membership in a state will be revealed as stronger than more local tensions when the state is placed at risk.

Arguments in support of the diversionary theory are especially prevalent in the historical analyses of European wars. For example, Fritz Fisher and David Kaiser argued that all German foreign policy after 1897 was a response to the “internal threat of socialism and democracy.”⁵² Similarly, James Joll takes the view that Austria-Hungary’s policy toward Serbia was “wholly the product of its internal politics.”⁵³ There is a large body of history based on the diversionary theory as applied to such wars as the Thirty Years’ War, the French Revolutionary Wars, and World War II.⁵⁴

In the same vein, many historians take the view that French policy in the Crimean War was driven by Louis Napoleon’s need to bolster his support by French Catholics by opposing Russian Orthodoxy. White⁵⁵ and Langer⁵⁶ argue that Russia welcomed war with Japan in 1904 to quiet revolutionary agitation.

52. See Fisher, Fritz, *War of Illusions*, New York, 1975; and David Kaiser, “Germany and the Origins of the First World War,” *Journal of Modern History* LV, 1983, p. 443.

53. Joll, James, *The Origins of the First World War*, New York, 1984, pp. 92–122.

54. See, for example, Gutman, Myron P., “The Origins of the Thirty Years’ War,” *Journal of Interdisciplinary History* XVIII, 1988: pp. 749–770; T. Blanning, *The Origins of the French Revolutionary Wars*, New York, 1986, and J. Hughes, “The Origins of World War II in Europe: British Deterrence Failure and German Expansionism,” *Journal of Interdisciplinary History* XVIII, 1988: pp. 831–891.

55. White, J., *The Diplomacy of the Russo-Japanese War*, Princeton: Princeton University Press, 1964.

56. Langer, W., “The Origin of the Russo-Japanese War,” in Langer (ed.) *Explorations in Crises*, Cambridge: Harvard University Press, 1969.

Indeed, the diversionary theory has such a following in the historical literature that Mayer⁵⁷ argues that most, if not all, wars in the developed world since 1870 have their origins in “domestic politics rather than. . .foreign policy and international politics.” According to him, the war aims of national leaders is “to reestablish political and civil society along lines favorable to the hegemonic [domestic] bloc, notably to certain factions, interests, and individuals within that.”⁵⁸ Specifically, he says that the diversionary theory explains World Wars I and II, the Franco-Prussian War, the French Revolutionary Wars, and the Crimean War.

Support for the diversionary theory is to be found outside of history scholarship, as well. Rosecrance (1963)⁵⁹ used historical case studies to explain why some periods in Europe have been peaceful and others conflictual. By assessing nine European international systems between 1790 and 1960, he was led to conclude that, regardless of period or regime-type, the internal security of political elites was the primary determinant of peace and war decisions by those elites. When those elites were secure (however they defined security), peace was likely. When internal elites felt themselves threatened by domestic happenings, external conflict was made more likely. Haas and Whiting⁶⁰ reached similar conclusions.

57. Mayer, A., “Domestic Causes and Purposes of War in Europe, 1870–1956,” in Krieger and Stern (eds.) *The Responsibility of Power*, New York: Doubleday, 1967; _____, “Industrial Crises and War Since 1870,” Bertrand (ed.) *Revolutionary Situations in Europe, 1917–1922*, Montreal: Inter-University Centre for European Studies, 1977.

58. op. cit., p. 122.

59. Rosecrance, R., *Action and Reaction in World Politics*, Boston: Little Brown, 1963.

60. Haas, E. and A. Whiting, *Dynamics of International Relations*, New York: McGraw-Hill, 1956.

The explanatory power of the diversionary theory was heightened for these scholars by a proposed explanation for its workings, the so-called in-group/out-group or conflict cohesion hypothesis arising from sociology and anthropology. The hypothesis predicts that increases in tension with an out-group produces increased cohesion among the in-group. Simmel⁶¹ was the first who applied the in-group/out-group hypothesis to international relations, if only descriptively.

The in-group/out-group hypothesis also suggests that under some circumstances, increased tension with the out-group will aggravate in-group tensions rather than alleviate them. Simmel acknowledged this risk for national politicians, which is why he felt that using war as a diversion was a policy of truly last resort. Coser⁶² went a step further and posited the conditions in which in-group coherence would be reliably reduced rather than exacerbated. He said one or more of the following conditions must be obtained for the policy to succeed:

- a. The in-group must have existed as an entity prior to the crisis;
- b. The in-group cannot have fallen below some minimal level of cohesion;
- c. The in-group members must be conscious of their membership, regard it as positive, and believe that it distinguishes them from nonmembers; and/or
- d. The threat to the in-group must be perceived as equally threatening the entire group more or less, rather than one segment, which could be sacrificed.

The first three of these conditions are tautological in that they seem inherent in the definition of in-group. However, the last condition is interesting, and corresponds to a

61. Simmel, G., *Conflict*, Glencoe, Illinois: Free Press, 1956.

62. Coser, L., *The Function of Social Conflict*, New York: Free Press, 1956.

common tactic for dismantling an in-group: threaten an unpopular segment of an in-group, and promise that one's demands will cease with its liquidation.

The in-group/out-group hypothesis has received considerable empirical support at the small group level by social psychological research. Indeed, the evidence for the existence of in-group/out-group behavior was deemed so strong that Dahrendorf⁶³ suggested it should be accorded universal legal status along with the "Iron Law of Oligarchy." Specifically, he says that, "It appears to be a general law that human groups react to external pressure by increased internal coherence."⁶⁴

Though the empirical evidence for the in-group/out-group hypothesis was confined to small groups, Wright⁶⁵ felt free to extrapolate from this research to assert that, "War or fear of war has often been used to integrate states." He felt so sure of this point as to posit rather majestically that "the direct relationship between political revolution and war, whether as cause or effect is in fact such a historical commonplace as to need no elaboration."⁶⁶ Unfortunately, for a relationship cited with such certainty, empirical research in international relations has often yielded surprisingly equivocal, ambiguous, and inconsistent results when applied to the level of national decision making.

Though, it may be the customary usage, I will not use the term diversionary war to describe the phenomenon of regimes making war and peace decisions driven by

63. Dahrendorf, R., *Class and Class Conflict in Industrial Society*, Stanford: Stanford University Press, 1964.

64. *Ibid.*, p. 17.

65. Wright, Q., *A Study of War*, 2nd ed., Chicago: University of Chicago Press, 1965, p. 257.

66. *Ibid.*

domestic politics. National leaders may well use international events to divert their constituents' collective attention from domestic difficulties. But they also may use international events, not so much by divert, but rather to provide an occasion for showing to their constituencies, their leadership strengths or for reminding their constituencies that the national ties the bind should overcome domestic travails. The occasion for making these points to political supporters may not be confined only to periods of political weakness. For this reason, I prefer the formulation "externalized conflicts" to diversionary wars.

As usual with many lines of research, investigation of the diversionary war hypothesis has come in distinct waves. Although seen as axiomatic by Wright and others, the first wave of this research method failed to find convincing evidence of such a relationship. Even in those studies where some correlation is found, the relationship was deemed weak at best.

The earliest study, which strives for current standards of rigor, is Sorokin's.⁶⁷ He aggregated longitudinal data on the internal and external conflict behavior of major states beginning with ancient Greece, Rome, and Byzantium and finishing with European Great powers of the late 1920s. Given the size of the time period he considered, Sorokin felt compelled to aggregate his data into 250-year units. He performed no formal statistical analysis beyond simply inspecting it. He found no evidence of any systematic relationship between internal and external conflict.

Obviously, Sorokin's work is vulnerable to criticism, though not for his decision to avoid formal statistical analysis. Inspection of his data should have

67. Sorokin, P., *Social and Cultural Dynamics*, Vol. III: *Fluctuation of Social Relationships, War, and Revolution*, New York: American Book Company, 1937.

revealed a relationship, if one was there to found. More troublesome were his 250-year data “chunks” which would have prevented all but the most blatant relationships from being visible.

The earliest research to apply formal quantitative methods is that of R. Cattell.⁶⁸ A sociologist, Cattell was interested in discovering patterns of highly aggregated societal behavior, which could be associated with cultural differences. To that end, in his work of 1950, he performed a factor analysis on a large number of variables representing the national characteristics of 69 states from 1837 to 1937. This produced 12 orthogonal factors. He deemed two of these factors to represent internal and external conflict respectively. Factor 1, entitled internal conflict, was composed of numbers of political assassinations, riots and local rebellions, and secret treaties. Factor 2, entitled external conflict, was composed of clashes with other states, participation in interstate wars, and treaties with other states. With the exception of treaties, Factors 1 and 2 were uncorrelated. Hence, Cattell reported no relationship between internal and external conflict.

Cattell’s data on 29 of the 69 states was spotty and unreliable. Therefore, for follow-on research published in 1951, he dropped those 29 and redid the factor analysis for the remaining 40 states. He obtained quite different results. Factors 1 and 2 could be reproduced. However, each loaded on a third factor. (See Table 1)

68. Cattell, R., “The Dimensions of Culture Patterns by Factorization of National Characters,” *Journal of Abnormal and Social Psychology* 1949, 44: pp. 443–469; “The Principal Culture Patterns Discoverable in the Syntal Dimensions of Existing Nation,” *Journal of Social Psychology* 1950, 31: pp. 215–253; and H. Bruel, H. Hartman, “An Attempt at a More Refined Definition of the Cultural Dimensions of Syntality in Modern Nations,” *American Sociological Review* 1951, 17: pp. 408–421.

LOADING	VARIABLE
0.70	Clashes with other states
0.60	Riots
0.60	Treaties
0.60	Secret treaties
0.58	Interstate Wars

Table 1: The Relationships between Cattell's Internal and External Conflict Factors

This result suggested that a third variable may mediate the relationship between internal and external conflict—the type of state or governing regime. By dropping 29 states with poor data, Cattell eliminated mostly poor, authoritarian Third World states and retained mostly wealthy, democratic industrialized states. The Soviet Union and East European states were also included so that the inadvertent taxonomy of Cattell's was crude. However, he unquestionably skewed his experimental population in the direction of a particular type of state and political system. In that skewed population, he obtained evidence of a relationship between certain types of internal and external conflict. In a general population, he found no relationship. This basic finding, that state- or regime-type may mediate the internal-external conflict relationship, is the only positive relationship that has received consistent support from other studies.

Rummel⁶⁹ performed a similar, though more ambitious, study using the Dimensions of Nations data. This study quickly became the “gold standard” for

69. Rummel, R., “Dimensions of Conflict Behavior Within and Between Nations,” *General Systems* 8, 1963: pp. 1–50.

researchers of the domestic policy/foreign policy linkage, so it's appropriate to describe Rummel's results in some detail. The DON project contained the following coding categories, which were judged to be related to domestic or foreign conflict behavior.

DOMESTIC CONFLICT BEHAVIOR MEASURES

- Number of assassinations
- Number of general strikes
- Presence/absence of guerrilla warfare
- Number of major government crises
- Number of purges
- Number of riots
- Number of revolutions
- Number of antigovernmental demonstrations
- Number of people killed in all forms of domestic violence.

FOREIGN CONFLICT BEHAVIOR MEASURES

- Number of antforeign demonstrations
- Number of negative sanctions
- Number of protests
- Number of countries with which diplomatic relations were severed
- Number of ambassadors expelled or recalled
- Number of diplomatic officials of less-than-ambassador rank expelled or recalled
- Number of threats
- Presence/absence of military action
- Number of wars

- Number of troop movements
- Number of mobilizations
- Number of accusations
- Number of people killed in all forms of foreign conflict behavior.

Data were coded according to these categories for all states with populations over 800,000 (77 states) for the years 1955–1957.

It is a mark of Rummel's care that he ran three checks to assure himself that such a narrow span of years was representative and adequate to support generalization. Each of his checks involved comparison with other large data bases of national behavior: Lewis F. Richardson's data on war from 1825–1945, Harry Eckstein's data on domestic conflict from 1945–1959, and Raymond Cattell's five measures of conflict behavior from 1837–1937. He obtained high correlations between his 3 years of data and the other three data bases suggesting that the period 1955–1957 were not atypical.

Rummel first performed separate factor analyses on the Domestic Conflict Behavior Measures and the Foreign Conflict Behavior Measures. The results are reproduced in Table 2.

Measures	Factor Matrix				Orthogonal		
	D ₁	D ₂	D ₃	Turmoil	Revolutionary	Subversive	H ²
Assassinations	(62)	17	(61)	(59)	-03	(66)	78
Gen. Strikes	(75)	05	-24	(52)	(60)	05	63
Guerr. War	48	(-57)	(59)	-04	28	(90)	90
Maj. Gov. Crisis	(52)	35	-10	(60)	21	-04	41
Purges	(67)	-14	-34	32	(71)	03	60
Riots	(76)	38	-03	(79)	31	09	73
Revolutions	(66)	-43	-36	09	(85)	13	75
Antigov. Dem.	(74)	46	14	(85)	17	19	79
Num. Killed	(80)	-40	-05	23	(75)	42	79
% Common Variance	65	18.0	16.0	39.0	37.6	23.4	100.0
% Total Variance	45.8	13.3	11.7	27.7	26.7	16.6	70.8

Table 2: Rummel's Factor Analysis of Domestic Conflict Measures

Rummel describes this matrix as supporting three factors, which he labels Turmoil ("nonorganizational, spontaneous" conflict), Revolutionary ("overt, organized" conflict), and Subversive ("covert, organized" conflict). As is so often the case, the process of ordering the data with factors analysis raises important questions. For example, these factor labels are confusing, since the distinction between revolution and subversion is not that one is overt and the other covert. Revolution denotes overturning of a state's political and social structures. Subversion is a tactic, which may or may not be associated with revolutionary action. In any case, revolutionary activities usually must have a large covert element for obvious reasons.

This would be a purely semantic curiosity except that the awkwardness of the factor labels is a reflection of a more general difficulty in making coherent sense of Rummel's results. Why should "Major Governmental Crises" be associated with "Riots" and "Antigovernmental Demonstrations" but not with "Purges?" Why should "Guerrilla Warfare" not be associated with "Revolutions" or "Domestic Number Killed?" Why should "Purges" be associated with "General Strikes." In summary, there may be three domestic factors obtained, but it is very difficult to understand what they are and why they should be considered "factors" other than that a factor analyses said they were.

The same problem afflicts the factor analysis of Foreign Conflict Measures reproduced in Table 3.

Measures	Factor Matrix				Orthogonal		H ²
	F ₂	F ₂	F ₃	War	Diplomatic	Belligerency	
Anti-Foreign demonstrations	(59)	14	48	13	42	(63)	60
Negative Sanctions	(65)	11	44	20	41	(64)	62
Protests	(78)	22	-13	(62)	49	22	67
Severance of Diplo. Relations	45	-46	(54)	13	-17	(82)	71
Expulsions of Ambass.	10	(67)	12	-16	(66)	-08	47
Expulsions of lessers	(53)	44	-06	33	(60)	08	48
Threats	(91)	07	05	(65)	43	48	84
Mil. Action	(73)	-48	04	(65)	-14	(57)	77
Wars	(65)	-04	(-57)	(85)	15	-10	75
Troop Mov.	(73)	33	01	47	(59)	28	64
Mobs	(61)	-34	-10	(60)	-08	35	49
Accusations	(89)	01	-05	(70)	35	41	79
Num. Killed	(80)	-19	-36	(87)	10	19	80

Table 3: Rummel's Factor Analysis of Foreign Conflict Measures

Again, the origin of these factors is more tortured than one would like. Rummel named the first rotated factor "War" because of its high loadings of "Wars," "Mobilizations," and "Number Killed." The problem is that "Protests," "Threats," and "Military Action" also load heavily on the first factor "War." However, Rummel elected to disregard those loadings because "Protests," "Threats," and "Military Action" are

correlated $\pm .4$ or greater with one or more of the other dimensions. This decision is entirely arbitrary and created an artificially distinct factor, which Rummel promptly ramified by naming it “War.”

The second factor he labeled “Diplomacy” to reflect “nonviolent” conflict behavior. But why didn’t “Severance of Diplomatic Relations” also load on this factor? On the other hand, why did “Troop Movements” load heavily on this factor. “Troop Movements” may not be synonymous to violence, but it surely exceeds nonviolence, as well.

Finally, Rummel named the third factor “Belligerence” to characterize “an actively hostile mood” independent of “War” and “Diplomatic.” But “Military Action” loaded almost as heavily on “Belligerence” as on “War,” so it is difficult to see how the two factors should have been considered independent. Also, why did “War” load *negatively* on “Belligerence?” Intuitively, one would assume that an “actively hostile mood” is *positively* associated with the outbreak of fighting. One’s assumptions are frequently wrong, but very counterintuitive relationships call for explanation.

Again, these points would be of less concern if Rummel had not promptly begun utilizing his factor names as though they were not his inventions. For example, he said that “a war continuum accounting for the most variance . . . may not perhaps appear strange to students of international relations, who look upon war as a prime mechanism through which the international systems adjusts to changes within the system.” This would be true except that Rummel and “students of international relations” each use the term “war” to mean quite different things. The same is true of the other two factors, which seem to have only weak unifying themes.

Rummel then employed multiple regression to assess the extent to which the Domestic Conflict Behavior Dimensions (Turmoil, Revolution, and Subversion) explained the Foreign Conflict Behavior Dimensions (War, Diplomacy, and Belligerence) and vice versa. The regression matrix in which the Domestic Conflict Behavior Dimensions are the independent variables appears in Table 4.

Dependent Variable	SD	SE	Multiple R	R ²
War	2.40	2.36	.26	.07
Diplomacy	1.49	1.46	.26	.07
Belligerence	1.00	.97	.31	.10

Table 4: Rummel's Matrix Produced by Regressing Domestic and Foreign Conflict Dimensions (Domestic Are Independent Variables)

When the Foreign Conflict Behavior Dimensions are independent, the results are in Table 5.

Dependent Variable	SD	SE	Multiple R	R ²
Turmoil	2.43	2.31	.37	.14
Revolution	1.73	1.70	.27	.07
Subversion	1.00	1.01	.14	.02

Table 5: Rummel's Matrix Produced by Regressing Domestic and Foreign Conflict Dimenisons (Foreign Are Independent Variables)

Plainly, the dimensions obtained by Rummel's factor analysis are almost unrelated. *However, this is not logically equivalent to concluding that internal and external conflict is not related.* Given the inconsistencies in the constituents of the factors, it is difficult to know whether or not they adequately represent internal and external conflict to the degree necessary to have confidence that the two are not related.

Tanter replicated Rummel's study for the years 1958–1960.⁷⁰ The time period he assessed contained 83 rather than 77 states as with Rummel. Tanter used the DON data set and employed virtually the identical methodology. Because of the difference in the time period, Tanter obtained a slightly different factor structure than did Rummel, which caused him to combine Rummel's "Subversion" and "Revolution" variables into one entitled, "Internal War." Aside from that, the factors were identical between the two studies.

70. "Dimensions of Conflict Behavior Within and Between Nations, 1958–60," *Journal of Conflict Resolution* 10 (1), 1966: pp. 41–64.

Tanter's results echo Rummel's quite closely in suggesting, at best, only "a small relationship between 1958–1960 domestic and foreign conflict which increases with a time lag. Not surprisingly, Tanter's study also replicates Rummel's weaknesses.

It is probably not an exaggeration to say that the Rummel and Tanter studies created a stir among scholars expecting to see the common wisdom validated.

Nicholson⁷¹ characterized the findings as "startling," and Calhoun,⁷² an historian, suggested that "the worldly. . . historian might be amazed at the finding offered by Rummel." Obviously, not all the reactions were favorable, but the Rummel/Tanter research provided a reference against which virtually later work has been evaluated.

Therefore, it is useful to summarize the strengths and weaknesses of this work, and what one can and cannot say based on it.

Its primary strength was the researchers' care, explicitness, meticulousness, and intellectual integrity. Their diligence in providing all details of what they did and why make it a model for quantitative IR research. These characteristics permit one to retrace their steps and quarrel with some of their judgments.

Its primary weakness is the general weakness of this genre: it is atheoretical. It is precisely for that reason that Rummel and Tanter are compelled to use such a broad-gauge methodology as factor analysis. The problem is, absent some prior idea as to how internal and external conflict might be related, Rummel and Tanter are forced to make several problematic methodological decisions.

71. Nicholson, M., *Conflict Analysis*, London: English Universities Press, 1971, p. 50.

72. Calhoun, D., "Review of M. Stohl, *War and Domestic Political Violence*," *Journal of Interdisciplinary History* 1978, (9): pp. 189–190.

First, they choose as an N virtually all states. This means that they assume that the internal-external conflict relationship must function in more or less the same in every state regardless of the many differences, which would otherwise distinguish. For example, the states differ in wealth, military capabilities, and regime-type, all of which might bear on the I-E question. By treating them all identically, Rummel and Tanter risk drowning out the more subtle relationships that could exist within subpopulations of states.

The second point to note is Rummel and Tanter's implicit assumption that quantity of events and their weight or influence are more or less synonymous. For example, five assassinations are considered in the Rummel methodology a more weighty stimulus to foreign conflict than one. But, as a substantive matter this simply is not so. The assassination of the Archduke Ferdinand in Sarajevo in 1914 may have been the proximate (though surely not the fundamental) cause of World War I. It occurred at precisely the "right" moment with precisely the "right" actors involved. These factors of timing and context may determine the weightiness of this assassination as a cause of wars far more than number of people killed. The same weakness can be associated with all their measures; indeed it is an inevitable difficulty for this type of methodology. So the fact that, in some cases, five assassinations are not at all associated with war, but, in another case, one assassination is associated with an enormous war is not evidence that assassinations are unrelated to war—even though the correlation in this hypothetical would be essentially zero.

The third and related point to note is that Rummel and Tanter are necessarily made captive to measures of conflict, which can be counted more or less easily. This is a particular problem with the "Domestic Conflict Behavior Measures," for the degree to

which domestic conflict is public may depend heavily on the character of the state and regime. Public, and thereby countable, displays of conflict may be more likely in democratic states or those authoritarian states with only weak powers of internal suppression. In states with large, efficient police organizations, domestic conflict may be considerable but not in the easily measured manifestation Rummel and Tanter consider. For example, counting the numbers of purges may be a poor way of determining whether an authoritarian regime is in conflict with its supporters. What if the regime is too weak to purge its opponents safely? What if a purge of a sort does take place covertly as apparently has happened regularly in the current Iraqi regime. Certainly the workings of different sorts of states and regimes give *prima facie* reason to expect that domestic conflict can take many different forms, not all of them appropriately measured by aggregate quantitative and not all of them even detectable using any analytical method.

The effect of not remedying this problem will be to underweight nonpublic manifestations of domestic conflict—which may be precisely the ones most significant. They also increase the potential importance of Cattell's finding a decade earlier that a positive association between some types of internal conflict and external conflict does exist for modern industrialized states. The combined impact of these problems may be severe. Certainly they do not permit the general conclusion that internal and external conflicts in international politics are unrelated. Rummel's results do suggest (and no more than that) that external conflict may not be related to certain public forms of internal conflict, and even that conclusion must be qualified by the possible impact of aggregating all the states together.

Considering these problems, it makes sense to examine whether the simple bivariate correlations between each internal conflict variable and each external conflict variable contain any statistically significant relationships obscured by the consolidation into factors. In fact, there are three such bivariate correlations: (a) Purges and Accusations, (b) Government Crises and Severance of Diplomatic Relations, and (c) Riots and Anti-Foreign Demonstrations. Presumably the last relationship, (c), can be dropped because it is almost tautological: riots of one sort frequently are associated with riots of another.

However, relationships (a) and (b) potentially are very interesting. According to Rummel/Tanter's definitions, a "governmental crisis" is "any rapidly developing situation which threatens to bring the immediate downfall of the present government. . . evidenced by the declaration of military law, a state of siege or the suspension or abrogation of the constitution." A "purge" refers to "the systematic elimination by the political elite either of opposition within their ranks or of opposition within the country by jailing or execution.

Thus, the strongest bivariate relationships between internal and external conflict have a common theme: government weakness or instability. "Purges" generally are undertaken out of fear of possibly disloyal domestic opposition powerful enough to pose a serious threat to the group in power. "Major government crises" is descriptive of domestic political weakness, as well. A simple bivariate correlation (or two) is not sufficient to conclude that an important casual relationship exists. However, it is suggestive, occurring as it does in both Rummel's and Tanter's research, and constitutes an interesting focus for additional research on the role of regime—or state—type on the relationship between internal and external conflict.

Wilkenfeld⁷³ addressed this question by investigating the hypothesis that “within certain types of nations, classified according to type of nation, there tends to be a relationship between internal (domestic) conflict behavior and external (foreign) conflict behavior.” He used the same DON data as did Rummel and Tanter to cover the period 1955–1960. Attempting to mirror Rummel and Tanter, Wilkenfeld used the same measures of domestic and foreign conflict behavior grouped by means of factor analysis into three domestic conflict dimensions (Turmoil, Revolution, and Subversim) and three foreign conflict dimensions (War, Diplomacy, and Belligerence).

Unlike Rummel and Tanter, Wilkenfeld then sought to divide the states in his population according to their regime-type. He elected to apply the Banks-Gregg taxonomy⁷⁴ which used a Q-factor analysis to establish five groups: centralist states (institutionalized, highly authoritarian states, such as the USSR and the PRC), personalist states (weakly institutionalized, personal dictatorships, such as some Latin American and African states), elitist states (regimes created to govern after a colonial power exits), traditional states (states governed by hereditary monarchs or tribal leaders, such as Saudi Arabia and Kuwait), and polyarchic states (pluralistic, decentralized regimes, such as the governments of the U.S. and Western Europe). The problem in using this taxonomy is that, while the Rummel-Tanter factors of domestic and foreign conflict were based on data from 1955 to 1960, the Banks-Gregg factors were based on data from 1955 to 1963. A large number of new nations were created from former colonies during the 1960 to

73. Wilkenfeld, J., “Domestic and Foreign Conflict Behavior of Nations,” *Journal of Peace Research* 1, 1968: pp. 56–69; _____, “Some Further Findings Regarding the Domestic and Foreign Conflict Behavior of Nations,” *Journal of Peace Research* 7, 1969: pp. 147–152.

74. Banks, A. and P. Gregg, “Grouping Political System: Q-Factor Analysis of a Cross-Policy Survey,” *The American Behavior Scientist* 9 Nov. 1965: pp. 3–6.

1963 period. For this reason, Wilkenfeld decided to use only the states included in the Rummel (1963)⁷⁵ study, which required the exclusion of 38 states, which Banks, and Gregg had used in their factor analysis. As a result, the Elitist and Traditional-states categories were reduced so much that Wilkenfeld elected to collapse what remained of these two groups into the Centralist-, Personalist-, and Polyarchic-state groups. Unfortunately, this meant that a nontrivial number of states had to be placed into groups, which poorly represented their regime-types. Since, regime-type is the crucial variable of the study, this categorization problem is potentially troublesome.

Wilkenfeld's analysis was conducted in two steps. In step 1, he used the factor scores obtained by Rummel which express the extent to which each state in the test population engaged in the three types of domestic conflict behavior (Turmoil, Revolution, and Subversion) and the three types of foreign conflict behavior (War, Diplomacy, and Belligerence).

He found significantly positive relationships between domestic and foreign conflict for each of the three groups of states, but no relationship held across all three groups. For Personalist states, Turmoil and Subversion were significantly related to Diplomatic tension. For Centralist states, Turmoil was significantly related to Belligerency and Diplomatic tension and Revolution to war. For Polyarchic states, Turmoil was related significantly to War and Revolution to Belligerency.

The reason these results are interesting is that they suggest Wilkenfeld's hypothesis about the impact of regime-type may be correct. However, as they stand, the results are not interpretable without "finer-grain" analysis, which Wilkenfeld carried out in step 2.

75. Rummel, R., op. cit.

In step 2, Wilkenfeld elected to use Rummel and Tanter's *raw scores* (which were available for each state, for each of the six measures of domestic and foreign conflict, for each year between 1955 and 1960) in place of the *factor scores*, which were available only in an aggregate form for the entire test period. He combined the raw scores to create dimensions of foreign and domestic conflict. In order to carry out this step, he relied on some data transformations (grouping according to geometric progression and logarithmic), which make his manipulations less than transparent. In any case, he emerged from this process with a raw score for each nation on each of the six dimensions (Turmoil, Revolution, Subversion, War, Diplomacy, and Belligerence) for each of the six years studied. It must be pointed out that, although these dimensions have the same titles as those of Rummel and Tanter, their contents may or may not be the same. One really cannot know, since their construction from transformed raw scores obscures their relationship to dimensions obtained from the factor scores.

Having arrived at a set of data by year, Wilkenfeld was in a position to test for time lag effects between internal and external conflict. Again, regime- or state-type produced a strong effect. For the Personalist states (mostly Latin American dictatorships), all types of internal conflict were most strongly related to external conflict behavior in the Diplomacy category. Put operationally, internal conflict in these states often was accompanied by expulsions or recalls of ambassadors. Introducing a time lag weakens that relationship. Unfortunately, while the strongest relationship was to be found between internal conflict and Diplomacy, other significant relationships were also present; e.g., between Turmoil and Belligerency and between Subversion and Belligerency. Wilkenfeld could not explain these other relationships. Nor is it clear why he attended less to them

than to that between internal conflict and Diplomatic tension. Indeed when one looks at the correlations, it is hard to resist the impression that they are scattered more or less randomly. See Table 6.

	-2	-1	0	1	2
TUR-WAR	.22	.15	.14	-.05	.01
TUR-DIP	-.01	.23*	.29*	.22	.16
TUR-BEL	.17	.25*	.20	.21	.40*
REV-WAR	.23	.00	.13	-.05	-.16
REV-DIP	.12	.15	.29*	.23*	.22
REV-BEL	.29*	.18	.14	.18	.31*
SUB-WAR	.33	.28	.24*	.14	-.01
SUB-DIP	.05	.34*	.49*	.11	-.04
SUB-BEL	.38*	.37*	.34*	.31*	.40*

*=Significant Correlations

Table 6: Wilkenfeld's Lagged Correlations between Internal and External Conflict

Note also the disorderliness of many of the rows as they rise and fall erratically. These problems afflict Wilkenfeld's conclusions about other types of states as well.

Centralist states evidenced a relationship between Turmoil and Belligerency for the time lag = 0 case. However, unlike Personalist states, Centralist states experienced external conflict one or two years after Revolutionary-type internal conflict. However,

that is also true (though less significantly) of Turmoil and external conflict. Again, it is not clear why Wilkenfeld felt free to ignore that latter relationship except to have done so would have made his results less striking.

For Polyarchic states, Wilkenfeld acknowledged that no “clear pattern of relations between internal and external conflict behavior” emerged. Though, here again, the lack of pattern in this case seems no greater than in the Personalist-state group. The strongest of the scattered relationships was Turmoil and all forms of external conflict. Wilkenfeld conceded that “no ready answer is available to deal with this phenomenon, except to suggest that the answer may lie in the nature of the components of the “Turmoil” dimension and their particular relevance to the type of nation characterized as Polyarchic.

Wilkenfeld took the view that the analysis confirmed his two hypotheses:

- a. “Within certain groups of nations, classified according to type of nation, there tends to be a relationship between internal (domestic) conflict behavior and external (foreign) conflict behavior.”
- b. “Within certain groups of nations, classified according to type of nation, there is a tendency for internal (domestic) conflict behavior and external (foreign) conflict behavior to co-occur, or for the occurrence of one to be followed in time by the occurrence of the other.”

It is hard to see how the second hypothesis can ever be unsupported so long as the first is. To say that two variables either co-occur or follow one another in time is to exhaust all possibilities, and therefore is effectively no hypothesis at all. The question then is whether the first hypothesis was supported by these results.

Perhaps, the best way to respond is with uncertainty—no small thing, given the strongly negative results of Rummel and Tanter. Unlike them, Wilkenfeld obtained strong relationships between internal and external conflict by controlling for the third variable, state or regime-type. Beyond that it is difficult to go. The meaning of those relationships is unclear, since they are left purely at the statistical level. The author did not interpret them; indeed, he cannot without connecting his work with the comparative politics literature. Also, the relationships he unearthed are hardly dispositive, since they do not sort “cleanly” on the basis of state or regime-type. Finally, the amount of data manipulation the author felt compelled to do had the effect of moving the data he actually used a considerable distance from the data he actually collected. As with heavily processed food, one is left unsure whether much of the original carrots have been left in the stew.

Still, despite these difficulties, one result seems robust: the inclusion of the third variable, regime-type, makes a substantial difference in the Rummel-Tanter results. To his credit, Wilkenfeld tried to deal with some of the limitations of his first study with some follow-on research.⁷⁶ Unfortunately, he applied himself entirely to ameliorating the technical difficulties rather than the more important interpretive ones.

In his follow-on work, Wilkenfeld addressed two of the technical weaknesses that undermined confidence in his first study. First, in his original research, he had used Rummel’s original factors even though Wilkenfeld’s data extended into a time period Rummel had not included in his factor analysis. Specifically, Wilkenfeld based his six dimensions of domestic and foreign conflict behavior (Turmoil, Revolution, Subversion,

76. Wilkenfeld, J., *op. cit.*

Belligerence, Diplomacy, and War) on the factors extracted by Rummel for 1955–1957 data. However, Wilkenfeld’s research covered the 1955–1960 period. In principle, the inclusion of more years may have changed the factor structure of the data.

Second, Wilkenfeld was criticized for the way he used raw scores in his research. As described above, he combined both raw and transformed raw scores to obtain year-by-year scores for the six dimensions of domestic and external conflict behavior. For example, to arrive at a nation’s year-by-year Subversion scores, Wilkenfeld simply added the raw and/or transformed raw scores for Guerrilla Warfare and Assassinations—the two components of the Subversion dimension. A problem with this method is that it gives equal weight to each of the two components. But Rummel’s dimensions were not comprised of components with equal loadings. The Subversion dimension contained Guerrilla Warfare with a loading of 0.9 and Assassinations with a loading of 0.66. Therefore, the former’s transformed scores should have been weighted more than the latter’s.

Wilkenfeld tried to remedy these problems by performing a new set of factor analyses for the entire 1955–1960 set. He extracted year-by-year factor scores for each nation thereby eliminating the need to use the raw scores. The results amplify my sense that Wilkenfeld’s findings support the broad view that regime-type matters in the relationship between internal and external. But his analysis cannot support any of the more specific conclusions he would like to reach as to whether time lags matter and whether particular regime-types are especially associated with particular relationships between internal and external conflict.

The new factor analysis for the 1955–1960 period did produce some important changes, which Wilkenfeld reported but did not interpret. First, he was compelled to

reduce the number of domestic conflict behavior dimensions from three to two by combining Revolution and Subversion into a new factor called Internal War. This would be unremarkable except that his first study's strongest finding was ostensibly that each regime-type was associated with a unique combination of domestic and foreign conflict.

Wilkenfeld was not able to obtain results with the foreign conflict variables "as clear-cut as that for the domestic variables." His results with the domestic variables were not so clear-cut either, but it is certainly true that Wilkenfeld's new foreign conflict factors are troublesome. Specifically, there was poor correspondence between Wilkenfeld's newly derived Diplomacy factor and those of Rummel and Tanter. This is also true of Wilkenfeld's Belligerence dimension. He cannot explain exactly why these strong incommensurabilities emerged beyond plausibly suggesting that they arose out of differences in data compilation. However, the impact of these differences is to cast doubt on the extent Wilkenfeld, on the one hand, and Rummel/Tanter, on the other, are really using analogous factors in their analyses. It is a perfect example of the pitfalls of naming factors.

Wilkenfeld used the new factor scores in a correlation analysis to assess relationships between domestic and foreign conflict. Not surprisingly, his results differ significantly in detail from what he obtained in his first study. For Personalist regimes, Wilkenfeld's new analysis shows correlations between Internal War and Belligerence. The problem is that the correlation is significant only for the time-lagged cases of -2, -1, +1, and +2 years. The relationship is not significant for the zero years time-lagged case. How is one to interpret such pattern correlations? Wilkenfeld provides none. Note also that Wilkenfeld's new and old results are impossible to compare, since Internal War did not appear in the old analysis.

For Centralist regimes, Wilkenfeld himself said that “an initial impression would be that these results are rather meager.” This is true, although he obtains very significant correlations between Internal War and War for this type of regime. However, no relationships of note appear elsewhere between Internal War and any other foreign conflict variable nor between War and any other domestic conflict variable. Again, Wilkenfeld did not go beyond simply reporting the result.

Only for Polyarchic regimes was Wilkenfeld able to find correlations that bore some resemblance to his earlier research. In both the new and the old, Turmoil was related powerfully to all three dimensions of foreign conflict.

Wilkenfeld assessed his results correctly when he described them as providing “at least partial confirmation for the original hypothesis” that “by controlling for type of nation. . .the domestic and foreign conflict behavior of nations.” (Perhaps “confirmation” is a bit strong.) However, he went on to note, again correctly, that “the generally small size of the correlation coefficients indicate that we have not explained a great deal of variance in foreign conflict on the basis of domestic conflict, and vice versa.”

To this point, the major quantitative studies of the link between internal and external conflict all employed factors analyses of large events databases coded for measures of each type of conflict. Wilkenfeld and Zinnes⁷⁷ were concerned that this choice of methodology might be confounding the negative results produced by Rummel and Tanter and the significant positive correlations found by Wilkenfeld.

Therefore, Wilkenfeld and Zinnes devised the following longitudinal model:

$$F_n, D_n \longrightarrow F_{n+1}$$

77. Wilkenfeld, J., and D. Zinnes, “A Linkage Model of Domestic Conflict Behavior,” in Wilkenfeld (ed.), *Conflict Behavior and Linkage Politics*, New York: David McKay Company, Inc., 1973.

Where:

- F_n denotes the level of foreign conflict behavior by a state at time n .
- D_n denotes the level of domestic conflict behavior by a state at time n .
- F_{n+1} denotes the level of foreign conflict behavior by a state at time $n + 1$.

This model represents the effect of a state's domestic and foreign conflict behavior on the state's subsequent foreign conflict behavior.

Wilkenfeld and Zinnes performed three analyses using this model. The first examined the probability of F_{n+1} as a function of different levels of F_n with D_n held constant. The second examined the probability of F_{n+1} as a function of different levels of D_n with F_n held constant. The third, and most subtle, analysis is of the researchers' hypothesis that states attempt to maintain proportionality between D_n and F_{n+1} . "For example, if domestic conflict is at level one, $D_n = 1$, then the transition probability between $F_n = 1$ and F_{n+1} should be greater than the transition probability between $F_n = 1$ and any other value for F_{n+1} ." This maintenance of proportionality they called matching behavior. The Rummel/Tanter data for 1955–1960 were used. Factor scores were obtained for each of 74 states for each year and for each measure of domestic and foreign conflict behavior. These factor scores were then sorted among three levels of "intensity": 0, 1, and 2 using a cutoff scheme based on the range of scores. The lowest factor score range was assigned factor intensity 1, the middle was assigned factor intensity 2, and the highest, factor intensity 3. They used these to construct a series of 3x3 matrices. Examples are given in Table 7.

Turmoil=0					Turmoil=1					Turmoil=2			
War _{n+1}					War _{n+1}					War _{n+1}			
War _n	0	1	2		War _n	0	1	2		War _n	0	1	2
0	.63	.34	.03		0	.55	.43	.02		0	.64	.36	.00
1	.44	.53	.03		1	.40	.47	.13		1	.33	.53	.13
2	.11	.11	.78		2	.13	.27	.60		2	.29	.57	.14

Table 7: Example of Wilkenfeld and Zinnes's Sort of Factor Scores by Intensity

The cell values are proportions summing to 1.00 across each row. The proportions represent the fraction of times states made a transition from one level of foreign conflict behavior to another in the following controlling for a level of domestic conflict or level of foreign conflict. Thus in the left-hand matrix, where Turmoil was low or nonexistent, 63 percent of the time states that had not engaged in behaviors coded in the war category continued not to one year later. States that had engaged in level 1 war behavior terminated that behavior one year later 44 percent of the time.

Using a chi square test, Wilkenfeld and Zinnes found that the level of foreign conflict behavior has a strong positive effect on subsequent foreign conflict behavior. However, as with Rummel and Tanter, Wilkenfeld and Zinnes found that the level of domestic conflict is unrelated to the probability of transition to higher levels of foreign conflict behavior.

They then performed the same analyses after sorting the states in their research population according to government type: Personalist, Centralist, and Polyarchic. This step showed that the strong positive association between F_n and F_{n+1} , controlling for D_n ,

was true only for Centralist and Polyarchic states, not Personalist. Similarly, D_n and F_{n+1} were positively associated, when controlling for F_n only for Centralist and Polyarchic states. Again, no consistent relationship was evident for the Personalist group.

The authors concluded that their results are generally supportive of Wilkenfeld's earlier work. But many of the problems of Wilkenfeld's research are present here as well, including sorting decisions that raise doubts about the Personalist category. For this reason, the differences between those states versus the Centralist and Polyarchic states found in this research probably are not trustworthy. Further, the analytical methods used by Wilkenfeld and Zinnes are so exceedingly complex that the results are difficult to interpret beyond the broadest, crudest level. At that level, however, their results support the view that regime or government type makes a difference in the association between internal and external conflict. As to what kind of difference specific regime-types make, this research cannot say.

Burrowes and Spector⁷⁸ elected a fundamentally different approach to their research on the I-E connection. They reasoned that the proper starting point was to pose an exceedingly easy test for I-E hypothesis to pass. If the hypothesis failed that test, there would be no point in going further. Therefore, they chose, as a single case, a state in which historians and area specialists believed the connection between internal and external conflict was both strong and consistent. That state was Syria in the years 1961–1967.

Burrowes and Spector made explicit their motivation to pose a very rigorous test of the Rummel/Tanter results. In particular, the authors sought to remedy the following defects of the Rummel/Tanter research:

78. Burrowes, Robert, and Bertram Spector, "The Strength and Direction of Relationships between Domestic and External Conflict and Cooperation: Syria, 1961–1967," in *Conflict*, 1973.

- a. Dependence on the *New York Times Index* and *Deadline Data*.
- b. Paucity of data for many of the indicators and states.
- c. Lack of time lags.
- d. Overly gross time periods. Instead of a highly aggregated approach, the authors focused on one state using carefully collected data subjected to multiple time-series analysis in which time lags were incorporated.

To this end, the authors coded data from a large number of public sources, several focused on the Middle East for the period 1961–1967. They used a large number of more narrow coding categories of external and internal conflict in an effort to detect more subtle interactions than did Rummel and Tanter. The coded data were analyzed in the following steps:

- a. Bivariate correlations and factor analysis of the measures of internal conflict.
- b. Bivariate correlations and factor analysis of the measures of external conflict.
- c. Bivariate correlations and factor analysis of both sets of measures together.
- d. Multiple step-wise regression of the internal measures and the external and vice versa.
- e. Steps a. through d. with the introduction of time lags of various lengths.

The bivariate correlations, factor analysis, and multiple regression analysis between the internal and external measures of conflict revealed virtually no relationship between internal and external conflict in Syria. Introducing time lags made almost no difference. In sum, the results obtained by Burrowes and Spector conform to those obtained by Rummel and Tanter, despite basic differences in research design.

The authors themselves could be described as bemused by these results. Indeed, they say that “it is still quite probable that the failure to find these relationships is due less to their absence than to crudeness of the various designs used to study them.” They include their own study in that criticism: “Despite its several refinements, the design of this study is quite naive.”

That said, how is one to explain these results, especially when so much care was taken to choose a case where strong internal-external connections were felt by area specialists to be the norm? Of course, the scholars could be wrong and these results correct. That would be scientific research at its best in exposing a counterintuitive reality. However, other possibilities must be considered as well. First, as with all the previous studies, Burrowes and Spector had to design coding categories that were gross enough to appear in public sources of information and be seized upon by the coders. More subtle kinds of domestic conflict, such as competition within the ruling elite, usually remain hidden or ambiguous until they break into the open with arrests, firings, and the like. By the time they become public, the regime’s option to create external tension may be irrelevant or unnecessary. Indeed, should the internal conflict take the form of large, public demonstrations, the state’s military resources may be conserved for internal use making external tension less rather than more likely. The authors reported a number of weak, but negative, internal-external relationships that may be attributable to this cause.

Second, unstable regimes may turn to external conflict only when they can perceive a way that external conflict will ameliorate their situation. In other words, they

may not take this course blindly or reflexively, but only when they can devise a “theory” whereby external tension will strengthen their domestic situation. That “theory” may not be very good, but perhaps the Syrian leadership could not devise any at all.

Third, related to the second point, all of the studies, including that of Burrowes and Spector, test for an internal-external connection over an entire set of time intervals. This constitutes an implicit assumption that, if such a connection does exist, it will be fairly invariant across a period of time. But this need not be so, any more than that the connection be true of all states—as Rummel and Tanter assume. In fact, the authors reported some “preliminary correlational analysis” that suggested that an internal-external connection did exist between 1960 and 1964 but not between 1964 and 1967.

At this point, it would be appropriate to consider a related line of research. Thus far, the principal elaboration of the Rummel/Tanter work has been the inclusion of a regime or government type variable. Several researchers have assessed the impact of a different variable, the character of the society, on the relationship between internal and external conflict. These researchers have explored the hypothesis that internal and external conflicts are related in states that are subject to high levels of societal stress of various sorts.

Leo Hazlewood undertook the first of these in 1973.⁷⁹ He used a path analysis methodology to explore the impact of “General Societal Diversity,” “Population Diversity,” “Ethnic Diversity,” “Economic Expansion,” and “Turmoil” on the level of a state’s foreign conflict defined by the Rummel/Tanter dimensions, Diplomacy, Belligerence, and War.

79. Hazlewood, L., “Externalizing Systematic Stress: International Conflict as Adaptive Behavior,” in Wilkenfeld (ed.), *Conflict Behavior and Linkage Politics*, New York: David McKay Company, Inc., 1973.

It is very difficult to interpret Hazelwood's results, which are a collection of relationships in the 0.2 to 0.3 range. Specifically, the strongest relationship reported is that between a state's record of going to war and future propensity to do so again. This is followed by only the slightly less strong relationship between "Technological Capacity" and "Relative Size" and its likelihood to go to war.

Warren Philips used the Rummel/Tanter data set to investigate the impact of other societal characteristics on a state's propensity for external conflict.⁸⁰ Using canonical analysis, he found that states that "have tended to experience unlawful changes of offices in the recent past" are likely "to respond militarily to their environment." This study suffers from many of the difficulties already raised. However, the strong association between regime instability and external conflict must be viewed with more confidence by virtue of the converging findings by Rummel, Tanter, and Wilkenfeld.

Though different methodologically, John Collins⁸¹ is animated by an idea similar to that of Burrowes and Spector: focus the search for I-E connections on states where there is a particular reason *a priori* to believe they exist. Burrowes and Spector selected Syria for this reason. Collins selects what he calls the African International System. He includes all countries on the African continent, which had attained independence by January 1, 1964, with the exception of the Republic of South Africa. His rationale for this exclusion is related to the fact that, unlike virtually all who preceded him, Collins approached his research with a bit of a theory.

80. Philips, Warren, "The Conflict Environment of Nations: A Study of Conflict Inputs to Nations in 1963." *Research Report No. 42*, Dimensionality of Nations Project, 1970.

81. Collins, John N., "Foreign Conflict Behavior and Domestic Disorder in Africa," in Wilkenfeld (ed.), *Conflict Behavior and Linkage Politics*, New York: David McKay Company, Inc., 1973, pp. 251–293.

Collins begins with the Simmel/Coser hypothesis of the in-group/out-group interpretation of the I-E connection. Collins reasons that one should find strongest evidence for this sort of I-E connection in states that most lack internal cohesion and therefore most need the stimulus of an out group. He argues that embryonic states involved in what he calls the “solidarity-building” stage of development are the most fertile ground for the externalization of conflict, and the new African states best fit that stage of development. This is why he excludes the Republic of South Africa, since he presumed that state had advanced well beyond that point.

Collins used an events data approach, deriving his data from the usual news sources, special African-focused new organizations, and assorted yearbooks. Like Rummel and Tanter, he used factor analysis to reduce his large body of data. Collins found six “Domestic Disorder Consolidated variables.”

- a. Anomic outbreaks.
- b. Subversive activities.
- c. Revolutionary activities.
- d. Elite instability.
- e. Number killed in domestic violence.
- f. Domestic suppression.
- g. Number of political arrests.

He obtained eight “Foreign Conflict Behavior Consolidated variables.”

- a. Diplomatic hostility.
- b. Negative behavior.
- c. Military violence.

- d. Number killed in foreign violence.
- e. Antiforeign unofficial activity.
- f. Negative communications alleging internal interference.
- g. Negative communications alleging hostile policies.
- h. Negative communications making general criticisms.

In any case, Collins first performed zero-order and multiple correlation analysis to identify relationships between the two sets of variables. He found that number Killed in Domestic violence, domestic Suppression, and number of Political arrests had high correlations with diplomatic hostility, negative behavior, numbers killed in foreign violence, antiforeign unofficial activity, and general criticisms.

He then used multiple regression to assess how well the full set of Domestic Disorder Variables predicted individual Foreign Conflict Behavior Variables. Negative communication alleging internal interference was most strongly predicted with an $R=0.85$, followed by foreign unofficial behavior ($R=0.76$), and negative behavior ($R=0.74$). Least well predicted were negative communication making general criticism ($R=0.27$), negative communication alleging hostile policies (0.40), and numbers killed in foreign violence (0.50).

Compare these results with those of Rummel who found Belligerency the most strongly predicted variable ($R=0.31$). Collins' analogous variable was negative behavior for which $R=0.74$. Collins was correct when he said that his results differ markedly from those of Rummel and Tanter in the generally strong association between internal conflict (in some forms) and external.

Collins then performed time-lagged correlations in the hope of finding some evidence of causation if domestic conflict consistently preceded external. He obtained results similar to Tanter and Wilkenfeld. Time lagging generally did not improve correlations, and when it did, the results were uninterpretable.

The Collins research represents a substantial step forward in this area, because it was the first to carry out an experiment to assess a prediction, which, in turn, was the product of a loose theory of national development. This is the first instance for which at least a tentative explanation could be provided for the positive results obtained. Wilkenfeld could also have achieved this if he had provided an explanation of *why* regime or state type should make a difference. Collins provided one.

However, beyond generally high correlations suggesting the importance of regime-type, Collins' results are difficult to interpret. It is hard to know what to make of the fact that negative communications making general criticisms receive strong support whereas other types of negative communications do not. What is badly needed here are case studies, which illustrate in context and detail the ways internal and external conflict are related. Then the unadorned correlations could be connected intellectually to actual behavior and decision making.

The Collins study concluded the first wave of intense investigation into the relationship between domestic affairs and decisions to go to war. The second wave is an extension of the *DPprop* literature. The initial *DPprop* research was focused on whether the monadic and dyadic propositions exist and why. The following *DPprop* work looked more narrowly at specific details of the *DPprop* phenomenon. Thus it was natural that attention would turn not just to the war proneness of democracies and, to a lesser extent,

other regime-types, but to the “deeper structure” of when and how democracies do rely on armed conflict. This, in turn, led to fresh consideration of the diversionary war hypothesis and whether or not democratically elected leaders, most often U.S. presidents, indulge in it.

Ostrum and Job argued that presidents have to make war and peace decisions, while hemmed in by an array of complex interacting constraints arising both from the international environment and, at least equally, from the internal domestic environments.⁸² Note here that, by “political” use of force, the authors did not mean the decision to use to go to war to influence a president’s domestic situation. Rather they meant “political” in the sense of using force short of war to influence the international situation. Nevertheless, they hypothesize and find support for the view that presidents do factor domestic political considerations and constraints heavily into their decision making on peace and war. Such considerations include the public’s aversion to war, a version of Reagan’s mystery index, presidential approval ratings in polls, polling data on a president’s overall success, and the nearness of national elections. Indeed, the authors found that domestic political factors outweigh international factors in affecting presidential decision making on war and peace. The authors did not explicitly extend their conclusions to other democracies or regime-types, but they do find strong evidence for the fundamental domestic-international connection.

Obviously these kinds of findings constitute a serious challenge to Realist predictions. It was precisely these implications for Realism that motivated James and Oneal.⁸³ They modified the Ostrum and Job study by using what, in their view, was a

82. Ostrum, Charles W. and Brian L. Job (June 1986), “The President and the Political Use of Force.” *American Political Science Review* Vol. 18, No. 2: pp. 541–566.

83. James, Patrick and John R. Oneal (June 1991), “The Influence of Domestic and International Politics on the President’s Use of Force.” *Journal of Conflict Resolution* Vol. 35, No. 2: pp. 307–322.

better indicator of the state of the international environment: severity of ongoing international crises. They found that the new independent variable did increase the statistically measured influence of the international environment on presidential decision making. But the measures of domestic political influence James and Oneal measured still remained the most influential factor on the president as Ostrum and Job originally reported.

Gaubatz focused on the nearness of presidential elections as the independent variable.⁸⁴ He found, as did Ostrum and Job, that election timing exerted a significant influence on the decision making of democratic leaders on peace and war. Specifically over the last 200 years, democratic states were more likely to become involved in wars shortly after leaders were elected and less likely shortly before leaders stood for reelection or where new candidates of the leaders' political party stood for election. Interestingly this relationship was obtained both when the democracy initiated war and the democracy was the target of attack.

DeRouen focused on another variable from Ostrum and Job, domestic economic performance.⁸⁵ The authors reasoned that existing research⁸⁶ showed that presidential approval polls are strongly influenced by the state of the national economy. Similarly, other research⁸⁷ has suggested that presidents may conduct

84. Gaubatz, Curt Taylor (June 1991), "Election Cycles and War." *Journal of Conflict Resolution* Vol. 35, No. 2: pp. 212–244.

85. DeRouen, Carl R., Jr. (December 1995), "The Indirect Link: Politics, the Economy and the Use of Force." *Journal of Conflict Resolution* Vol. 39, No. 4: pp. 671–695.

86. See, for example, Brace, Paul and Barbara Hinckley (1992) *Follow the Leader*. New York: Basic Books.

87. See, for example, Morgan, Clifton and Kenneth Bickers (1992), "Domestic Discontent and the External Use of Force." *Journal of Conflict Resolution* Vol. 36: pp. 205–252.

military operations in response to failing public opinion polls. Therefore DeRouen reasoned that there should be a link, albeit indirect, between poor performance of the economy and a president's decision to initiate or become involved in an international militarized conflict. This is precisely what the author found: economic performance did lead to sagging approval ratings, which, in turn, was associated with increased recourse to armed force. There were exceptions to this relation such as when presidents believed that military action would not raise their approval ratings, or when the public was opposed to the use of force or if the nation had just emerged from an armed conflict. Wang reported similar results.⁸⁸

Alastair Smith focused on the election cycle at an even greater level of detail: How do reelection prospects affect presidential decision making about war and peace?⁸⁹ He found that the relationship between reelection prospects and war proneness described a U-shaped functional form. War proneness is unaffected when a president's reelection chances were either very good or very bad. When, however, the reelection was close enough that international events could have an important impact, Smith found that presidents are more likely to embark on "violent, adventurous foreign-policy projects"; again, weighing 1996 reached a similar conclusion.

Thus far, the *DPprop* diversionary war literature focused on the behavior of democracies. Gelpi expanded the literature's scope to compare the tendencies of democratic versus authoritarian regimes to externalize domestic conflict for international

88. Wang, Kevin (March 1996).

89. Smith, Alastair (1996), "Diversionary Foreign-Policy in Democratic Systems." *International Studies Quarterly* Vol. 4: pp. 133–153.

crises between 1948–1982.⁹⁰ Gelpi found that democracies became more war prone in response to nonviolent domestic dissent whereas authoritarian regimes became less so, presumably because they can and prefer to suppress the domestic dissent coercively.

Leeds and Davis also expanded the question to include the other democracies rather than the other regime-types.⁹¹ The authors explored whether leaders of democracies, other than the United States, also showed increased war proneness in reaction to rising popular discontent. To that end, they assessed the relationship in 18 advanced industrialized democracies among economic performance, the election cycle, and military action during the period 1952–1988. Unlike the previous researchers, Leeds and Davis found no consistent evidence to support the view that democratic leaders were influenced in their war and peace decisions by economic performance or the election cycle. Their interpretation of these results was interesting. They attributed the absence of the internal-external connection to the strategic interaction hypothesis: Potential adversaries of democracies understand when internal affairs in a democracy are conducive to wars, and, at those times, they minimize the strategic interaction opportunities between themselves and the potentially hostile democracy. To that end, Leeds and Davis also tested to see if foreign leaders, who might be antagonistic, refrain from provocative actions when democratic elections are close or when a democracy's economy is doing poorly. They found strong support for this argument. In other words, while democratic leaders may be eager to use force under certain domestic circumstances, potential foreign targets are even more eager to deny them the

90. Gelpi, Christopher (April 1997), "Democratic Divergence, Governmental Structure and the Externalization of Domestic Conflict." *Journal of Conflict Resolution* Vol. 41, No. 2: pp. 255–282.

91. Leeds, Bret Ashley and David R. Davis (December 1997), "Domestic Political Vulnerability and International Disputes." *Journal of Conflict Resolution* Vol. 41, No. 6: pp. 814–834.

opportunity. I think this is a plausible hypothesis, but it does not explain why, unlike other researchers, Leeds and Davis found no domestic political effect in any democracy other than the United States. One obvious explanation is the U.S. behavior is unique, but that in turn requires further explanation. The authors are the only ones to use a pooled time series design, which can have the effect of masking important variability among the performance of the individual states. Also the authors did not measure popular approval directly by reference to polls, but indirectly using economic performance as a proxy and by assuming that the period shortly before national elections will be every leader's window of greatest vulnerability. One can reasonably suppose that these potentially problematic ways of measuring the crucial independent variable, domestic discontent, could be driving their results.

Fordham reexamined the Leeds and Davis study in order to better understand the source of their divergent results.⁹² Using the COPDAB and WEISS events data sets, instead of the MIDS data set used by Leeds and Davis, and focusing on the United States and her international rivals, Fordham reproduced the results that showed a strong relationship between poor U.S. economic performance and U.S. presidents' war proneness. He also found evidence of the strategic interaction hypothesis of Leeds and Davis. Potential targets of the United States apparently did try to lower their risks, but Fordham concluded that, as a practical matter, the actions the potential adversaries can take to manage their risk of being attacked by the United States are much weaker than the motivations of the U.S. presidents to use force when they believe it can benefit them. Therefore, Fordham argued that potential targets of U.S. attack may try to be

92. Fordham, Benjamin O., "You Can Run, But You Can't Hide." A paper prepared for presentation to the annual meeting of the International Studies Association, March 23–27, 2002, New Orleans, Louisiana.

unprovocative, but the available ways of doing that are likely to be ineffective. These results strengthen the majority position in this area that U.S. presidents, if not other democratic leaders, are strongly influenced by domestic affairs, but still left open is the extent to which this is true of democratic leaders more generally, if at all.

Fordham pushed more deeply into the area of U.S. presidential behavior and focused next on the comparative war proneness of Democratic versus Republican presidents.⁹³ He teased out two components of economic performance, the unemployment and inflation rates, and he tested to see whether Democratic and Republican presidents differed as to what kinds of economic distress would stimulate their war proneness. Fordham found that Republican presidents became more war prone as unemployment increased, and he surmised this was because of traditional Republican reluctance to inflate the economy. On the other hand, Democrats were more likely to use military force when faced with high inflation, because to act directly against the inflation rate carried the risk of driving up unemployment, a Democratic “hot button” issue.

Miller returned to the question broached by Gelpi: Do democratic and autocratic regimes behave differently, with respect to externalizing domestic conflict?⁹⁴ Specifically, he assessed whether potential targets for military intervention attempted to limit their strategic interaction opportunities with both democratic and autocratic regimes at times of leadership vulnerability, and, if so, whether democratic and autocratic regimes are differentially affected by that tactic. With respect to democratic regimes, Miller replicated the results of Leeds and Davis. As they did, he found that democratic leaders

93. Fordham, Benjamin O. (August 1998), “Partisanship, Macroeconomic Policy, and U.S. Uses of Force, 1949–1994.” *Journal of Conflict Resolution* Vol. 42, No. 4: pp. 418–439.

94. Miller, Ross A. (June 1999), “Regime-type, Strategic Interaction and the Diversionary Use of Force.” *Journal of Conflict Resolution* Vol. 43, No. 3: pp. 388–402. 44.

did not become more likely to become involved in wars during periods of poor economic performance, and again like Leeds and Davis, Miller attributed that more to the ability of potential targets to limit their strategic interactions than to the disinclination of the democratic leaders to use military force. He found, however, that autocratic leaders did become involved in more wars during periods of economic distress, especially by choosing to become involved in ongoing conflicts. Therefore, Miller concluded, autocratic leaders must be less sensitive to the attempts by potential targets to limit their exposure.

In the process of this analysis Miller also tested to see if democratic and autocratic leaders responded in the same way to domestic protests and rebellion. As did Gelpi, Miller found that democracies and autocracies seemed to behave differently. Domestic protests exerted a strongly negative effect on the increased democratic tendency to become involved in wars, whereas the war proneness of autocratic regimes was unaffected in either direction by protests. Miller proposed that this result could be interpreted as suggesting that democratic leaders conclude that, once discontent reaches the levels of overt protest, externalizing the conflict is no longer an effective alternative. Autocratic leaders can, on the other hand, simply coercively suppress the protests.

As with those of Leeds and Davis, these results are puzzling. Like Leeds and Davis, Miller used a pooled time series design with its liabilities, but putting those issues aside, only two substantive interpretations of these results seem available: If we assume that the findings about increased U.S. war proneness are correct, then the findings of Leeds and Davis and Miller suggest the United States is unique among democracies. On the other hand, if we assume that the findings about the United States are incorrect and that the United States is not unique among democracies, one is left to explain the large

and impressive literature to the contrary. One strong possibility, at this point, is that U.S. presidents are not unique in their occasional domestically driven interests in foreign military adventures. However, it may well be the case that European publics are less apt than their U.S. counterparts to find military conflicts a reason for increased support of a national leader and at least in cases that fall short of a national emergency or direct attack by an adversary. Finally the Leeds and Davis and Miller results may also be driven by the indubitable fact that no other democracy is remotely as capable as the United States to launch a foreign military operation at least without considerable national mobilization. Put another way, the military as an instrument is far more effective for U.S. leaders to wield than for any other democratic leader.

Regime Strength and Weakness as an Independent Variable

The concept of “regime strength and weaknesses” arises from attempts to measure the strength of nations. “Strength here refers to all the capabilities states possess, military, social, economic, and the like, that potentially can be used to pursue international or domestic policy objectives. The concept’s most straight forward application is in the context of the Realism paradigm where international power distribution is a key issue. Such measures as military forces, population, energy production, have been used for decades as indicia for divining the international pecking order and its shifts.”⁹⁵

However, the weaknesses of this approach were obvious and manifold. First, it tends to emphasize quantitative measures over qualitative. Yet the history of international relations, and especially, war, contains a myriad of examples of quality looming larger than quantity. Second, in a similar vein, such measures tend to emphasize the tangible

95. Mansfield, Edward D. and Jack Snyder (June 1997), “A Reply to Thompson and Tucker.” *Journal of Conflict Resolution* Vol. 41, No. 3: 457–461.

and material over the conceptual and less tangible. Examples are competence, flexibility, cohesion, efficiency, know-how, and the like. Again, history is replete with examples illustrating the criticality of these factors in determining the outcome of international and domestic events.

Finally, of most interest here, the simple counting of national resources (tanks, megawatts, etc.) says nothing about a regime's capability actually to mobilize and use those resources to achieve its goals, not least among them, staying in power. Consider the case of Tsarist Russia, especially in the late 19th and early 20th centuries. There was no question that, on the basis of resources *per se*, Russia was a great power. Yet as the Russo-Japanese war and WW1 illustrated, the Tsarist regime's ability to extract and use those resources effectively was very limited. In a metaphorical sense, Russia possessed vast potential energy, but very little kinetic. The Tsarist regime is an example of what is meant by a weak regime, a point expanded upon later in this section.

Attempts to capture this issue of regime strength and weaknesses were pursued by the classical expository methods of historians and intelligence analysts until the late 1960s and early 1970s. At that time Huntington⁹⁶, Coleman⁹⁷, Verba⁹⁸, and Holt and Turner⁹⁹ published increasingly careful studies of what then was called the "political development of nations" i.e., the capacity or the effectiveness of regimes to do their job.

96. Huntington, S. (1967) *Political Order in Changing Societies*. New Haven: Yale University Press.

97. Coleman, J. (1971), "The Development Syndrome: Differentiation-Quality-Capacity," in L. Binder (ed.) *Crises and Sequences in Political Development*. Princeton, NJ: Princeton University Press.

98. Verba, S. (1971), "Sequences and Development," in L. Binder (ed.) *Crises and Sequences in Political Development*. Princeton, NJ: Princeton University Press

99. Holt, R. and J. Turner (1975), "Crises and Sequences in Collective Theory of Development." *American Political Science Review*, Vol. 69, No. 3 (September): 979-994.

One way around some of the problems of simply counting national aggregates is to develop indicators of the degree to which regimes are able to *extract* those resources from the nation and society. A straightforward approach is to compare states on the basis of percentage of GDP in the state sector or consumed by government spending, but this too has serious problems. First, though it is assumed that the extracted resources are expended in pursuit of regime objectives, the measure by itself says nothing about whether the resources are expended effectively, efficiently, or competently. Second, regime strength could be related to percentage GDP wielded by the government but not linearly. Presumably, the most effective regimes are those that can extract what they need, no more and no less. If they take too much, the states capability to execute regime policies is damaged. If they take too little, government resources are insufficient. Therefore, it is not true that the higher the percentage, the stronger the regime. In principle, the relationship should have functional form approximating an inverted “U”. But simply calculating the measure tells one nothing about the specifics of that functional relationship, so, in a deep sense, percentage of GDP used by the government is probably meaningless without some way of relating the statistic to state requirements.

W. Arthur Lewis attempted to come to grips with this problem by roughly calculating what he deemed to be the minimal expenditures necessary by developing states to achieve a basic level of national welfare in education, health, public works, and support of enterprise¹⁰⁰. The figure he thus derived was approximately 20 percent. That is, the regime of a developing state has to spend at least 20 percent of its GDP to provide basic services.

100. Lewis, W. Arthur (1966), *Development Planning: The Essentials of Economic Policy*. New York: Harper and Row.

Omitted from Lewis's calculations are two types of government expenditures that developing states usually find necessary: creating and sustaining a military and servicing foreign debt. Migdal¹⁰¹ estimated that these requirements add another 10 percent to Lewis's figure to produce a total government spending norm of 30 percent of GNP.

So far as I can determine, Organski and Kugler¹⁰² were the first to explore quantitative methods for measuring regime strength as part of net assessments of national power. They defined regime strength as "the capacity of the political system to carry out the tasks imposed upon it by its own political elite, by other important national actors, or by the pressures of the international environment." The authors based their work on earlier, economic research measuring the extent regimes are able to extract tax revenue from its state.¹⁰³

The authors proposed what they called the "index of tax effort."

$$\text{TAX EFFORT} = \text{REAL TAX RATIO} / \text{TAX CAPABILITY}$$

Where:

$$\text{REAL TAX RATIO} = \text{TAX REVENUES} / \text{GNP}$$

$$\text{TAX CAPABILITY} = \text{THE STATE'S GENERATION OF TAXABLE WEALTH}$$

Put another way, the denominator expressed the theoretical limit of extraction and the numerator the degree to which that limit is approached by a regime. The closer the expression approaches 1, the greater the effort the regime is able to muster to extract the resources of the state.

101. Migdal, J. (1988), *Strong Societies and Weak States*. Princeton, NJ: Princeton University Press.

102. Organski, A.F.K. and J. Kugler, (July 1978), "Davids and Goliaths: Predicting the Outcomes of International Wars." *Comparative Political Studies*, Vol. II, No. 2: 141–180.

103. Lotz, J. and E. Morss (1967), "Measuring Tax Effort in Developing Countries." *International Monetary Fund Staff Papers*, Vol. 16: 478–499; Chellian, R. (1971) "Trends in Taxation in Developing Countries." *International Monetary Fund Staff Papers*, Vol. 18: 570–610.

But how is one to know whether any particular tax effort reveals high government effectiveness or low? The authors attack this problem by “norming” each regime’s performance to the *mean* performance of regimes in economically similar states.

This basic approach has been adopted with variations in all later quantitative attempts to measure regime strength, with the exception of those most recently developed by the World Bank. The problem of the approach is its limits. Though Organski and Kugler, and later, Kugler and Domke¹⁰⁴ recognized the difference between the sheer ability of a regime to extract resources and its ability to use those resources competently, the tax-effort measure does a much better job of capturing the latter than the former. Papa Doc Duvalier may have possessed that ability to extract (steal) the resources of the Haitian population, yet that fact alone seems an entirely inadequate basis to argue his regime was strong or effective, in any but the most narrow sense. So inclusion of some measures of competency seems necessary for measuring regime strength of weakness.

Researchers in the international political economy have taken a different approach to exploring regime strength so as to avoid the methodological weaknesses of the quantitative methods summarized here. For example, Katzenstein differentiated weak and strong regimes by assessing the “number and range of policy instruments” available to them. Regimes with large numbers and wide ranges of such instruments are stronger, because they possess what Katzenstein calls a more effective “policy network.”¹⁰⁵

The problems with this approach are several. First, the users of this approach cannot explain well why quantity of policy instruments is associated with their quality.

104. Kugler, J. and W. Domke (1986), “Comparing the Strength of Nations.” *Comparative Political Studies*, Vol. 19, No. 1: 39–69.

105. Katzenstein, P.J. (Autumn 1977), “Confusion: Domestic Structures and Strategies of Foreign Economic Policy.” *International Organization*, Vol 31: 892.

Put another way, one can imagine a very strong regime, which relied on a few, very powerful policy instruments, and a weak regime possessed of many, only moderately effective policy instruments.

Second, the approach does not reflect well the different ways regime strength can be manifested in different societies. For example, Katzenstein argues that, based on his measures of policy instruments, Japanese regimes are stronger than U.S. regimes. This is because Japan's democracy is more centralized than the American, which reserves some powers for the states. Yet it is widely accepted that Japanese regimes are often hamstrung by the ossified permanent bureaucracies and political parties. American political regimes, on the other hand, probably are able to produce more continuous change in U.S. society than any other regime in the world. So, in what sense, can Japanese regimes be construed to be stronger? It is notable that Katzenstein reached his conclusions in the late 190s when it seemed as though the Japanese policies of powerfully directing the economy and setting investment strategy seemed to be producing such good results. One wonders whether he would hold to the same conclusions today.

Third, the Katzenstein method is best suited to comparing developed states. Developing states generally have so few policy instruments that counting and comparing states on that basis does not produce much differentiation.

Migdal¹⁰⁶ employs yet another approach to the problem of measuring regime strength or state strength in his parlance. Migdal views regime strength mostly through the experience of the developing world. In most of those states, he noted, strong and authoritative social structures exist beneath the level of the regime or government.

106. Migdal, J. (1988), *Strong Societies and Weak States*. Princeton, NJ: Princeton University Press.

These social structures may rest on the tribe, clan, family, ethnic group, and the like. In order for a regime to be strong, Migdal argues that it must be able to penetrate into those social structures with its policy instruments. In other words, to be strong, a regime must be recognized by members of those social structures which compete with the regime for power, as in some sense, the highest source of authority in the state. Therefore, Migdal defined regime strength in terms of the degree to which a regime can claim this position.

Migdal's book consists of theoretical discussion of these concepts with applications to several regimes in the Middle East and Latin America. I find it to be an excellent book in all respects. The problem is that its methodology does not adapt well to research, such as this, which seeks to compare regime strength among a large number of states. For that, the quantitative approach, with all its limitations, seems the method of choice.

Fortunately, quantitative work in this area has recommenced after a long hiatus. Beginning in the late 1990s, the World Bank has undertaken a research program with the goal of measuring the quality of governance in all the world's states.¹⁰⁷ I use the World Bank's data base as I discuss in the following chapter on methodology and analysis.

107. See, in general, www.worldbank.org (wbi) [governance. Note particularly, Kauffman, D. A. Kraay, and M. Mastruzzi; June 2003] "Governance Matters III – Governance, Indicators for 1996–2002." *World Bank Staff Papers*; Kauffmann, D. and A. Kraay (Fall 2002), "Growth Without Governance." *Economia*, Vol. 3, Vol;1-8; Kauffman, D. (2003), "Rethinking Governance: Empirical Lessons Challenge Orthodoxy." *World Bank Staff Papers*; Kauffmann, D., A. Kraay, and P. Zoldo-Lobaton (1999), "Governance Matters." *World Bank Staff Papers*; Kauffman, D., A. Kraay and P. Zoldo-Lobaton (1999), "Aggregating Governance Indicators." *World Bank Staff Papers*; Beck, T., G. Clarke, A. Groff, P. Keefer, and P. Walsh (2000), "New Tools and New Tests in Comparative Political Economy: The Database of Political Institutions." *World Bank Staff Papers*.

Weak Regimes and the Developing World

There is an interesting, though small, body of literature, which supports the notion that weak autocratic regimes are especially prone to take external action because of internal political instability. This is the literature on national security policy in the Third World. Buzan,¹⁰⁸ Ayoob,¹⁰⁹ Bobrow and Chan,¹¹⁰ and Azar and Chung-in¹¹¹ make the following points.

In an important sense all states are alike by virtue of their claim to the right of self-rule. Asserting that right in an anarchical society imposes a host of like tasks and structures for carrying them out. Similarly, all states are equal legally. In what meaningful ways can states be distinguished? Realists like Waltz argue that “power” is the most important variable, perhaps the only important one.

However, exclusive focus on power, usually operationally defined as ability to coerce, ignores the critical sense in which many Third World states differ from those in the First World. Many Third World states, creations of colonial map-drawing are at a phase of state building which Europe passed through in the 18th and 19th centuries. One can characterize such states as half-formed. They are characterized by lack of social cohesion among different ethnic or religious factions, little or no coherent sense of

108. Buzan, B. (1988), “People, States, and Fear; The National Security Problem in the Third World,” in Azar and Chung-in (eds.) *National Security in the Third World*, Aldershot: Edward Elgar Pub. Ltd.: pp. 14–43.

109. Ayoob, M. (1986), “Regional Security and the Third World,” in Ayoob (ed.) *Regional Security in the Third World*, Boulder: Westview Press, 1986: pp. 3–23; _____, “The Security Problematic of the Third World,” *World Politics*, Vol. 43, 1991: pp. 257–283; _____ “Third World in the System of States: Acute Schizophrenia or Growing Pains?” *International Studies Quarterly*, Vol. 33, 1989: pp. 67–79.

110. Bobrow, D. and S. Chan, (1988) “Simple Labels and Complex Realities: National Security for the Third World.” *National Security in the Third World*, Aldershot: Edward Elgar Pub. Ltd.: pp. 44–76.

111. Azar, E. and M. Chung-in, (1988), “Rethinking Third World National Security,” in Azar and Chung-in (eds.) *National Security in the Third World*, Aldershot: Edward Elgar Pub., Ltd.: pp. 1–13.

common membership in a state, no widely held theory of the legitimacy of government, and (often) insufficient coercive power to force the compliance of the population and competing groups. Buzan characterizes this condition as “weak sociopolitical cohesion.” Thus, by introducing the dimension of cohesion, one can describe a country as militarily powerful but politically weak. With few exceptions, he says, the Third World is comprised of politically weak states, though some may possess strong military forces. Among the weakest are such states as Chad, Uganda, and Lebanon (perhaps prior to the Syrian intervention). Somewhat less weak are Angola, Ethiopia, and Mozambique. States like Syria, Iraq, Pakistan, and Egypt he describes as “ordinary” weak states where local loyalties and state repression are in a fragile equilibrium. India, Argentina, Brazil, and Mexico he would place in a “middle” position between weakest and strongest. These states can depend less on outright repression because popular loyalties are more balanced between national identification and more local ties, then in states like Egypt where popular loyalties are more local.

The regimes of weak states are able to govern because they can maintain a dynamic balance among the various sources of power within their states. Ayoob¹¹² describes the mechanism quite well. This “balancing act” is accomplished through reward and coercion. Coercion is usually straightforwardly physical. Jill Crystal¹¹³ has argued convincingly that rewards can take material or ideological forms. For example, in the Arab world, she points out that “appeals to tradition or Arab nationalism. . .are better seen as deliberate and careful attempts by rulers to deal with a crisis of

112. M. Ayoob, “The Security Problematic of the Third World,” *World Politics*, 43, 1991, pp. 257–283.

113. Crystal, J. “Authoritarianism and Its Adversaries in the Arab World.” *World Politics*, Vol. 44: pp. 175–184.

legitimacy either by invoking a carefully crafted, selective, and often inaccurate past or by offering promises, perhaps willingly false, of future. . .gain. . .to fragment [the political] opposition. Although the regime's justifications appear in local dress, unveiled they bear a strong family resemblance to appeals to tradition and progress made by rulers throughout the Third World.

The reason for this difference between First World and Third World national security is only a manifestation of the more basic differences in the ways states in each group came into being, in the ways elites have been recruited, and in the ways political regimes are established and maintained.

Modern developed states represent several hundred years of what might be called "political and social natural selection." These states represent the outcomes of a slow process of resolving the tensions between an array of centrifugal and centripetal forces, particularly national versus local authority.

Joseph Strayer,¹¹⁴ the medieval historian, summarizes the process:

"While the sovereign state of 1300 was stronger than any competing political form, it was still not very strong. . .It took four to five centuries for European states to remedy their administrative deficiencies, and to bring lukewarm loyalty to the white heat of nationalism."

Charles Tilly,¹¹⁵ an historian of Europe in the Middle Ages and the Age of Revolution, points out that the appearance of solidity of developed states in the late 20th century is quite a recent development.

"The seventeenth and eighteenth centuries. . .has us dealing with periods in which for most of Europe, both the primacy and the ultimate form of

114. Strayer, J. (1970) *On the Medieval Origins of the Modern State*. Princeton: Princeton University Press: pp. 23, 57.

115. Tilly, C. "Reflections on the History of European State-Making," in Tilly (ed.), *The Formation of National States in Western Europe*. Princeton, Princeton University Press, 1975: p. 7.

the state were much in doubt. Perhaps that is the most important historical insight this book has to offer: as seen from 1600 or so, the development of the state was very contingent; many aspiring states crumpled and fell along the way.”

The process of state-building is by no means finished. Witness the various secessionist movements in Wales, Northern Ireland, the Basque region, Normandy, and northern Italy. Indeed, the perennial debate in the United States over the proper balance between national versus local governments is an example, though a benign example. But, even though some of these tensions can still produce violence, the process of state-building in the developed world can be likened to a formerly volcanic area, which remains geologically active. The major questions have been settled in these states: the concept of “state” as a social and political organizing principle is accepted as legitimate, as are the political regimes which govern them, and the populations identify themselves saliently as members of a state.

By contrast, in many if not most Third World states, none of these questions have been settled. One reason for this is time. Most of these states were created after WWII by former colonial power. The administrative boundaries of the colonies often did not coincide with nations, and the colonial period was not one of successful state-building—though a technically skillful elite might have been created in many colonies. However, individual administrative competence has little to do with establishing the state as a legitimate form of social and political organization. During the period of decolonization, states were created largely along the same colonial boundaries. As a result, Third World regimes as a class have been hard pressed to create in their populations a sense of strong, habitual identification with the state. It must also be said that many of the regimes themselves may not feel this identification. This combination of embryonic state formation and illegitimate

regimes is self-reinforcing in that such regimes lack the ability to produce the higher levels of sociopolitical cohesion needed. Their alternative is resort to coercion and cruder rewards, which may buy compliance but not cohesion.

In addition to time, poverty is often cited as contributing to the Third World's problem of state-building. Almost by definition, the members of the Third World are quite poor. This has the effect of removing the buffers that allow richer states to deal with painful issues without risking disintegration. For many Third World states, questions of politics, governance, and resource allocation are literally life-and-death questions. Thus proponents of different views bring a high level of ferocity to the "marketplace of ideas." Horizontal and vertical cleavages are exacerbated in such an environment, which, in turn, amplifies the precariousness of the internal balances of power on which many regimes are based.

As a consequence, many of these states are ruled by narrowly based, authoritarian regimes, military and which have acceded to power by force or by agreement of a narrow elite backed by the threat of force and hang on to power in the same way. Virtually the entire Arab Middle East is ruled by such regimes. The current popularity of democracy may be altering this pattern, but that remains to be seen.

CHAPTER 3

CASE STUDIES

The statistical relationships between regime strength and conflict are powerful and suggestive. However, it is also important to descend below the aggregate level, and identify the mechanisms and processes that characterize the way regimes react to their weakness by selecting violent external conflict. My strong preference would be to pursue this question quantitatively. In principle, if a events database existed that tracked domestic political developments attached to the strength or weakness of regimes, then it would be possible to investigate statistically the relationship between changes in regime strength and the initiation of external violent conflicts. Unfortunately, no such database currently, at least for the span years covered here. Rummel's DON database contained domestic variables, but it is far from current and of questionable usability. The IPI database tracked such domestic developments at a great level of detail, but only for a small number of states and years. *The World Handbook of Political and Social Indicators III*, tracks these data, but only extends to 1982 with notable omissions even within that period.¹ Not surprisingly, tracking subtle regime strength indicators is difficult in closed societies and in those states with few news sources.

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1. Taylor, C. L. and D. A. Jodice (1986), *World Handbook of Political and Social Indicators III: 1948–82*. Compiled by C. L. Taylor, Virginia Polytechnic Institute and State University. 2nd ICPSR ed. Ann Arbor, MI.

As a result, I have elected to use a mix of informed judgment and case studies. There purpose to inject life into the aggregate statistics. There is an important methodological point underlying the combination of statistical analysis and cases. Statistics expose large-scale patterns and relationships. They can suggest causality. But to show causality in human behavior, one need to show more than behavior A follows behavior B. One needs to show that the actor intended that A cause B and that he perceived and understood A and B to be causally related. The cases are intended to provide that element of intentionality missing in statistical reporting.

The cases are chosen for two reasons. First, they are particularly good examples of the relationship between domestic conditions leading to regime weakness leading to war and peace decisions. Second, they are cases for a a substantial, diverse, and high quality secondary literature exists. I can make no claim that they are typical or even representative. What I can claim is that they illustrate powerfully the arguments made in this dissertation, to the effect that the statistical analysis can be supported existence proofs from recent history.

George's² focused comparison method is used to structure the cases. Each case follows a common protocol to address the following questions:

- a. How was the regime organized, and which institutions and individuals were important sources of political power?
- b. On what basis did the regime's hold on political power rest? What institutions, factions, and individuals supported and opposed the regime and why?

2. See George, A. (1974), *Deterrence in American Foreign Policy: Theory and Practice*. New York: Columbia University Press: pp. 94–97.

- c. In the period prior to the outbreak of external conflict, did the regime believe its hold on political power was weakening so significantly as to require remedial action? What was the source of the threat to its power? Did unsupportive institutions, factions, and individuals become stronger? Why? How did they use their increased power? Did supportive institutions, factions, and individuals become less so? What difference did that make?
- d. Did the regime develop an explanation for its crisis, which connected another state's actions or simple existence to the domestic crisis? Did that state support internal adversaries? Did it threaten internal friends?
- e. Did the regime develop a plan of action or strategy in which external conflict with the state was expected to ameliorate the domestic crisis by weakening internal adversaries or strengthening internal friends?
- f. Did the regime undertake an external conflict with that state in order to ameliorate the domestic crisis?

What follows are five of the many cases that illustrate the powerful ways in which concerns for domestic stability lead already fragile states to aggressive international risk-taking. One concerns a vulnerable, though elaborate, totalitarian regime; another a weak, partially formed totalitarian regime; two involve authoritarian regimes; and one a weak democratic regime. These cases are as follows:

- The Chinese decision to intervene in Korea in 1950
- Argentina's invasion of the Falkland Islands in 1982
- Egypt's decision to remilitarize the Sinai in 1967

- India's attempt to coerce the Chinese over a disputed border area in 1962
- Iraq's invasion of Kuwait in 1990.

Nasser's Decision to Remilitarize the Sinai, 1967

"Political action is not undertaken by angels but by human beings. Political leadership is not a ruthless and sharp sword but rather a process of balance . . . between various possibilities and, in many cases, between obvious risk."

— *Gamal Abd al - Nasser*

Between 1922 and 1952, Egypt existed in a state of chronic social and political crisis due, in large part, to a three-way competition for domestic power between the Egyptian monarchy, the Wafd Party, and the British. The monarch sought to maintain autocratic rule. The British objective was preserving her imperial interests. The Wafd pursued two aims: dispossessing the monarchy and evicting the British.³

The Wafd Party was dominated by what might be called the Egyptian bourgeoisie elite: upwardly mobile entrepreneurs, moderate religious leader, and professions. Though the Egyptian middle class was small, the Wafd succeeded in mobilizing the support of the major elements of the Egyptian population: peasants, farmers, small businessmen, bureaucrats, and the massive urban poor. The weight of this movement was sufficient in February 1922 to extract Egyptian independence from Britain. A constitution creating a Parliamentary democracy with a titular monarch on the British model was promulgated in 1923.⁴

Unfortunately, the government envisioned in this document never proved feasible due to the unwillingness of the monarch, King Farouk (backed by the British) to refrain from sabotaging the fledgling democracy. This took the form of

3. Dekmejian, R. (1971), *Egypt Under Nasir: A Study in Political Dynamics*. Albany: SUNY Press: pp. 17–18.

4. *Ibid.*, p.19.

frequent and arbitrary dissolution of Parliaments and Cabinets, appointments of incompetent Cabinet Ministers, and continual efforts to wrest political authority away from the Prime Minister.⁵

The result was national, political paralysis, and the creation of an environment encouraging to radical and violent ideologies and groups. The Wafd proved incapable of moderating this process. The most important group to fill this vacuum was the Muslim Brotherhood, which transformed itself in the late 1920s and early 1930s from a small religious reformist group into a mass revivalist movement. The basis of the group's ideology was a form of fundamentalist Islam extended into all aspects of political, economic, and social governance.

The Muslim Brotherhood and similar though smaller groups became increasingly active and violent, especially once the war was over. Interestingly, until this time, the Egyptian military as an institution had refrained from political action - though the junior officer ranks had been deeply infiltrated by the Muslim Brotherhood, communists, and assorted others. The military had never entered the political struggle, nor had it been used to repress one group or another.⁶

However, political ferment was quite widespread among middle-level officers. This was no coincidence for these officers were among the first to be admitted to the Egyptian military academy in the 1930s. At that time, the Academy was opened to Egyptians of non-aristocratic and non-Ottoman origins. Small nuclei of these officers

5. *Ibid.*, pp. 20–21.

6. Nutting, A. (1972), *Nasser*. New York: E. P. Dutton & Co., Inc.: pp. 11, 19–20.

formed in their opposition to the continuing British presence in Egypt, especially during WWII. The Arab debacle in the Israeli War of Independence of 1948 was decisive in driving many officers into these proto-organizations.

In the wake of the 1948 war, the Association of Free Officers was formed with the explicit, though secret, agenda to seize political power. Significantly, unlike some of its competitors within the military such as communists and the Brotherhood, the Free Officers had no central ideological theme other than dissatisfaction with the status quo. The growth of the Free Officers was due to the administrative and tactical competence of the inner core of eleven officers, led by Gamal Abdul Nasser, who comprised the Executive Committee.⁷

The Free Officers seized power in a coup in mid-1952, and promptly changed the name of the Executive Committee to the Revolutionary Command Council (or RCC). Beyond this, Nasser and his colleagues had neither political, social, or economic program nor any particular competence in developing or implementing one. The period from 1952 through 1955 was spent attempting to mix Western-style democracy and military government with little success. Attempts at agrarian reform produced turmoil, and the Muslim Brotherhood and other military factions conspired to fill the ideological vacuum of the Free Officers. In response, the Free Officers government became increasingly repressive as illustrated by Figure 5.⁸

7. Heikal, M. (1973), *The Cairo Documents*, New York.

8. Dekmejian, *op.cit*, p.35.

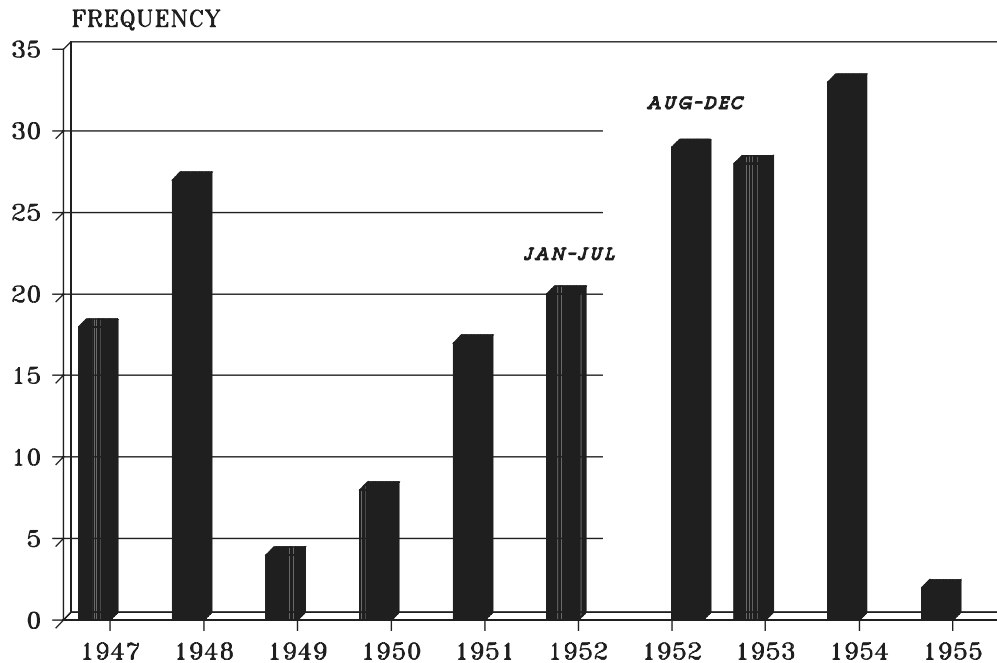


Figure 1: Acts of Violence

The bar graph suggests that the campaign of repression succeeded by the end of 1954. One can say that the coup launched in 1952 was only completed two years later. The RCC of the Free Officers was in firm control of Egypt, and Nasser was in firm control of the RCC. His problem, now deferred for almost three years, was to find a vehicle for transforming himself and his organization from destroyers into builders. In other words, to what source would Nasser turn to provide legitimacy to his regime beyond simply being the one that dislodged the former one?

The answer Nasser literally stumbled upon was foreign policy and the personal appeal he could develop by actions in that sphere. Ironically, Nasser found the legitimacy he needed within Egypt by joining in himself two threads of international or transnational

policy: resistance to the West and Pan-Arabism. This orientation saved him, temporarily, from confronting the difficult domestic task of developing a broad-based program. Indeed, he never did. Nasser's first major step into the foreign affairs stage was in early 1955 when he rejected membership in the Baghdad pact, after Turkey and Iraq accepted. Nasser represented the proposed defense arrangement as a mechanism for the promotion of two ex-colonial powers, Turkey and Great Britain. It is not clear what Nasser anticipated, but the popular response in Egypt and elsewhere was enormous and approving. He followed this up by playing a leading role at the Bandung Conference of the so-called non-aligned nations in April 1955. He recognized the People's Republic of China and concluded an arms purchase agreement with the U.S.S.R. in the same year. Each of these actions seemed to resonate deeply with the near complete alienation of many Arabs from the West, an issue not fully understood by the West and by other Arab leaders.⁹

These steps led the U.S. and Great Britain to withdraw their offer of support for the Aswan High Dam; Nasser retaliated by nationalizing the Universal Maritime Suez Canal Company in July 1956, and the Suez crisis ensued. The attack by Britain, France and Israel and its disastrous results finished the process of making Nasser a charismatic, personalist leader. He ruled Egypt on the basis of his international role in the Middle East rather than domestic policy. Indeed, Figure 6 evidences the small proportion of Nasser's utterances relating to Egyptian domestic welfare and national interests.¹⁰

9. This point is made by Dekmejian, *op cit.*, pp. 42–43, Nutting, *op. cit.*; pp. 99–100, 100–101, and Heikal, *op. cit.*, pp. 27–28.

10. Dekmejian, *op. cit.*, p. 94.

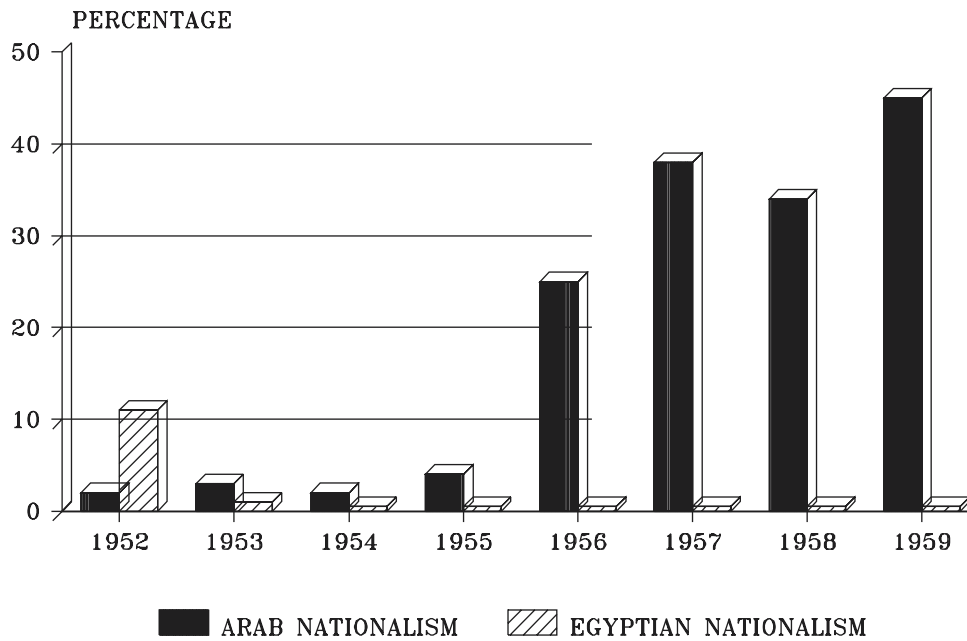


Figure 2: Ideological Change (Yearly Index)

However, having based his domestic legitimacy on being a paladin for the entire Arab world, Nasser was compelled to produce success even while putting the Egyptian national interest at risk. He could never avoid becoming trapped into undertaking any high risk adventure or supporting any movement associated with anti-Western or Pan-Arab causes, even when his own long-term interests, as well as those of his state, militated against such commitments. Personal charisma being as volatile as it is, Nasser felt he had a limited ability to withstand the criticism that would arise by avoiding certain fights or arriving at constructive compromises with

the West.¹¹ Any reverses that could cast doubt on that claim were exceedingly serious. Yet such reverses were inevitable if only because Nasser's claim to lead the Arab world was so ambitious.

The first of these reverses occurred in 1961 when Syria elected to leave the short-lived United Arab Republic. In 1958, Nasser had been compelled to form and lead the UAR when a group of young Syrian officers requested immediate union with Egypt. The leader of Arab nationalism could scarcely reject this first step toward doing away with "artificial" national boundaries, and the agreement was consummated in Cairo, February 1, 1958 to great acclaim.¹² In fact, Nasser was never pleased with this situation, which he viewed to be a Syrian attempt to get Egyptian cover to pursue a reckless foreign policy.¹³ Further, the ruling elites of Iraq, Turkey, Lebanon, Jordan and Saudi Arabia viewed the UAR as Egyptian imperialism.¹⁴ Nor were the Soviets pleased to share Syrian loyalties with Egypt.¹⁵

But, worst of all, the signing of the agreement created internal instability in Syria, particularly among the Baathists, over Nasser's ambitions in Syria. Ultimately, the Syrian military and the Baath seized power in September 1961, and dissolved the union with Egypt—though Nasser insisted that Egypt continue to be called the UAR.¹⁶

11. Sadat, A. *In Search of Identity*, p. 92.

12. Nutting, *op. cit.*, pp. 217–218.

13. *Ibid.*, pp. 219–220.

14. Seale, *op. cit.*, p. 54.

15. Nutting, *op. cit.*, p. 221.

16. Dekmejian, *op. cit.*, pp. 60–61.

This setback could not have come at a worse time, because Nasser faced a second foreign policy disaster at roughly the same time in Yemen. In 1962, the ruler of Yemen, Iman Ahmed, died and his son, Saif al-Badr, was named to succeed him. However, dissident Yemeni military officers seized the royal palace, declared a republic, and appealed to Nasser for help. Badr, having escaped to Saudi Arabia, announced a counter-revolution with the support of loyal Yemeni tribes. Once again, Nasser was entrapped by the self-created relationship between the survival of his regime and his support of any movement espousing Arab nationalism. Within a year, a quarter of the Egyptian army was fighting in Yemen with little success. By 1964, the Egyptians had doubled that deployment,¹⁷ and ultimately, in their desperation for success, overtly used poison gas on fellow Arabs.

Thus by mid-1965, Nasser and his regime had received two severe blows to his claim to legitimacy: infallible leadership of the Pan-Arabism movement. Until this time, Nasser had faced little internal opposition. However, 1965 began a period of increasing internal turmoil in Egypt leading the June war of 1967 with Israel.

The proximate causes of the increased internal criticism of Nasser were three:

- a. The unsuccessful war in Yemen;
- b. Serious food shortages.
- c. Cleavages in Egyptian elites, especially within the original Free Officers.

First, during the summer of 1965, Nasser acknowledged serious conspiracies in Egypt aimed at assassinating members of the top leadership.¹⁸ Indeed, crowds urged on

17. Nutting, *op. cit.*, p. 322.

18. *Arab Observer*, 13 September 1965, pp. 8–12.

by members of the Muslim Brotherhood, staged a violent demonstration against the Cairo police. Ironically, the primary conspirators and instigators of the demonstration probably were motivated most by opposition to Nasser's socialism rather than his international failures. However, what concerned Nasser was the degree of popular willingness to participate, which he felt would not have happened had he some clear-cut military or diplomatic victory around which to mobilize support.

Second, Egypt had begun to suffer from major food shortages in late-1964, and these continued in 1965. The basic cause was traceable to Egypt decrepit economic infrastructure, growing population, and the general incompetence of the military government in the management of economic development.¹⁹

Third, between 1956 and the early 1960s, Nasser had dismissed several of his old comrades in the RCC. By 1964, only seven of the original twelve who launched the 1952 coup remained in government. Nasser had ceased to be merely first among equals, and this and his mistakes produced a quasi-opposition group led by Abdul Hakim Amir, Commander-in-Chief of the Armed Force and RCC member.²⁰

Amir had skillfully used his position to surround himself with officers whose careers were linked to his. Due to this independent power base, Nasser had avoided direct confrontations with Amir, preferring rather to limit his authority to meddle in affairs outside the Army. However, Amir's explicit criticism of Nasser's dismissals of old comrades and failures in Yemen spurred Nasser to contend more directly with Amir. This he did by compelling Amir to replace the commanders of the air force, navy, and

19. *New York Times*, 23 October, 1965, p. 1.

20. Dekmejian, *op. cit.*, p. 172.

artillery, his allies, with Nasser loyalists. Nasser, himself, became Commander-in-Chief of the Armed Forces, and Amir was demoted to Deputy. Defense policy was entrusted to a Defense Council, presided over by Nasser.²¹

Yet, in spite of these constraints, Amir's allies were sufficiently influential that Nasser was compelled to annul his change in command of the air force and gradually the Defense Council became no more than a *pro forma* organization. By early 1964, two more old comrades resigned from government leaving only five of the twelve original Free Officers. Finally, in the fall of 1966, General Shams al-Din Badran, an ally of Amir, was appointed Minister of War. This constellation of Badran and Amir was to play an important role in shaping Nasser's action in the crisis leading to the outbreak of the 1967 war.²²

In response to his domestic economic difficulties, Nasser appointed a new Prime Minister in October 1965, Zacharia Mohieddin to carry out the needed internal reforms and improve internal security. Mohieddin's economic strategy was to relax the centralized controls of Egyptian government, permit market forces to function to a greater degree, and provide an environment more attractive to foreign capital investment.

Unfortunately, Nasser thwarted this ambitious plan for at least three reasons. First, in the late summer and early fall, Egyptian prices on staples rose steeply in response to shortages and the continuing high level of government expenditure on defense and inefficient industrialization. Attempts to secure a loan from the IMF failed when Egypt rejected strict controls on the money supply for fear of inciting greater domestic disorder.

21. *Ibid.*, p. 239.

22. *Ibid.*, pp. 239–240.

Second, the Egyptian bureaucracy proved highly effective at resisting the Prime Minister's efforts. Conservative estimates suggest that the size of the Egyptian government more than doubled between 1962 and 1965 in terms of both personnel and payroll. By 1966, the government payroll accounted for 73 percent of total government spending. It is not surprising that Mohieddin's efforts to decentralize government and eliminate many government organizations was highly unpopular with current government employees, the economic organizations which benefited from customary relationships with the regime, and the Egyptian middle class for whom government was a career.

Finally, and most important, Nasser was not prepared to delegate meaningful authority to Mohieddin. Indeed, Nasser could not delegate to anyone. Arguing that the Prime Minister was appointed specifically to deal with domestic problems, Nasser excluded him from the foreign and defense policy decisions that were largely responsible for Egypt's domestic economic problems.²³

In April 1966, Moheiddin submitted his resignation, which was refused. He grimly went through the motions until September of that year, when Nasser reshuffled his cabinet. Moheiddin was returned to his former position, vice-president of the UAR, a purely ceremonial office. The three ministers concerned with the economy were fired and were replaced by military officers.

Nasser's close associates take the view that his decision reflected his growing paralysis when confronted by the need to modernize the economy by inflicting additional pain at a time when the population was increasingly restive at the pain it was already

23. Nutting, *op. cit.*, pp. 375–380.

being asked to bear.²⁴ He was increasingly hemmed in by the Badran-Amir group, which opposed any fundamental reallocation of resources away from defense. The dismissal of the relatively pragmatic and market-oriented Prime Minister and the appointment of more military officers suggest a desire to avoid confronting this dilemma. One can find current parallels in the recent history of the USSR where the Party chose morale building and anti-alcoholism as an alternative to systematic reform. Not surprisingly, the Egyptian food shortages did not improve, and dock strikes occurred in Port Said in the fall of 1966.

These events constitute the domestic context in which Nasser made his peace and war decisions in the months preceding the outbreak of the 1967 war with Israel. The steps leading to that crisis illustrate the way domestic weakness and the actions of other states can dovetail to make war a response to internal instability, or threat.

A faction of the Syrian Ba’thist party seized power from a rival faction in February 1966. Salah Jalid became head-of-state and named his fellow conspirators to the other positions of high national leadership. Hafez al-Asad became Minister of Defense. This group took authority in a state of high emotion and paranoia, convinced, partly with reason, that numerous plots threatened them internally. They were also convinced that the CIA, Israel, and the conservative Arab regimes in Saudi Arabia and Jordan were supporting these conspiracies. Certainly Israel had the Syrian government well penetrated and had broken many of its codes. Jordan, Saudi Arabia, and Lebanon had accepted the emigration of thousands of disaffected Syrians, and Jordan permitted a clandestine anti-Jalid radio station to operate from its soil.²⁵

24. Sadat, *op. cit.*, pp. 71–73.; Heikal, *op. cit.*, pp. 201–203.

25. Seale, *op. cit.*, pp. 104–109.

At this unpropitious time, Syria's border tensions with Israel sharply escalated. Since the war of 1948 three patches of land located respectively on the eastern shore of the Sea of Galilee, near the Hula marches to the north, and at the tip of the so-called Galilee Finger, still further north, had been in dispute. By the terms of the armistice, the patches were demilitarized with their sovereignty left uncertain. With some reason, Israel felt it had a legitimate claim, and engaged in a process of creeping annexation. The Syrian military responses were met by Israeli retaliation. By 1964, Israel was in possession of most of the disputed areas, developed settlements, and, in particular, completed the so-called National Water Carrier project to divert water from the Jordan.²⁶

Syria regarded Israeli control of the disputed areas as unacceptable and was particularly provoked by the diversion of water it regarded as its own. Therefore, from the early 1960s, the Syrians shelled the Israeli disputed settlements and supported Palestine guerrilla operations in the area. By 1966, the level of violence in this area had risen sharply with both sides fighting nightly small unit actions, air battles, and engaging in long-range bombardments.

Three factors combined to turn this volatile situation in Egypt and Syria into war. The first was Nasser's state of mind in late 1966 and early 1967, beleaguered as he was by domestic problems, isolated from his old associates, and lacking recent successes in his foreign relations. The second was the more intensive introduction of the Palestinian guerrillas to the conflict. The third was the Israeli's sense that advantage was to be found in this situation.

26. Safran, *Israel*, pp. 384–385.

Israel had been a chronic source of vulnerability for Nasser. For, in spite of his flamboyant rhetoric, he had always counseled caution when confronting Israel. This position left him open to charges of cowardice, a dangerous accusation to someone who bases his legitimacy on being almost reckless in facing down such adversaries as France, Great Britain, and the U.S. So concerned had he become over a possible Syrian-Israeli war that Egypt would get sucked into that he organized an Arab summit in Cairo in 1964 to use the weight of the collective Arab leadership to inhibit the Syrians. He repeated this performance in 1965 and 1966. However, by the 1966 summit, Nasser's credentials were not so impeccable and his strategy of "containment" and "restraint" came under serious, public, vitriolic challenge both at the conference, within Egypt, and over Syrian and Jordanian official radio. This resistance was bolstered by the new Palestine Liberation Organization, which leveled the charge that Nasser had no intention of liberating Palestine.²⁷

Nasser's position had reached a crucial point. Though, the level of overt, organized public domestic resistance to his regime remained low, he anticipated more severe eruptions stimulated by his failure to confront Israel, support Syria and the Palestinians, his continuing stalemate in Yemen, and the ouster of his friends and supporters in other states: Ben Bella of Algeria, Mehdi Ben Baraka of Morocco, and Sukarno of Indonesia. As a result, Nasser moved to co-opt his critics by taking a more aggressive position toward Israel, even though he did not seek war. Indeed, he felt that the Arab armed forces were largely unready and would remain so for several more years.²⁸

27. Dekmejian, *op. cit.*, pp. 241–243.

28. Seale, *op. cit.*, p. 122–123; Nutting, *op. cit.*, pp. 388–389; Heikal, *op. cit.*, pp. 212, 213; Sadat, *op. cit.*, p. 157.

His first attempt to diminish Syrian criticism was to sign a bilateral defense pact with them in late 1966, and the two states restored diplomatic relations, which had been broken when Syria seceded from the UAR in 1961. The pact pledged Egyptian aid if Syria were attacked, and committed each side to create a joint command headquarters. However, this cosmetic step did little to improve relations between the two states. Nasser suspected the Syrians of seeking to entrap Egypt in a war with Israel in the name of Pan-Arabism. The Syrian leadership correctly believed Nasser hoped for change of regime in Damascus with a consequent reduction of Syrian aggressiveness. King Hussein of Jordan believed the affect of the pact would be to give the Syrians greater latitude in supporting Palestinian Guerrilla operations in the disputed areas to which Israel generally retaliated by attacking Jordan.²⁹

Indeed, within a week after the pact was signed, Israel launched the largest retaliatory raid to date against the Jordanian village of Samu in the Hebron hills. The effect was to provoke massive riots in Jordan, which nearly brought down the King Hussein. Ultimately, the Jordanian army plus 20,000 Saudi troops were needed to restore order. King Hussein, the Saudis, and the PLO publicly accused Nasser of cowardice for not providing air cover over Samu and not creating counter pressure in the Sinai to diminish Israeli freedom of action. Amir and his supporters in the War Ministry and by the Muslim Brotherhood echoed these criticisms within Egypt. Nasser attempted to rationalize his lack of action by pointing out that the UN force in the Sinai, the UNEF, prevented Egyptian action there. This explanation produced derision in Syria and Jordan, which pointed out that the UNEF, was in place with Egyptian consent.³⁰

29. Seale, *op. cit.*, p. 124.

30. Safran, *op. cit.*, p. 386; Nutting, *op. cit.*, p. 392.

This fragile situation continued until the next large-scale engagement of Israeli and Syrian forces. On 7 April 1967, Syrian artillery fire into one of the disputed areas was answered by large Israeli air strikes on the guns. The Syrian air force responded, and the Israelis shot down 6 MIGs over Damascus in full sight of the people in the street. Indeed, Israeli aircraft performed victory roles over the city itself.³¹

It is clear that Nasser did not want war with Israel.³² Though Amir and Badran fed him optimistic reports about Egypt's prospects, he was not convinced, and he could see no benefit to himself or to Egypt in sustaining a defeat by Israel. On the other hand, his failure to act in response to the 7 April battle left him entirely exposed to extreme pressure both from within and without. Israel was viewed as a Western proxy, and the Arab world, which he claimed to represent, was calling for Egyptian action. Even the conservative Saudi regime, normally fearful of Nasser's secular threat to its legitimacy issued the following statement over official Radio Jidda:

“Anyone who imagines that Egypt will wage any kind of battle against Israel, to defend Syria or anyone else, will wait a long time.”³³

Jordanian broadcasts played on the same themes, pointing out that Egyptians could have flown over the UNEF areas to bomb Eilat, and that Nasser was permitting Israeli shipping to transit the Straits of Tiran while Arabs were being killed.

According to Heikal, Nasser saw himself as having two unattractive alternatives. First, he could stay out of the growing conflict between Israel and Syria. If there were war between the two without an Egyptian front, the Syrians would be destroyed quickly,

31. Nutting, *op. cit.*, p. 396.

32. Heikal, *op. cit.*, pp. 216–217.

33. Seale, Patrick (1988), *Asad: The Struggle for the Middle East*. Berkeley: University of California Press.

and Nasser's regime (if not his person) could not survive that event. Second, he could actively support Syria by counter pressure in the Sinai. This would increase the chances of war with Israel, which Nasser feared, particularly if Nasser left it to the Syrians to escalate tensions. As between an unacceptable alternative and one that is merely almost unacceptable, the latter must be relatively more attractive.³⁴ Nasser selected the second option for this reason, but sought to manage the risks by wrestling management of the situation away from the Syrians. He hoped he could walk the narrow strip between too much caution, which would destroy his regime, and too much aggressiveness, which would produce war with Israel.³⁵

The course of action he took was to shift the center of gravity away from Syria, where his control was weak, into the Sinai. To this end, on 16 May 1967, he called from the United Nations Emergency Force to withdraw from its positions on the Sinai border between Egypt and Israel. He hoped to manage his risk by specifically *not* requesting removal of the rest of the UNEF force from its more sensitive position at Sharm Al-Shaykh, (which controlled the Straits of Tiran) and in the Gaza Strip (which permitted direct access into the Israeli heartland). Unfortunately, UN Secretary General U Thant replied publicly that such a partial withdrawal was not possible. This left Nasser no choice but to request complete evacuation by the UNEF on 18 May, a request U Thant obeyed with alacrity. Egyptian forces then occupied the UN positions in the Sinai, but refrained from doing the same at Sharm Al-Shaykh, the announced Israeli *casus belli*.³⁶

34. Heikal, *op. cit.*, pp. 220–222.

35. Nutting, *op. cit.*, p. 399.

36. Seale, *op. cit.*, p. 130.

Syrian and Jordanian official news ridiculed Nasser's caution. More importantly, Badran, Amir, and other high commanders in the Egyptian armed forces demanded he go forward, citing the lack of warnings from the U.S. and Israel. (Of course, the reason no warning was given by the U.S. was to avoid placing Nasser in a position where he would be seen as backing down from a U.S. threat. The reasons for the Israeli silence are unknown.) Nasser could not hold out against this pressure, and he ordered Sharm Al-Shaykh occupied and the Straits of Tiran closed on 21-22 May 1967.³⁷ Israeli attacked on 5 June.

The key point is that, even at this point, Nasser's objective was not war. There is strong evidence that he remained pessimistic about Egypt's chances throughout, in spite of the optimistic situation reports being sent him by Amir and Badran. Indeed, Nasser's assessment essentially was the mirror image of Israel's. The Israeli leadership may not have directly sought war, but they regarded the entry of two-thirds of the Egyptian army into Sinai as a tremendous opportunity. Their only major debate was over how long to wait before attacking. Both Nasser and the Israelis knew that the Egyptian army's deployment into the Sinai was chaotic in the extreme. No logistics were in place, communications were not secure or reliable, and units were intermixed and strung out through the desert. A third of the Egyptian army remained in Yemen. Even the Syrians, as bellicose as they were, were not prepared for war, as Nasser knew. The Syrian armed forces had repeatedly purged its officer corps to the point that few senior and middle-level officers remained with any experience or competence. The Syrian Chief of Staff, General Suwaydani, called for immediate promotion of all officer cadets. No joint plans existed between Syria, Egypt, and Jordan.³⁸

37. Seale, *op. cit.*, p. 130.

38. Stephens, R. (1971) *Nasser*. London: Penguin Press: pp. 481–482; Seale, *op. cit.*, pp. 136–137; Dekmejian, *op. cit.*, p. 243; Nutting, *op. cit.*, p. 408.

Yet Nasser viewed the risk of war to be the lesser risk, even though he clearly understood the consequences of occupying Sharm Al-Shaykh and closing the Straits. The likelihood of war with Israel, while very high, was not certain. The likelihood of defeat, also high, also was not certain. Nasser's assessment of the consequences of doing nothing, loss of the regime, made the risks of action acceptable.

- a. How was the regime organized, and which institutions and individuals were important sources of political power?

After the 1952 coup, the Egyptian government went through several configurations before settling on a system resembling somewhat the French: a strong President, a Prime Minister, and a Cabinet. Of course, in the Egyptian case, the President did not stand for election, and he appointed the Prime and Cabinet Ministers.

As discussed in the case, power resided almost entirely with Nasser, though other institutions were capable of challenging Nasser when he was vulnerable. First among these was the Egyptian military, especially the young, nationalistic, middle-class officer corps which Amir and Badran cultivated skillfully. Second, was the Muslim Brotherhood and similar, though smaller, organizations, which could mobilize the population on issues of Pan-Arabism and Islam. Third was the state bureaucracy which more than doubled under Nasser's leadership and proved a major source of resistance to Nasser's half-hearted reform attempts in the early 1960s.

Though the secret police was large and active during Nasser's regime, there is no evidence that it was viewed as a potential challenger or independent power source or that it ever attempted to be.

- b. On what basis did the regime's hold on political power rest?

Nasser held power almost entirely on the basis of extra-national interests: Pan-Arabism and anti-Western sentiment among all sectors of the Egyptian population. In some sense, Nasser was leader of more than Egypt, and therefore had a surfeit of constituencies to please. His claim of legitimacy directly flowed from his ability to stage dramatic foreign policy triumphs such as Suez 1956, development of the Soviet connection in the face of U.S. protests, construction of the Aswan High Dam without Western contributions, and a prominent role in the non-aligned movement.

The Muslim Brotherhood opposed Nasser as a secular nationalist, but that challenge was manageable so long as Nasser's forward momentum could be maintained.

The institution he took pains to cultivate was the Egyptian military. He tolerated the "cult of personality" Amir created for himself. Indeed, Nasser declined to push confrontation with Amir in the early 1960s when attempts to curtail his power failed. Nasser's primary source of reward was large defense budgets and prerequisites afforded the Egyptian officer corps.

- c. In the period prior to the outbreak of external conflict, did the regime believe its hold on political power was weakening so significantly as to require remedial action?

In the months prior to the June War, Nasser felt himself substantially threatened by the intense criticism directed at him by other Arab capitals and the Palestinians. The source of the criticism in facing Israel, thereby seemingly abandoning the Palestinians, Syria, and Jordan to their fates. The criticisms gained in effectiveness in direct relation to the success of the spectacular Israeli operations, which drew regional and international attention.

Nasser's restraint in the face of this pressure is a measure of his reluctance to fight a war with the Israelis and his belief that Arab rearmament was still insufficient.

The internal Egyptian institution benefiting most was the Muslim Brotherhood, which was able to mobilize large public demonstrations against Nasser's betrayal. Amir and Badran used this occasion of Nasser's weakness to insist on development into the Sinai and eviction of the UNEF.

- d. Did the regime develop an explanation for its crisis, which connected another state's actions or simple existence to the domestic crisis?

Nasser's explanation for his predicament focused on Israel, whose military pressure on Syria, Jordan, and the Palestinians were providing justification for opposition to Nasser.

- e. Did the regime develop a plan of action or strategy in which external conflict with the state was expected to ameliorate the domestic crisis?

Therefore, Nasser conceived his plan to remilitarize the Sinai to deter the Israelis from acting with a free hand on their Northern and Eastern fronts. Success in that would restore Nasser's legitimacy as the only Arab leader of international stature.

- f. Did the regime undertake an external conflict with that state to ameliorate the domestic crisis?

Nasser did not seek war with Israel, though it is unclear how pessimistic his estimates of a potential conflict were. His own timetable for conflict was not for several years, and he manifested his caution by attempting to avoid crossing Israel's pre-announced red line, closing the Straits. Yet, ultimately, when confronted with the likelihood of war with Israel or not entering the Sinai, Nasser chose the latter.

Could the Egyptians have been deterred from pursuing confrontation, especially in May 1967? As with the cases of China and Argentina, there is evidence that Nasser could have been deterred. Although highly motivated by political considerations, Nasser devoted considerable attention to the question of the military balance between the two sides. His closest associates suggest that he would not have continued to escalate had he not concluded that Israel was militarily inferior (see Safran, 1978, pp. 397–398).

One reason for his misimpression was the Egyptian-Syrian defense agreement, which placed Syrian forces under Egyptian command. In May 1967, Jordan agreed to do the same, and arrangements were made to bring Egyptian and Iraqi troops into Jordan. The result was to compel Israel to fight a three-front war, its worst case. Nasser was undeterred by Israel *not* because he doubted Israel's resolve, but because he doubted Israel's capability. He did assess military balances, even in the midst of intense crisis, and that assessment was instrumental in his decision to go forward. Therefore, the Israelis should have been able to affect that decision, if they could have altered Nasser's view of the military balance. Thus Nasser was deterrable. Indeed, as was true with the Chinese in 1950, despite great differences in culture, values, and political system, the Egyptians and Israelis spoke a common deterrence language, albeit unsuccessfully.

China, 1950

The Chinese decision to enter the Korean War in the winter of 1950 is an excellent example of the ways in which domestic fragility can drive a state's international behavior. In addition, this decision also demonstrates the strength of motivation that results when domestic stability is deemed to be at stake. This case is informed by a number of classic works on this subject and by recently obtained Chinese documents:

Mao's Korean War telegrams to Stalin, Chou en Lai, and Chinese military commanders in the field.³⁹ These data provide a clear picture of Mao's sense of Chinese domestic weakness in the fall of 1950. The Chinese revolution was only two years old at this point; Taiwan was in Nationalist hands; and China was isolated in the world. Her economic situation was dire, and Mao was very concerned that the counterrevolutionary potential in China was dangerously high.

Mao's fears were twofold. First, he was concerned that U.S. and Republic of Korea forces would push into Manchuria and seize China's industrial heartland. However, he was relatively sanguine about this prospect compared to his greater fears that a liberated Korea and a U.S. presence on the Chinese border would imperil the internal order. Such a scenario would have this effect, because it would tie down troops that were needed to menace Taiwan and maintain internal security, be very expensive at a time when China had little cash, and embolden counterrevolutionaries.

Specifically, as Whiting and others have noted, the position of the new Chinese revolutionary regime was quite precarious in 1950. Internal armed resistance had not been entirely suppressed. Large areas in western and southern China remained potentially hostile because of their continued connections to the anticommunist opposition—Kuomintang forces, traditional sources of feudal authority, and non-Chinese ethnic groups. Mao's decision to enter the war was not taken as a crude diversion to the troubled population,

39. See Whiting, A. (1960), *China Crosses the Yalu: The Decision to Enter the Korean War*. Stanford: Stanford University Press; N. Lebow (1981), *Between Peace and War: The Nature of International Crises*, Baltimore: Johns Hopkins Press; and A. George and R. Smoke (1974), *Deterrence in American Foreign Policy: Theory and Practice*, New York: Columbia University Press; T. Christensen, "Threats, Assurances, and the Last Chance for Peace: The Lessons of Mao's Korean War Telegrams," *International Security*, Summer 1992: pp. 122–154. In particular, see Christensen (1992), pp. 122–154. This article discusses Chinese decisionmaking in light of recently released Chinese archival materials, including some of Mao's Korean War cables.

although the “diversion” theory may be applicable at times. Rather, he sought to remove an external influence that, if left unrestrained, threatened to amplify critically indigenous sources of instability.

With stakes as high as regime preservation, one would predict that China would have been very difficult to deter. In fact, that seems to have been true. Mao recognized clearly the risk the United States could pose of striking urban-industrial targets from the air with conventional and *nuclear* weapons. China, after all, had no means to respond directly to this threat. Ironically, this threat led Mao to take the most aggressive course he could. He reasoned that the presence of U.S. forces anywhere in Korea posed an unacceptable risk to Chinese domestic arrangements. A Chinese offensive to drive the United States merely below the 38th parallel would be unsatisfactory. The United States still would be left in possession of Korean bases from which it could wage a strategic air war against China. Indeed, Mao’s worst-case scenario was a stalemate on the Korean peninsula leaving the U.S. free and motivated to undertake strategic bombardment of China from within easy range. Therefore, Mao elected to commit forces sufficient to drive the United States entirely off the peninsula in one blow as quickly as possible. In that way, the U.S. capability to strike Chinese targets could be reduced to a “more tolerable scope and duration.”⁴⁰

This case is a good example of the domination of the costs of inaction over the risks of all other courses, at least in the eyes of the Chinese leadership. The presence of U.S. forces north of the 38th parallel was deemed a mortal threat; so to do nothing was unacceptable as soon as the U.S. forces crossed the 38th parallel. Mao stated this

40. Christensen, T. “Threats, Assurances, and the Last Chance for Peace: Lessons of Mao’s Korean War Telegrams,” *International Security*, 1992, pp. 137–138.

explicitly in his wires to Marshall Peng Duhai and Stalin. Apparently, no buffer arrangement with the United States along the Yalu would have ameliorated his fears. Therefore, Chinese strategic decision making shifted to evaluating ways in which the benefits of an inevitable and risky war could be maximized. In that process, Mao accepted the possibility that China could be struck severe blows. There is no evidence in his telegrams of confidence that the Soviets would or could deter the United States from attacking China. Thus, Mao deemed nuclear bombardment of Chinese cities less risky than tolerating U.S. forces on China's border.

Seen in this light, the prospects for deterring the Chinese were substantially reduced after the 38th parallel was crossed. This resolves the question posed by Thomas Schelling as to why the Chinese prepared their attack in secret, and launched it by surprise, if their objective was to deter the United States from crossing the Yalu.⁴¹ In Mao's view, deterrence of the United States had failed much earlier and, accordingly, deterrence of Chinese intervention became extremely difficult—but, perhaps, not impossible.

Mao's Korean War telegrams provide evidence that, as motivated as the Chinese were, they were not nondeterrable.⁴² In the telegrams, Mao discusses his worst-case scenario: a failure to eject the U.S. forces from the Korean peninsula, followed by prolonged stalemate and U.S. bombardment of the mainland. It follows logically that Mao's strategic calculations might well have been affected *if the United States had been able to pose a credible threat that a Chinese attack would have resulted in Mao's worst case*. Presumably, this would have required the United States to establish a declaratory

41. *op. cit.*, p. 141.

42. *op. cit.*, pp. 137–140.

policy of this intention, and to reinforce and configure its advancing forces in Korea to make them clearly capable of resisting a Chinese attack. Ground forces would have been necessary for this task, since the Chinese had assessed correctly that the air forces of that period could not halt a Chinese offensive with the coalition's ground forces deployed as they were.

- a. How as the regime organized and which institutions and individuals were important sources of political power?

It is important to realize that the Korean War began, though not the Chinese intervention, with the Chinese revolution significantly unfinished. Nationalist units were still operating inside of China in border areas. In a real sense, there were no governing institutions other the Party apparatus, which had existed for decades.

In 1950, a constitution was promulgated creating a national government. The supreme state decision making body was the Central people's Governing Council led by Mao with high-ranking PLA officers as his deputies. The supreme administrative body was the Government Affairs Council, led by Chou En-lai, Mao's closest civilian associate. In truth, of course real power lay with the parallel Party organizations, the POLITBURO and the People's Revolutionary Military Council.

- b. On what basis did the regime's power rest?

Mao and the CCP's constituency was overwhelmingly the rural peasantry, who made up a large part of the PLA. Therefore, one can say that Mao's hold on political power depended upon three components: the CCP rank-and-file and second tier leadership, the rural peasantry, and, in particular, the PLA, the most coherent and disciplined institution in China.

Opposed to the regime were parts of the intelligentsia, professional classes, property owners, and seeming paradoxically, the rural peasantry. The latter was true in prospect, because, at this time, Mao and the CCP had not yet embarked on thoroughly communizing China. Land had been redistributed in small parcels to the peasantry, but it was held by them as private owners rather than state property. Considerable resistance was anticipated when collectivization started in earnest.

- c. In the period prior to the outbreak of external conflict, did the regime believe its hold on political power was weakening so significantly as to require remedial action?

The evidence is compelling that Mao felt reasonably secure in his hold on power on the eve of the North Korean attack. However he clearly viewed the U.S. decision to intervene as highly threatening to that hold, depending upon the various paths the Korean war might take.

The threat he perceived was not a direct attack by U.S. ground forces into China. Rather, he feared that a U.S. force on his border, having defeated a communist state and changing its regime, would have a strongly energizing effect on the nascent counter-revolutionary elements in China. As a corollary, he feared that any lack aggressiveness on his part would alienate the senior PLA leadership, a central pillar of Mao's regime.

- d. Did the regime develop an explanation for the crisis, which connected another state's actions or simple existence to the domestic crisis?

The CCP had developed an extensive, effective, and sophisticated propaganda apparatus down to the grass roots level. It was used to prepare the country for war. The line taken depicted an aggressive U.S. intent on attacking and occupying China, deposing the regime, and reintroducing the Nationalists.

But this did not exactly reflect the Chinese leadership's true concern, which was not that the U.S. could occupy China, but that the revolution was sufficiently vulnerable to internal forces alone. Obviously this analysis was too dangerous to share with the public.

- e. Did the regime develop a plan of action or strategy in which external conflict was expected to ameliorate the domestic crisis?

Mao and his associates did develop such a plan, which was to engage the U.S. forces and expel them from the Korean peninsula, or, failing that, to drive them away from the Chinese border and permit the survival of the communist regime in the North

- f. Did the regime undertake an external conflict with that state to ameliorate the domestic crisis?

The Chinese implemented this strategy with positive results, assuming Mao's concerns about the fragility of the revolution were accurate in the first place.

Argentina, 1982

Argentina's calculations that led to its attack on the Falklands (Malvinas) illustrate the way war and peace decisions can be crucial to a regime's struggle to retain legitimacy. In this case, Argentina's leaders attempted to buttress their hold on political

power through a demonstration of military competence. The reasons they felt compelled to take certain steps illustrate the relationship between internal regime weaknesses and aggressive external behavior.⁴³

The military junta in Argentina seized power in March 1976 in response to the incompetence of the Peronistas in managing internal security and the economy. The bloodless coup received widespread popular approval because of the expectation of the military's effectiveness in both of these areas. By early 1982, the junta's incompetence had proved to equal that of the previous regime, and Argentina's middle and upper classes, the military's main supporters, had become disaffected.

During World War II, Argentina had been the most prosperous and advanced state in South America. By 1976, when the junta took power, the Argentine economy had reverted to Third World status because of extensive corruption, national subsidies for various industries and groups, massive public-sector unemployment, and inflation. The junta attempted to rectify this situation through anticorruption and anti-inflation measures and free-market restructuring. Initially, the regime was able to obtain good results, as inflation dropped below 100 percent per year and national growth reached 7 percent in 1979. However, the widespread corruption and other weaknesses of the nation's financial institutions led to the collapse of its leading banks, which produced cascading business failures. By 1981, growth had dropped to less than 1 percent; inflation had reached an annual rate of 150 percent; real wages declined by 18 percent; and unemployment

43. For general information on the Falklands War, see Lebow, N. (1985), "Miscalculation in the South Atlantic: The Origins of the Falkland War," in R. Jervis *et. al.*, *Psychology and Deterrence*. Baltimore: Johns Hopkins University Press: pp. 89–124; M. Hastings and S. Jenkins (1983), *The Battle for the Falklands*. New York: W. W. Norton & Co.; "Falkland Island—The Origins of a War," *The Economist*, June 19, 1982: p. 43.

exceeded 15 percent.⁴⁴ The junta reacted in the traditional way of authoritarian regimes in this situation: It relaxed the austerity measures, used the public sector to absorb unemployment, and reinstituted government subsidies to prevent the loss of more jobs. Unfortunately, accelerating inflation was the result of the increased money supply needed to maintain these large-scale social programs.

The junta was no more successful in matters of internal security, an area of supposed expertise. In 1976, several left-wing insurgencies were active in Argentina, of which the Montoneros were the best known. The military encouraged the expectation that these groups would be overcome quickly and easily. Instead, what became known as the “Dirty War” ensued, which involved widespread repression, torture, and murder. As many as 20,000 may have been murdered, and many more were arrested or tortured. This “Dirty War” ultimately reached into many of the segments of Argentine society that had supported the 1976 coup.

As a result of its demonstrated incompetence, the junta became increasingly unpopular, especially between 1980 and 1982. Organized labor, which had been badly hurt by the junta’s economic policies, staged large protests in major cities in 1982. Farmers and small-business people were openly critical, as well, although they were not well organized. Consistent with the “juggling act” characteristic of authoritarian regimes, the junta mixed repression with a certain measure of increased freedom. As a result, by June 1981, Argentina’s five largest political parties joined together in a “common front” to demand open party activity and elections. The junta could not afford simply to deny these demands, and in November 1981 it issued new, freer guidelines for political activity.

44. As cited in Lebow, *op. cit.*, pp. 97–98.

Similarly, newspapers became bolder in 1980 and 1981 in their criticisms of the junta. Indeed, *La Prensa* and the *Buenos Aires Herald* openly advocated the reinstitution of civilian national leadership.⁴⁵ These and other newspapers also used continuing British sovereignty over the Falklands as illustrative of the junta's shortcomings. When the labor demonstrations reached their peak in March 1982, the junta felt compelled to use the Falklands as a means of reasserting its legitimacy through a demonstration of military competence.

The growing domestic crisis in Argentina dovetailed perfectly with the Conservative electoral victory in Great Britain in 1979. The prior Labor governments of Wilson and Heath had conducted continuous negotiations with Argentina over the sovereignty of the Falklands. Although very slow, these talks had produced movement toward the transfer of sovereignty to Argentina. However, the Thatcher government was much less friendly to this outcome. Negotiations stalled in 1981 and were broken off by the Argentines in early 1982.

Directly after the repudiation of the negotiations on March 3, 1982, the Falklands crisis began when a group of Argentine workmen raised the Argentine flag over South Georgia. They had been landed on the island as part of a long-term contract to remove British scrap metal. It is not clear whether or not the junta knew what the group intended; however, it is clear that Argentine public opinion had been fully aroused over the Falklands by this time. As a result, the junta seized on public approval of the workmen's actions and announced that the Argentine navy would give them "full protection." On March 26, 1982, over 100 Argentine troops landed on South Georgia; on April 2, Argentina invaded the Falklands.

45. As quoted in Lebow, *Ibid.*, p. 98.

Could Argentina have been deterred by the British? Approximately one month passed between Argentina's first hostile action (support for the workers on South Georgia) and the full-blown invasion of the Falklands. During that period, the British were cautious to the point of passivity. As is so often the case, the problem the British government faced was ambiguous intelligence, a strong predisposition to believe that the Argentines would not act "irrationally," and a classic concern that aggressive British action would provoke rather than deter.⁴⁶ This is not the place to assess British policy. Rather, the question is whether Argentina could have been deterred from invading the Falklands after the South Georgia event. Obviously, this question is impossible to answer, but several general points can be made.

The junta did not believe itself to be in control of events. This sense of compulsion is typical of a regime that deems its stability to be at stake. Endangered regimes usually believe they have no other choice but to take a desperate course of action. However, they seldom choose a course of action that they understand at the time to be virtually certain to fail.⁴⁷ Therefore, could the British have presented the Argentines with that prospect?

The British chose the most difficult option, which was to execute an amphibious landing and a subsequent ground campaign. There is good evidence that Argentina's planners thought that the British prospects of success in this endeavor were small and that the British themselves shared that assessment. In Argentina's view, once Argentine

46. See Jervis, R. (1976), *Perception and Misperception in International Politics*. Princeton: Princeton University Press for a discussion of the "spiral" model. The spiral model describes the conditions in which threats intended to be inhibiting are actually stimulating to an adversary.

47. As we said, the exceptions to this generalization are states in such dire straits that literally any alternative is preferred to doing nothing. But such states are rare.

troops (regardless of their poor preparation) reached the Falklands, the British would be compelled to negotiate. Thus, as long as the Argentines felt certain that they could deploy a force into the Falklands, they could not be deterred by the British threat to expel them.

However, the British had the naval capability to have greatly increased the risks to Argentina's troop deployments, the bulk of which were moved by ships. The British could have used attack submarines to isolate the Falklands from the mainland. Indeed, David Owen made exactly this point (Stein, 1992, p. 109). It would have been possible to implement this policy if one or two submarines had been dispatched to the South Atlantic at the time of the South Georgia incident. Also, tactical aircraft or surface-to-air missiles could have been deployed to the Falklands airstrip to prevent Argentina from airlifting troops instead of sea lifting them. Though Argentina might have contemplated contesting air superiority over the Falklands, it would have been helpless to contest control of the sea—a reality its leaders understood fully. Therefore, there are good reasons to suppose that the announcement that British submarines would prevent reinforcement of the Falklands would have been an effective deterrent, especially if coupled with an offer to renew negotiations over sovereignty. No doubt, the Argentines might have tested the British to ensure against a bluff. The British submarines could have given a graphic demonstration of their capabilities, and Argentina would have been entirely helpless to rectify their situation. Indeed, when H.M.S. *Conqueror* sank the *General Belgrano* on May 3, Argentina's surface navy never ventured again into the theater of operations.

One can debate whether the threat should have been made publicly or privately. Certainly, one characteristic of submarines is that they can be covertly deployed, thus making it possible to keep threats private. This threat, public or private, would have

confronted the junta with a virtually assured risk of failure. Unless the junta believed its chances of political survival to be even less likely, or it simply disbelieved the British threat, it likely would have been deterred by such a British move.

- a. How as the regime organized and which institutions and individuals were important sources of political power?

The supreme decision making body was the *junta* itself, on which each of the military services was represented by its chief of staff. There were no civilians permitted at the *junta*-level, International and military policy were entirely in the hands of the *junta*. The economy, on the other hand, was led by a civilian, so-called superminister, Martinez de Hoz, who could rule by decree in all matters economic, with one important exception. All the state-owned enterprises, a considerable portion of the economy, were directed by military officers and were off-limits to Hoz.

Supporters of the regime included business, which benefited from de Hoz's free market policies and his drive to bring in foreign investment and the middle and upper classes, which looked to the *junta* for internal order and economic stability. Labor, specially organized, Peronistas, were strongly opposed to the junta, as were political elements of the intelligentsia and students. The urban and rural poor were mixed in their support.

- b. On what basis did the regime's power rest?

The *junta*'s sole claim to legitimacy was competence and efficiency in suppressing the rampant internal violence and restoring the economy.

- c. In the period prior to the outbreak of external conflict, did the regime believe its hold on political power was weakening so significantly as to require remedial action?

In 1981 and 1982, the *junta* felt compelled to return to the Peronist policy of extreme deficit spending to cope with the growing popular anger with the economy's lack of growth, inflation, and increasing unemployment. This economic decline was stimulated in part by a sharp decrease in foreign investment flowing into the country due to international uneasiness over the tactics used in the "Dirty War" and the apparently uncontrollable corruption at high levels of business management. That compelled the *junta* to loosen its suppression, which in turn promptly led to an upsurge in internal violence.

The loosening also led to large, increasingly frequent public demonstrations, many of which led by Peronist organized labor. The civilian political parties, heretofore excluded completely, announced the formation of a common anti-*junta* front. Finally newspapers, previously censored and extremely cautious, began testing the limits by publishing critical news and editorials.

- d. Did the regime develop an explanation for the crisis, which connected another state's actions or simple existence to the domestic crisis?

The *junta* can be charitably described as incoherent in its public explanations, in part due to its own incompetence and in part due to conflicting objectives. The *junta* members, the Argentine military services, all had to agree for any policy to be undertaken. A single dissent meant action was impossible. Each of the military services received considerable benefit from the corruption afflicting the Argentine economy; indeed a good deal of the budgets of each service were so derived. Regime supporters, such as business, also benefited. Therefore, the *junta* could only refer generally to

domestic trouble makers, Peronistas, and communists. At the same time, the *junta* desperately needed foreign investment, and so were precluded from show “crack downs,” as well as from blaming other states, possible sources of capital.

So, in short, the *junta* could not develop an effective explanation for its situation.

- e. Did the regime develop a plan of action or strategy in which external conflict was expected to ameliorate the domestic crisis?

The *junta* did indeed expect the seizure of the Malvinas would strengthen their position, though, again, for incoherent and incompetent reasons.

The *junta* believed strongly in the power of international prestige, especially that gained by military force, as a way of attracting foreign capital. They planned the Malvinas campaign in the belief the British would not resist and the U.S. would be indifferent. The victory would vindicate siphoning off national income for inordinate amounts of useless military hardware, create a “rally round the flag” popular reaction, demonstrate the *junta*’s competence to the public, and, most important, reopen the flow of foreign investment capital.

- f. Did the regime undertake an external conflict with that state to ameliorate the domestic crisis?

The *junta* deliberately and explicitly did exactly that.

India, 1962

The Sino-Indian war in October 1962 provides the last example of the intermingling of domestic stability and international behavior and of how the desire to avert loss (usually loss of political control) leads to increased risk-taking. It also illustrates, as do most of the cases we examine, that a propensity to take high risks is not

synonymous with being undeterrable. The Indian leadership was concerned with the military balance with China, and a particular assessment of that balance was crucial in Indian decision-making.⁴⁸ It follows that, had China been able to alter that particular assessment, the chances of deterrence success would have been substantially increased.

This war was the result of India's decision to contest Chinese occupation of a disputed border area, the Aksai Chin. The relative equities of the Chinese and Indian claims to the Aksai Chin are irrelevant to this discussion. Interested readers are directed to an extensive literature on the long and rich history of the question.⁴⁹ Suffice it to say that the dispute had its origins in British colonial decisions over where the Indian border should be drawn. As such, the contest for sovereignty over the Aksai Chin was an outgrowth of one of the many colonial-era negotiations that resulted in troublesome administrative lines drawn on a map.

Following independence in 1947, India asserted its claims to the Aksai Chin. However, a confrontation with China did not occur until the mid-1950s, when India began introducing reconnaissance patrols into the area. These patrols discovered a strategic road that the Chinese had built in the early 1950s to link Sinkiang with Tibet. The discovery became a domestic crisis for Nehru, the Indian Prime Minister and founder of the Congress Party.

48. See Lebow, R. (1981), *Between Peace and War: The Nature of International Crises*. Baltimore: Johns Hopkins University Press: pp. 164–169; Y Vertzberger (1984), *Misperceptions in Foreign Policymaking: The Sino-Indian Conflict, 1959–1962*. Boulder: Westview Press; S. Hoffman (1990), *India and The China Crisis*. Berkely, University of California Press; L. Kavic (1967), *India's Quest for Security: Defense Policies, 1947–1965*, Berkeley, University of California; F. Moraes (1956), *Jawaharlal Nehru: A Biography*. New York: McMillan; M. Edwards (1972), *Nehru: A Political Biography*. New York: Praeger.

49. Fisher, M., L. Rose, and R. Huttenback (1963), *Himalayan Battleground: Sino-Soviet Rivalry in Ladakh*. London: Pall Mall; A. Lamb (1964), *The China-India Border*. London: Oxford University Press.

Nehru had pursued a consistently friendly policy toward China, even to the point of acquiescence to Chinese occupation of Tibet in 1950. But large and politically weighty elements of the Indian military, the Congress Party, and the foreign policy establishment were quite hostile to China, suspecting it of hegemonic ambitions. Nehru's domination of the Congress Party, indeed of his own cabinet, was partial at best. Many of those who opposed Nehru over his China policy also opposed his broad social and economic reforms. So the border dispute was used to jeopardize Nehru's national domestic objectives; it was used as a proxy for those who had built up a "dislike of Nehru and his charisma, his claim of superiority, his indispensability, his concept of social and economic revolution . . ."⁵⁰ As a result, Nehru had very little room to negotiate a compromise with the Chinese. This became especially true as the Indian population became mobilized over the border dispute in the wake of a sharp firefight between Indian and Chinese troops in August 1959. Nehru is quoted as having told a colleague, "If I give them that (a negotiated settlement), I shall no longer be Prime Minister of India."⁵¹

His position was further constrained by a 1960 decision of the Indian Supreme Court, which ruled that any cession of Indian territory required a constitutional amendment. Thus to cede part or all of the Aksai Chin to the Chinese would have required a two-thirds vote in the Parliament plus simple majorities in eight of India's fourteen legislatures, a very difficult process to say the least.⁵²

In spite of these constraints and pressures, Nehru attempted (as did Nasser in 1966–1967) to walk a balanced course between China and his domestic enemies. To this

50. Edwards, *op. cit.*, p. 287.

51. Quoted in Hoffman, *op. cit.*, p. 286.

52. Lebow, *op. cit.*, p. 188.

end, he felt compelled to continue the Indian patrols in the Aksai Chin, although he did not increase them. A second exchange of fire occurred in October 1960. Public opinion reacted with bellicosity toward China. In a speech to the nation, Nehru attempted to rein-in the inflamed Indian sensibilities by taking a moderate line on the shooting. For his pains, he was accused in newspaper editorials of appeasement, weakness, and an “over-scrupulous regard for Chinese susceptibilities and comparative indifference toward the anger and dismay with which the Indian people have reacted.”⁵³

Nehru never took a conciliatory line again in this matter and responded to this criticism by increasing the Indian presence in the disputed area. A program was begun to construct a network of Indian forward outposts, and deployed Indian forces were ordered to fire on Chinese forces threatening them. A series of clashes ensued, increasing in intensity and cost throughout the remainder of 1961 and 1962. They culminated in a successful Chinese offensive in October 1962, in which China seized the Aksai Chin.

Why did the Chinese fail in their attempts to deter the Indians? As with the other cases, the Indian leadership believed that its domestic political position was weak and that demonstrations of external “strength” could ameliorate that condition. Such behavior can be imperfectly analogized to “overcompensation” in psychodynamic theory. An individual may respond to weakness in one area by hyperaggressiveness and rigidity in another. Such behavior is not the exclusive province of Third World leaders. But Third World regimes tend to be fragile, so events that might be difficult for any leader become “life threatening” for them. Therefore, Nehru and India plainly fall into the “hard-to-deter” category. But “hard to deter” does not mean “impossible to deter.”

53. Moraes, *op. cit.*, p. 302.

I make this point because of the strong evidence that Nehru was very interested in the Sino-Indian military balance. Specifically, he had quite an optimistic (although misguided) view of the capabilities of the Indian forces to defeat the Chinese.⁵⁴ The fact that such an assessment was unrealistic misses the point that consideration of the balance was central to Nehru's risk-taking behavior. Further, his view was widely shared, even among Indians alarmed by the aggressiveness of Nehru's actions. It follows that the Chinese might have deterred the Indians had they found a way to communicate effectively their actual capabilities. The Indians reportedly took Chinese caution prior to their October attack as evidence of the correctness of the Indian assessment.⁵⁵

- a. How as the regime organized and which institutions and individuals were important sources of political power?

India's government was (and is, of course) a parliamentary democracy. A president, is head of state and a prime minister is the leader of the government. At this time, the two positions were unified in Nehru.

The basis for Nehru's political strength was the Congress Party, secular and democratic. Its constituency was comprised of a coalition of the middle class, moderate labor and peasantry, and the intelligentsia. But, of course, India had suffered from a history of particularly virulent and lethal opposition groups of various sorts, e.g. Hindu nationalists seeking a theocracy, radical leftists seeking orthodox socialism or

54. See Hoffman, S. (1990), *India and the China Crisis*. Berkeley: University of California Press, Chapter 9.

55. Note the parallel between the effects of caution in this case and those in the Falklands invasion. It suggests that the period following the first provocation by the aggressor is especially important for communicating credible deterrence. Passivity at this point can be very dangerous.

communism, conservatives seeking a capitalist and militarily powerful state allied with the West, and geographical separatists seeking secession or autonomy for particular ethnic communities or regions.

By holding the center, by fabricating a reasonably successful mixed socialist and free market economy, and by acquiring international recognition through the India-led Non-Aligned Movement, Nehru was able to minimize the impact of the opposition groups and enjoyed hefty majorities in the parliament.

b. On what basis did the regime's power rest?

See a. above.

c. In the period prior to the outbreak of external conflict, did the regime believe its hold on political power was weakening so significantly as to require remedial action?

In the late 1950s, Nehru's coalition came under severe pressure from both the left and the right. The causes were three. First, the Indian economy weakened leading socialists and capitalists to blame one another and press for reforms in their preferred directions, greater movement toward complete socialism v. expansion of the Indian market economy. Second, relations with Muslim Pakistan over Kashmir remained highly adversarial, thereby keeping Indian Hindu nationalists at a high state of agitation. Third, pressure from China over the contested border area threatened the intellectual underpinnings of a foreign policy claiming to be independent of the protection of both superpowers.

Matters came to a head in the period 1959 to 1960. In 1959, Nehru elected to move somewhat in the direction of expanding socialism to include introduction of communal and co-op agricultural. This smacked of collectivization and triggered

significant disaffection among the agrarian peasantry and free marketers, as well the socialist groups which claimed the moves were too small and slow. Also, in 1959, Nehru felt compelled to dissolve the duly elected communist government of the state of Kerala. This prompted even greater criticism from the left, particularly since the Keralan government had avowed sympathies for the Chinese.

In response to his deteriorating situation, Nehru undertook two actions designed to shore up his position, and both of them were military. He invaded and took possession of the Portuguese colony of Goa, and he very publicly inaugurated the so-called Forward Policy by sharply increasing Indian military activity in the disputed border area with China. Firefights with Chinese troops soon became more frequent.

These steps did relieve some of the pressure on Nehru, but the public Forward policy placed him in a position with little room to maneuver vis a vis China.

- d. Did the regime develop an explanation for the crisis, which connected another state's actions or simple existence to the domestic crisis?

As with the Argentine *junta*, Nehru did not attempt to persuade the Indian public that another state, China, was responsible for India's problems. Rather he used the public's indignation at China's pressure over the disputed territories as an occasion to impress the public with his effectiveness and nationalism.

- e. Did the regime develop a plan of action or strategy in which external conflict was expected to ameliorate the domestic crisis?

Nehru instituted the Forward Policy for precisely this reason, but he and his cabinet were entirely confident that war with China would not be the result.

- f. Did the regime undertake an external conflict with that state to ameliorate the domestic crisis?

Like Nassar, Nehru did not undertake a deliberate policy to wage war against China nor did he believe that China would attack India. The Indian political, diplomatic, and military establishment were extraordinarily self-deluded in their understanding of the consequences of the Forward policy and China's intentions. At the same time, having used the policy so politically as a means of repairing his political position, Nehru had virtually no flexibility in negotiating with the Chinese or in drawing down the Indian forward military presence.

Interestingly, after the war was over, Nehru successfully used the Chinese attack as precisely the rallying event he had hoped his pre-war policies would be. This bolsters the earlier findings that losing such wars need not bring an end to a politician's career, although I must point out that that point was made only for autocracies and not for democracies.

Iraq, 1990

The motivations for Iraq's invasion of Kuwait have been widely debated, primarily over what dominated Saddam Hussein's motivations: desire for gain or desire to avert loss.⁵⁶ While probably not in the desperate domestic straits of Japan in 1941 and North Korea in 1950, many analysts argue that Iraq's situation contained important parallels. This may explain some of Iraq's willingness to accept risks that surprised Western observers during the Gulf crisis of 1990–1991.

56. See, for example Stein, J. (1992), "Deterrence and Compellance in the Gulf 1990–91: A Failed or Impossible Task?" *International Security*, Vol. 17,; pp. 147–179; P. Davis and J. Arquilla (1991), *Detering or Coercing Opponents in Crisis: Lessons from the War with Saddam Hussein*. Santa Monica, Calif: RAND R-4111-JS; L. Freedman and E. Karsh (1993), *The Gulf Conflict, 1990–1991: Diplomacy and War in the New World Orders*. Princeton: Princeton University Press; E. Karsh and I. Rautisi (1991), "Why Saddam Hussein Invaded Kuwait," *Survival*, January/February 1991.

From 1980 through 1989, Iraq incurred \$80 billion in foreign debt, mostly to sustain its war with Iran. About 50 percent was owed to Arab states, especially Saudi Arabia, Kuwait, and the United Arab Emirates (UAE).⁵⁷ By 1990, Iraq's creditors had become extremely concerned over Iraq's ability to meet its debt-servicing schedules. As a result, additional credit became difficult for Saddam to obtain.

The nub of his problem was financing national reconstruction after the ruinous war with Iran. Estimates of the cost of reconstruction were in excess of \$230 billion. Yet the national budget of Iraq was already running a deficit of about \$10 billion per year just to maintain the status quo. Obviously, the additional burden of reconstruction was unaffordable without new sources of capital. In principle, this could be obtained from three sources. First, the existing debt could be forgiven or rescheduled. The creditors rejected forgiveness, and rescheduling could provide only small relief. Second, new credit could be extended, but the creditors rejected that, too. Finally, Iraq could obtain hard currency from increased revenues from oil sales. Unfortunately, a higher oil price was needed. In the late 1980s, the price of oil had begun to fall, a trend that was still under way in early 1990. Iraq's attempt to arrest this slide by lowering production quotas was rejected by Kuwait and the UAE, the two biggest violators of the existing Organization of Petroleum Exporting Countries (OPEC) quota. By the beginning of 1990, OPEC was exceeding its production quota by 2 million barrels per day, with Kuwait and the UAE accounting for 75 percent of the surplus.⁵⁸

Beginning at the Arab Cooperation Council summit in February 1990, the Iraqis began an intense diplomatic effort to induce or coerce Kuwait and the UAE to agree to

57. Stein, *op. cit.*, p.157.

58. At that time, OPEC's production quota was 22 million barrels per day. Actual production was 24 million barrels per day.

lower quotas, reduce their own production, and forgive Iraq's wartime debts. Kuwait and the UAE remained indifferent to Iraq's pleas and threats. By mid-July 1990, Iraq had begun its military buildup opposite Kuwait.

The important deterrence question is to what extent Iraq's debt situation and lack of additional credit imperiled Saddam Hussein's regime—or, at least, that this was believed by the Iraqi leadership. To the extent the regime was endangered, Saddam's decision making could be said to be motivated by a desire to avert loss, i.e., his hold on power. Decision makers in this predicament have a propensity to accept high risks and so are hard to deter. To the extent the regime was not imperiled, Saddam's decision making could be said to be motivated by desire for gain, i.e., Kuwait's vulnerable oil riches. Such decision makers most often are chary of risk and easier to deter. Certainly at the time of the crisis, the prevailing view in the United States was that Saddam was motivated primarily by the latter rather than the former. This presumption may explain some of the continuing surprise at Saddam's stubbornness in trying to hold Kuwait in the face of such overwhelming force. Many observers felt that the Iraqis would surely retreat from Kuwait at the last moment under the cover of Russian or French mediation. Of course, this did not happen, a pattern of behavior more consistent with a desire to avert loss than purely a matter of gain. For this reason, a number of analysts of the Gulf War take the view that Saddam's "political survival and his long-term ambitions" hinged on the national reconstruction that could not be undertaken without some debt relief.⁵⁹ If so, Iraq would have been difficult to deter from invading Kuwait (though probably not impossible) and even more difficult to compel to leave. So it proved.

59. See Stein, *op. cit.*; Freedman and Karsh, *op. cit.*; and Karsh and Rautsi, *op. cit.* For an opposing view, see L. Mylroie (1993), "Why Saddam Hussein Invaded Kuwait," *Orbis*, Vol. 37.

The deterrability of Iraq in 1990 is most difficult to discuss because of the dearth of direct information that has proven so valuable in the other cases. Instead, we need to rely primarily on two well-done analyses of this question.⁶⁰

Both studies interestingly focus on what we would agree is the central question: Saddam Hussein's motivations. Was he an opportunity-driven aggressor or a vulnerable leader motivated by need?⁶¹

Davis and Arquilla describe this as Model One and Model Two. Model One corresponds roughly to the "vulnerable leader motivated by need" and Model Two to the "opportunity- driven aggressor".⁶²

Both studies conclude that Saddam Hussein probably could have been deterred from invading Kuwait, regardless of which motivation dominated his calculations. Stein notes particularly the usefulness of combining deterrence threats with promises of rewards for desirable behavior. In the case of Iraq, that reward would have taken the form of debt relief.

Davis and Arquilla are more specific in their conclusions. They argue that a U.S. preinvasion tripwire force in Kuwait "might well have deterred his (Hussein's) invasion".⁶³ They also emphasize the importance of early action before the adversary has committed himself and before the problem becomes one of compellence rather than deterrence.

60. Davis, P. and J. Arquilla (1991), *Thinking About Opponent Behavior in Crises and Conflict: A Generic Model for Analysis and Group Discussion*. Santa Monica, CA: RAND, N-3322-JS; J. Stein (1992), "Deterrence and Compellence in the Gulf, 1990-91," *International Security*, Vol. 17: pp. 147-79.

61. Stein, *op. cit.*, p. 155.

62. Davis and Arquilla, *op. cit.*, pp. 12-15.

63. Davis and Arquilla, *op. cit.*, p. 69.

- a. How as the regime organized and which institutions and individuals were important sources of political power?

Saddam Hussein's regime was highly personalistic and, as a consequence, was not terribly dependent on the support of semi-autonomous, large, technical institutions. Even in the cases of military and intelligence organizations, Saddam exerted highly personal control much as Hitler attempted to do: by creating a myriad of special directorates and units with over-lapping jurisdictions. Unlike communist party organization, the Baath party did not constitute a parallel government, nor did it hold substantial governing power.

- b. On what basis did the regime's power rest?

Saddam's sources of power were small relative to size of the Iraqi population, but powerful. In a majority Shiite country, Saddam protected Sunni interests and pushed a disproportionate fraction of state revenues to that community. At the same time, he was supported by the secular elements of Iraqi society including the professional, technical, and business communities. Finally, Saddam was fervently supported by the Baathist leadership, and the various elite and specialized units of the military and intelligence apparatus.

- c. In the period prior to the outbreak of external conflict, did the regime believe its hold on political power was weakening so significantly as to require remedial action?

Saddam's regime was significantly weakened by developments in the wake of the Iran-Iraq war. First the war, started by Iraq, had been enormously punishing to Iraqi society with no tangible gain the regime could claim. Second, the war had effectively

bankrupted Iraq, which meant that Saddam's supporters were in danger of losing much or all of the rewards they had become accustomed to receiving in return for their support of the regime.

- d. Did the regime develop an explanation for the crisis, which connected another state's actions or simple existence to the domestic crisis?

The regime explained the post-war situation in terms of the perfidy of the Gulf states, especially Kuwait, which had advanced loans to Iraq to wage war with Iran and were demanding repayment by a bankrupt country. Also, of course, Iraq had long claimed Kuwait as apt of its national territory.

- e. Did the regime develop a plan of action or strategy in which external conflict was expected to ameliorate the domestic crisis?

Clearly the regime felt there existed a nexus between the seizure of Kuwait and rectification of its weakness. Its invasion plans were developed explicitly for that purpose, hence Iraq's decision not to advance into Saudi Arabia when it had the chance. No particular nexus was perceived by the Iraqi leadership linking their problems with Saudi Arabia.

- f. Did the regime undertake an external conflict with that state to ameliorate the domestic crisis?

They did, and the decision probably would have been successful had the U.S. not become galvanized. . . which apparently was a very near run decision.

CHAPTER 4

IMPLICATIONS FOR POLITICAL SCIENCE AND U.S. GRAND STRATEGY

This research suggests strongly, though preliminarily, that regime strength is an important independent variable influencing a regime's propensity for becoming involved in armed interstate conflicts. This has interesting implications for Political Science theory and research. As discussed at length in the literature review, the regime attribute most extensively researched has been regime type. Based on this research, the next step would be to assess the relative weights or influence of these two regime attributes, strength and type. Having not investigated regime strength as an independent variable, and given my hypothesis that democratic regimes tend to be strong, it would not be entirely surprising to find that, to some extent, the *DPprop* researchers have attributed to democracy what may also be a product of regime strength. In that case, depending upon the effect of regime strength as an independent variable, it may make sense to consider a *Strong Regime Peace Proposition (SPprop)*, as well, or even instead of, a *Democratic Peace Proposition*

A *SRprop* offers explanations for some of the puzzles that bedevil the *DPprop*.

For example, as was discussed in the literature review, considerable effort has been made to explain why democracies do not fight other democracies. Normative and structural explanations have been examined with only partial success at best. Another puzzle in the

DProp literature is why democracies seem to become war-prone for the first three years or so of their existence. Similarly, why do U.S. Presidents become more prone during periods of economic downturn, especially close to elections? Yet another puzzle is the recurrent finding that not only do democratic regimes not war against other democratic regimes, but autocratic regimes tend not to war against other autocratic regimes. These phenomena become clear if the variable at work is regime strength. The logic runs as follows:

1. Essentially political regimes seek to remain in power and will use all means open to them to do so. The means open to them are determined by the state's political system, culture, history, norms, etc. of each state.
2. The primary mechanism for staying in power is servicing the constituencies that support a regime. The nature of those constituencies is specific to each regime and state. Servicing constituencies means partially or wholly satisfying their interests. The tools for doing that are domestic and international policies.
3. It follows that strong regimes are the result of successfully meeting the interests of constituents and of assembling new constituents. A record of successful constituent servicing and the gathering of new constituents results in strong regimes for two reasons. First, constituents who have been consistently satisfied are less likely to shift their allegiances, if the regime undertakes a policy that a constituency dislikes. Second, the growth of a regime's constituency buys that regime resilience if components of its constituency defect. This is why Parliamentary regimes can be so weak. It is

specifically why a robust democracy like Israel was scored as having a regime strength in the 40s throughout the 1985–2001 period.

4. Strong regimes can be, by definition, more independent of the wishes of any particular constituency. Obviously a regime has to exercise care in what part of its constituency it chooses to defy and for how long. But there is a direct and powerful relationship between the strength of a regime and its policy independence.
5. Exactly the opposite is true of weak regimes. They are weak precisely because their attachment to the constituencies critical to keeping them in power is fragile. When constituent dissatisfaction can be directly and immediately translated into removal from power, a regime must hew closely to policies agreeable to its constituents. The result is that a weak regime has little policy independence.
6. What do strong regimes do with their policy independence? The answer depends upon the regime, and especially, the top leadership. If the regime is strongly ideological, then the leader is free to pursue the goals dictated by that ideology including interstate war. Hitler's Germany is a good example. If the regime is personalistic, like Stalin's the regime will pursue the policy preferences of the leader, whatever those are.

Interestingly, if a strong regime is invested with the principles espoused proponents of the Realism paradigm, the regime will steer the state in directions that support Realism's predictions. This may well be the reason

that, though the Realism paradigm does have some degree of empirical support, it has fallen short of universalism.

7. Again, the situation of a weak regime is the reverse. Weak regimes are bound to search for policies that will hold their coalitions together internally and that will retain their support of the regime. Therefore, the policy orientation of such regimes must directly reflect that of the constituency.
8. What happens when an already weak regime is threatened with decisive loss of a constituency or when a strong regime enters a period of weakness? The regime will search with urgency for policy steps that will prevent the loss of strength. The range of choices is driven by the specifics of the internal politics of the regime, its constituency, and the nation as a whole. For example, if a regime's constituency is its peasantry and displaced farmers, then the regime shores up its position by scapegoating land owners, expropriating their property, or expelling outright as is Mugabe of Zimbabwe or Idi Amin in Angola.

In some cases, there may be a nexus between the interests of a constituency and a dispute with another state. Usually such disputes will concern the ownership of territory around which the constituency can be rallied, a friendly ethnic group which is allegedly being persecuted by a neighbor, or action by a neighbor deemed to be implicated in the diminishing strength of the regime. In these sorts of cases, the weakening regime may see the initiation of an external violent conflict as a way to halt the loss of strength.

This is why and when weak regimes resort to so-called diversionary wars. Note that this choice of strategy is not restricted to chronically weak states. The reason why

research suggests the U. S. Presidents seem to resort to external violent conflict during economic downturns or as they near elections, is that U. S. regimes weaken at precisely these times. This also is why regimes in transition have an elevated propensity for interstate violence. Regimes in transition are inherently weak and fragile, as are newly revolutionary states.

The thrust of this argument is that the key to understanding a regime's propensity for external violent conflict is by tracking its strength or weakness. Weak regimes tend to be violent regimes.

For the reasons discussed, we would predict that weak regimes would attempt to externalize internal weakness more often than strong ones. Exactly how often they have recourse to this strategy is contingent upon whether a nexus exists between the domestic weakness and some external dispute, such that making dispute violent would be politically helpful. Therefore, without further research described below, I can see no way rigorously to establish a useful statistic for weak regimes' propensity to rely on diversionary war. A regime may be quite willing to take that step, but sees no profit in it if it can find no nexus between the regime's weakness and the possible external violent conflict. So the absence of diversionary wars may say nothing about the propensity to use them. It may simply be due to lack of opportunity.

Obviously, I believe following the leads identified in this research would be fruitful for the political science community. Specifically, I can identify several substantial political science research programs.

First, the comparative influences of regime strength and regime type must be tested in a more demanding and methodologically sophisticated than I was able to muster.

Ideally, that would involve replicating as much of the seminal research on the *DPprop* as possible with the insertion of the additional independent variable, regime strength. To proceed along this course would constitute a most direct and efficient way forward, in part because the authors of the seminal pieces could hardly mount methodological criticisms without danger of self-contradiction.

To move along this path, it would be imperative to develop a measure of regime strength built with data extending back at least 100 years. There exists today no database of that size that can be directly used to measure regime strength, so a regime strength variable would have to be created on data not originally intended for that purpose. The most obvious candidate would be economic data of various sorts. In principle, regime strength should have economic manifestations, which could be used as proxies. As discussed in the literature, early work on regime strength focused on a regime's ability to raise resources, usually through taxation, with which to pursue its policies. Those early attempts to use taxation were not satisfactory, but later work by the World Bank along these lines is much more sophisticated. I suspect that research could serve as the basis for creating a satisfactory regime strength variable.

Second, the link between regime strength and an increased propensity for militarized interstate disputes is suggested by the results reported there. And, examples of the link between regime strength and so-called diversionary wars, a subspecies of militarized interstate disputes, have been provided. A missing link is ability to develop a strong statistical argument linking explicitly domestic events, regime weakness, and entry

into militarized interstate disputes intended to remedy that weakness. To do this with some rigor; a domestic events database would be required. The DON data set used to collect these sorts of data, as did the *World Handbook of Political and Social Indicators*.

Smaller, more recent efforts have collected these data, such as the Easterly-Levine, the International Political Interactions, and the State Failure data sets. All of these suffer from one or more critical shortcomings: currency, partial coverage of states, and, especially, the categories of data collected.

As discussed earlier, the domestic causes of regime weakness and, in turn, the domestic manifestations of that weakness have to be informed by theory, in particular, theory explaining the sources of strength and weakness in democracies and autocracies. The indicators of democratic and autocratic strength and weakness are likely to be quite different. In democracies, weakness might be manifested by public opinion polls or the inability of the president or prime minister to obtain legislative support. But, in an autocracy, weakness can be manifested by subtle and hard-to-detect events, such as arcane bureaucratic changes and the like. Of course, these can be more overt indicators, too, such as party purges, cabinet changes, disappearances, trials, and the like. Even with these, secrecy is an ever-present difficulty.

The point is that assembling a useful domestic events database would assuredly be a substantial, difficult, long-term undertaking. Given the increasing acceptance of the intimate connection between domestic and international behavior, however, I think the logic for developing such a data set is fully as compelling as that for the COW data set.

Third, little systematic study exists on what makes for strong regimes. This is due to the neglect of regime strength as an important attribute for study and because regime

strength has been viewed as synonymous with democracy. I hope this study has illustrated that both of those beliefs are erroneous. The starting place for an investigation of the requirements for strong political regimes would be two hypotheses:

1. Most democracies have strong regimes and
2. Some autocracies have strong regimes.

It follows that strong democracies and autocracies ought to have traits in common. Coming to grips with what those might be would be an important research program for the comparative politics community with strong practical implications. As I discuss below, it may be more sensible and feasible for U.S. strategy to be focused more generally on the proliferation of strong regimes and less specifically on democracies. It follows that a good understanding of what makes for strong regimes is the crucial pre-requisite for implementing such a U.S. strategy.

This research also has important implications for international relations as practiced by the United States.

First, a priority for U.S. national strategy is the spread of both democratic regimes and free market capitalism. The underlying assumptions of this strategy parallel the findings claimed by proponents of the *DPprop*—though it is not clear that U.S. policymakers have been directly influenced by that literature, President Clinton’s mention of it notwithstanding. These assumptions are, as follows:

1. Democracies do not fight other democracies, and
2. Democracies are, in general, more pacific than nondemocracies.

I believe this research suggests that these assumptions may importantly incomplete and require qualification. Regime strength seems an importantly weighty

determinant of the behavior of states. Given the DPprop research on the behavior of U.S. presidents at times of political weakness, this is likely to be true of both developed and developing states, democratic or autocratic . Put another way, regime-type is only one of two powerful domestic variables, strongly influencing the international behavior of states.

That finding means that U.S. national security strategy should stress not only the spread of democracy, but also the spread of strong regimes. I think it is reasonable to hypothesize that certain characteristics of democracy conduce in direction of regime strength, among these being public accountability and predictable, transparent, and relatively open mechanisms for replacing one regime with another. But strength and democracy, while correlated, are not identical. There are strong autocracies and weak democracies. Strong democracies tend to act more like strong autocracies than like weak democracies.

It follows that U.S. strategy ought to be made more complex and nuanced. The simple establishment of a democratic political system per se does assure that the behavior of that regime, if weak, will be benign towards its neighbors or the United States. Democracy certainly has never assured the benign behavior of the U. S. where its serving regime has felt itself to be weak, such as where elections approach during a period of economic distress. Nor is it the case that an autocratic regime, if strong, will pursue policies inimical to the U.S. and its neighbors. Taking this line of reasoning a step further, the rapid proliferation of weak democracies in the developing world can easily result in a notably unstable and conflictual international security environment, presumably the opposite of what the U.S. seeks to accomplish.

This leads to the fascinating question of what produces strong regimes, whatever the political system in which they happen to be embedded. There is no direct

literature on this subject that I have been able to discover, but history is replete with tantalizing hints. For most of the history of humane civilization, regimes have been autocratic. In our current period, we would characterize all those regimes as entirely unsatisfactory, certainly as far less evolved than democratic regimes. Yet, historians and individuals at the time describe many regimes, certain monarchs, for example, as “good” regimes. “Good” here denotes regimes deemed effective by the governed at the time and by historians retrospectively at providing their populations what was expected of them: internal and external security, a satisfactory justice system, and an economy permitting individuals and families some basic level of subsistence. Certain pharaohs accomplished this, assuredly, did some Roman emperors, as well as many rulers through the 19th century. Today, strong autocracies are successful when they provide the same basic requirements. The autocratic regimes that exist entirely by coercion are characterized as weak both by the World Bank indices of effective governance and by the ICRG scale.

The explanation for the success of these autocratic regimes shares much with that of successful democratic regimes: a strong regime of any type is able to provide its governed a level of security, justice, and prosperity that is acceptable to them. That assuredly does not mean that state coercion is not an important tool for all autocratic regimes, strong or weak. It also does not mean that strong autocracies provide to the governed the levels of justice and prosperity that they *could* provide, if popular welfare were their primary concern. It generally is not. But it does mean that the “social contracts” offered by strong autocracies tend to be acceptable (and not necessarily more than that) to the bulk of their population.

Democracies usually do better than a basic level of acceptability, which is why the bulk of strong states are democratic. But democracy, *per se*, does not lead inevitably to the sort of acceptable social contract that characterized strong regimes. Indeed, weak democracies such as Pakistan, cycle between democracy and autocracy for exactly that reason. The danger of this destructive cycling exists with many Latin American and African states, such as Venezuela and Senegal. That danger does not exist in the case of Singapore.

So, to return to the question of U.S. strategy, if offered the choice, would the U.S. prefer that Singapore be a weak democracy or the strong autocracy that it is? The answer is obvious regardless of the importance placed on spreading democracy. By the same token would the U.S. prefer that weak democracies like Venezuela, Senegal, South Africa, occasionally Pakistan, Argentina, Colombia, Guinea-Bissau, India, Macedonia, Niger, Nigeria, Paraguay, Sri Lanka and others not be transformed into Singapore-like states, thereby trading regime strength for the currently preferred regime type? Again I believe the answer is obvious—or it should be. Finally, would the international security environment be made more stable and less conflictual, if these weak democracies were to become strong, economically effective autocracies, like Singapore or perhaps Malaysia, Tunisia, or Morocco in the developing world? Practical judgment, as well as the literature on the international behavior of states transitioning to democracy, suggest that the answer is “yes.”

Therefore, the question for the United States is not how best to institute democracies across the globe, but rather how to institute strong regimes across the globe, which may or may not be democracies. In truth, little is known in any

systematic way about how to undertake such a project. I suspect that the key is less the theory of regime creation and more the ability to identify individuals who can lead or participate in autocratic regimes while retaining a sense of moderation and a genuine concern for the welfare of the population. One should note that we are not any better informed as to how to create democracies. Certainly we know how to stage certain processes associated with democracies like elections. But, of course, creating democratic institutions like a genuinely free and motivated press, a truly independent judiciary, and a representative assembly that works by compromise is a far more daunting task. Indeed, debate still exist as to how and why *we* were able to develop these things for ourselves.

Shifting our sights from creating democracies to creating strong regimes brings with it an important advantage over our current policies: We will be able to escape the trap of trying (or appearing to try) to create little Americas every where regardless of history, culture, norms, social structure, and the like. We thereby finesse the reflexive “push back: of many societies who understandably come to believe that we seek to “steal their souls.” Strong regimes, as a concept, are not culture-bound or alien to any society or culture, although the specifics for each certainly must be driven by the particular and unique local conditions that obtain.

In so doing, U.S. policy acquires flexibility and sophistication that it does not currently enjoy. We cease to have to endlessly and ineffectually defend ourselves against the charges of political imperialism, arrogance, insensitivity and a certain rudeness. Instead, we can adapt as the local conditions demand. We also can defuse the problem of explaining to ourselves and to the world why we espouse democracy, on the one hand,

and support nondemocracies. That apparent inconsistencies disappear when we reorient the issue away from regime type and towards regime strength.

This is all well and good, but we are still left with the problem of developing a strategy for limiting the threats weak regimes pose to international stability and peace. There are many facets of such a strategy, because weak regimes are dangerous in several different ways. They are highly likely to be involved in internal war, which brings with it all of the problems of genocide, starvation, mass migrations to mention just a few. Weak regimes, as I hope to have demonstrated, also have an elevated propensity to externalize its internal weaknesses by resorting to interstate war of various sorts, including international guerrilla war, otherwise known as terrorism.

Now I wish to turn to the subject that introduced this work: the role of deterrence in U.S. post–Cold War national security strategy.

Thus far, the discussion has illustrated the vulnerability of certain regime-types common to the Third World to domestic pressures to externalize internal conflicts and the ways those pressures can result in acceptance of high risks. By definition, a willingness to accept risks suggests that deterrence of such regimes is difficult. Indeed, it proved so in the cases cited here.

The next question is whether such cases are deterrable or whether states in this predicament fit the description of “non-deterrable” or “crazy state.” The first step toward addressing this question is to review some of the basic issues at play in deterrence.

Broadly defined, deterrence involves dissuading a leader, group, or state from acting against another’s interests by threatening to impose some sanction or cost. At this level, deterrence applies equally well to individual behavior and to the behavior between

states. Deterrence, thus defined, is part of a larger set of strategies for influencing a country's behavior. In general, one can dissuade an opponent from acting against one's interests by offering rewards or inducements if the opponent acts according to one's wishes, or by threatening sanctions or retaliation if the opponent does not. The latter is the domain of deterrence. The actions one wants to discourage can range from the acquisition of particular weapons to overt military attacks. Obviously this research focuses on the restricted set of proscribed actions in which a hostile regional power threatens to use military force against a U.S. ally or regional interest.

The findings of prospect theory are pertinent here, even though directly importing them into national security decision-making is not straight-forward. The evidence is very strong that decision-makers accept greater risks to prevent losses than to achieve gains. It seems to me suggestive that so many of the cases discussed here seem to involve analogous situations where the loss the decision-maker fears is domestic political power. For this reason, I include a discussion of prospect theory here.¹

Prospect theory explicitly accounts for the fact that decision makers appear to weigh losses more heavily than gains in ways not accounted for by comparing the apparent utilities of alternatives.² It is as though the concept of "loss" has a different psychological connotation than the concept of "gain," independent of the utilities

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1. The application of prospect theory to conventional deterrence has also been discussed in Davis, P. and J. Arquilla (1991), *Thinking About Opponent Behavior in Crisis and Conflict: A Generic Model for Analysis and Group Discussion*. Santa Monica, CA: RAND, N-3322-JS.
 2. Risk aversion in expected utility theory is represented by a concave utility function (i.e., one exhibiting diminishing utility with higher gains). Risk-seeking behavior, on the other hand, is represented in prospect theory by a convex utility function (i.e., one exhibiting diminishing utility for larger losses, where the utility domain has now been divided into gains and losses). When facing a choice between a certain loss and a gamble on a larger loss (where the expected loss is equal in both cases), utility is maximized by taking the gamble.

involved. The result is that, all else being equal, decision makers usually accept greater risks to avert a loss than to achieve a gain, even when the expected utilities of the choices would predict the opposite.³ Indeed, decision makers will accept particularly high risks to avert a serious or irremediable loss. This idea is captured by the phrase “the strategic costs of inaction,” i.e., the costs of accepting prospective losses may be too high for leaders not to act. The distinction between these two occasions for risky decisions—opportunity to gain versus aversion of loss—helps one understand why deterrence can be difficult.

Thus, leaders facing losses can be expected to choose a course of action that runs the risk of greater losses so long as this choice contains the possibility of averting the loss. How much of a gamble depends on the risk-taking propensities of individual leaders. This risk-taking propensity is particularly acute if the decision maker seeking to avert the loss, weakening political power, for example, already has a marginally acceptable status quo and anticipates that the impending loss will make the status quo unacceptable. Indeed, at the limit, such decision makers become literally nondeterrable if they believe they have nothing to lose by acting.⁴ By the same token, decision makers seeking to improve on a status quo that is already satisfactory are easiest to deter, because they are least inclined to take risks that might jeopardize their agreeable situation.

3. A risk-averse decision maker is one who prefers a gain with certainty to a gamble of achieving the same expected gain or perhaps even a slightly higher expected gain. A risk-prone decision maker is just the opposite: He prefers a gamble to a certain outcome. The reader can easily demonstrate risk aversion for gains by asking whether one would rather receive \$100 for certain or gamble on a 50-50 chance of receiving \$210 or nothing. Most people prefer the certain outcome of \$100 over the bet, even though the bet has a slightly higher expected return, i.e., \$105. On the other hand, when facing losses, most people prefer a 50-50 gamble on losing \$210 to the certain loss of \$100.

4. The condemned convict awaiting execution is an appropriate example. If he is convinced that no pardon will save him, there is no sanction that will deter him from taking any risk. What threat is more fearsome and certain than the one that awaits him?

Although this discussion is based on the crucial distinction between two seemingly dichotomous types of risk-taking situations—averting loss and seeking gain—I do not mean to imply that any decision is entirely one or the other. Rather, all decisions involve mixes of the two. The more the desire to avert loss dominates the mix, the greater the propensity for risk-taking and the harder that leader or state is to deter, other things being equal. The more the desire for gain dominates, the greater the propensity for risk avoidance and the easier it is to deter that type of leader.

Many weak regimes find themselves consistently confronted with averting loss, balancing the risks of war with risks of loss of power. Given the premium political regimes place on retaining power, it follows that deterrence will be difficult in many circumstances. But, it is probably safe to say that true nondeterrability is likely to be quite rare at any time, although North Korea and Cuba may become the latest candidates for nondeterrability status. Thus far, both have behaved as though each has something left to lose. For example, North Korea has not yet decided that its future is so grim that “rolling the dice” on a military option is preferable. Again, the next few months are likely to show whether this is because the North truly does have something left to lose or merely because its preparations are not complete.

The related notion of so-called crazy states should be mentioned here in connection with nondeterrability (see Dror, 1980).⁵ In principle, a state may be nondeterrable because it is too irrational to be sensitive to a deterrent threat. Since deterrent threats often are rather crude, this degree of irrationality has to be pronounced. Based on the cases we examined, we found very few, if any, clear

5. Dror, Y. (1980), *Crazy States*. NY: Kraus.

examples of leaderships irrational to this degree. Many leaderships of regional states can be characterized as paranoid or perhaps sociopathic, but very few can be characterized as disabled by psychopathology. The process of competing for and holding power has much in common with rivalry between states, so the domestic political process may weed out individuals who are highly incompetent in domestic and international politics. Idi Amin of Uganda and Emperor Bokassa of the Central African Republic may be exceptions; although even their respective disabilities became pronounced only after they had held power for a time. Certainly, many highly unpleasant individuals have held power in Third World states, but few could be called crazy in the sense of being so irrational as to be nondeterrable.

Let us turn now to the specific issue of U.S. deterrence strategy against weak regimes. In most cases, these regimes will challenge us, deliberately or inadvertently, in periods of increasing weakness. The sequence of actions leading a regime to confront the U.S. will be undertaken in the belief that needed political strengthening will result.

Deterrence is likely to be most feasible when one or both of the following conditions obtain:

1. When the weakness of the regime, though serious, is not literally "regime life threatening," or when the regime's leadership can identify another, less risky way to shore up its position.
2. When a regime's leadership is sufficiently patriotic as to be averse to risking the general welfare of the nation for the sake of political survival.

When one or both of these conditions obtain, the cases suggest that successful deterrence can be possible, if the adversary can have been persuaded to a high degree of

certainty that he could not achieve his military objectives and that efforts to do so would leave him in a dangerously poorer *status quo*. The primary criteria that these regimes used in determining the likelihood of their success were *military*: Could the objectives be achieved against the military defenses likely to be encountered? If the answer was thought to be “no” to that question, the evidence supports the view that the attackers would not have initiated the conflicts. In spite of the strength of the domestic needs—the costs of inaction—none of the regimes we examined was prepared to commit national suicide. They all had a theory of victory by which their military forces would succeed, and in no case were those theories irrational or crazy—although they involved high risks.

The implication of this reasoning is that such adversaries are deterrable by military measures that convincingly invalidate the adversary’s theory of victory. Obviously, deterrence in these instances is narrow and limited. The hostile intentions of the adversary are left unchanged. Only the specific military action has been deterred, leaving the situation unchanged and the adversary likely to try again tomorrow. However, this sort of deterrence success is no small thing and, in any case, is all that can be reasonably expected of deterrence. Deterrence, in a sense, is a superficial policy, for it cannot affect the roots of the problem; it can only stifle it. Nevertheless, in a world of flawed and crude instruments, that may be the best one can do.

But deterrence is increasingly difficult to the extent that neither of the conditions are met. At the limit, when regime disintegration is virtually certain (in the view of the leadership) if no action is taken and that no alternative exists but embarking on a course of action that endangers U.S. interests, and the regime is indifferent to the welfare of its nation and population, then deterrence will prove impossible for all practical purposes.

What of the situation that is not quite so dire as this? Logically, deterrence ought to be possible, if difficult. The deterring threat needs to be directed with high credibility at precisely what the regime most values, its own survival in power. Contrast this sort of threat with the more customary ones aimed at damaging the assets of the nation. These are likely to be worse than useless; indeed they may be an incentive to a weakening regime. to challenge the U.S.

What sort of threat, then, can credibly and reliably bring about the destruction of a regime's hold on power? This is remarkably unclear, as our current situation in Iraq powerfully illustrates. Presumably the capability to threaten precisely the physical survival of the regime's leadership and prominent constituents has a role, but, again as Iraq currently illustrates, accomplishing this objective without inflicting unacceptable harm on an innocent population is not something we know how to do very well, yet.

The sad and unfortunate fact is that we do not possess the deep and reliable theoretical and concrete knowledge of how weak regimes function, their sources of political power, and how to break that link. This problem is pronounced both in the U.S. intelligence community and in the U.S. political science community. Realistically, I do not believe this situation will change for a long time.

So, in the end, what can be said about post-Cold War deterrence. First, it may well prove to be the case that the critical tools to deterring weak regimes are partially or entirely political and financial, as opposed to military. Threatening to seize personal assets, for example, may be far more harrowing to these regimes than high explosives.

Second, no doubt there will occur individual cases of regimes about which we possess especially intimate knowledge. On these occasions, we may be able to

discern what is required for deterrence. But, in most cases, we will lack this level of information. Therefore, we can anticipate that successful deterrence will be difficult and episodic. Militarily defeating these adversaries will, in most cases, be less difficult and more reliable. So we can expect that, unlike the Cold War, the immediate future will be characterized more by use of military forces and less by the *threat* of their use.

Finally, in my view, the most dangerous regimes are, unfortunately, going to be the most difficult to deter. These are the regimes that believe themselves to be in situations resembling that facing the Japanese in 1941: either embark on a course of action with *almost* certain disastrous consequences or do not so embark and face the *certainty* of disastrous consequences. In these cases, the only policy that might prove successful at dissuading an adversary is the opposite of deterrence: a credible promise to *strengthen* the regime. Obviously this course of action brings with it another set of risks, the high risk of domestic political resistance to what will be viewed as appeasement and moral distaste at helping unsavory regimes.

So, in sum, I believe we face three when confronted with weakening regimes in this category:

1. Attempt to deter with poor prospects of success, or
2. Attempt to strengthen an adversary regime, and suffer domestic political damage, or
3. Resign ourselves to having to use military force, usually at a high short- and long-term cost.

Fortunately, as the cases are meant to illustrate, most weakening regimes tempted by a risky course of action are deterrable. But, in my view, for the reasons identified in this research, that will still leave a considerable minority that cannot be.

APPENDIX A

THE JAPANESE DECISION TO ATTACK PEARL HARBOR

The Japanese decision to enter into war with the United States and Great Britain represents a classic example of the sort of decision making driven by assessments of the costs of inaction. It is a profitable case to study not only because it is a striking example of risk assessment under extreme conditions, but also because it illustrates well some of the factors at play (in a less extreme way) in the decision making of more commonly encountered regional adversaries.¹

In the Japanese government of the time, decisions as to whether or not to go to war were the formal province of the cabinet. However, the bulk of the deliberations prior to cabinet considerations was conducted in the Liaison Conferences attended by a committee of high military and civilian government leaders. A somewhat expanded National Security Council is the best U.S. analogy to this Japanese organization. The Liaison Conference sessions were attended by the Prime Minister, Foreign Minister, Navy Minister, War Minister, the Chiefs of Staff of the Army and Navy, their Deputy Chiefs, and assorted high-level civilians representing economic ministries. This committee met several times during the latter half of 1941 and reached the decision to take Japan into war. The accounts of these meetings are available virtually in stenographic form, and they provide a remarkable look into the analytical processes employed by the participants.² As always, these processes were a mixture of clarity and

1. This discussion of the Japanese decisionmaking relies generally on several sources. See Wohlstetter, R. (1962), *Pearl Harbor: Warning and Decision*. Stanford: Stanford University Press, 1982; L. Mortaon (1954), "The Japanese Decision for War," *U.S. Naval Institute Proceedings*, Vol. 80: pp. 1325–1337; S. Sagan (1988), "The Origins of the Pacific War," *Journal of Interdisciplinary History*, Spring; J. Toland (1970), *The Rising Sun*, New York: Random House; Y. Maxon (1957), *Control of Japanese Foreign Policy: A Study of Civil-Military Rivalry, 1930–1945*. Berkeley: University of California; *Hearings Before the Joint Committee on the Investigation of the Pearl Harbor Attack*. 79th Congress, Washington, DC: U.S. Government Printing Office, 1946; N. Ike (ed.) (1967), *Japan's Decision for War: Records of the 1941 Policy Conferences*. Stanford: Stanford University Press.

2. Ike, N., *op. cit.*

chaos, detachment and manic passion. But, in our view, what comes through clearly was the sentiment that the almost certain costs of inaction for Japan exceeded the uncertain, although potentially very high, costs of action. For the most part, the participants understood Japan's weaknesses and the implications of U.S. strengths. The overall level of self-delusion and impetuosity, while undoubtedly present, seems low. Rather, even the most pessimistic Japanese officials seem to have believed that the current and prospective status quo for Japan was inconsistent with her most vital objectives—membership among the Great Powers with an established sphere of interest—and that the only course of action open to Japan with any chance of success (even if low) was war.

On July 2, 1941, the Japanese cabinet endorsed the view developed in the Liaison Conferences that Japan had to develop the Greater Asia Co-Prosperity Sphere to include all British, Dutch, French, and Portuguese possessions in the Far East, as well as the Philippines, India, and Australia. Although leaving open the prospect of achieving these objectives by negotiation, the minutes of the meeting are explicit that the Co-Prosperity Sphere was to be pursued “no matter what obstacles may be encountered” and “no matter what international developments take place.”³ Indeed, the decision memorandum refers explicitly to the necessity of an “advance into the Southern Regions.” The Japanese decision makers hoped that the United States could be kept out of this war, but they accepted the necessity of planning for the disappointment of those hopes.

The Liaison Conference met again on October 23 to consider whether or not the United States could be kept out of a war while Japan pursued the Co-Prosperity Sphere. The conferees agreed that there was no chance that the United States would accept

3. Quoted in Wohlstetter, R. *op. cit.*, p. 346.

Japanese objectives in the Far East and drop its economic sanctions. They further agreed that compliance with U.S. terms to resolve the sanctions crisis would mean, “Japan would be compelled ultimately to withdraw entirely from the (Asian) continent.” At the same time, the conference concluded that it was not possible for Japan to fight the United States separately, that U.S. war potential was seven or eight times that of Japan, and that “there were no means of directly vanquishing the United States in case of war against her.”⁴ Yet, this conference reached a unanimous or, at least, majority decision to go to war with the United States if negotiations were unavailing—as all expected them to be. Why was this decision made?

The military members of the Liaison Conference were quite optimistic that Japan could achieve major successes in the Pacific against the United States and Britain in the first six to twelve months of war. However, they were equally pessimistic about Japanese chances if the war continued beyond that point. This assessment was entirely realistic. On the other hand, they believed that continuation of diplomatic activities was foolish, since they were unlikely to bear fruit and would erode even Japan’s short-term advantages as U.S. war preparedness accelerated. Therefore, as Navy Minister Shimada concluded, “though there is a great risk in beginning the war now, we must realize that there is also great risk in depending on negotiations unless we can be certain of the final outcome.”⁵

Ultimately, the Japanese chose war in the hopes that, in some way, it could be kept short. But, as they were aware, they had no plan to ensure that it would be short, other than to hope that the United States would elect to cut its losses in an area of the

4. Togo, S. *The Cause of Japan*. New York: Simon and Schuster: pp. 125–127.

5. Quoted in Wohlstetter, R., *op. cit.*, p. 351.

world of less interest than Europe. In other words, the Japanese could not deprive the United States of the freedom of action or the means to continue the conflict. The Japanese chose this course—which U.S. planners discounted as grossly irrational—because all other courses seemed worse.⁶

Indeed, as Roberta Wohlstetter has pointed out, the very notion of “choice” is slippery in such situations.⁷ The Japanese felt compelled to go to war; they did not see themselves as free *not* to do so. This notion of compulsion occurs frequently in the assessments of states in similar circumstances and illustrates the difficulty of even less extreme, more common international situations. Inherent in the concept of deterrence is that the adversary has some freedom to refrain from the undesirable behavior. If he thinks he has none, deterrence must fail. If he thinks he has at least some freedom to refrain (as most states do), deterrence is possible even if difficult.

A final point is in order about the problem these situations pose to U.S. intelligence analysts. The assessments of U.S. planners about the consequences of war with Japan were identical to those of the Japanese. The U.S. estimate found the disparities in national power to be so great that “national sanity would dictate against such an event” (referring to war).⁸ But this U.S. conclusion omitted an assessment of the status quo and its future prospects as viewed by the Japanese and provides a good illustration of how frequently such considerations are dropped from deterrence

6. Quoted in Sagan, S., *op. cit.*, pp. 894, 904.

7. Wohlstetter, R., *op. cit.*, p. 357.

8. Hearings Before the Joint Committee on the Investigation of the Pearl Harbor Attack (1946), Part 14, p. 1056.

calculations. The United States never weighed seriously the Japanese view that the alternative to war was “gradual exhaustion” without ever having struck a single blow.⁹

Interestingly, as desperate as the Japanese believed their situation to be, they clearly were deterrable, at least in a limited military sense. Admiral Nagumo, the commander of the Japanese naval force sent to attack Pearl Harbor, was under orders to abort the operation if his approach was detected. The Japanese planners believed that surprise was essential if the attack was to have any prospect for success. Therefore, the United States could have deterred the Japanese by taking steps that suggested to them that surprise had been lost.

North Korea and Cuba are current candidates for nondeterrable status. Both are facing prospects that appear catastrophic for the survival of the existing political regimes. The forces pushing in this direction seem irresistible if left unremediated. Remediation, in these cases, would require some benign, external intervention to shore up these regimes in much the way the Soviets did. This seems unlikely, short of coercion by the failing regime. So far as we know, Cuba has no coercive means sufficient to this task. Humanitarian intervention can be expected in Cuba, but nowhere near the magnitude needed to preserve the Castro regime. North Korea, on the other hand, has managed to create better prospects by skillfully using its putative nuclear capability to extort economic and diplomatic benefits from Japan, South Korea, and the United States. Whether it can extort enough to make a difference is an open question.

9. Wohlstetter, R., *op. cit.*, p. 354.

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