EVOLUTIONARY PROCESSES IN WORLD POLITICS

Puzzles of the Democratic Peace

Theory, Geopolitics, and the Transformation of World Politics

Karen Rasler and William R. Thompson

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First published in 2005 by
PALGRAVE MACMILLAN™
175 Fifth Avenue, New York, N.Y. 10010 and
Houndmills, Basingstoke, Hampshire, England RG21 6XS
Companies and representatives throughout the world.

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Library of Congress Cataloging-in-Publication Data

Rasler, Karen A., 1952–
Puzzles of the democratic peace : theory, geopolitics, and the transformation of world politics / Karen Rasler and William R. Thompson.
p.cm.—(Evolutionary processes in world politics)
Includes bibliographical references and index.
ISBN 1–4039–6823–3 (hc)—ISBN 1–4039–6824–1 (pb)
1. Peace. 2. Democratization. 3. Democracy. 4. International relations. 5. World politics—1989– I. Thompson, William R. II. Title. III. Series.

JZ5538.R37 2005
327.1'72—dc22 2004054199

A catalogue record for this book is available from the British Library.

Design by Newgen Imaging Systems (P) Ltd., Chennai, India.

First edition: March 2005

10 9 8 7 6 5 4 3 2 1

Printed in the United States of America.

For our parents

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ACKNOWLEDGMENTS

Various parts of this book have seen the light of day before but often in different form. A portion of chapter one was published as "Democracy and Civil Society as Constraints on Major Power Warfare," in *The Waning of Major Power War*, ed. Raimo Vayrynen (London: Frank Cass, 2004). A shorter version of chapter two first appeared as "The Democratic Peace and the Sequential, Reciprocal, Causal Arrow Hypothesis." *Comparative Political Studies* (2004) 37: 879–908. An earlier version of chapter three is "Democracy and Peace: Putting the Cart Before the Horse?" *International Organization* (1996) 50: 141–74. The much revised ancestor of chapter four is "Intermittent Republics and Democratic Peace Puzzles." *Review of International Studies* (1997) 23: 93–114. Chapter five first appeared as "Rivalries and the Democratic Peace in the Major Power Subsystem." *Journal of Peace Research* (2001) 38: 657–83. Chapter seven was first published as "The Monadic Democratic Peace Puzzle and an 'End of History' Partial Solution?" *International Politics* (2003) 40: 5–27. A version of chapter eight came out as "Structural Change and Democratization in the Major Power Subsystem." *International Politics* (2003) 40: 465–91. Parts of chapters one and two were presented at colloquia held in the Departments of Political Science at the University of Colorado, Boulder and the University of Nebraska, Lincoln, respectively. Our thanks to Michael Ward and David Rapkin, equally respectively, for arranging those presentations. As noted above, we have benefited from the journal editorial services of Jim Caporaso, Scott Gates, Laura Neack, John Odell, Paul Taylor, and Raymo Vayrynen, as well as a plague of anonymous reviewers over the past few years. We also have benefited from comments on individual chapters at various stages from Michael Colaresi, Jeff Isaac, George Modelski, Jeffrey Pickering, David Rapkin, James Lee Ray, Bruce Russett, and Richard Tucker. Regrettably, we cannot blame them for any of the faults of our argument. We also greatly appreciate David Pervin's assistance in bringing this book project to fruition.

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PART ONE

Introduction

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CHAPTER ONE

Developing Perspective on Democratic Peace Phenomena

The study of international relations is currently undergoing one of its periodic bouts of greater-than-usual intellectual turmoil. The world out there seems to be changing on us in ways that are not easy to fathom. Stripped of our Cold War anchor and other things once thought to be immutable (such as the U.S.-Soviet Cold War, South African apartheid, the relative absence of foreign terrorism in the United States, and the improbability of a Palestinian state), it is not always clear how we should interpret current events or from what perspective.

A case in point is the finding that democracies do not fight other democracies that has come to be regarded as a law-like cornerstone of knowledge about international politics.[1] Findings are one thing, explanations of this "law" represent something else entirely. There are at least three problems with explanations of the democratic peace. One is that there is absolutely nothing resembling a consensus on why democracy or, more accurately, joint democracy should be capable of pacifying world politics however selectively. There is substantial agreement on the empirical relationship between joint democracy and a decreased probability of war. Yet there does not appear to be any substantial movement toward adopting one of many arguments for why this should be the case. On the contrary, the number of explanations has proliferated. As a consequence, we have either a plethora of explanations or one rather ambiguous explanation that it seems to have something to do with democracy.

A second problem is that changes in regime type represent a phenomenon that requires explanation in its own right. Democracies have been slow to emerge. They have hardly been the outcomes of immaculate conception. How can we be sure that whatever helped produce democracies is not also responsible for the observed democratic peace? Rather than assuming that democratization is responsible for something we choose to call the democratic peace, perhaps we should be looking for broader explanations that are capable of accounting for democratization and the democratic peace.

Finally, explanations focusing on democracy suffer some potential threat of "presentism." We sense behavioral transformation and look around for something relatively new to account for it. Democratization certainly fits the novelty requirement. But perhaps the contemporary transformation in selected peaceful relations, if that is what it is, has older roots. Perhaps we have seen similar transformations in the past and simply do not recognize the more general factors at work, in the past and now.

Rather than fight pitched battles over paradigmatic ideologies or even pursue different slants on democratic virtues, our suggestion is that we may be barking up the wrong analytical tree in assuming that interactive democracies are primarily responsible for generating a peace we choose to call democratic. Zones of relatively peaceful relations do appear to be emerging. But we need to take a step back and contemplate where democracy may fit in to a larger canvas encompassing the possibility of multiple paths to less conflict-prone dyads.

A quick way of summarizing the problem is suggested by table 1.1. Contemporary warfare tends to be concentrated where democratic political systems are most scarce. Most of the nineteenth-century warfare took place in Europe and Latin America prior to the emergence of democratization processes. By the latter half of the twentieth century, most warfare had shifted to the right-hand side of the table (Africa, Asia, and the Middle East) while the concentration of democracies is clearly manifested on the left-hand side (North America, western Europe, and some parts of Latin America) of the table.

But is this not precisely what democratic peace explanations are designed to explain? War is most rare where democracies are least scarce. Given the historical pattern of democratization and warfare, it is entirely conceivable that these two processes are so closely intertwined that more democratization implies less warfare—at least between democracies. Yet it is also possible that other things are going on, in addition to democratization, that are critical to the outcomes we are observing currently. The association between

Table 1.1 The spatio-temporal distribution of democracies and war

Year	*North America*	*West Europe*	*Latin America*	*East Europe*	*Middle East*	*East Asia*	*Africa*	*Oceania*
1816–34	1/0	0/1	0/0	0/0	0/1	0/0	0/0	
1835–54	1/1	0/3	0/1	0/0	0/1	0/0	0/0	
1855–74	1/0	2/6	0/4	0/0	0/1	0/0	0/0	
1875–94	1/0	3/0	2/2	0/0	0/1	0/2	0/0	
1895–1919	2/0	6/1	1/3	0/4	0/0	0/3	0/0	
1920–34	2/0	15/0	1/1	4/0	2/0	0/2	0/0	2/0
1935–49	2/0	14/1	1/0	1/1	2/1	0/5	0/1	2/1
1950–69	2/0	13/0	1/1	0/1	2/2	3/3	0/0	2/0
1970–88	2/0	15/0	7/1	0/0	3/4	4/2	2/2	3/0
1989–2000	2/0	18/0	18/1	3/0	3/1	3/0	5/1	3/0

Note: The number of democracies are listed to the left while the number of interstate wars are listed to the right within each historical interval.

regime types may be entirely spurious. Alternatively, it may be that the association is quite genuine but that we are giving far too much credit to democracy in bringing about a selective transformation of international relations.

After a fairly cursory introduction to some of the problems associated with minimal and maximal democratic peace explanations, the focus of this chapter shifts to a review of four earlier efforts [those of Quincy Wright, Karl Deutsch and colleagues, Nazli Choucri and Robert North, and Richard Rosecrance] to account for the evolution of more peaceful relations. The point of such a review is neither nostalgia nor a felt need for a longer literature review. The study of international relations is very much like technological development. At any given point in time, multiple technological solutions to some economic problem are apt to exist. For some reason, one technological solution, as in the case of the QWERTY keyboard, wins out over others and it is not always because the winner represents the superior technological solution. Democratic norms/institutions' explanations of the "democratic peace" appear to be winning on the basis of default and the assumption that the "peace" must be a democratic one. Alternative explanations from the International Relations (IR) analytical technology shelf suggest that this equation may be both too simple and too narrow. If what is going on in world politics is broader than the democratization process, we need to develop equally broader perspectives. A highly selective review of work that emerged prior to the discovery of democratic peace phenomena provides a foundation for the suggestion that we need to give more attention to alternative pathways encompassing such topics as peaceful niches, rivalries, the eclipse of bellicose aggressors, and systemic leadership.

All of these topics, in addition to regime type, are pursued at greater length in subsequent chapters. Yet it is not enough to simply insist on multiple routes to pacific interactions. An integrated framework is also needed. We develop this framework toward the end of this chapter. Chapters two through nine then proceed to test various aspects of the argument that reduces to the idea that the world system has evolved to the point where coercive strategies are becoming more costly while more cooperative strategies are becoming less costly—at least for some states. Explaining why this might be the case needs to be put off for the moment while we first establish a context within which the coercive–cooperative argument can be best advanced. Part of the context requires a brief overview of the various ways in which the "democratic peace" might be explained—a subject to which we now turn.

Minimal Versus Maximal Approaches to the Democratic Peace

The approaches that might be pursued in specifying "democratic peace" constraints can be reduced to two generic categories. The minimalist

approach involves taking the "democratic peace" term at its face value. Democratization of political regimes leads to reduced conflict at least among democracies even if we are not quite sure why. The maximalist approach involves the consideration of some ten complexes of variables, all ostensibly intertwined, that currently appear to be linked to the prospects of greater peace and security. The ten complexes to be discussed are labeled for present purposes as community norm building, civil society, electoral punishment, transparency/signaling, economic growth and development, economic interdependence, external threat, external status quo satisfaction, external institutional support, and systemic leadership. No single complex is viewed as sufficient to explaining the "democratic peace." Nor are any of them mutually exclusive. The problem is the opposite. They all seem so excessively intertwined that analytical disaggregation is difficult.

One of the main democratic peace explanations involves the construction of interstate communities predicated on shared values, institutions, and culture.[2] Democracies are said to be characterized by norms of nonviolence in political contestation. Electoral losers surrender gracefully knowing that they can always renew the contest in the next election. Electoral winners refrain from persecuting the defeated side knowing that they may not be victorious the next time around. As a consequence, democratic populations and politicians, in particular, are schooled in avoiding violent solutions to their political differences. Negotiated outcomes involving some degree of compromise are the expected or standard method of operation. Consequently, two democracies, sharing this type of political culture, will expect that their mutual interstate differences will be treated similarly. Violent solutions will be avoided (Dixon, 1993, 1994). Mediation and negotiation should prevail (Raymond, 1994). As the democratic community expands, a zone of peaceful, inter-democratic interactions emerges.[3]

The "civil society" argument(s) is a societal-oriented and highly normative variation on the democratic culture idea. Democratic politics are easier to bring about and maintain if nonpolitical spheres of human interaction are also democratic in operation (Dahrendorf, 2000). One thing to look for is a psychological predisposition toward long-term procedures for advancing private interests. If one believes that current losses can be turned around in the future, moderation, trust, tolerance, and restraint in dealing with the demands of others are more likely (Maddison, 1998: 115–16). The existence of independent associations that can be utilized to pursue private interests is another critical prerequisite. If all social institutions are highly centralized, there is little room for, and likelihood of, change. There is also less likelihood that groups will be able to contest powerful institutions (the state, churches, economic corporations) and carve out space for private rights and autonomy. The combination of moderation, tolerance, and voluntary organizations thus creates a liberal culture that can reinforce a liberal political system. Such a cultural context also creates an environment in which further improvements toward liberalization can be attained. Yet it is conceivable and historically demonstrated that the agencies of civil society and

the state need not act as checks on one another's relative power. Trentmann (2000), for instance, notes that nineteenth-century nationalism and imperialism were popular foci for British and German civil societies. Hence, the development of civil societies can lead to variable outcomes depending on historical context. Put another way, the emergence of a civil society can work to constrain political decision-makers but it also can be mobilized to intensify interstate rivalries and conflict.

The other main explanation for the democratic peace emphasizes institutional constraints on decision-makers in democracies. Democratic decision-makers are periodically subjected to the discipline of electoral contests in which they can be punished for engaging in rash foreign policy adventures or merely for presiding over costly external combat.[4] Whether they win or lose these external combats, voters remember the economic and physical sacrifices and penalize the decision-makers thought to be responsible. Democratic politicians are perfectly aware of this potential retribution and, in order to avoid it and maintain their posts, they will evade opportunities for its exercise. Even if they are not always mindful of electoral punishment, democratic decision-makers are thought to be institutionally handicapped in foreign affairs. They often need authorization from other elected bodies (for instance, legislatures) to go to war. They will also need a relatively high degree of domestic consensus to be able to mobilize resources for external combat. Unless a democratic state is actually attacked, these types of support may be difficult to create. In any event, they will usually take time to cultivate. All of these considerations suggest that democratic decision-makers will need to be more cautious than their autocratic counterparts in committing to foreign policies involving violence and coercion. Ultimately, they are constrained in one way or another by the average citizen's dislike for risk, death, and economic sacrifices.

The transparency/signaling cluster is related to the electoral punishment cluster. Transparency draws attention to the difficulties democracies experience in cloaking their intentions (Kydd, 1997). If decision-makers require institutional and popular support for their activities, they must engage in public discussions of motivation and intentions. Secret arrangements or public deceptions are apt to boomerang once they are brought to light and may lead to audience costs (Fearon, 1994) and electoral punishment. If democracies must be relatively more public in their announcing their intentions, the signals they send are more likely to be congruent with those publicized intentions. External opponents should be able to read these signals with less chance of misinterpretation—although it may also make democracies more vulnerable to attacks at different points of the electoral cycle (Gaubatz, 1991). Democratic decision-makers should also be more able to demonstrate resolve and make lasting commitments because allies and adversaries can assume that what they are being told is sincere and not duplicitous.[5] Resolve is good for communicating deterrent threats that may prevent the need to escalate to violence. Lasting commitments are good for enduring alliance arrangements that can augment capability against external threats.

The economic growth explanation has developed at least three tracks. The older track (Lipset, 1959, 1994) argues that economic growth reduces inequalities and diffuses resources, including more wealth and education, to individuals throughout the economy. As individuals become more affluent and educated, they will insist on greater participation in the political system. They will also demand property rights and the rule of law. They should also become more tolerant of minority rights. All of these tendencies should be expected to promote democratization. A second track (Barro, 1997) argues for a reciprocal effect. Democratized political systems should experience greater economic growth because they are less prone to capricious governmental intervention in the economy and violation of property rights. The most recent variant (Przeworski et al., 2000) to emerge argues that democratization does not require antecedant economic development to come about, but if a democracy is to survive, some minimal level of economic development is a prerequisite. Accordingly, economic development supports democratization even if it does not cause it.

Closely related to the economic growth arguments is the emphasis on the peaceful implications of economic interdependence.[6] Economic growth and development is likely to lead to greater economic interdependence. Greater economic interdependence is also thought to benefit further economic growth and development. Equally important, however, is the probability that increased economic interactions across borders will create and expand domestic pressures for avoiding the disruption of those interactions. External conflict tends to disrupt and distort trading patterns. As a consequence, people whose livelihoods depend on uninterrupted trade should be expected to lobby for decisions that avoid serious interruptions in their businesses. Another way of looking at this is that economic interdependence should expand the size of foreign policy "doves" and "soft-liners." Autarky, on the other hand, should be associated with "hawks" and "hard-liners" in a two-way causation scheme. "Hawks" and "hard-liners" will prefer to reduce their dependency on the outside world so that they are able to better cope with external threats. A reduced dependency on the outside world also means that there are fewer incentives to lobby for maintaining the existing pattern of international interactions.

The external threat argument suggests that to some extent the democracy $\rightarrow$ peace relationship needs to be reversed.[7] The relative absence of external threat is conducive to the initial emergence of democratic practices. A high degree of external danger encourages hierarchical centralization of authority to deal with foreign threats. Individual and minority privileges tend to be superceded by the overriding need to thwart the intentions of malignant external opponents. Thus, "nasty" neighborhoods can forestall the development of democratization. They can also erode it once it has developed. Moreover, external threats can also affect economic development by increasing the actual and opportunity costs of national security preparations and engagement in external conflict. If the economic development argument(s) is right, these economic costs should

also reverberate in the political system's proclivity for more open practices. Therefore, if "nice" neighborhoods facilitate and support the emergence and maintenance of democracy, to what extent are those same "nice" neighborhoods responsible for the ostensible democratic peace outcome? The answer might range from all to none, with the most likely probability falling somewhere in between.

The external institutional support dimension is straightforward.[8] Democracies have a vested interest in encouraging the development of democracy elsewhere. They need allies to defeat autocratic foes. They also wish to nip autocratic foreign policies at the bud, so to speak, by encouraging more democratization in former and continuing autocracies. Thus, democracy begets more democracies to the extent that democracies support and lobby for democratization. As the size of the democratic community expands, pressures for continued democratization in less democratic territory should also expand. These pressures are likely to be manifested in the foreign policy practices of democracies and also in the international institutions they create and control. A reverse spiral effect can also be imagined. As the size of the democratic community shrinks, it should be increasingly difficult to stem internal and external pressures to autocratize in order to respond to a deteriorating economic and political environment at home and abroad.

The external status quo explanation focuses on the likelihood that democracies are more likely to support the existing status quo than to be revisionists.[9] To the extent that democracies have been wealthy and powerful for some time, they are likely to be beneficiaries of existing structural arrangements. To the extent that democracies are firmly embedded in economic interdependencies, they are apt to value the maintenance of existing arrangements as most useful for advancing their interests. Turmoil threatens wealth, stability, peace, and community. The point here is that supporters of the status quo are less likely to be the agents of disruption and conflict. Again, then, some portion of the democratic peace outcome may be attributable to a disinclination to rock the international boat in the first place.

Finally, the tenth complex of variables is based on the premise that the modern history of democracy can be traced to a considerable extent to the emergence of small trading states on the fringe of western Europe, the success of which created a succession of global system leaders committed to increasingly democratic political practices (i.e., Venice, the Netherlands, Britain, and the United States).[10] The early commercial specialization encouraged the development of domestic political systems emphasizing limited governmental intervention in the economy, general constraints on governmental behavior, and an inclination toward the usually nonviolent rotation of competing groups of political elites. These practices set the foundation for subsequent movements toward expanded franchises and political rights that, in turn, established formulas for success and emulation by the rest of the world. In this respect, democratization depended (and depends) on the successes of the global system leaders. This dependence is

most dramatically observed in the intermittent battles between coalitions of democracies and expanding autocracies in global warfare. The point is not that the democratic coalitions were exclusively composed of democracies. They have not been. Nor is the point that global warfare is solely about the relative merits of democracy versus various types of autocracy (aristocratic, fascist, communist). However, the more democratic side has triumphed consistently in these battles. If they had not, the world would be a different place and undoubtedly characterized by less democracy rather than more. In general, then, the democratization of the global system leaders encourages the democratization of other states in the system through external institutional support, just as the same system leaders also stimulate economic growth and development, and create and defend the existing status quo against external threats.

The problem with the maximal approach is that it is difficult to know where best to begin. There are so many variables potentially relevant that a maximally oriented analyst could easily be paralyzed into inactivity by the enormous breadth of the task. What we need are some clues about simplifying the undertaking. One place to look for clues is the pre-democratic peace literature on the emergence of peaceful relations. Four studies, in particular, appear especially helpful.

Oldies But Goodies

There are at least four explanations that were developed prior to the current emphasis on the democratic peace that deserve further or renewed consideration. The four that come most readily to mind are Quincy Wright's (1942/1965) interpretation of democratic–autocratic interactions, Karl Deutsch and company's (1957) discussion of security communities, Nazli Choucri and Robert North's view of what they refer to as peace systems, and Richard Rosecrance's (1986) trading state theory. The argument we develop is not that any or all of the four offer a compelling alternative to the current mania for regime type but that they suggest some alternative paths to peaceful relations. With some synthesis, they can be used to construct an alternative theoretical interpretation of the "democratic peace" that can be tested empirically.

Path one: the pluralistic security community. The problem that Karl Deutsch and his team (1957) sought to explain was how some areas had managed to eliminate war. They were equally interested in "amalgamated" and "pluralistic" situations. In the former, political units that had once had conflictual relations were combined to form a larger political unit. This is basically a state-building process. Of more direct interest are the pluralistic cases in which states retain their sovereignty but develop a very low probability of going to war with one another.[11]

Another core concept is the "security community." Security communities constitute groups of political units that have evolved toward widely

held assumptions that disputes will be settled in some other way than resorting to physical combat. These groups have become "integrated" in the Deutschian sense, which only means that they have created a sense of community, institutions, and practices that permits the interstate group to anticipate that processes of peaceful change will prevail.

How do we know that such groups exist? Two fundamental indicators are proposed. Do political elites appear to believe, for some reasonable length of time, that peaceful change is highly probable? Do decision-makers prepare for the possibility of war against other states within the group? When the first question can be answered affirmatively and the second one negatively, the group qualifies as sufficiently integrated to be considered a security community.

Security communities sound very much like democratic dyads that have ruled out war due to processes related to shared institutions or norms. The emergence of Deutschian security communities, however, is not predicated necessarily on considerations involving a particular type of shared regime type. A glance at the cases that are discussed by Deutsch and his colleagues, enumerated in table 1.2, quickly reveals a variety of regime type pairings.

Table 1.2 lists 18 cases described as pluralistic security communities in the general North Atlantic area studied by Deutsch and colleagues (1957). Whether the 18 cases qualified as pluralistic security communities at the times given should not be our immediate concern. The more interesting feature of table 1.2 is that the 18 cases can be evenly divided into two groups: one is composed of dyads that would probably satisfy most conceptions of democratic dyads while the other consists of dyads that were at least not democratic when they first emerged. This was not an element emphasized by the 1957 study but it is clear that Deutsch and colleagues did not require the presence of democratic norms or institutions for the development of zones of peace.

Table 1.2 Deutschian pluralistic security communities

More clearly democratic dyads	*Less clearly democratic or less consistently democratic dyads*
United States (1781–89)	Switzerland (1291–1847)
Canada–United States (since the 1870s)	England–Scotland (1560s–1707)
Britain–United States (since as early as 1871 or the end of the century)	Prussia–German states [excluding Austria] (1815–1866/71)
Belgium–France (since some time in the nineteenth century)	Britain–the Netherlands (since perhaps 1815 or 1928)
Norway–Sweden (since 1907)	Austria–Germany (1876–1932)
Britain–Norway (since 1910 or earlier)	Denmark–Norway (since late nineteenth/early twentieth century)
Britain–Sweden (since 1910 or earlier)	Denmark–Sweden (since late nineteenth/early twentieth century)
Britain–Belgium (since 1928 if not earlier)	Britain–Denmark (since 1910 or earlier)
Britain–Ireland (since 1945)	Mexico–United States (since the 1930s)

Source: The identification of pluralistic security communities is extracted from the discussion in Deutsch et al. (1957).

Instead of democratic norms and institutions, the 1957 authors stressed three conditions as very important to the emergence of pluralistic security communities. Major values had to be compatible. Just which values are considered to be major depends in large part on what the participants consider to be salient. The three that were most prominent in the 18 cases included political ideology, principles of economic organization, and religion. Thus, shared democratic values could be facilitative in the political ideology column. But, presumably, so could other types of political ideologies. It is also unclear what to anticipate with cases in which some values are shared and others are not. For example, should we expect the same probability of successful community formation with two democratic states that possess unregulated economies as in the case of two democratic states in which one operates on socialist economic principles and the other leans more toward free enterprise? Can Muslim democracies (autocracies) work equally well with Hindu, Christian, or other Muslim democracies (autocracies)?

The 1957 authors might have said that this was an empirical question but it is interesting to note that they did suggest (Deutsch et al., 1957: 126) that the relative weight of different values depended on whether the value(s) in question can be depoliticized. The specific example they gave was that if a strong state such as West Germany were to become increasingly undemocratic in regime type, it would not necessarily be a cause for friction in a North Atlantic security community. What would make a dangerous difference is whether some form of militaristic expansionism became entangled with an increasingly centralized political ideology. But then this example assumes that West Germany had already become a member of a security community and then changed its regime type. So, we might have different weights for different types of values at different stages—as in the case of initial community formations as opposed to community maintenance. On the other hand, it is not clear that NATO decision-makers treated Portugal differentially according to the regime type in place since 1949. Perhaps then different weights are not necessarily in order by the stage of the process. Alternatively, perhaps the problem lies with the casual insertion of the capability dimension (a strong Germany versus a weak Portugal?) in the 1957 example.

The states involved also had to be mutually responsive, as well as mutually predictable. What these two criteria mean is that the integrating states had to possess the appropriate infrastructure for not only receiving signals from each other, but also responding to the signals by incorporating the other side's preferences into decisions made about how the two states would treat each other. The predictability criteria, one would think, would require the mutual responsiveness and the avoidance of violence to be operating over some length of time.

Just how critical these last two criteria are seems debatable in view of the capability asymmetries found in table 1.2. Were England and Scotland, Austria and Germany, or the United States and Mexico, mutually responsive

and predictable throughout the periods designated as security community phases? Perhaps most important is a criteria that Deutsch et al. (1957: 66) introduce immediately prior to discussing the three most important conditions. A pluralistic security community "implies acceptance by both parties of a political situation which neither side expects to change by force." The precise status of this maxim in the 1957 theory is unclear. We are inclined to treat it as a fourth very important criteria and, without benefit of case analysis, equally inclined to suggest that it may well be the most important criteria.

Later in their discussion (Deutsch et al., 1957: 115), the authors return to this question, refer to it as the "outstanding issue leading to pluralistic security communities," and note that there are a variety of ways that the acceptance can come about. War might be rejected as a plausible option because it was too likely to be indecisive (United States–Canada in earlier times). War might be rejected because it increased the probability of a third party intervening in a way that could lead to a worse outcome than if nothing was done (Norway–Sweden in 1905). Or, war with a neighboring state might seem increasingly unattractive if it was deemed an unpopular issue in the domestic politics of one or both states (United States–Mexico in the 1920s/30s). One might easily extend this list of scenarios to include situations in which one side was sure to lose decisively while the other side had little to gain or circumstances that lead one side to simply accept subordination to a stronger neighbor or rival.

In sum, there are a variety of theoretical and empirical problems lurking in the Deutsch et al. (1957) analysis. Yet there are also intriguing insights and interesting arguments about how zones of peace can come about in a number of ways. *Shared democratic values can be facilitative in this view but they are neither necessary nor sufficient causes in creating zones of peace.*

Path two: peace systems versus war systems. In 1972, Nazli Choucri and Robert North applied their lateral pressure model to the analysis of four states (Sweden, Norway, Denmark, and the Netherlands) in hopes of distinguishing how these more peaceful states diverged in behavior from the major power interactions that led to World War I, their initial focus of inquiry.[12] Their model is driven by two motors, population growth and technological development. Change in both spheres lead to the need for more resources and the greater the need, the more likely a society will develop specialized capabilities to acquire them, et ceteris paribus. To the extent that these resources are sought outside the boundaries of the society, the greater is the probability that two or more extending states will intersect and collide. The more intense these intersections and collisions over resource acquisition, the greater is the probability that interstate competition will assume militarized forms and become violent.

However, there are multiple ways that extra-societal extensions can take place. Colonialism or expansion at the expense of people with fewer capabilities and large-scale combat between rival major powers are two modes

especially likely in war systems. Other modes of extension (commerce, investment, exploration, mineral extraction) are not necessarily equally conducive to conflict and violence. The Choucri–North position is that if modes of extension are pursued that avoid partitioning space into spheres of influence that must be defended against interloper encroachment, peace systems are likely to emerge. The point is to somehow evade a war system's intense intersections and consequent militarized competition by taking part exclusively in behavior characterized by minimal threat and violence.

Choucri and North's complicated simultaneous equation modeling, that need not be explored in any detail here, yielded strong linkages between population growth–economic development, military preparedness, and participation in external violence in their 1870–1914 major power analysis and only weak to nonexistent linkages in their Scandinavian–Dutch sample. Their specific finding was that "most links in the war-prone conflict model do not hold for Sweden and Norway and few links are significant for Denmark and Holland" (Choucri with North, 1972: 239). While one may view this outcome in a variety of ways, its interesting implications for our present purposes are not connected directly to the empirical findings or even the specific causal model. What are more interesting are their peripheral comments about the cases upon which they are focusing.

Toward the end of their analysis, they note that the Scandinavian states, for all practical purposes, opted out of participating in the [Baltic] war system as early as 1815.[13] The decisions to opt out are attributed somewhat vaguely to internal and external transformations. They do not discuss the nature of the internal transformations that were involved but the timing (1815) would suggest that they preceded any discernible movement toward substantial democratization.[14]

Choucri and North are more forthcoming about the possible external transformations. One possible reason for opting out that they mention is that the small states of Europe found themselves hemmed in by more powerful states (Russia, Germany, Britain, and France) after 1815. The authors are not impressed by this explanation because they note that other small states have tried to remain within war systems by building up their capabilities and seeking more powerful allies. But that misses two interesting points about Sweden, Denmark, and the Netherlands. First, they were not always small and relatively weak states in the European region. Both Sweden and Denmark had once been major powers, at least by northern European standards, and Sweden, briefly, had proved its competitive capability on a wider scale in the seventeenth century. The Netherlands had once been more powerful on an even wider scale. In the late sixteenth and for most of the seventeenth centuries, it was the leading maritime power and a major innovator, as were the Swedes, in European land warfare.

Historically, then, Sweden, Denmark, and the Netherlands were not simply small powers in the late eighteenth/early nineteenth centuries. They had become small powers after a period of being something else because other adjacent states with larger populations and greater access to resources

eclipsed them. It was not so much a matter, then, of their opting out of the war system as it was a matter of being forced out as major competitors because the stakes and entry costs had risen and these three states could no longer afford to play in the major power league. It took a number of years for them to fully realize that they were no longer as competitive as they had once thought they were.

A second external dimension that Choucri–North mention as something that they have not taken into account is rivalries. For many years, Sweden and Denmark were bitter and violent rivals. That process played itself out with their mutual exhaustion and the emergence of new threats in the Baltic region (Russia and Germany). Sweden slowly learned that it could not compete with the Russians; the Danes learned their lesson about the Germans a bit more quickly. Much earlier, the Dutch had also engaged in highly significant strategic rivalries with the Spanish, the English, and the French. By 1815, those rivalries were dead issues. They had long since been played out.

Hence, one could argue that Choucri–North's empirical analysis was somewhat beside the point. By focusing only on data in the nineteenth and twentieth centuries, they have succeeded primarily in confirming that the Dutch and the Scandinavians were no longer major players in European warfare—something they already knew before the analysis was undertaken. A more impressive test would have been to examine data, if only it existed, spanning the fifteenth and twentieth centuries in order to look for break-points in the transition from war systems to peace systems. Nevertheless, Choucri and North's analysis sensitizes us to additional systemic factors in bringing about zones of peace. *Some states are forced out of competition by the emergence of more powerful competitors and an increasing ante to be able to play the major power game. Some states also are able to reduce their external competition because the issues they once fought over in rivalries become moot or can no longer be pursued in a changing strategic environment.*

Path three: the Wright interpretation of democratic peace. Much of the current democratic peace discussion focuses on democratic–democratic dyads. These are the dyads that rarely, if ever, fight one another and, so, explanations of the democratic peace concentrate on the "dem-dem" attributes that are thought to be particularly pacific in effect. Quincy Wright also thought in terms of dyads but, writing in the late 1930s and early 1940s, his inclination was to focus on an earlier generation's most significant dyad—democratic–autocratic pairs. Given such a focus, the usual explanatory emphasis is reversed: what is it about these dyads that make them more likely to go to war with one another?

Wright (1942/65: 842) was not convinced that either democracies or autocracies were more or less war-prone:

> Probably there are tendencies toward both peace and war in democracies, as there are in autocracies—tendencies which approximately

> neutralize each other and, under present conditions, render the probabilities of war for states under either form of government about equal.

But each type of state possessed tendencies and attributes that not only differentiated them but also gave one type an advantage over the other. An advantaged type of state, it was assumed, would exploit that edge when either its position or its prestige was threatened and could not be protected in the absence of war.

One difference between democracies and autocracies was that democracies are handicapped in participating in the bluffing and bullying games of international politics. Democracies by definition encourage debate and mass participation in politics. Decisions therefore come relatively slowly if a large number of political participants have to be consulted in some fashion. Dissidents cannot be silenced easily. Moreover, external opponents can always be counted on to encourage internal opposition to policies deemed undesirable. In contrast, autocracies can suppress their domestic dissent. They can also act quickly and more deceptively because there is less need to convince many people of an enemy's harmful intentions or its own need to take some sort of action in a crisis situation. Again, by definition, the circle of influential people who need to be consulted is thought to be smaller in an autocracy than in a democracy.

A second difference is that very different types of leaders are selected in the two basic types of political systems. Democracies, according to Wright, tend to promote conciliatory, welfare maximizers who possess strong professional incentives to prefer butter over guns. To survive, autocratic leaders must be aggressive power-seekers who are not too constrained by rules and laws. It follows that guns, for domestic and external purposes, are more likely to be given priority over butter and domestic welfare considerations.

Still another difference is that democratic political system processes are more likely to be characterized by the primacy of domestic politics than are autocracies. Public opinion must be placated to stay in office. Yet public opinion knows little about foreign policy, cares less about foreign policy than closer-to-home problems, and, if ignored even temporarily, can become a target of opportunity for parties seeking an opening to greater domestic power. Moreover, as long as political parties rise and fall from public favor, it is difficult to contemplate long-term strategies. The vicissitudes of domestic politics places a premium on short-term survival tactics. In the long run, domestic leadership turnover is the only sure thing.

A fourth difference is that some of the most significant advantages of democracy emerge only after war is underway. Autocracies enjoy an edge in starting wars but fall apart more readily than democracies do when states experience the societal shocks associated with defeat. In addition, since democracies tend to privilege economic growth over military capability development, democracies also have the edge in surviving long wars of attrition.

These tendencies, in turn, have three implications. First, it is extremely difficult for democratic decision-makers to do what must be done to maintain balances of power. Preventive strikes are difficult, if not impossible. Bluffs are hard to execute. Military preparations are expensive and unpopular. Mobilization against external threat is slow and hard to achieve unless the threat is very acute and by then, it may be too late to do anything but fight defensively.

Second, the alternative strategy of building international institutions that promote democratic organization and policy-making at the international level is also difficult to pursue. It requires surrendering sovereignty, or some portion of it, to superordinate organizations. Yet any such action is likely to encounter immediate and sustained domestic opposition. As long as domestic problems and politics enjoy primacy, serious constraints on assuming the obligations that go along with effective multilateral organizations can be expected.

The third implication is a product of the first two. Democratic political systems create or facilitate international situations that are hazardous to their own health. If there are autocratic leaders willing to assume some level of risk, circumstances will encourage attacks by autocracies on democracies. Wright went so far as to suggest that as the number of democracies increases, the expected gains from the perspective of autocratic decision-makers should also increase. The underlying rationale appears to be that "the greater the number of sheep, the better hunting for the wolves" (Wright, 1942/65: 266).

In moving to more systemic considerations, as is implied by referring to the number of democracies, Wright adds a fourth, more macrostructural element to the explanatory stew. While democracies have been handicapped in playing power balancing games, the emergence of Britain as a balancer in the eighteenth and nineteenth centuries helped offset the structural odds against democracies surviving. Britain's geographic insularity, dominant sea power, industrial and financial resources, and liberalism—all interacted to create an umbrella of protection for democracies. The catch was that the Pax Britannica had to be strong. When it was strong, democracies could expect protection and encouragement. The prestige of Britain and its political system also meant that other political systems would be more likely to emulate democratic institutional arrangements.

A powerful Britain, as at the end of the Napoleonic Wars and through part of the nineteenth century, was conducive to periods of liberalization, democratization, and prosperity. A Britain in decline meant that the initiative returned to the autocratic states that usually have an edge in manipulating balance of power politics to their favor. Fewer democracies and less prosperity could also be anticipated. Thus, Wright argued that the general rule was that periods of general peace and democratization followed general wars only to be succeeded by periods of instability, autocratization, and general war. Wright's version of a long cycle of war and peace led him to suggest even further that it was peace that produces democracy rather than

democracy producing peace. This was not the dyadic-driven democratic peace with which we are most familiar in current discussions. Rather, the idea was that it was systemic peace, underwritten by a dynamic system leader, that facilitated and fostered the emergence of democratic political systems.

This conceptualization dovetails nicely with leadership long cycle arguments. In the leadership long cycle framework, democratization is one of several key, co-evolving processes that are intertwined and mutually reinforcing. Others include economic innovation and naval power concentration. As argued by George Modelski and Gardner Perry (1991: 33):

> The role of global politics in all this is essential. The mechanisms of diffusion and clustering [of democratization] are set in motion by the long cycle. In that process the role of global leadership has served as a source of innovation and, via demonstration effects, as a center of innovation diffusion, as well as the nucleus of the emerging global democratic community. From the success of nation-states performing that role (the Dutch Republic, Britain, and the United States) has sprung the process of reinforcement that is essential to learning (the key to which is the proposition that "reinforced behavior becomes more frequent"). The world powers have been the preferred models of imitation, and, successively, the center for gravity for community organization. When these centers, at times, ceased to "hold" (as in the 1930s or [around] 1800, and in the 1670s or the 1580s), the prospects of democratic organization dimmed. The long cycle of global politics, itself a learning process, intermeshes with, supports, and in turn derives strength from the evolution of the global democratic community.

In sum, Wright saw democracies and autocracies as equally war-prone if considered as monads, despite the attribution of different strategic advantages and liabilities. From his dyadic perspective, *autocracies were thought to be more likely to attack democracies (than democracies were to attack autocracies) if the circumstances were facilitative. Systemically, the circumstances are least facilitative for autocratic attacks when the system is led by an economically strong, democratic, naval power. Systemic peace, therefore, encourages peace for democracies. For a variety of reasons spelled out above, these same periods of strong systemic leadership are optimal for the expansion of the number of democratic states. Periods of systemic leadership decline and general wars are most facilitative for autocratic expansion in both the military and institutional senses*.

Path four: Rosecrance's trading state model. Richard Rosecrance's (1986) trading state model begins with an emphasis on two strategies. One is labeled "military–political" and is focused on states that base their relative power on how much territory is controlled. Territorial expansion is useful for both establishing independence and self-sufficiency, as well as for improving one's competitive standing in the struggle for international

primacy. Given the probability of resistance to territorial expansion gambits and the competition for primacy, occasional bouts of warfare are a likely outcome as competitors determine how far they can expand.

The second strategy is referred to as the "trading state" approach. Trading states do not seek self-sufficiency and regard such a goal as inefficient. Some division of labor is preferred, with differentially endowed economies occupying various specialized niches, and the strong likelihood of increasing dependence on trading partners is accepted as par for the course. Trading states also avoid the temptations of territorial expansion in preference for a concentration on domestic economic development and trade relationships that are apt to be interrupted by warfare.

Rosecrance envisions a situation in which decision-makers are constantly choosing between these strategies. Neither strategy is entirely mutually exclusive and, historically, most states have opted for some combination. However, with some notable exceptions, the modal combined strategy has given greater weight to the military–political route than to the trading option. One exceptional era is attributed to the outstanding success of British industrial and naval prowess in the mid-nineteenth century. For a few decades, a trading world characterized by decreasing barriers to trade and increasing interdependence began to emerge only to be terminated as World Wars I and II brutally returned the systemic emphasis to military–political issues. The trading world "experiment" resumed only after 1945, this time with German and Japanese exemplars leading the way.

Several changes had taken place prior to and by the second half of the twentieth century to make the expansion of the trading world feasible. A series of industrial revolutions beginning in the late eighteenth century altered the demand for natural resources needed to fuel industrialization. Since few developing states possessed access to these resources at home, their economic dependence on obtaining the commodities through trade increased. The same industrial revolutions reduced transaction costs as transportation on land, sea, and through the air became less costly and faster. Then, too, the most successful industrializers developed production capacities that exceeded the demands of their home markets. The cultivation of, and continued access to, foreign markets became increasingly critical to sustaining domestic growth and employment. Perhaps ironically, the ballooning size of the international system after decolonization and the independence of many small and weak states in the former third world also left most of these new states with few choices but to pursue economic development and trade over territorial expansion. Economic independence was never a real option for the newly independent states.

At the same time, the costs of warfare had risen, while the probable payoffs from winning wars had declined. Technological advances in military weaponry meant that force could be projected at greater distances and with greater lethality but with little improvement in the ability to defend the home base. Vulnerability to attack therefore had increased even as the cost of military preparations had escalated immensely. If military conflicts were

increasingly likely to be highly destructive, conquering some target that would be destroyed in the process began to lose some of its appeal. Even more of the appeal was lost if the victor was also likely to be destroyed in the process. Even if maximum destructiveness could somehow be avoided, conquered populations had become increasingly resistant to being absorbed readily into the victor's territorial domain.

In general, the costs of military–political strategies had risen enormously, especially after 1945. Fortuitously, the costs of economic development and trade were declining as the barriers to trade were being whittled away in the increasingly prosperous, postwar era. *Decision-makers, faced with rising military–political costs and declining trading costs, make the rational choice and opt for participation in an expanding trading world*. Substantial defections from the military–political world might have occurred after World War I—the war to end all wars. But as long as a few aggressors remained and even fewer defenders were prepared to restrain their aggression, the inter-war years and World War II had to first be experienced to give the trading world approach its strongest opportunity to thrive after 1945. Only ideological conflict and differences in domestic political constitutions persisted in suggesting that some benefits might be attained via warfare and military–political strategies.

Rival/Complementary Interpretations to the Democratic Peace

So, what are we to make of these older perspectives that predated the current vogue for democratic peace explanations? What could be more straightforward than to explain the democratic peace with a democratic variable? Yet what may seem the most straightforward and parsimonious path is not necessarily the most satisfactory one. If there is a strong possibility that dyadic peace has come about through multiple pathways, we have all the more reason to be cautious about jumping on the democratic peace bandwagon. An increasing number of dyads may be displaying pacific tendencies or, at least, discounting the possibility of war with selected dyadic partners. Regime type may have something to do with this trend. The appropriate question is how much does it have to do with the apparent, if partial, pacification of world politics? Is there a bigger picture that current emphases that often focus almost exclusively or heavily on regime types are overlooking?

Seven elements emerge from the four older studies that seem worth pursuing further. They include limited roles for democracy, multiple paths to reduced probabilities of the use of force, peaceful niches, eclipses, rivalry life cycles, ideological conflict, and systemic leadership. For instance, one interesting common denominator of the four older interpretations is that they subordinate regime type to other emphases. For Deutsch et al., regime type was pertinent to the question of shared political ideology, but it is not determinative and may not even be all that important. For Wright, regime

type had both monadic and dyadic implications, but it is not the democratic dyad that received theoretical attention. Rather, the emphasis was on mixed dyads and their tendency to fight in the absence of structural protection for democracies. For Choucri and North, regime type is not really very prominent. The emphasis is on the choice of modes for satisfying resource needs—a process that predates more recent shifts in regime type. Democracy is missing altogether from Rosecrance's emphasis on rising military costs and declining trading costs.

If it seems reasonable to suggest that regime type is insufficient to carry the explanatory day, then it should also be fairly agreeable to suggest that these older perspectives are highly suggestive for developing a more comprehensive attack on the emergence of pacific tendencies. None of the four is likely to provide an adequate platform exclusively, but each can suggest something worthwhile for further consideration. The Deutschian perspective reminds us that security communities have developed in the absence of democracy. Wright's interpretation reminds us that democratization has always been an international phenomenon, in addition to national developments. Democratization has required external protection and assistance from a democratic system leader with variable capability over time to provide protection and assistance. At a minimum, if the twentieth century's two global wars had worked out differently, there would probably be far fewer democratic dyads than there are today. Choucri and North remind us, perhaps somewhat inadvertently, that some states shift their modes of interaction from more dangerous strategies to less war-prone ones due to being eclipsed by more powerful competitors and/or because they have simply exhausted themselves trying the more hazardous modes.

Deutsch and company also provide the more general question that we should be pursuing. Rather than ask why democratic dyads are so peaceful, we should be asking why is it that a dyad moves from a situation in which the parties perceive some probability of an armed attack from the other member of the dyad to a situation in which neither side perceives much likelihood of such an attack. Shared regime type may be facilitative of this process; it is hard to imagine, though, how it could be *the* dominant influence.

If democratic regime type needs to be "put in its place," what other factors should we be looking at that might offer more explanatory leverage, and, in the process of doing so, inform us as to just what might the appropriate place for regime type be? In many respects, Rosecrance's argument, with some historical tweaking and theoretical modification, provides an alternative framework that is capable of subsuming some of the major points made by the other, earlier studies reviewed in the preceding section. The Rosecrance argument reduces to a ratio of the net costs associated with war and trade. As the war/trade costs ratio rises (war become more costly and trade less costly), more states should adopt trading strategies that include avoiding the costly disruptions of intense conflict as much as is feasible. In a world characterized by territorial expansion, multiple

threats, and intermittent warfare, political–military strategies must predominate as a matter of survival. As environments become more characterized by exchange, interdependence, and development, trading state strategies should become more predominant.[15]

Rosecrance couches his argument in a decidedly Westphalian genesis story. From 1648 to 1945, with some deviation from the script in the mid-nineteenth century, international relations approximated the political–military end of the continuum. After 1945, strategies began to move toward the trading end of Rosecrance's dualistic behavioral spectrum as the world became increasingly interdependent. Yet we contend that these war/trading options have been available as long as people have been engaged in war and trade. More to the point, as western Europe moved into the center of the world system after 1500, some of its most prosperous states sought to disentangle themselves from regional affairs to better focus on controlling the flow of American and Asian trade to Europe. Portugal, the Netherlands, and England sought as much as possible to keep some distance from the territorial conflicts in their home region. Ultimately, as in the case of their prototypical predecessor, Venice—a model trading state for its time, the first two in the western European trio were ultimately unsuccessful. They were invaded and conquered by Spain and France, respectively, but not before establishing records as precociously successful trading states in the early-modern European context.

Rosecrance would probably dispute this characterization. The difference of opinion can be reduced to how one views Portuguese, Dutch, and English coercive tactics outside of Europe. If they chose to specialize in African, Asian, and American conquests, how does that distinguish them from Spanish activities in the Americas or Spanish and French territorial expansion in Europe? The answer is that Portuguese, Dutch, and English agents in long-distance trade always began with the premise that territorial conquest should be avoided as much as possible. The gradual acquisition of first bases and then political–military control adjacent to the bases in ever-increasing scale was neither entirely premeditated nor did it represent successful strategy. Many imperial acquisitions proved to be drains on profit-making in trade. The Portuguese were usually too weak to penetrate very far inland. The Dutch essentially were bankrupted by the success of their initially reluctant efforts to control Indonesia. The English were more successful after they lost their first empire but in the process they also became something other than what they had started out as. One can also speculate whether the British might have been more likely to have retained their nineteenth-century industrial lead, or at least a leading role in technological change, in the absence of an extensive empire on which to fall back.

Nonetheless, there is a long lineage of trading states, the leading powers of the global system delineated in table 1.3, in the history of contemporary (the last 500 years) European international relations. The older trading states engaged in predatory behavior outside of Europe but, within Europe, they

Table 1.3 The evolution of global systemic leadership

Century	*Lead global power*	*Resources*
Sixteenth	Portugal	Lead in Asian trade, global military reach (navy)
Seventeenth	The Netherlands	Lead in European and Asian trade, capital markets, industry, global military reach (navy)
Eighteenth	Britain	Lead in Asian and American trade, industry, insular position, global military reach (navy)
Nineteenth	Britain	Lead in industry, trade, finance, liberalism, insular position, global military reach (navy)
Twentieth	United States	Lead in industry/scientific technology, liberalism, insular position, global military reach (navy and air forces)
Twenty-first	United States?	Lead in industry/scientific technology, liberalism, insular position, global military reach (navy and aerospace forces)

tended to conform to Rosecrance's trading state strategy expectations unless they were attacked by the predominant military–political powers of continental Europe. Of course, the fighting between the trading states and predominant land powers is a major feature of modern European history. Every hundred years or so (1494–1517, 1585–1608, 1688–1713, 1792–1815, and 1914–45), a coalition of trading states and certain land powers would suppress the occasional threat of a European hegemony being established—a threat that also had important implications for the security of the global trading network.

The point here is not only one of emphasizing the pre-1648 emergence of trading states. The early trading states attempted to organize intercontinental trade and thereby provided systemic leadership that corresponded to their relatively weak resource platforms. The nineteenth-century British iteration of this trading state trajectory combined the traditional commercial edge along with a comparatively novel lead in industrial technology. Thus what was a premature nineteenth-century experiment in the predominance of trading states strategies according to Rosecrance was in actuality a continuation and intensification of an older pattern already ongoing for hundreds of years.

Nor did the British nineteenth-century "experiment" end the succession of global system leaders. One of the more puzzling features of the Rosecrance historical interpretation is the placement of the late twentieth-century United States in the "political–military" world. How one ignores the U.S. lead in establishing a postwar regime for the management of political economy questions in emulation of the earlier British lead is difficult to explain. Thanks to a pioneering lead in technological innovation, the most successful trading state is likely to become the global system leader and to reorganize the trading world along lines reflecting its preferences, interests, and strengths.

Relatively weak states often need protection from predators so that they can pursue trading strategies. This type of protection, most pronounced after 1945, is an important function of systemic leadership. It allowed, among other things, Germany and Japan to redevelop their economies (and predominant strategy in world politics) for trading purposes. Other small trading states, such as the Benelux states, also have enjoyed considerable protection in the most recent era of international politics. All states can hardly be said to enjoy free choice in the strategies they pursue. Most do what they can given their resource endowments and opportunities. But if the external threats are much greater than they can manage on their own resources, a security shield can be very helpful in creating safe niches to pursue cooperative strategies. Britain accomplished some of this shielding on a very selective basis in the mid-nineteenth century. The United States has done much more along these lines in the second half of the twentieth century.[16] In this respect, shifts to different predominant strategies have relied heavily on Pax Britannicas and Pax Americanas that are created by system leaders to preserve the world orders and trading networks they construct after successfully defeating the last hegemonic challenge.

The reliance on systemic leadership is not simply a matter of external protection. System leaders are critical to generating the economic basis for eras of increasing interdependence. System leaders lead in a variety of areas. They control the system's lead economy that generates pioneering innovations in commerce and industry. These hard and soft technological innovations bring about paradigmatic shifts in how economies are structured and how they interact. They produce new products, increase supply and demand for trade, and lower transaction costs (as in steamships, railroads, and jet engines). The ability to outproduce all other economies gives the leader substantial incentives to take the lead in reducing barriers to trade that the lead economy no longer needs. Yet system leaders can maintain their edges only for finite periods. Other economies catch up by adapting and often improving upon the pioneering technology of the leader. Trading interdependence is especially encouraged by these periods of catch-up and increased specialization. Then, too, lead economies, also for a time, become the system's primary source for investment capital and loans. All of these factors are critical to spirals of interdependence and lowered costs of trade. Without them, economic processes would be fairly static and characterized only by the slow-moving, incremental change enshrined in equilibrium analyses.

While missing the critical ingredient of systemic leadership, Rosecrance (1987: 18) does acknowledge that trading strategies are often chosen by states that are no longer competitive in the political–military world. This propensity deserves more emphasis. The ongoing transformation of international relations reflects the fact that a number of contenders for power and position have been defeated, exhausted, and eclipsed in the last 500 years. Spain, the Ottoman Empire, Sweden, Denmark, Austria, France, Italy, Germany, Japan, and Britain have all dropped from the list of main

contenders. Russia's omission from this list may prove permanent as well. If the most powerful territorial expanders are removed or disarmed, the transformational opportunities expand commensurately. Their removal or demotion does not mean that small states will not make "trouble" from time to time. But the level of pervasive threat is much less and sufficiently so as to facilitate the expansion of alternative strategies. The costs of competitive warfare have certainly increased, thereby raising the minimal thresholds for competition and interstate rivalry at the elite level. Few states can afford the military implications of contemporary major power status. Those that thought they could in the past have largely been eclipsed by larger, stronger, and wealthier states that demonstrated their positional superiority in competition. Many of the past's political–military dinosaurs have either died or been transformed into birds.

Finally, another missing element is democracy. Rosecrance (1987: 27) leaves an opening when he notes that one of the domestic features facilitating participation in the trading world was merchant autonomy to freely participate in external networks of exchange. Barriers to trade had to be avoided domestically as well as internationally. Not surprisingly, early trading states had been characterized by merchant participation in politics and while these earlier forms (as in the case of Venice or the Netherlands) were more accurately described as merchant oligarchies, they did help establish a republican tradition that clearly contrasted with aristocratic formulae and contributed to subsequent democratization efforts by making a case for limited state intervention in internal processes. Domestic economic development also tends to expand the proportion of a population with sufficient resources to expect and to demand some form of participation in political decision-making. An emphasis on trading strategies thus tended to encourage democratization.

The first wave of democracies initially restricted the extent of participation to property holders above a certain threshold and then gradually expanded the scope of franchise inclusion by lowering the threshold. But one could say that groups of people were more likely to be enfranchised as they became regarded as important contributors to economic development and/or threats to domestic stability that was conducive to economic development. We know from a large number of studies that economic development is an albeit imperfect predictor of democratization. Emphasizing economic development and trade as the predominant national strategy, therefore, may also facilitate democratization. Put another way, the most successful trading states have been the states that have been the most likely to be democratic. In turn, they tend to encourage other states—both by example and the provision of resources—to imitate their economic and political institutions.

So far, we have demonstrated that a modified Rosecrance framework can encompass earlier emphases on not privileging democratic institutions (as in Deutsch et al.), on incorporating defeated and eclipsed competitors (as suggested in Choucri and North), and bringing in systemic leadership and the

democratic need for protection against external threats (as urged by Wright). But these influences and transformations are not likely to emerge evenly. Political–military strategies can be expected to be sticky in the sense that they will persist in some places because the control of territory and other values are still being pursued coercively. Paraphrasing Deutsch and his colleagues, why and where do situations persist in which states cannot preclude the possibility of armed attack?

One good bet that has received greater priority in recent years is the study of interstate rivalry. Most states have very low probabilities of going to war. In contrast, states involved in rivalry relationships—in which states view their rivals as threatening competitors or enemies—are definitely more likely to go to war with their designated adversaries than with other states. Not surprisingly, most of the wars of the past two centuries (almost 80 percent) have involved confrontations between rivals. These observations should not be viewed as suggesting that rivalries alone account for four-fifths of the warfare that has occurred. Rather, the point is that it is (or should be) difficult to account for warfare in the absence of information on ongoing rivalries.

Another attribute of rivalries is that they have life cycles. As in the case of wars, they have beginnings and endings—even if rivalry conceptions and terminations are apt to be more hazy than those for warfare. Of particular interest here, some regions have become rivalry-free while other regions have increased the number of ongoing, interstate rivalries. North America and western Europe are good examples of the former. North American rivalries ended when the British gave up their attempts to contain the expansion of the United States. The last interstate rivalry in western Europe (France–Germany) had ended by 1955. The independence of new states in the Middle East, Africa, and Asia, on the other hand, have resulted in a number of new rivalries among proximate states. These are the same parts of the world to which the incidence of warfare has been shifting (and away from the areas in which rivalries have atrophied). The obvious question, therefore, is whether war and peace is due more to the presence or absence of rivalry as opposed to the presence or absence of paired democracies? There is no need for these factors to be "rivals." It may turn out that regime type and rivalry relationships are intertwined and complementary influences on conflict propensities. Our expectation is only that ongoing rivalry relationships should be a partial indicator of the relative salience of political–military strategies because states in interstate rivalry relationships often have good reason to anticipate the need for coercive strategies.

Testing the Modified Trading State Theory in the Context of Democratic Peace Puzzles

In many respects, the modified trading state theory is no more easy to test conclusively than the minimalist arguments associated with democratic peace theory. However, it is possible to play them off against one another.

The broader argument of the two is the modified trading state theory because it can subsume democratic peace theory. The democratic peace theories cannot subsume the modified trading state theory, although there is no inherent incompatibility. We may well find empirical support for both approaches to explaining the emergence of more peaceful international relations. Yet there are also chicken-and-egg considerations. Minimalist approaches to democratic peace theory imply that the effect of democratization is substantial. The modified trading state theory implies that democratization, at best, is only one of several transformations taking place more or less simultaneously. To some extent, democratization is also seen as derivative of other, more central changes taking place.

Accordingly, if democratization must share causal credit with a slate of other ongoing processes, its impact on international politics should appear more limited when some of the other ongoing processes are examined at the same time. Some of the alleged effects of democratization may also turn out to be nonexistent when examined in a broader context. Then, too, we think that we are dealing with transitional phenomena. That is, the way the world works, and has worked, may be changing in at least some parts of the world. Thus, empirical relationships will not necessarily hold equally well throughout the period of transition. For instance, a relationship between relative capability and war initiation (e.g., stronger states attacking weaker states is the most likely scenario) may be fairly strong prior to 1945 and relatively weak or nonexistent after 1945 and increased constraints on the nature of warfare among the strongest powers. These considerations give us three overarching hypotheses that we will be probing in various ways throughout this study:

H1: *The influence of democratic regime type is only one of a number of factors encouraging less conflict in contemporary world politics.*
H2: *The emergence and survival of democracies, and any consequences thereof, is embedded within a larger context of structures and processes that share responsibility for subsequent reductions in conflict behavior.*
H3: *Evolutionary tendencies in world politics work against the overtime stability of relationships between conflict and its antecedents.*

The remainder of this book is divided into four parts. Chapters two and three focus on the "reversed causal arrow" question. Many scholars have argued that democracy leads to peace (at least for some pairs of states). What happens if we reverse the causal question? Do relatively peaceful niches facilitate the emergence of democracy? If so, we may have been giving too much credit to the transforming effects of regime type without asking the a priori question of in what external contexts is democracy most likely to emerge in the first place. Instead of democracy → peace perhaps the more complete causal path is peace → democracy → peace. Then again, it may be that periods of relative peace foster all sorts of beneficial consequences including democratization and the prospects for more pacific interactions.

The "reversed causal arrow" question feeds into our concern with the long-term development of inducements to avoid war and seek trade and

cooperation by altering the nature of the democratic peace question. Rather than taking the democratic institutions and practices as givens, the first question becomes under what conditions are democratic institutions and practices more likely to emerge? Hostile environments, in particular, seem non-conducive to the development of political arrangements that involve power sharing and slow-moving deliberative assemblies. Political systems that are slow to react to external attacks have been less likely to survive. A hostile environment, seemingly from the beginning of recorded history, has encouraged the emergence of political hierarchy to best accomplish the protection function of states. Centralized political decision-making is very useful in mobilizing and maintaining the resources needed for defense against external attacks. The more likely these external attacks are thought to be, it stands to reason (and other things being equal), the more likely the tendency to centralize political power.

A good illustration of this phenomenon is that democracies tend to centralize political power in wartime. The presumption is that the normal ways of doing things are inappropriate and inefficient in times of emergency. While these power consolidations tend to be temporary in states that have already established democratic practices, highly centralized autocracies emerged in pre-democratic times in a number of states. Not coincidentally, these strong autocracies, once they emerged, proved highly resistant to subsequent democratization pressures. The question then is whether hostile environments played a selection function in sorting out states that had better (limited hostility) or worse (great hostility) chances of developing democratic institutions and practices.

States with highly centralized political power tend to maintain that high degree of centralization by ensuring that resources (e.g., land, wealth, control of military forces) remain equally centralized. Again, it is not coincidental that observers have long associated democracy with middle classes or have argued that land reform is critical to democratization chances. The very existence of a middle class implies some deconcentration of resource concentration. Middle classes have some access to resources (some moderate level of wealth, education, and economic position) that they can convert to making political trouble. Landless peasants, on the other hand, have few resources, other than their lives, to use in demonstrating political dissent.

Thus, our initial focus is on situations of high external threat-creating conditions that lead to domestic resource concentration and, consequently, a limited likelihood of democratization. These same hostile environments are also likely to be conducive to the continued appreciation for the need to make war in order to defend the homeland and to defeat external opponents. At the same time, states that focused much of their attention on territorial defense and expansion in the home region were least likely to become involved in long-distance trade. The most successful trading states, especially in the European context, tended to be located on the peripheries of Europe and insulated to some degree from their troublesome neighbors

by natural defenses—as exemplified by swamps in Venice, canals in the Netherlands, or the English Channel. For the most part, these trading states were able to avoid the development of powerful autocracies and landed aristocracies. In the Portuguese and English case, civil wars were required to defeat, although not to totally suppress, the local aristocracies and autocrats that had developed. Civil wars occurred elsewhere in early modern Europe but without the same outcome.

Instead of the usual equation: democracy → reduced threat among democracies → democratic peace, we will look at the following relationship: external threat → domestic resource concentration → low probability of democratization → low probability of democratic peace. If we find that the external environment deserves some share of the blame/credit for the extent of conflict that results, with democracy at best an intermediary influence, then the question becomes whether we still find evidence of a democratic peace, after controlling for some of the a priori influences. These considerations lead to our first three hypotheses:

H4: *The greater is the perceived external threat, the more likely a state is to develop and maintain high levels of domestic resource concentration.*
H5: *The greater is the level of domestic resource concentration, the less likely a state is to democratize.*
H6: *Controlling for external threat and domestic resource concentration, the greater the level of democratization, the more likely it is that state's foreign conflict behavior will be constrained, especially with other democratic states.*

These causal arrow questions are examined in chapter two while chapter three focuses on a slightly different, but related question. What happens to domestic political regimes when foreign policy ambitions expand and become more aggressive in search of regional primacy? Are democratizing trends likely to be suppressed? Is democratization probable as long as regional primacy remains a foreign policy priority? Or, does democratization only become more likely after regional primacy ambitions are extinguished the hard way—by devastating defeat in regional combat and the eclipse of competitive status?

H7: *States heavily involved in the pursuit of regional primacy and coercive expansion have been the least likely to develop democratic forms of government.*

Both chapters two and three focus on the appeal or perceived benefits of coercion highlighted in the modified trading state theory. States in dangerous neighborhoods are more likely to regard war as a desirable option in some circumstances than states in relatively protected ones. At the very least, protected states have more choice in whether they participate in warfare or not. Then, too, the attractions of warfare will also depend on the nature of a state's foreign policy goals. States that seek territorial expansion and geopolitical predominance in their home region will be more likely to accept the "necessity" of warfare than states with less ambitious foreign policy goals. In both types of cases (neighborhood contexts and foreign

policy ambitions), the benefits of war are likely to exceed the perceived costs of war. Regime type or more specifically, democracy, is likely to be one of the casualties of external environments and goals.

The next section of the book takes this thread farther by grouping four chapters on the relative strength of regime type vis-à-vis other variables in coercive contexts. Chapter four looks at the role of various path dependencies—including foreign policy ambitions, irredenta, and rivalries—in the histories of states that have oscillated in the extent to which their domestic political system can be characterized as democratic. The point is that as long as the foreign policy path dependencies persist, success in transforming domestic political institutions is less likely. So, too, are the prospects for domestic political institutions overcoming the path dependencies in foreign policy—especially those dependencies that promote war or make war appear to be an attractive policy alternative. As a consequence, different types of domestic regimes end up pursuing similar external strategies that are relatively bellicose.

H8: *Certain path dependencies (for instance, foreign policy ambitions, irredentism, and strategic rivalries) encourage more aggressive foreign policies in spite of domestic political constraints on aggressive behavior.*

Chapter five takes this path dependency question one step further by comparing the effects of rivalry relations with democratic dyads on conflict proneness. Strategic rivalries are closely linked to net perceptions about the benefits of war. External threats and enemies may require coercive behavior that is missing from the relationships between two democracies. But which type of relationship has been more potent historically? We think it is likely that the threats implicit in rivalry relationships are more potent than the constraints linked to democratic institutions. But, are the relative potencies changing over time, with, say, the democratic structures becoming stronger as the rivalry structures become weaker? This shift over time could be anticipated to the extent that democratic regimes defeat their nondemocratic adversaries. Another way of looking at this same expectation is that mixed dyads (pairs of democratic and nondemocratic rivals) have been especially conflictual in the twentieth century as the number of democratic states gradually expanded despite nondemocratic opposition.

H9: *Democratic dyads are less prone to militarized disputes and wars than are nondemocratic dyads.*
H10: *Strategic rivalry dyads are more prone to militarized disputes than are dyads not engaged in strategic rivalry.*
H11: *The effect of strategic rivalry on militarized dispute and war behavior is greater than is the effect of dyadic regime type.*
H12: *The relative explanatory value of strategic rivalry and regime type are unlikely to be constant. The relative explanatory contribution of dyadic regime type vis-à-vis strategic rivalry should increase over time.*
H13: *Mixed regime type dyads are more prone to militarized dispute and war behavior than are autocratic dyads. Both of which, in turn, are more prone to militarized disputes and war behavior than are democratic dyads.*

Chapter six takes a different tack. Democracy is often said to be useful for winning wars. For instance, they may be slow to mobilize, but once they do mobilize they can fight with the support of greater domestic consensus and resources. While largely democratic coalitions triumphed in World Wars I and II and the Cold War, it is less clear that democratic regime type is an asset in winning wars more generically. On the other hand, democracies were once more rare than they are now. It may be that their war-winning virtues might not become apparent until more recent times. Putting these propositions to the test, chapter six reports that regime type is not found to be especially important in winning wars historically, but the role of variable may be becoming more significant.

H14: *Capability and initiation are more important to war outcomes than is regime type.*
H15: *The relative effects of capability, initiation, and regime type are changing over time, with regime type becoming a stronger influence on war outcomes.*

Shifting gears to a more systemic level of inquiry, chapter seven argues that it is the ideological clashes of the late nineteenth and twentieth centuries that have led to the analytical controversy over whether democratization has pacific elements in general or only within democratic dyads. The problem is that as long as emerging democracies have had to vie with nondemocratic enemies, one of the byproducts of increasing the number of democracies in the system, it will be difficult to assess the pacifying effects of changes in regime type. This generalization can be examined by inspecting the conflict histories of democracies and autocracies with their respective rivals. Democratic rivals tend to be autocratic while autocratic rivals are less concentrated by regime type. Only after the system moves through this transitional phase will it be possible to fully assess the strength of the pacifying effects of democratization.

H16: *In general, autocracies and democracies appear equally belligerent in the frequency of their overall conflict behavior.*
H17: *Both autocracies and democracies are equally likely to distinguish between enemies and non-enemies in their conflict behavior.*
H18: *Democracies are likely to distinguish between democracies and autocracies in their conflict behavior while autocracies are less likely to do so.*
H19: *Democracies are most likely to engage in conflict with autocratic enemies and least likely to do so with democracies. Autocratic non-enemies, as opponents, fall somewhere in between.*
H20: *Autocracies are most likely to engage in conflict with enemies, whether democratic or autocratic, and least likely to do with non-enemies, whether democratic or autocratic.*

Chapter eight examines the past two centuries and demonstrates the significance of democratic–autocratic conflict as states espousing sequentially different principles of domestic organization (aristocracies, fascists, communists, democracies) clashed ideologically in militarized disputes and major power wars. One implication of these examinations is that the apparent

triumph of democracies in these ideological wars and the resultant democratic peace cannot be viewed exclusively as a matter of domestic democratization processes. Systemic considerations are especially vital in bringing about a world in which democratization can proceed and flourish. Systemic leadership, in particular has been critical in defeating malign autocracies, fascists, and communists and also creating world economies in which trade and cooperation can flourish.

However, recognition of these fundamental relationships do not tell us exactly how democratization has altered the nature of systemic conflict. One argument is over the specific nature of the relationship. Some argue that expanding the number of democracies leads to a curvilinear relationship—little conflict at first, then considerable conflict as more democracies are available to fight, and then less conflict as democracies come to outnumber the nondemocracies. Others argue that there is simply no way to anticipate what might happen at the systemic level, given increasing democratization. It only takes one nondemocratic troublemaker to start a war. We can appreciate the logic underlying both arguments but our expectation is that the relationship has been more positive than it has been curvilinear—at least so far. The most simple reason is that curvilinearity assumes that all democracies and nondemocracies are more or less equal in significance and capability. As long as powerful nondemocracies persist, mixed regime dyads are likely to continue to conflict. Alternatively put, the third phase of democratic "supremacy" is slow to emerge.

H21: *Democratization and systemic conflict are positively related.*

Yet that sort of interpretation does not imply that the rate of democratization alone drives systemic conflict. Contrary to examinations that find that structural variables drop out of the explanatory equation as democratization becomes more important, we argue that the significance of structural change persists. It may depend on which types of structural change are examined. We have earlier argued that systemic conflict can be explained as a conjuncture of two opposing propensities—the atrophy of global concentration and the upswing of regional concentration. The relative decline of global system leaders and the emergence of regional hegemons and challengers of the global order creates a situation highly conducive to global war. When we control for democratization, does information about global and regional concentration continue to be important? The answer is yes. Structural change is not rendered, or at least has not yet been made, redundant by the liberalization of domestic political regimes.

H22: *Global concentration is negatively correlated and regional concentration is positively correlated with systemic conflict, even when the influence of democratization is controlled.*

Chapter nine continues the emphasis on systemic considerations by looking at first the pace of democratization and then alliance making. We first look at some of the claims put forward by Quincy Wright (reviewed

in chapter one) and others about the interaction between and among global war, systemic leadership, and the expansion of the number of democratic states in the world. Is there a clear relationship between systemic peace and prosperity and the pace of democratization? At the same time, others have argued that democracies have a special affinity for democratic alliance partners. This is another generalization that is difficult to assess during the transitional twentieth century. System leaders have been important in creating postwar alliances structures but they have not done so consistently throughout the nineteenth and twentieth centuries. The Pax Britannica era was not based on alliance structures while the post-1945 Pax Americana most definitely was. Thus, it is difficult to tell how frequently democracies might have allied with other democracies in the absence of a change in system leadership behavior. One index—whether democracies are disproportionately allied with other democracies—however suggests that it is system leadership that has been more important to alliance-making than regime type. Not only are democracies not consistently allied with other democracies—more than one might expect other things being equal—their tendencies to ally with other democracies also weakens as one moves forward into the postwar era.

H23: *Global war and systemic leadership are related to the pace of democratization.*
H24: *Systemic leadership is a major influence on democratic states' alliance formation propensities.*

We realize 24 hypotheses is a large number of generalizations. The task of keeping them straight is made all the more difficult by being forced to deal with a few at a time in any given chapter. Our last chapter brings them back together in order to show how they address and provide support for our alternative interpretation of the "democratic" peace. Moreover, chapter ten provides a conclusion to the examination by returning explicitly to the contrast between the modified trading state theory and minimalist approaches to the democratic peace. The findings of chapters two to nine are summarized and linked directly to the overarching theoretical questions at stake (see hypotheses H1–H3). The nature of the theories do not lend themselves readily to once-and-for-all contests in which one theory emerges the clear victor. But, the argument that the effects of democratic regime type remain significant yet weaker than sometimes contended, is amply supported by the evidence examined. The effects of secure niches, the eclipse of belligerent, expansive actors, rivalries, ideological conflicts, systemic leadership, and multiple paths to more peaceful relations are all demonstrated. Part of the analytical problem is that transformational processes are easily studied in static examinations. Transformations, which are clearly taking place, are characterized by historical evolution. We need to look for movement from one type of system to another, while recognizing the odds are that we are caught in between the old and the new ways. Yet another part of the analytical problem is attributing causality to the transformations underway. We do not argue that democratization has no

role—only that its explanatory role has been exaggerated. A broader, more maximal interpretation—as exemplified by the modified trading state theory—that focuses on increasing disinclinations to use force, increasing interest in economic development, trade, and cooperation, and fluctuations in systemic leadership, appears to offer a better and more comprehensive explanation of processes currently underway, even though these processes are manifested tentatively and unevenly.

PART TWO

Contexts in Which Democracies Emerge: Chickens or Eggs?

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CHAPTER TWO

External Threat, Domestic Power Concentration, and Disputatious Foreign Policies

When we observe a correlation between democratic dyads and reduced conflict propensities, several possibilities exist. Democratic regime type may be responsible to some extent for the reduced conflict in the identified dyads. This is the now well-known democratic peace argument.[1] Alternatively, the reduced conflict propensities may be responsible to some extent for the democratic regime types.[2] This is sometimes called the reversed causal arrow hypothesis in the sense that it changes the direction of causality in the democratic peace argument 180 degrees. A third possibility is that both interpretations are correct. Democratic dyads produce less conflict within their dyads and, reciprocally, the reduced conflict propensities encourage democratization within the dyad. Or, it may be a sequential relationship in the sense that relatively peaceful neighborhoods encourage democratization which, in turn, increase the probability of reduced conflict with other democratic regimes. A fifth possibility is that the linkage between democratic dyads and reduced conflict propensities is simply spurious. Some other, unidentified factor(s) is (are) responsible causally for generating both democratic dyads and reduced conflict propensities.

All five possibilities are plausible and most have their proponents. Although the democratic peace argument has certainly received the lion's share of attention, some of the five have been subjected to variable amounts of empirical testing. So far, tests for spuriousness have not yielded outcomes that unambiguously eliminate support for the democratic peace argument. One can argue that it has been demonstrated that the democratic peace is neither necessary nor sufficient for reduced conflict propensities and that, thus, it is possible to exaggerate the credit bestowed on democratic regime types. Yet the significance of a relationship between shared democracy and more pacific interactions has survived a number of empirical challenges.[3] The most likely conclusion is that there must be something going on in the relationship that is genuine. However, one area of doubt that remains is the question of reversed causality. What is the possibility that we have the causality of the relationship backwards? It may seem to be the case

that democratic states treat other democratic states less conflictually but that may misinterpret contexts in which niches of reduced conflict encourage democratization.

The possibility of causal misinterpretation is not an easy one to address empirically. Nevertheless, we think it is possible to do so and that the effort is a worthwhile one. While our answer is no more likely to be *the* definitive answer, our results are straightforward. We find that reduced conflict propensities, or, more exactly, reduced threat in the "neighborhood," has encouraged democratization tendencies in major powers over the past two centuries. Such a finding offers definite empirical support for a reversed causal arrow argument. Yet we also find that same neighborhood threat does not account for subsequently reduced conflict propensities among democratic dyads. Thus, our findings are best viewed as supporting the sequential interpretation that peace encourages democratization and democratization subsequently encourages more pacific behavior, albeit selectively. Implicitly, at least, this also suggests support for a variant of the reciprocal (peace $\leftrightarrow$ democratic dyads) interpretation.

There is some inclination in the literature to treat these questions of reversed/reciprocal/sequential causality as methodological questions. Our preference is to treat them first as a theoretical problem. Accordingly, we first review an argument on war making and state making that leads to the anticipation of a peace $\rightarrow$ democracy relationship. Next, we briefly review previous empirical analyses that have addressed various aspects of this relationship and suggest that a different approach is worth attempting. After developing a relatively complex causal map of possible linkages, a set of hypotheses are derived. We then develop what we regard as appropriate indicators and tests for hypotheses selected from the set linking external threat, domestic power concentration, democratization, and monadic and dyadic conflict behavior.

The War Making–State Making Argument

The war making–state making argument is more complicated than it often seems.[4] The well-known aphorism, war made the state and the state made war is a good summarization but it is also a gross understatement of the multiple processes at work. Actually, there are at least three main arguments. One is about state resource mobilization, a second concerns domestic power concentration, and a third emphasizes territorial expansion and efforts to attain regional primacy. The three arguments converge sufficiently to treat them as belonging to a single model, but it may help to communicate the arguments most effectively if we first discuss them separately.

To make war, states need resources. To acquire the sinews of war, state leaders need to develop taxation systems which, in turn, require bureaucratic agents to make the taxation systems work. If a state's wealth is primarily agrarian, tax collection becomes more difficult because it is easier to cheat. Bureaucratization should be more extensive in agrarian systems than in states that are able to rely on taxing trade goods at central points of

entry and exit. At the same time, state leaders often must give something to the population being taxed in order to expect any level of voluntary participation in the resource mobilization process. One item exchanged has been citizenship, political rights, and political participation in legislatures and decision-making. Thus, war making can have contradictory outcomes. On the one hand, it makes for larger and more powerful states. It enhances the autocracy potential of states, assuming victory in warfare. On the other, war making also opens windows of vulnerability for beleaguered decision-makers who make desperate bargains with various segments of their population to obtain sufficient resources for making war. Greater political participation and democratization can be one result.

One of the deals historically made by state makers seeking war making resources has been the surrendered control of land in exchange for military service. Some sort of feudal arrangement would ensue in which soldiers would be rewarded with land for services rendered and the future expectation that the state would be provided with trained personnel and equipment when needed. Aristocracies were created in this fashion, with variable linkages to the prospects for future state autonomy. To the extent that the state was controlled by aristocratic elements, there would be less reason to anticipate democratization or attempts at equalizing landownership. People who enjoy the benefits of power concentration are loathe to give up their privileges without a fight. But aristocrats are also good for manning bureaucratic and military posts, even if their competencies were not always commensurate with their social rank. In this sense, state making could become dependent on power concentration for its leadership in war making activities. One of the ironies of the history of these processes is that some states became more autocratic in order to escape the control of the nobility. Aristocracies were often hindrances in fully developing a state's ability to make war. They did not have to be eliminated but they did have to be subordinated. Only a powerful state could accomplish the subordination of the nobility. While this particular process could sometimes lead to coalitions between royalty and commoners, with a political payoff for the commoners, it could also be executed without surrendering any decision-making powers. Again, the outcome could lead to greater democratization or greater autocratization.

The third argument is about foreign policy ambitions and regional contexts. To best escape the implications of dealing with extensive and intensive external threats, state makers should desire a geographically insular position. No neighbors meant no need to develop large armies to protect against the possibility of an attack. Large armies, in return, meant resource mobilization, bureaucratization, and, quite often, the expansion of the state. Large armies could also be used to control the domestic population. No neighbors would also suggest a very limited temptation to expand one's own territorial borders by coercion. Extensive foreign policy ambitions that involved territorial expansion also led to large armies and frequent warfare. Even if a state could avoid the temptation to expand, being in a neighborhood harboring some expansive aspirants meant one had to

develop adequate defenses against the possibility of attack. The more imposing the threat of some state aspiring to regional primacy, the greater the need for defensive capabilities to match the expansive capabilities. Tough neighborhoods thus meant everyone had to play the same realpolitik game lest they be overrun by a local conqueror. Only the states with some natural or artificial insularity (for instance, a maritime barrier or the protection of a strong ally) could afford to opt out and allocate their resources to other types of activity. These same states could build navies instead of armies. They could also work toward reducing domestic tendencies toward power concentration without becoming easy prey for external predators.

Writing more than a century earlier, Seeley (1886: 133) summarized this third argument rather elegantly:

> It is reasonable therefore to conjecture that the degree of government will be directly proportional, and that means that the degree of liberty will be inversely proportional, to the degree of pressure. In other words, given a community which lives at large, in easy conditions and furnished with abundant room, you may expect to find that community enjoying a large share of liberty; given a community which has to maintain itself against great difficulties and in the midst of great dangers, you may expect to find in it little liberty and a great deal of government.

Of course, these three arguments are highly intertwined in practice. The more successful the state's resource mobilization for warfare, the more tempting ambitious foreign policy schemes may become. Alternatively, successful resource mobilization is likely to frighten neighbors into emulating the same practices. Resource mobilization for war making may also be responsible for concentration in landholding in the first place. The concentration of power makes it difficult to resist the expansion of the state, either at home or abroad. Moreover, it is often argued that aristocracies needed foreign adventures to employ the sons who would not inherit family land and to provide them with opportunities to win their spurs in combat. The other side of the coin is the argument that popular participation in politics restrains foreign policy ambitions. Warfare is costly and dangerous and not a practice most citizens would be likely to encourage if given a choice.

Associated with these multiple arguments is yet another complication that, in effect, constitutes a fourth argument. Once democracies began to emerge, they developed behavioral propensities that have led toward the protection/maintenance and expansion of the democratic community. Democratic metropoles encouraged their colonies to adopt the institutions of the metropole. The adoption of democratic institutions by new states carved out of defeated empires has also been encouraged. Powerful democracies have protected, albeit selectively and not always successfully, weaker democracies from the potential of attack from an adjacent and relatively powerful autocracy. Weak democracies that have been taken over by

autocratic foes have been defeated in intensive global combat by coalitions organized and funded by the most powerful democracies in the system. Democratic victors imposed democratic institutions on some of their leading autocratic foes to ensure less chance of a rematch. If things had worked out differently in the two global wars of the twentieth century, the proportion of the world occupied by democracies would no doubt be much smaller than otherwise has become the case. Other types of aid have also been provided, including occasional subversive intervention to prevent the wrong types of parties from being elected. Even if the powerful democracies did nothing at all, their mere existence and success provides an example for comparison and emulation.

One consequence of these arguments is that it is more difficult to contemplate the popular linkages between political regime type and peace as sufficient. Both regime types and peaceful regions have emerged from more complicated processes than can be extrapolated from the overly narrow perspective on what differences regime type may make on selective pacific propensities. To focus only on the correlation between regime type and conflict behavior is tantamount then to ignoring how democratic regime types emerged in the first place. It also ignores the likelihood that different regional contexts made differences in interstate conflict highly probable. In some cases, zones of peace could have emerged without any assistance from the right sort of regime types. In other cases, democratic intervention was undeniable. In still other cases, though, zones of relative peace may have been critical to the emergence of democratic regimes.[5] In any of these cases, crediting pacific propensities solely to the presence or absence of certain regime types seems inappropriate. The history of world politics has been much more complicated than the democracy → peace relationship suggests.

But is there any evidence for this point of view? There is, but it is both inconclusive and contradictory. It is also quite recent in origin, which suggests that we are just beginning to tackle these questions empirically. There are at least two analyses focusing on case studies that argue for a reversed, democratic peace, causal arrow. Layne (1994) looked at four crises—the United States versus Britain in 1861 and 1895–96, Britain versus France in 1898, and France versus Germany in 1923—and concluded that realist variables and not regime type accounted for the non-escalation to war in each case. While he did not test specifically for a reversed causal arrow, he concluded that democratic peace arguments had interpreted the situation backward. Democracies are not more pacific in behavior but their ability to emerge was strongly influenced by the relative absence of external threat. Thompson (1996), on the other hand, examined four cases—Scandinavia, revolutionary France, North America, and Taisho Japan—in order to test the argument that democratization was more likely in relatively cooperative, geopolitical niches that insulated political systems from harshly competitive, regional international politics. Unlike Layne, Thompson did not conclude that the democratic peace argument was

spurious. Rather, the conclusion was that regime type did not deserve all the credit for more pacific relations, geopolitical context also deserved some of it. States in less competitive regions were more likely and more able to behave in less bellicose ways.

Eight quantitative studies have addressed the question of whether the democracy → peace relationship also holds reciprocally.[6] Modelski and Gardner (1991, 2002) demonstrate that democratization has been facilitated by the outcomes of World Wars I and II in which a coalition headed by democracies defeated coalitions of autocracies. If the outcomes had been different, democratization would have been seriously set back. As it was, the outcomes of the wars offered various types of opportunities to expand the postwar number of democracies. Mitchell et al. (1999), in some respects, generalize this finding by arguing that since (1) war losers are more likely to experience regime changes and (2) democracies are more likely to win wars than are nondemocracies, nondemocratic war losers are more likely to change their regime types than more successful democracies. They also suggest that this differential tendency facilitates the promotion of democratization by democracies in the aftermath of war.

Midlarsky (1995) finds that hostile environments, as operationalized by aridity and many land borders, encourage autocratization and therefore work against the emergence of durable democracy both in ancient and more contemporary political systems. The other three studies are more explicitly concerned with endogeneity or the possibility of simultaneous causation in the democracy–war relationship. James et al. (1999) argue that war and preparations for war diminish the chances for democratization. They employ simultaneous equation modeling and find that the peace → democracy relationship is somewhat stronger than a nonexistent democracy → war relationship for the 1950–86 period.[7] Mousseau and Shi (1999) are also most concerned about the possibility that states become more autocratic as they prepare for war. They look systematically at 1, 3, and 5 years prior to war onsets and find no support for their anterior autocratization hypothesis. Crescenzi and Enterline (1999) examine all of the possible relationships among the frequency of war, the proportion of states that are democratic, and changes toward greater democratization. They find systematic support for the idea that democracy and war are reciprocally related, but that it is manifested very unevenly across time and space. Accordingly, one of their conclusions is that analyses that examine all states at once may be least likely to find support for the two-way relationship.

Thus, the studies described above have not all addressed the same question(s), the same time periods, or used the same indicators or methods—all of which is quite normal in the contemporary study of international politics. Some studies are supportive of a two-way relationship while others are much less so. But, most importantly, all of the studies highlighted here look only at some of the possible ways in which a peace → democracy relationship might be found. None of the studies exhaust the implications of

what we here call the war making–state making perspective. Indeed, most of the studies of this type hardly even begin to tackle the complexities of a historically protracted, state making process interacting with the evolution of world politics over a number of centuries.[8]

There are good reasons for previous studies not taking on all of the complexities associated with the evolution of states and world politics over the past five or more centuries. There are clear limitations in our ability to model all of the processes at work—or to do so in one fell swoop. Thus, we do not criticize previous studies for not doing the impossible. Rather, our fundamental point is that, arguably, there are multiple processes at work and that previous empirical studies have only touched upon some of them. At the same time, we would be among the first to acknowledge that all four of the paths outlined earlier do not lend themselves readily to simultaneous modeling. We and others have examined some of the suggested relationships in earlier empirical studies.[9] For instance, the empirical linkages between war and the expansion of state revenues and public debts have been examined. Another study looks systematically at the linkages among war making, resource mobilization, and army size. While all of these studies have so far proved to be quite supportive of the war making–state making interpretation, some selectivity in the scope of any given analysis is imposed by the realities associated with available longitudinal data series. However, our perspective on the historical processes that seem most important for the linkage between regime type and foreign policy behavior does suggest a different model than has been customary in earlier modeling of the democracy ↔ peace question. We think the modal model found in empirical studies of the reciprocal, democracy–peace relationship is whether the bivariate relationship works both ways. This is certainly one way to proceed and, no doubt, it is the most obvious way.

In contrast to the modal approach, our preferred model is outlined in figure 2.1. It suggests that there is much more to the reversed causal arrow argument than simply a question of endogeneity. We make no attempt to introduce all of the variables that might be introduced. Instead, we restrict

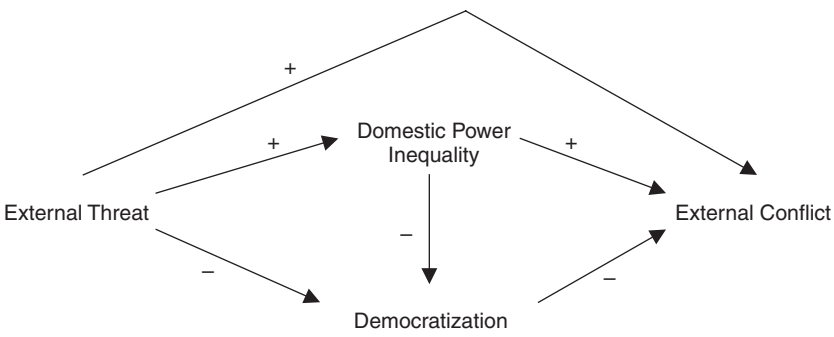

Figure 2.1 From external threat to external conflict

ourselves to the following set of three hypotheses extracted from the first three of the war making–state making arguments discussed above.

H4: *The greater is the perceived external threat, the more likely is a state to develop and maintain high levels of domestic power concentration.*
H5: *The greater is the level of domestic power concentration, the less likely a state is to democratize.*
H6: *Controlling for external threat and domestic resource concentration, the greater the level of democratization, the more likely it is that a state's foreign conflict behavior will be constrained, especially with other democratic states.*

These three propositions address a number of the linkages connecting regional neighborhood (external threat), foreign policy ambitions/war making (foreign conflict behavior), landholding concentration/power concentration (domestic power concentration) in figure 2.1. They also allow us to deduce three hypotheses about domestic power concentration, democratization, and foreign conflict behavior. Proposition H4 tells us that domestic power concentration is a function of external threat. The combination of hypotheses H4 and H5 suggests that democratization is a function of external threat and domestic power concentration. Putting all three statements together links foreign conflict behavior to a function of external threat, domestic power concentration, and democratization.

External threat creates incentives for fostering power concentration, either at the chief executive level and/or at the more general elite level. Decentralizing power in the face of threat would seem inefficient and highly dangerous, perhaps even inviting attack. A much more likely response to perceived threat is an increased probability of preparations for, and participation in, war. War preparations and participation, in turn, lead to attempts by states to mobilize resources and to build up military forces, bureaucratic extraction agencies, and the state. The prospects for sustaining some level of decentralization that may already have existed are not necessarily extinguished by these war making processes. Whether war preparations lead to greater power concentration will depend on other factors such as geopolitical location, foreign policy ambitions, and alternatives available to decision-makers for mobilizing resources, such as taxing trade or confiscating church land. Other things being equal, however, high and persistent levels of threat should increase the likelihood of power concentration.

This argument applies both to older historical situations and to more contemporary ones. For instance, in the medieval and early modern European setting, a state surrounded by threatening enemies either moved toward greater political centralization or faced invasions, dismemberment, and absorption into some larger unit. Not coincidentally, the number of states in Europe declined greatly between 1,000 and 1,900, while the size of the absolutist regimes expanded greatly. Only states or areas with some natural or political insulation from threats were able to evade the harsh choices of greater centralization or extinction.

By the beginning of the twentieth century, most of the ancien régimes were disappearing, only to be replaced by experimentation with alternative forms of domestic orders including democracies, military governments, and fascist and communist single-party regimes. Space considerations do not encourage an extensive discussion of the contours of the experimentation. But an underlying argument is that popular representation processes failed or were set back after World War I in cases in which external threats were greatest, and survived where they were least pervasive. The development of the single party regimes also reflected, to some degree, a perception of a threatening capitalist world in which liberalism and laissez faire economic policies would not suffice to catch up with the most prosperous states, or to compete with one's most dangerous adversaries. Survival encouraged a centralization of resources and policy-making powers.

Once a state is characterized by high levels of domestic power concentration, attempts to achieve greater democratization would be viewed as an internal threat to the privileges of the beneficiaries of power concentration. Those with power are unlikely to surrender it voluntarily. Democratization in such cases depends on openings provided by some gradual or abrupt erosion in power concentration.

Clearly, we are not attempting to fully specify the process(es) of democratization in hypothesis H5. Nor are we proposing a univariate explanation. The literature on explaining how democracy emerges and is maintained is extensive and complex.[10] We are prepared to accept arguments that economic development, political attitudes, balances among contending groups, elite negotiations, industrialization timing, and external influences are all worthy of consideration. We are also sympathetic to the problems encountered in attempting to develop one argument that fits all circumstances. It does seem perfectly plausible that the first wave of democratizing states experienced situations that were considerably different from those encountered by more recent converts to democracy, thereby suggesting the possibility of multiple trajectories existing over time.

Yet while we acknowledge these problems, we still see the likelihood of there being some significant relationship between power concentration and democratization. Following Vanhanen (1997), political competition depends in part on access to resources with which to compete. The more concentrated these resources are, the more likely it is that the distribution of political power in a society will also be highly concentrated, and that this power concentration will be manifested institutionally by some form of autocracy. Democracy, in contrast, assumes some level of resource distribution that makes political competition feasible.

A number of the empirical concomitants of democratization seem to correspond to this fundamental proposition. Many studies have found economic development to be a critical precondition.[11] Just why that might be the case—attitudinal modernization, coalitions between towns and kings against the aristocracy, the demands of business for freedom from governmental regulations, expanding middle classes, the emergence of working

classes, greater literacy, and so forth—is disputed. But some amount of resource dispersal usually accompanies economic development if only in trickle-down form. Some resource dispersal also seems related to all of the intervening variables that might link economic development to democratization processes. The problem is that the resource dispersal precondition's relationship to democratization does not appear to be constant over time. Empirical problems are most likely to emerge when we try to mix older cases of democratization with newer cases that have been encouraged to democratize before the facilitating preconditions are in place. As a consequence, we have a number of weak democracies and intermittent backsliding. While this is a major problem with attempting to model democratization cross-nationally, it should not prove to be as much of a problem with the elite major power group on which we will be focusing because of our primary interest in the external threat–democratic peace question.

The third statement sounds as if it has been borrowed from the democratic peace, as opposed to the war making–state making, literature. But it stems from some of the implications of low levels of domestic power concentration thought to be linked to democratization. The beneficiaries of high power concentration have often believed that their privileges and way of life are best maintained by an expansive and aggressive foreign policy involving warfare. This type of reasoning can incorporate exporting landless sons of the nobility as a domestic stabilizer to the diversionary hypothesis in which besieged elites distract attention from domestic problems by provoking foreign crises. It can also encompass highly ideological, revisionist states attempting to catch up quickly and coercively in elite status. From a different slant, there is also the Kantian approach, which emphasizes essentially that the more people involved in decisions related to war, as one might anticipate in dispersed resource circumstances, the more likely the majority will prefer to avoid the likely costs of war, as long as the situation falls short of a full-fledged national emergency. Traditional elites could be more cavalier about these costs since they were less likely to bear them fully. Then, too, their place in the stratification system was often predicated on ambitious plans for territorial and state expansion. Doing away with the aggressive expansionary schemes undermined the rationale for aristocratic service as military officers and imperial agents. As aristocratic and bureaucratic elites became more dispensable, they also became weaker opponents to demands for greater political participation.

So, for democratization to make headway in states that were once highly autocratic, the same states had to first become less expansionary in orientation. Otherwise, the rationales for maintaining high levels of concentration would be sustained. Severe defeat or resource exhaustion in warfare, being eclipsed by the emergence of much stronger rivals, and military occupation by victorious enemies were some of the ways in which this pacification of foreign policy goals might come about. Of course, all three could and, in some cases did, happen simultaneously. In other cases,

these external trauma had to occur more than once to make their impact fully felt.

States that enjoyed natural insulation from external threats could contemplate withdrawal and at least intermittent isolation from the hurly-burly of international relations. The diminished threat meant less political power concentration, less state extraction of resources, and smaller armies. Such states were too weak to contemplate or execute expansion against opponents that were stronger. Opponents that were weaker and more distant were a different matter. Maritime insulation from adjacent threats also implied some probability of developing oceanic commerce that not only contributed to resource dispersion, it also created lobbies for avoiding interruptions of economic interdependencies. These lobbies did not necessarily prevent warfare; they simply added another constraint on the outbreak of war with trading partners.

States that were not so autocratic at the outset but not very well insulated from adjacent threats usually were forced to maintain low profiles in international relations. This meant avoiding conflict if possible, and especially conflict with more potent neighbors. The diametrically opposite strategy, the Prussian path, involved extremely high risks of failure and accelerated movement toward domestic power concentration and territorial expansion.

Thus, we do not regard all democratic peace arguments as inherently alien to war and state making perspectives. But one question that arises is if domestic regime type exerts dyadic and monadic behavioral constraints, how did these different regime types come about in the first place? By attempting to answer this a priori question, the theoretical route to reduced conflict is different but the end result can be similar. We need not dismiss the possibility of domestic regime type constraints in order to recognize that there are geopolitical constraints in operation as well. The initial niches occupied by states, in conjunction with their various endowments, encouraged the adoption of different strategies for coping with a hostile, war-making world. In some cases, states were encouraged to become aggressive and expansive. In others, the best strategy appeared to be selective engagement in regions in which the opposition's capabilities were strong. Not coincidentally, different strategies for managing one's foreign conflict behavior also had ramifications for domestic political processes. Aggressive and territorial expansive states were more likely to have high levels of domestic power concentration and least likely to democratize. Less aggressive states had better possibilities of evading high levels of domestic power concentration and, thereby, improved their chances of democratization.

Testing Considerations

The three hypotheses linking threat, domestic power concentration, democratization, and interstate conflict seem reasonably straightforward. However, their testing raises a number of problems. The most general one

is that we are attempting to tap into a very long run set of sequential processes. Domestic power concentration, in particular, has been going on for many hundreds of years. Some nineteenth-century aristocrats who opposed early efforts at democratization could trace their privileged lineages back a thousand years before. Some of the "structured" external threat patterns also can be extended back just as far. For instance, Anglo-French rivalries in some respects went back to 1066 and Franco-German hostility can be traced as far back as the division of Charlemagne's empire in the ninth century, if not before. The point is that since most empirical tests are restricted by the nature of available data to the nineteenth and twentieth centuries we are unlikely to be able to tap into anything more than more contemporary segments of the processes under investigation. Just how that might distort our findings is not clear but it is a potential temporal selection effect that could make it difficult to discover any significant relationships. If we can discover anticipated relationships despite this handicap, our confidence in the substantive meaning of the results should be bolstered.

Another complication from previous studies pertains to the level of analysis. Earlier studies of the relationships among external threat, democratization, and external conflict have been conducted either at the systemic or aggregated dyadic levels.[12] Our hypotheses suggest that we begin on a monadic basis (threat → domestic power concentration → democratization → conflict) and switch to the more conventional dyadic basis when we move to a focus on selective conflict propensities (democratization → dyadic interstate conflict). Whether we could have confined ourselves to the dyadic level all along (threat, democratization → dyadic interstate conflict) remains to be seen. However, the sequential shift in the level of analysis corresponds to our preference for interpreting war making–state making as an extended sequential process. By almost all accounts, regime types do appear to behave differently in terms of their dyadic conflict propensities. But first we must ask the antecedent question concerning the origins of the different regime types.

Variables

Our sample is restricted to the nine states that have been viewed as major powers in the past two centuries: Austria-Hungary, Britain, China, France, Germany, Italy, Japan, Russia, and the United States. All nine have not been major powers at exactly the same time. China, Italy, Japan, and the United States were latecomers to the elite circle. Austria dropped out after World War I. Britain, France, Italy, Germany, and Japan dropped out, in comparison with the superpowers, after World War II although it is fairly common practice to extend Britain and France's major power status into the current period. However, rather than exclude these states from the sample when they were not major powers, in this chapter all nine are considered full sample members for the 1816–1992 period. This procedure does not eliminate all censoring biases, but at least it reduces the censoring to the pre-1816 period.[13]

Confining the analysis to a major power sample obviously reduces our ability to generalize to the full universe of states. Yet these states have been the most powerful and, arguably, the most troublesome in terms of generating interstate conflict. They have also served as models for the rest of the state population, not only in terms of shaping what states look like but also in terms of democratization. As a group, the major powers encompass some of the earliest and most prominent democracies and their fiercest opponents. If there are meaningful relationships among external threat, democratization, and conflict, they should be observable in this elite, highly combative group.

But there are other reasons for focusing on the major powers. The history of contemporary democratization is characterized by substantial heterogeneity in terms of the origins of changes in regime type. States have democratized in waves and more recent conversions have not necessarily followed the same path as did states in the first wave.[14] Some of the more recent cases appear to be responding more to external cues than to internal developments. This observation suggests that we need a sample that eliminates cases in which domestic developments (even if subject to external influences) played a less than prominent role. At the same time, we want a sample that exhibits variation on the democratization variable and one in which all of the states in the sample might have democratized during the longest time period we could devise given data constraints. This consideration eliminates many states that only became independent in the twentieth century. Finally, we also thought we could construct a plausible index of external threat utilizing a new rivalry database. The convenient thing about the major powers, besides meeting the other criteria, is that they all have rivals—some of which are contiguous and some of which are not. Some nonmajor powers have rivals, some do not. Nonmajor power rivals are also much more likely to be contiguous than noncontiguous. Yet nonmajor powers can also face external threats from non-rivals (for instance, French threats to Belgium and Switzerland in the nineteenth century or Soviet threats to Norway and Turkey in the twentieth century). This type of external threat (and external protection against such threats) is difficult to capture systematically. Confining the sample to a small group of highly significant actors of roughly equivalent capability thus facilitates a more simple research design. There are fewer threats to validity that might be introduced (and overlooked) in a much more heterogeneous and ambitious sample. Put another way, all of these states might have been members of the first wave of democratization, but most were not. To what extent can we account for their variable approaches to world politics as a response to the threats posed by external rivals?

Hypothesis H4 relates perceived external threat to levels of domestic power concentration. Threat is, of course, a perceptual variable. Ideally, we would have indices of decision-maker's perceived level of threat on an annual basis. Such data do not currently exist and, quite possibly, may never become available. As a surrogate, we combine information on strategic rivalry, military capability, and contiguity.[15] Strategic rivals are threatening competitors

designated as enemies. Decision-makers identify their main adversaries and give priority to those states categorized as most threatening to national security. Rivals, therefore, must be seen as competitors, perceived as threatening in some potentially militarized way, and identified as enemies.

Strategic rivals do not constitute the only sources of external threat. Another state can be threatening but not considered a competitor. For instance, a state could be viewed as too powerful to be a genuine competitor but still constitute a serious threat. Alternatively, another state could be viewed as too weak to be a genuine competitor but still constitute enough of a threat to be a nuisance or merely a problem. For the major powers, though, these distinctions are less of a problem. Major powers may still perceive differentials in relative power within their elite group, but all members of the group are sufficiently powerful to be viewed as competitors—although not necessarily as strategic rivals.

Some strategic rivals are contiguous while others are not. Some are quite powerful while others are less so. We assume that the threats that are most influential are those that are close to home in the immediate regional neighborhood. Other analysts have emphasized the effects of neighborhood on both conflict and democratization propensities.[16] The idea is that tough neighborhoods encourage conflict and discourage democratization. "Nice" neighborhoods have the opposite effect. To weight the nearby presence of powerful strategic rivals, we first compute the total sample's combined number of military personnel and then calculate each state in the sample's proportional share.[17] The shares of strategic rivals that are contiguous to each state in the sample are aggregated to represent the nature of the immediate threat. This index ranges from a maximum of 1.00 for a situation in which all members of the sample were contiguous and strategic rivals to 0.00 in which no members of the sample are contiguous and strategic rivals. Table 2.1 outlines some of the basic information used to create these contiguous rival threat scores on an annual basis between 1816 and 1992.[18]

Data pertinent to some dimensions of domestic power concentration are taken from Vanhanen's (1997) index of power resources. This complex index attempts to measure the extent of economic and intellectual power concentration by combining indices on urban population, nonagricultural population, students, literate proportion of the population, and the proportional area encompassed by family farms. Specifically, Vanhanen first computes the arithmetic means of the two population indices and the student and literate indices, multiplies them, then multiplies that product by the number of family farms and divides by 1,000.

Unfortunately, these composite indices are available only in the form of decadal averages for the period 1850–1979. To make use of this unique indicator, we are forced to convert all of our other data to comparable decadal units of analysis, giving us only 13 observations for each state.[19] At the same time, we do not claim that this measure taps every possible facet of domestic power concentration; rather, it emphasizes resource concentration that we assume is related to power concentration.

Table 2.1 Rivalry schedule

State	*Years in sample*	*Years as major power*	*Contiguous rivals*	*Non-contiguous rivals*
Austria	1816–1992	1816–1918	Germany (1816–70) Italy (1848–1918) Russia (1854–1917)	France (1816–1918)
Britain	1816–1992	1816–	France (1816–1904)	Germany (1896–1918, 1934–45) Italy (1934–43) Japan (1932–45) Russia (1816–1956) USA (1816–1904)
China	1816–1992	1950–	Japan (1873–1945) Russia (1816–1949, 1958–89)	USA (1949–78)
France	1816–1992	1816–	Britain (1816–1904) Germany (1816–1955) Italy (1879–1940)	Austria (1816–1918) Russia (1816–90) USA (1830–70)
Germany	1816–1992	1816–1945	Austria (1816–70) France (1816–1955) Russia (1890–1945)	Britain (1896–1918, 1934–45) USA (1889–1918, 1939–45)
Italy	1816–1992	1860–1943	Austria (1848–1918) France (1879–1940)	Britain (1932–45) Russia (1937–43)
Japan	1816–1992	1895–1945	China (1873–1945) Russia (1873–1945)	Britain (1932–45) USA (1900–45)
Russia	1816–1992	1816–	Austria (1854–1917) China (1816–1949, 1958–89) Germany (1890–1945) Japan (1873–1945)	Britain (1816–1956) France (1816–90) USA (1945–89)
United States	1816–1992	1899–		Britain (1816–1904) China (1949–78) France (1830–70) Germany (1889–1918, 1939–45) Japan (1900–45) Russia (1945–89)

Note: Strategic rivalries are not identified on a militarized dispute-density basis. Rather, they are identified by attempting to codify decision-maker perceptions about who their rivals were at given points in time. As a consequence, data collection procedures relied heavily on diplomatic histories because this type of information is readily available in descriptive accounts. The problems include the fact that historical sources use different terminology for the concept of rivalry, which do not always agree with the specific criteria used here of threatening competitors that are considered enemies. The sources do not always agree on the same adversary identifications. Nor are they always explict about beginning and ending dates. Thus, the systematization process requires scanning a large amount of information and making decisions about the adequacy of evidence on when the three criteria were satisfied and when they were not. The actual list of sources consulted runs to about 14 pages in length. In addition, the following major power dyads were treated as contiguous: Austria–Germany, Austria–Italy, Austria–Russia, Britain–France, China–Japan, China–Russia, France–Germany, France–Italy, Germany–Russia (1816–1918 and 1940–45), and Japan–Russia (from 1853). All other dyads were considered noncontiguous (based either on adjacent land borders or less than 150 miles of separation by sea). The one exception to this rule is the U.S.–Russian dyad that qualifies as contiguous in 1867 and the purchase of Alaska. While we are reluctant to make the exception, we also regard the geohistorical background for this dyad as an anomaly by major power standards. We do not have the sense that either side regarded the other as especially proximate prior to the Cold War era.

Data on levels of democracy attained are taken from two different sources. Vanhanen (1997) supplies one index. His measure combines data on competition (the percentage of votes won by the largest party subtracted from 100) and on participation (the percentage of the total population that actually voted). The democracy measure simply adds the two percentages and divides by two. The second source is the now standard Polity data set (see Jaggers and Gurr, 1995).[20] We subtract the autocracy score from the democracy score (both of which are based on 10-point scales). While both democracy measures are highly correlated ($r = 0.75$), we examine each separately to determine whether their different natures influence the outcome. To measure interstate conflict, we adopt the frequency of militarized interstate disputes taken from the MIDs data set.[21] We also wish to control for possible interstate war influences in the sense that losses in major wars can be expected to significantly impact the identity of external threats/rivals, domestic resource concentration, and regime types. War data for the major powers are extracted from the Correlates of War interstate war inventory (Small and Singer, 1982).

Modeling

The three hypotheses suggest that domestic power concentration is a function of threat, democracy is a function of threat and domestic power concentration, and external conflict is a function of threat, domestic power concentration, and democratization. This formulation might be thought to suggest a simultaneous equations approach, but we lack full specification (or other variables of possible importance). We also need to keep in mind that the number of observations with which we are working is small due to the decadal observations. There is, as well, a substantial collinearity problem in the sense that threat, domestic resource concentration, and democracy are all highly intercorrelated and characterized in addition by autocorrelation.

To investigate the relationships among disputes, democracy, threat, and domestic resource concentration over space and time, we first specify three regression models: domestic resource concentration is viewed as a function of changes in threat and lagged domestic resource concentration; democracy is modeled as a function of threat, changes in domestic resource concentration, war, and lagged democracy; disputes are conceived as a function of threat, domestic resource concentration, democracy, war, and lagged disputes.[22]

In addition to the models outlined above that are examined in terms of decadal observations, we also want to reexamine some of these findings by assessing the threat–democracy relationship with interval data over the 1816–1992 period. Preliminary analyses revealed that the serial correlation on the lagged dependent variable was near 1 suggesting that we needed to separate the short- and long-run impacts of the independent variables on democracy.[23] Therefore, our fourth regression model models democracy as

a function of change in democracy, change in threat, levels of threat, war, and lagged democracy. The short-term influences are represented by the change (or first differenced) variables while long-term influences are reflected in the variable levels.[24]

We also need to move away from the monadic approach implicit in the first four regression models in order to evaluate our monadic findings in the dyadic context in which democratic peace is often, but not always, couched. We estimate a fifth model relating threat and democracy to subsequent dispute behavior in which the log of disputes is estimated as a function of threat, democracy, peace years, war, and lagged disputes.[25]

Analysis

Table 2.2 summarizes the results of the pooled, cross-sectional time series regression of domestic power concentration on threat. As anticipated, our measure of external threat has a strong positive influence despite the controls for past levels of domestic resource concentration. This finding reaffirms the central point that dangerous neighbors have helped shape the distribution of internal power resources. Among the major powers, the more powerful the military capability of adjacent rivals, the greater has been the level of domestic resource concentration.

Table 2.3 reports the influence of threat and changes in domestic power concentration on the two democracy indices. The results are very similar both in models 1 and 2.[26] Threat and changes in power concentration share strong negative relationships with democracy, while controlling for the significant influence of past democracy values. This second outcome continues to provide encouraging support for our interpretation of democratization as at least a partial function of external menace and consequent reactions in internal power configurations. In this specific context (controlling for

Table 2.2 Domestic resource concentration on threat, 1850–1979

Variable	*Coefficient*	*Standard error*
Changes in threat$_{(t)}$	22.20**	5.02
Domestic resource concentration$_{(t-1)}$	0.88**	0.07
Constant	−1.95**	0.84
Residual Diagnostics		
Adjusted *R*-square	0.68	
Ljung-Box *Q* statistic ($df = 36$)	28.76	
Breusch-Godfrey *F*-statistic ($df = 2$)	0.97	
ARCH *F*-statistic ($df = 2$)	0.00	
Sample size	111	

Note: Panel corrected standard errors are reported below coefficients. Double asterisks indicate statistical significance at the 0.05 level or lower. Threat has been differenced due to autocorrelation problems.

Table 2.3 Democracy measures (Polity III and Vanhanen) on threat and domestic resource concentration, 1850–1979

Variables	*Coefficient*	
	Model 1 (Polity III)	*Model 2 (Vanhanen)*
$\text{Threat}_{(t)}$	−11.35**	−17.45**
	(2.64)	(4.20)
Changes in domestic	−0.26**	−0.49**
Resource concentration$_{(t)}$	(0.04)	(0.08)
$\text{War}_{(t)}$	−0.89	1.48
	(0.91)	(1.64)
$\text{Democracy}_{(t-1)}$	0.66**	0.64**
	(0.06)	(0.07)
Constant	2.67**	6.19**
	(0.61)	(1.33)
Residual Diagnostics		
Adjusted *R*-square	0.77	0.70
Ljung-Box *Q* statistic ($df = 36$)	36.40	32.85
Breusch-Godfrey *F*-statistic ($df = 2$)	0.32	1.31
ARCH *F*-statistic ($df = 2$)	0.07	0.96
Sample size	111	111

Note: Panel corrected standard errors are reported below coefficients. Double asterisks indicate statistical significance at 0.05 level or lower. Domestic power concentration has been differenced due to autocorrelation problems.

external threat, domestic resource concentration, and earlier levels of democratization), war has only a weak influence on democracy.

Table 2.4 reports the results of the pooled, cross-sectional time series regression of militarized disputes on threat, domestic resource concentration, and the two democracy measures separately (see models 1 and 2). Because the changes in the domestic resource concentration measure were correlated fairly highly with the democracy measures ($r = -0.64$ for the Polity III index and -0.71 for the Vanhanen index) and the bivariate relationship between disputes and domestic power concentration was zero, the concentration variable is estimated alone in model 3.[27]

The results in table 2.4 show that external threat has a strong positive influence on militarized disputes (model 1), while the coefficient for the Polity III indicator of democracy is negative but statistically insignificant.[28] Model 2, in contrast, shows that threat is statistically insignificant in the presence of the Vanhanen democracy index, which has a strong negative relationship with disputes. These results are not especially surprising given the strong bivariate correlations between threat and the two democracy measures. External threat is correlated at -0.61 with the Polity III measure and -0.62 with the Vanhanen measure. War and the lagged influence of disputes consistently have strong positive effects across all three models.

Table 2.4 Pooled cross-sectional time-series test of disputes on threat, democracy and domestic resource concentration, 1850–1979

Variables	*Coefficient*		
	Model 1	*Model 2*	*Model 3*
$\text{Threat}_{(t)}$	9.59**	5.64	11.16**
	(4.55)	(4.94)	(4.98)
Domestic resource concentration$_{(t)}$	—	—	0.01
	—	—	(0.06)
Democracy (Polity III)$_{(t)}$	−0.06	—	—
	(0.09)	—	—
Democracy (Vanhanen)$_{(t)}$	—	−0.13**	—
		(0.06)	
$\text{War}_{(t)}$	11.73**	11.82**	11.71**
	(1.85)	(1.85)	(1.87)
$\text{Disputes}_{(t-1)}$	0.25**	0.25**	0.24**
	(0.06)	(0.06)	(0.06)
Constant	3.84**	5.71**	3.71**
	(1.13)	(1.56)	(1.50)
Residual Diagnostics			
Adjusted *R*-square	0.65	0.65	0.65
Ljung-Box *Q*-statistic ($df = 36$)	41.60	41.40	39.04
Breusch-Godfrey *F*-statistic ($df = 2$)	0.84	0.38	0.73
ARCH *F*-statistic ($df = 2$)	1.98	1.63	1.79
Sample size	116	113	113

Note: Panel corrected standard errors are reported below coefficients. Double asterisks indicate statistical significance at 0.05 level or lower.

Collinearity problems aside, the outcomes reported in tables 2.2–2.4 reinforce the argument that democratization in the major powers has been influenced by their immediate threat horizons and domestic resource distributions, with the latter reacting to the former. All three variables appear to have some linkage to subsequent conflict behavior.[29] Yet the findings are based on only a few, aggregated-by-decade observations between 1850 and 1979 because that is the form in which the critical intervening variable of domestic resource concentration is currently available. Since our indices of threat, democracy, and conflict behavior are available in annual observations, we would be remiss if we did not take advantage of the opportunity to reexamine the relationships between threat and democracy, as previously examined in the results reported in table 2.3. Table 2.5 summarizes a statistical outcome that indicates that short-term changes in external threat are associated with a negative but weak and statistically insignificant change in the level of democracy. However, as hypothesized, long-term threats exert a very strong negative influence on democracy levels. Past levels of democracy have both short- and long-term positive influences on current levels of democracy while war has a positive but negligible effect.[30]

Table 2.5 Democracy on threat, 1816–1992 ($N = 1593$)

Variable	*Coefficient*
Changes in democracy	0.075**
	(0.027)
Changes in threat	−0.712
	(0.749)
Levels of threat	−0.638**
	(0.273)
War	0.052
	(0.137)
$\text{Democracy}_{(t-1)}$	0.977**
	(0.005)
Constant	0.146**
	(0.055)
Residual Diagnostics	
Adjusted *R*-square	0.98
Ljung-Box *Q*-statistic ($df = 100$)	91.30
Breusch-Godfrey *F*-statistic ($df = 2$)	1.89
ARCH *F*-statistic ($df = 1$)	1.15

Note: Panel corrected standard errors are reported below coefficients
Double asterisks indicate statistical significance at 0.01 level or lower.

Table 2.5, therefore, reconfirms that the immediate neighborhood makes some difference. Major powers with powerful contiguous rivals have been less likely to democratize than major powers in less threatening niches. Just why that may be the case is not pinned down in the equation but our earlier, decadal findings clearly point to domestic power concentration as an important part of the process. The outcome in table 2.5 suggests that it is not war participation per se that is responsible. That hardly precludes, however, the deleterious effects of constant war preparations as one of the principal processes historically inhibiting democratization. Nor does it eliminate the exceptional effects of specific wars—as in the cases of Germany and Japan in the aftermath of World War II.

We have established, therefore, that a type of peace—the relative absence of nearby external threat—encourages democratization. This is clear evidence, then, for a reversal of the causal arrow in the usual democracy → peace equation. The relative security insulation of the United States and Britain, a factor rarely overlooked in studies of their prominence, was also critical for fostering their leads in democratic development—precisely as anticipated by the war making–state making literature. The only difference is now we have some strong empirical support for a specific substantive interpretation of how the external environment makes some difference.

Whether this relationship is restricted to the major power elite sample is a question that will require more data and analysis. But, given the rather substantial role major powers have played in encouraging and discouraging democratization around the globe, the finding should retain significance beyond the small sample on which it is based. Another question that can be

addressed in the immediate context is whether the reversal of the causal arrow in the democratic peace equation is tangential or critical to the democratic peace argument. If a type of peace leads to democratization, is it possible that the immediate environmental effect is also responsible for the subsequent pacific behavior among democracies? Of course, a sequential effect need not be a zero-sum proposition. A less threatening environment and regime type could work together in producing democratic peace. This process could proceed over time with a more benign environment preceding the development of the appropriate regime type, with regime type then having direct consequences for subsequent regime behavior. Or, environment and regime type could work together in generating selected pacific behavior. The other possibility is that once we have established a peace → democracy linkage, regime type might fall out of the subsequent behavior equation as spurious. A relatively peaceful environment then might be directly responsible for the democratic peace.

To pursue this particular question, the data analyzed in table 2.6 were completely reconfigured from a monadic to a dyadic format. In doing so, we translate the individual state's immediate external threat into dyadic form by simply aggregating each pair of states' contiguous rival threats. Dyads with both states in relatively benign environments will enjoy the lowest external threat scores. Dyads with both states in relatively dangerous neighborhoods will continue to possess the highest external threat scores. Mixed cases should fall in between these ends of the dyadic threat continuum. The greater is the dyad's combined external threat, the greater should be their subsequent involvement in interstate conflict behavior. But we need to add information on dyadic regime type as well. Dyads are democratic if both members of the dyad qualify as democracies and nondemocratic if they do not. The actual analysis is based on equation 5 that also introduces statistical controls for autocorrelation in logit analyses.

Table 2.6 Logit estimates for militarized disputes, 1816–1992

Variables	*B coefficient*	*Robust S.E.*	*z-value*	*Odds ratio*
Threat	0.11	0.34	0.33	1.12
Democracy	−0.91**	0.32	−2.87**	0.40
Constant	0.10	0.18	0.57	—
Peace	−1.05**		−11.91**	
Years		0.09		0.35
Spline(1)	−0.03**	0.00	−8.67**	0.97
Spline(2)	0.01**	0.00	7.45**	1.01
Spline(3)	−0.00**	0.00	−3.47**	1.00
Log likelihood	−764.83			
Chi-square	434.30**			
Significance	0.00			
Sample size	2618			

Note: Democracy is coded as 0 and 1; 0 = nondemocratic dyads, 1 = democratic dyads. Robust S.E. refers to Huber/White estimates. Asterisks denote statistical significance at 0.05 level or lower. Spline coefficients are based on duration of Peace Years variable.

Table 2.6 demonstrates that while regime type is significantly related to militarized dispute behavior, as countless other analysts have discovered, external threat is not when we control for the number of years between disputes. Our preliminary analyses of the annual data showed that threat did have a strong positive influence on dispute behavior until we controlled for the influence of autocorrelation. Controlling for autocorrelation, the sign of the threat variable remains positive but the variable is no longer statistically significant. Another way of looking at this comparison of the two variables' impact on conflict is to focus on the odds ratio coefficients. The odds of democratic dyads being involved in disputes decreases by 60 percent in comparison to other types of dyads. External threat, in contrast, increases the odds by only 12 percent. Clearly, regime type is the more important of the two predictors for dyadic analyses of conflict.

No doubt, there are a variety of ways to view these last findings. Advocates of the democratic peace will suggest that they reaffirm the interpretative emphasis on the causal role of joint democracy in bringing about more pacific relations, albeit on a selective basis favoring one type of dyad. That is a possibility that cannot be denied based on table 2.6's outcome. The same findings could also be seen as discounting the asserted role of external threat in influencing conflict behavior. Our own view is that we need to proceed cautiously in moving from monadic to dyadic settings. Monadically, the empirical evidence supports quite strongly the external threat → domestic power concentration → democracy → external conflict causal chain. But the dyadic analysis takes the presence or absence of a democratic dyad as a given, without inquiring into the origin of each dyadic member as the monadic format permits. Thus, one quite plausible reason why regime type is stronger in the dyadic format than the external threat variable is because regime type represents an outcome shaped in the past by external threat. If that is the case, and we think that the monadic analyses suggest that it is, the two explanatory variables in the dyadic equation, to some extent, are measuring the same thing and the failure of both indices to retain statistical significance in the same equation is not surprising.

Conclusion

We view the full set of findings as further affirmation of a relationship between regime type and reduced conflict in general, but especially between democracies. Yet the present findings cannot be interpreted as suggesting that regime type alone influences subsequent conflict behavior—only that there is a significant relationship and it is one that cannot be attributed solely to the type of external environment in which the different regime types emerged. In chapter five, for instance, it is demonstrated that the presence or absence of strategic rivalries are more powerful predictors of conflict relationships than regime type. On the other hand, Oneal and Russett (1999) contrast the strength of democratic dyads with a large number of variables

as simultaneous predictors of militarized dispute involvement. Their evidence also suggests that the credit for reduced conflict behavior must be apportioned across several variables, and not just joint democratic regimes. Such findings caution against bestowing excessive credit on democracy for the democratic peace. But these findings also do not give any grounds for excluding regime type from the explanatory ensemble.

The relationships among external environment, regime type, and conflict behavior appear to be sequential as expressed in the following pattern:

Low External Threat → Low Domestic Resource/Power Concentration → Democratization → Less Conflict Behavior, both selectively and in general

One type of peace helps bring about democracy and democracy, among other types of influences, helps bring about another type of peace. This sequence is not so much a matter of reversing the causal arrow as it is one of delineating the temporal order of causal influences. The causal arrows are less reversed than they are reciprocal over the long run.

At the same time, we make no claim that democratization is exclusively influenced by external threat and domestic power concentration, but only that these are two significant influences on the probability of democratization. Other factors, such as economic development, ethnic homogeneity, and elite bargaining, can no doubt alter the probability of regime type change, or at least the probability that the changes persist, as well. We do not see the war making–state making emphasis as a rival explanation to other theses about the origins of democratization. Nevertheless, the war making angle on the balance of external and internal influences on regime type development has not been given adequate attention in other efforts to model democratization.[31] Hopefully, the present findings will work toward eliminating that bias in future studies.

This chapter has focused on only one of the dimensions of the democracy → peace versus peace → democracy problem. External threat apparently makes some difference in the probability of democratization taking place successfully. Yet another dimension of this problem is the threat-generation process. That is, the ambitious, expansionary states that create threats for their neighbors are also likely to thwart their own chances for successful democratization—presuming some movement toward more open domestic political systems is underway. This second dimension of the peace → democracy relationship is the subject of chapter three.

CHAPTER THREE

Questions of Regional Primacy

"Zones of peace" are neither necessary nor sufficient to produce democracies but, as demonstrated in the preceding chapter, they have been facilitative in fostering the development of liberal republican institutions and democratization. Similarly, the absence of peaceful environments has thwarted movements toward liberal democratization. When conflict diminishment tendencies have preceded the attainment of democratic status, we should be careful in attributing causality to the more recent development. It is of course conceivable that the relationship between democratic states and altered conflict propensities is reciprocal; but it is also possible that other variables are necessary to explain the origins of diminished probabilities of war between certain states.

One of the hitherto missing pieces of the puzzle pertains to the issue of regional primacy. Geopolitics must be given its proper due. The creation of zones of peace or areas in which states are much less likely to go to war with one another has as much, and perhaps more, to do with the settlement of, or restraints imposed on, regional primacy questions as it does the type of political system. In essence, most of the states that became (and remained) democratic in the nineteenth and early twentieth centuries had created or found themselves in relatively cooperative niches that insulated them from extremely competitive, regional international politics. The various ways in which these niches were established had important and positive implications for the likelihood of domestic democratization processes. Usually, the niches preceded substantial progress in democratization and, short of outright invasion, it is the geopolitical circumstances leading to the evolution of the niches that deserves some share of the responsibility for the consequences of diminished conflict probabilities among democracies.

The next section elaborates the rationale for the regional primacy argument. In a following section, four cases (Scandinavia, Revolutionary France, North America, and Taisho Japan) are reviewed. Two of the cases (France and Japan) represent situations in which fledgling democratization processes were suppressed by efforts to attain regional hegemony. The other two cases (Scandinavia and North America) show how democratization

was facilitated (and war between democracies made less likely) by situations in which the pursuit of regional hegemony was either exhausted early or constrained by extra-regional circumstances.

The Regional Primacy Angle

As we have seen in chapter two, a basic premise of the war making/state making perspective is that more liberal ruling arrangements tend to stem from exchanges with segments of the population made by war-making rulers hard-pressed for finances and manpower.[1] The rulers offered or were forced to surrender various degrees of political participation in exchange for the resources they needed to make and prepare for war. Initially, these exchanges tended to be restricted to bargaining within elite groups. Gradually, and increasingly so after the late eighteenth century, exchanges of manpower, taxes, and compliance for some semblance of political participation were extended to larger proportions of the adult population. In the nineteenth and twentieth centuries, not coincidentally, the scope of these franchise extensions depended in part on expansions in the number of people regarded as important to war making efforts, either as conscripts or war industry workers, and capable of paying taxes. How bureaucratized states became and precisely with whom (barons, merchants, peasants) rulers made their deals also depended on varying circumstances such as location, military technology, political economy, and the scale of warfare. Different types of political systems emerged as a consequence.

For instance, states that were heavily involved in trade could rely on customs levies to raise money for war purposes more easily than states that depended on taxing the more difficult-to-assess agrarian production and assets. Less bureaucracy would be anticipated in the former as opposed to the latter types of states. Similarly, states that could rely on natural defenses such as maritime barriers as opposed to large standing armies were less likely to develop authoritarian formulas for maintaining the defense of the realm. Alternatively, states dependent on cavalry levies for their military force were more likely to develop feudal arrangements. A powerful landed elite and a high degree of concentration in landholding were two likely consequences.

Whether a state's nobility maintained its concentrated powers and privilege depended in part on whether rulers could and/or needed to make different arrangements with other societal groups such as an emerging middle class or even the peasantry. Defeat in warfare was one of the more important factors in prompting reconsiderations of the wisdom of prevailing domestic winning coalitions. Defeat often opened opportunities, at least, for dismantling incumbent regimes and trial-and-error searches for new ruling formulas. Success in warfare and the expansion of state and empire, in contrast, tended to solidify the mutual dependence of the nobility and ruler—thereby diminishing the probability of occasions arising in which elites would have incentives to mobilize new political forces for political conflict.

Thus, the war–state building relationship is multidimensional. In some circumstances, war making can facilitate democratization while in others it can facilitate autocracy. More generally, though, frequent participation in warfare, and especially intensive warfare, tends to concentrate political power within a state because war making encourages and often rewards more authoritarian approaches to resource mobilization and decision-making. Even the threat of impending war can make decentralized power sharing arrangements seem relatively inefficient and undesirable. Whether relatively authoritarian or democratic at the outset, political systems are quite likely to become more authoritarian as they become engaged in crises of national security. Some systems may return to prewar arrangements at the end of the emergency period while others will not. Infrequent participation in warfare need not lead to less authoritarian political arrangements but it does make the diffusion of political power within a state at least conceivable.

Relatively peaceful regions, therefore, can facilitate the development of the gradual expansion of political participation processes that are geared to the nature of domestic political conflict, subject only to intermittent external stimulation. States in such regions enjoy some degree of insulation from the demands of external military competition. States not located in such regions lack this insulation unless more powerful states choose to protect them, thereby extending some degree of "artificial" and possibly temporary insulation. In the absence of meaningful and protracted external protection, political systems in regions characterized by high conflict will tend to become more authoritarian to the extent that they choose to pursue foreign policies of expansion or find themselves forced to defend themselves constantly against the threats of proximate states. An excellent example is provided by the central and east European experience between 1919 and 1939. A geopolitical opening (a weak Germany and Soviet Union and the victory of the older democracies in the context of new states and Wilsonian idealism) permitted and even encouraged a number of experiments with democratization in the early postwar period. The 1930s were a different story and few liberal or liberalizing systems in the region survived intact.

Thus, the geopolitical interpretation of the democratic peace being advanced here stresses, above all, both neighborhood contexts and expansionist foreign policies as two sides of the same coin. Other things being equal, the absence of expansionist foreign policies will lead to more peaceful regional neighborhoods. Expansionist foreign policies not only make democratization less likely in states seeking expansion and regional primacy but they also have implications for the likelihood of democratization (and survival) of other states in the same region. In addition, the interpretation has three further implications for war behavior. The abandonment of regionally expansionist foreign policies neither precludes expansive activity outside of the home region or even wars initiated by liberal democracies in their home regions, nor does it mean that liberal democracies are immune from attack from neighbors with different types of political systems.

It is coercive regional expansion that is most likely to have deleterious impacts on domestic power concentration. Expansion outside the home region, historically, could be done on the cheap particularly if the emphasis was on commercial expansion and the control of long-distance trade. And not only are cooperative strategies in the home region and aggressive expansion outside the region not contradictory, such behaviors are also probably interdependent. England's two empires came about only after the English were forced to abandon their territorial pretensions on the European continent after the Hundred Years War. Portugal and the Netherlands sought to stay out of regional European quarrels, to concentrate better on exploiting Asia and Brazil, but neither state managed to duplicate England's degree of home region insularity. Both lost their political–economic leadership partly as a consequence. Portugal lost its independence for 60 years. The Netherlands gradually evolved from a republic into a kingdom. In the nineteenth century, the scramble for Africa is attributable in part to the perceived costs associated with European territorial expansion. In this respect, expansion was displaced southward. In general, trading states preferred stability in their home neighborhood and, at times, other major powers found it more prudent to channel their aggressive impulses away from the home neighborhood.

All wars can not be attributed to schemes of regional primacy by one or both sides. But it happens that a good number of the European wars of the past five centuries, especially those involving Spain, France, and Prussia/Germany, did have something to do with regional primacy aspirations. Yet wars can be and are fought over extremely local issues of position and spatial control. Liberal democracies may initiate these disputes just as neighbors may initiate them against liberal democracies. These types of lesser-intensity conflicts may also be the best places to look for the operation of institutional and normative restraints. Even so, geopolitical factors should not be ignored in such settings either. For instance, republican Italy became more likely to attack autocratic Abyssinia in the late nineteenth century because it did not dare fight republican France over Tunisia.

While liberal democracies are unlikely to seek regional primacy through territorial conquest, the most prominent liberal democracies have had a historical propensity for becoming involved in thwarting the aspirations of regional hegemons, especially in western Europe. When regional wars became global wars twice in the twentieth century, the two most powerful liberal democracies, Britain and the United States, have had much to lose (in terms of determining the rules for the global political economy) and a great deal of incentive to create anti-hegemonic coalitions. A number of the older, smaller democracies were located in western Europe and in the way of the German challenges. States such as Belgium, the Netherlands, and Denmark were especially prone to invasion, as was in a different way, France—a natural target as the previous regional leader. Thus, democracies have been far from immune to warfare in their home regions. Any analysis of twentieth-century data on regime type and war participation, let alone

alliance patterns and international organization memberships, will find it difficult to control for the disproportionate impacts of the European balance of power wars and their outcomes in terms of the postwar world orders, the winning coalitions, and the structural hierarchies underlying them. The very structure of twentieth-century world politics is inextricably intertwined with the fate of democratic political systems.

A number of hypotheses on state making and internal/external linkages can be extracted from these arguments. Some of them have already been examined in chapter two. In this chapter, we confine our attention to the question of regional hegemonic aspirations and its relationship to democratization as another variation on the reversed causal arrow argument (peace → democracy).

H7: *States heavily involved in the pursuit of regional primacy and coercive expansion are the least likely to develop liberal republican/democratic forms of government.*

Regional hegemon aspirants were most likely to develop into authoritarian, highly bureaucratized, war-making machines. If war made states, then the states most involved in aggressively making and preparing for war (as implied by the pursuit of a coercive regional primacy strategy) were most likely to be shaped by their foreign policy ambitions and battlefield experiences. In European history especially, these same states, not coincidentally, were also likely to be characterized by strong economic inequalities, powerful nobility, and concentrated landholdings. In these states, rulers were most likely to make their exchanges with agrarian elites who dominated wealth making in these societies and who could help generate the resource mobilization needed for attempts at expansion. These same agrarian elites could provide the personnel to command the armies and manage the imperial bureaucracies. In return, their privileged share of political power and economic wealth was guaranteed at least as long as the expansive foreign policies remained successful.

Of course, not all states have attempted to gain regional primacy. But the ones that have are especially important to the democracy–peace thesis because it has been major powers with the capability to aspire to regional primacy that have been most likely to be involved in warfare.[2] Moreover, most warfare (in frequency terms if not necessarily in terms of battle-deaths) has tended to be regionally circumscribed. As a consequence, we do not really need to explain why widely separated democracies do not fight one another. States separated by considerable distance, again exempting the states with high capabilities, do not usually fight one another because they have so little opportunity to do so. The better question is why do democracies tend not to fight other democracies in their own home regions?

One easy answer is that many regions have not been populated by a sufficient number that could fight one another. But for those regions populated by two or more democracies, the principal answer is: (1) the possibility of democratization was facilitated by states either being forced to abandon their aspirations to regional hegemony, choosing not to pursue

regional hegemony, or some combination; (2) states in the process of becoming more democratic that also found themselves pursuing regional hegemony suppressed their immediate democratization chances in the process; or (3) states were not prevented from becoming more democratic in part because their region was not characterized by extensive interstate conflict in the first place.

This answer reverses the causal arrow in the conventional democracy–peace argument. The strongest version would say that it is not democracies that create the possibility of peace but the other way around. Some level of "peace," especially as indicated by the abandonment of expansionist foreign policies, made the historical development of liberal institutions and democratization feasible. Ironically, some experience with warfare was probably necessary to encourage the initial exchanges of rights and privileges between ruler and ruled. But the war making–democratization relationship is curvilinear. States that pursued regional expansion vigorously developed political systems characterized by concentrated political power. Once these states abandoned expansionist strategies, either because they chose to do so or were forced to do so, the possibilities for the domestic deconcentration of power were vastly enhanced.

How or whether political and economic power is concentrated affects the likelihood of the development and maintenance of a liberal republic/democratic form of government. Other things being equal, domestic power concentration probably also influences orientations toward regional expansion campaigns. Whether one stresses the old argument that the need to take care of landless noble offspring encouraged aggressive foreign policies or the more contemporary emphasis on authoritarian rulers seeking external diversions from domestic problems, the positive linkage is similar in effect.

But more important, political systems characterized by high levels of political and economic inequality are unlikely to be transformed into liberal democracies overnight. Privileged elites, threatened by the prospect of expanding political participation, will defend their vested interests and resist democratization. Thus, most democratization stories involve gradual, drawn out struggles between conservatives defending a status quo and reformers challenging the status quo. As long as political and economic power remains highly concentrated, conservative preferences possess an edge. It is only as domestic capability concentration erodes, barring revolution and foreign conquest, that the prospects for genuine reform become more probable.

Space limitations argue against developing a full-blown model of political liberalization/democratization here but the domestic concentration of power conceptualization is meant to encompass some of the arguments linking the development of less authoritarian political systems to the negative influences of such phenomena as the power of agrarian elites, highly concentrated landownership, economic inequality, restricted social mobility opportunities, limited presence of middle classes, low levels of literacy, low levels of economic development, and ethnic heterogeneity.[3] The implication is that these

ostensibly domestic phenomena are not givens. To a variable extent, they are created and shaped by actors attempting to cope with threatening environments while at the same time taking advantage of opportunities for advancing individual and group interests at home and abroad. This pulling and hauling among contending domestic groups (Rustow, 1970; Powell, 1973) is certainly critical to democratization processes.

There is no need to flatly deny some role for governmental type in restraining or decreasing the probability of external conflict. There is no need to substitute one single variable explanation for another. The argument here, however, is that the link from foreign policy orientation to war initiation probability is probably stronger than the link running from political system type to war initiation probability. How restraining institutional "checks and balances" really are is debatable, just as the contrasting image of capricious autocrats with nothing to lose in going to war is no doubt exaggerated. Legislatures do not always restrain executive branch war initiations. Authoritarian leaders are rarely free of all constraints on their own behavior. Nor are they always highly risk-acceptant.

Yet the argument (Morgan and Howard, 1991) that it is not political system type per se but simply the number of constraints that matters deserves further consideration. Alternatively, the status of the cultural/normative variant of democratization–peace arguments is more difficult to assess. If geopolitical circumstances create opportunities for liberalization/democratization that, in turn, creates institutional restraints and/or normative affinities, then it is probably the geopolitical circumstances that are most critical. Only if one could make a case for the normative affinities attributed to democratic dyads being responsible for the abandonment of expansionist foreign policies would we need to reverse the explanatory priorities. Yet once regionally expansionist foreign policies are abandoned by a state, it is not usually done in a way that discriminates among neighbors according to ideological compatibility. Thus, there does not appear to be any reason to treat the cultural/normative argument here any differently than the institutional argument.

Nonetheless, liberal democracies do encourage the formation of other liberal democracies in at least four ways. At least some of these activities also have pacifying effects. Strong democratic states have imposed democratic institutions on colonies that have been granted independence, major power foes that have been defeated, and new states that have been created as part of postwar settlements. However, these pressures are for the most part a twentieth-century phenomenon. Even so, the mere presence of democracies and successful transitions to greater democratization in the system stimulates and encourages liberalizing movements in more authoritarian systems. The American Revolution, the French Revolution and the European turmoil in 1830 and 1848 all had their reverberations among the populations of other states. Liberal democracies sometimes use foreign aid to encourage other states to develop or to maintain liberal governmental forms. The more powerful ones also attempt to discourage the destabilizing regional expansion policies of other states through alliance balancing, threats, bribes,

appeasement, and coercion. Hence, some liberal democracies have played crucial roles in protecting the very existence of other liberal democracies.

It is fairly easy to make the argument more complex. For example, the competitive political process within democracies might make it difficult to sustain a coercive drive for regional primacy. Anything short of fast and complete success would open the incumbents to criticism from the opposition. But there is no need to muddy further the conceptual waters. The main message is that some reduction in the probability of conflict propensities is viewed primarily as both an independent process and as an antecedent facilitator of the historical development of the older liberal democracies as opposed to strictly a consequence of regime type.

Rather than dwelling on the complexities of possible interactions between internal and external processes, it is more important to press on to a consideration of some of the evidence supporting the argument for "peace" preceding democracy as opposed to the other way around. Ideally, one might simply code all states for regional expansion histories and match the distribution and timing of this variable to the distribution and timing of democracy. The peace–democracy argument would be supported if, in most cases, regional expansion policies had been absent or abandoned prior to the advent of democratic regime types. Unfortunately, the argument does not lend itself readily to such a straightforward strategy at this time. Some states owe their democratic status at least in part to such varying influences as colonial legacies, defeat in war, and great power protection. Some states owe their democratic status to hundreds of years of development while other states have much shorter, relevant histories. And, perhaps most importantly, the regional primacy emphasis is only one facet of the argument essentially reversing the democracy–peace equation. It also applies best (even though it is not restricted) to major powers that have the capability of seeking regional primacy. Thus, an emphasis on expansionist foreign policies can only account for part of the puzzle even though it is oriented toward the states that are also most likely to have some influence on the striking absence of warfare between democracies.

Rather than seek to demonstrate definitively the relationship among regional primacy, war, and democracy, a more modest focus on a few cases demonstrating variance in their political system outcomes is examined here. Presented in chronological order, the first case is the Scandinavian region roughly between the fourteenth and early nineteenth centuries. The focus is placed primarily on Sweden and Denmark but also has important implications for Norway, Finland, and Iceland. The second case is Revolutionary France in the 1790s. The third case is the North American region in the nineteenth century, encompassing the interactions among the United States, Britain, and Canada. The fourth case, Taisho Japan, represented a situation involving substantial but short-lived movement toward satisfying minimal democratization criteria after World War I.

If the case selection bias is tilted toward major powers, it is also clear that the orientation is unorthodox. We do not normally think of Sweden,

Denmark, or the nineteenth-century United States as major powers. Yet Sweden and Denmark were once major powers in northern Europe, albeit not recently. The Scandinavian case is particularly interesting because it evolved from a highly conflictual arena to a relatively peaceful zone. As for the case of the United States, our tendency to date its major power status from 1898 betrays something of a Eurocentric bias. The two strongest powers in North (and South) America throughout most of the nineteenth century were Britain and the United States. Because of these positions, the two states clashed repeatedly from 1784 to 1895, but only went to war once. During this same time period, both states became more democratic and, if they had gone to war more frequently, the virtual absence of warfare between democratic states would have been less impressive.

The basic questions to keep in mind in reviewing these cases are: (1) whether a significant case can be made for a type of "peace"—absent or constrained regional primacy impulses—having preceded significant movement toward democratization, and (2) whether the presence or absence of this antecedent condition facilitated or diverted the attainment of some minimal threshold of democratization.

The Scandinavian Story

The Scandinavian states, primarily Denmark and Sweden (with Finland, Norway, and Iceland often subordinated to either Denmark or Sweden) represent the northern and southwestern sides of the Baltic region. Control of this region was hotly contested by Scandinavian states up to about the first half of the eighteenth century. As a consequence, relations between Denmark and Sweden and their immediate neighbors were characterized by war during the following periods: 1319–75, 1422–35, 1448–81, 1520–37, 1557–98, 1600–53, 1655–61, 1672–79, 1700–21, 1756–63, 1788–90, and 1805–15.[4] Many of these wars can be attributed to the rivalry between Denmark and Sweden for Scandinavian primacy. In both cases, regional warfare created domestic settings that were initially not conducive to democratization. Only when these states lost their capability to continue the struggle for regional dominance, and their decision-makers finally came to appreciate their changed circumstances, was it possible to reverse the domestic effects of years of attempted imperial expansion.

In the first phase of the period in which we are interested, ending roughly in the early seventeenth century, is a period of Hanseatic and Danish supremacies in the region. A second phase, about 1630–1720, is one of Swedish dominance. The third phase begins in 1721 with the Russian victory in the Great Northern War. After 1721, Russian dominance of the Baltic persisted until challenged by an ascending Germany in the late nineteenth century. One implication of this regional circulation of Baltic elite powers was the equally persistent downgrading of the ranks of the other actors in the region, including Sweden and Denmark.

In effect, both Sweden and Denmark were forced gradually to withdraw from great power competition even though both states continued to

participate in European fighting as lesser powers willingly or unwillingly in subsequent years. More to the point, the long-playing rivalry of Sweden and Denmark gradually became less hostile and less likely to break out into combat. After 1721, Denmark and Sweden opposed one another only twice more (1788–90 and 1807–14), but it was not until the end of the Napoleonic Wars that their rivalry was altered fundamentally. Denmark surrendered Norway to Sweden in 1814 while, at the same time, Sweden lost Finland to Russia. Sweden also lost its German territories in Pomerania at this time, thereby avoiding future territorial conflict with an expanding Prussia. Barton (1986: 361–62) stresses the significance of these territorial realignments in 1814–15 as a key to "the long peace in the North and the possibilities for internal development it allowed." Only Denmark, which retained control of German provinces, would be drawn into war over them in 1848–49 (with some Swedish assistance) and 1864.

Still, Denmark no longer shared a long border with Sweden. Sweden finally controlled adjacent territory it had long coveted but at the same time, it had lost its extended maritime (cross-Baltic) empire. The consolidation of its territorial control also was accompanied by a delayed coming to terms with its diminished status in regional politics. By 1815, Swedish decision-makers had accepted finally the demotion in international rank that had actually occurred in 1721.

But there was more involved in this change in status than simply matters of rank and foreign policy ambitions. The geopolitical changes in the Baltic region had significant domestic repercussions as well. Scandinavia largely escaped European tendencies toward feudalization but the imperial expansions of first Denmark and then Sweden encouraged the development of nobility heavily involved in military and bureaucratic service to the imperial state. War also encouraged attempts at the expansion of royal power. However, since these forays into absolutism usually came at the expense of aristocratic privileges, the phases of imperial expansion and decline were characterized by periodic constitutional reorganizations that ultimately worked in favor of reducing both royal and noble prerogatives.

At the end of the Thirty Years War, the Danish nobility had managed to reduce the powers of the monarch in exchange for accepting the ascension of Frederick III. The subsequent humiliation by Swedish armies in the 1658–60 war and the loss of Danish imperial territory east of the Sound led to a domestic reaction against the nobility who were blamed for the wartime debacle. A royal–middle class coalition acted in 1660 to give the king absolute powers. Denmark had had the most concentrated landowning pattern in Scandinavia. But by 1700, much of the land once held by the nobility was owned by other people and by 1807, 60 percent of the peasant farms were freehold (Barton, 1986: 371).[5]

The 1611–12 ascension of Gustavus Adolphus in Sweden was predicated on an agreement with the nobility to improve their status as well as not making war or peace without their consent. Some eighty years later, the Swedish King was granted absolute powers and had begun to reduce substantially the power and wealth of the nobility by transforming

approximately half of the nobility's estates back into crown land. As early as about 1750, Swedish land was equally divided among the crown, the nobility, and the peasants (Barton, 1986: 8–9).

Swedish absolutism proved to be short-lived. At the end of the disastrous Great Northern War, the *Riksdag* took advantage of the death of the heirless king to restrict severely the powers of the crown. Between 1718 and 1772, Swedish politics was characterized by the "Era of Liberty" and the quasi-parliamentary competition of the Hats and Caps factions. The Hats favored an expansionist foreign policy and were responsible for Swedish participation in the Seven Years War. The poor Swedish showing in the war undermined the support for the Hats and produced a Caps majority (favoring, among other things, coming to terms with Russian ascendancy in the Baltic) in the *Riksdag* between 1764 and 1769. However in 1772, a military coup dissolved the *Riksdag* and increased the powers of the monarchy. But more less-than-successful warfare between 1788 and 1815 and the opportunities to restrict the king's powers provided by subsequent succession problems reduced the level of political concentration (both royal and aristocratic) in the Swedish political system.

Both Norway and Finland were able to use the Napoleonic Wars as an opportunity to gain more local autonomy even though their formal allegiance was switched to Sweden and Russia respectively. Norway, in particular, had virtually no aristocracy and was able to adopt an unusually liberal parliamentary constitution for the times in 1814 and keep it as part of the negotiated transfer to Sweden a year later.

As H. Arnold Barton (1986: 370) contends,

> The decline of the nobility reflects the transformation of the Scandinavian kingdoms from expansive powers in the seventeenth century, requiring the services of a numerous administrative and military class and with ample means to reward it, to minor and contracting powers in the eighteenth century.

For Sweden, it took a bit longer than the eighteenth century to reconcile to the fact that it had become a "minor and contracting power," but the effect was similar. The relative power of Scandinavian monarchs, nobles, and non-aristocracy (and their share of land) oscillated between the sixteenth and eighteenth centuries, thanks in large part to the fortunes of war. However much credit war deserves, the oscillations ultimately worked in favor of constitutional reorganizations that facilitated the development of more liberal political systems in the nineteenth century.[6]

The Revolutionary France Story

In the early 1790s, France moved toward greater liberalization and democratization as part of the ongoing revolution that had begun in 1789. But its movement toward democratization was inextricably intertwined with

external threats and the subsequent expansion of France throughout western Europe. Precisely because internal and external politics were so interdependent, the probability that French democratization would continue indefinitely was low. In fact, the expansion of domestic political participation ended within a few years while the struggle to extend French hegemony on the continent continued for over twenty years (1792–1815). War and the last martial bid for European dominance undermined the initial prospects for democratization in France.

Just why the French Revolution began in 1789 is a hotly disputed subject and not one to which we need to devote much attention for immediate purposes.[7] For the most part, the piece of the story that is of most interest begins after 1789. The significant exception to this statement is the role played by the ancien régime's fiscal crisis due in large part to previous military expenditures. Most analysts accept this phenomenon as part of the revolution's causation. The dispute is over how much credit should be given to other factors. However one comes down on this question, the fact remains that the fiscal crisis triggered the revolution. Whether it was necessary or sufficient is a topic to be argued elsewhere. Suffice it to say that it is most unlikely that there would have been a revolution beginning in 1789 without the fiscal crisis.

The nature of the fiscal crisis was quite simple. The French government had accumulated debts due to its participation in earlier wars, most recently in the American War of Independence. As a consequence, much of its revenue was devoted to debt payments. This propensity was not unusual, either for France or other countries, but thanks to a long-term process of tax farming and other aspects of limited centralized administration, the French government actually received only a relatively small fraction of the revenues that were raised each year.[8] In 1788–89, governmental elites were surprised to learn that anticipated spending would greatly exceed expected revenues. To deal with the monarch's deficit crisis, meetings of groups of elites were initiated, thereby creating an opening for the traditional exchange of expanded elite privileges for increased financial support of the state. The question was how much it would cost the French monarchy politically in order to resolve its short-term spending problem.

Circumstances were less than propitious for a continuation of the old ways of doing things. France had never fully recovered from a long bout of economic depression in the 1750s and 1760s. The several years immediately prior to 1789 were also characterized by depression, bad weather, price fluctuations, and aggravated by the impression that a reduction in French protectionist regulations in 1786 had opened the door to a flood of British industrial goods. The apparent ineffectiveness of the government in dealing with all of these economic problems made it more vulnerable to attack and, at the same time, expanded the number of people interested in the concept of constitutional reform.[9]

France's economic problems and the perception of governmental ineffectiveness were paralleled by an equally long string of reversals and

failures in its foreign policy. In retrospect, one could argue that France had been declining relative to its European rivals ever since the 1714 defeat of Louis XIV.[10] Despite frequent involvement in eighteenth century warfare, no significant gains were discernible. Defeated by the British in 1748 and 1763, France had little to show for its victory in 1783. Britain's colonial losses, moreover, were overshadowed by the major gains in industrial development that it had realized in marked contrast to its continental rival across the Channel. France had also appeared powerless to prevent the 1772 Polish partition and, especially, the less remote 1787 Prussian intervention in the Netherlands against the internal party supported by France. Thus, the French monarchy appeared incapable of doing anything about either the state of France's economy or its declining position in continental and global affairs (Schama, 1989: 61–3; Stone, 1994: 56–63).

Other things being equal, it is conceivable that France might have become a constitutional monarchy in the early 1790s somewhat along the lines of Britain.[11] The monarch would have lost some of his authority to a legislature and government based on limited enfranchisement. But other things were not equal. The monarch lost much more than some of his powers, enfranchisement became universal, and several constitutional experiments were attempted. The end result was the ascension of Napoleon Bonaparte as emperor of France. All of these outcomes could be said to be byproducts of an escalating and reciprocal interaction between processes of internal and external warfare.

The crux of the situation was a divided set of politicians, including the monarch, in Paris debating the future course of France's governmental structure and faced with the threat of external intervention and civil war. Initially, the threat of external intervention was not as great as it may have appeared. For various reasons, Austria, Prussia, and Britain were reluctant to become involved in French domestic politics. All welcomed what appeared to be changes that promised to further weaken France and reduce its potential for threatening regional hegemony.[12] French émigrés, of course, were lobbying quite strongly for intervention but they were not having much success in their efforts. The threat of civil war and regional separatism was much greater, especially if the changes in Paris took on an increasingly radical hue—as they did.

The loss of power by moderates and the gaining of power by radicals was neither accidental nor did it occur in a vacuum. It was made probable by the interaction of real and perceived internal and external threats. Internal and external war must be given a lion's share of the credit for, among other outcomes, radicalizing the French Revolution.[13]

The initial movement toward war in late 1791 arose over the expressed desire to remove the threat to governmental survival posed by French émigrés clustered near French borders. The idea was appealing to a broad spectrum of competing elites for different reasons. More radical elites sought an assertive approach to the threat of counterrevolution that included attacking enemies in and outside of France. Hoped-for byproducts

were greater clarification within France on who supported and resisted the revolution and the forceful diffusion of the revolution throughout Europe. More moderate elites saw an opportunity to unite rival domestic groups in a fight with external opponents. It was also hoped that an invasion of the German principalities would reinvigorate the demoralized French army, which would then be more useful in establishing some semblance of domestic order. Even Louis XVI supported the idea of war. In his case, however, it was hoped that a French attack on the German principalities would stimulate Austrian and Prussian intervention that would lead ultimately to a reversal of the revolution and a restoration of the king's absolute powers.[14]

The one common denominator of the various war proponents was the desire to reassert France's once-leading position in Europe.[15] This sentiment clearly reflected continuity with prerevolutionary interests and is well captured in the following communication from Cardinal de Bernis to the Duc de Choiseul in 1759 (taken from Stone, 1994: 29–30):

> The object of the politics of this crown has been and always will be to play in Europe the superior role which suits its seniority, its dignity, and its grandeur; to reduce every power which attempts to force itself above her, whether by trying to take away her possessions, or by arrogating to itself an unjust preeminence, or, finally, by seeking to take away from her . . . her influence and credit in the general affairs [of Europe].

The way in which war actually broke out had something of a comic opera quality. The French first threatened the Elector of Trier with invasion if support for émigré activity did not cease. Although the elector and other German principalities immediately complied with the French ultimatum, Austria threatened to intervene if France moved onto German soil. According to Blanning (1986: 89), Austrian decision-makers thought their threat would help moderates and conservatives regain control over French policy, but the counterthreat instead had the complete opposite effect. Even though the original cause for the war no longer existed, the French desire for a war now shifted its focus to Austria because all or most of the initial motivations for war could still be served, in addition to the further aims of settling older, pre-Revolutionary grievances with Austria and deterring other possible major power interventions. France declared war first but Austria declared war on France before it learned of the French declaration.[16]

War between France and Austria led to a number of direct and indirect developments of particular significance. It encouraged the French to attack the Austrians in their Belgian territory in the belief that the Belgians, who had revolted unsuccessfully against the Austrians in 1790, would welcome their liberators. The initial French attack was entirely unsuccessful, which stimulated French politicians to intensify their search for scapegoats and

traitors and to accelerate the mobilization of resources to meet the newly acute external threat.

As part of the mobilization effort, the French army was transformed. Unsuccessful officers were removed creating unusual avenues of mobility for talented junior officers. Traditionally undermotivated ancien régime foot soldiers were augmented by large numbers of enthusiastic patriots. The large numbers proved useful in overwhelming opponents with smaller forces. New tactics were developed that exploited artillery, battlefield maneuverability, and the passion of the new recruits. Moreover, the relative scarcity of resources at home led to the decision that the French army would be forced to subsist on the resources it conquered abroad. To continue feeding and clothing large armies meant that more territory had to be conquered. It also changed the nature of logistical restraints on army movements. Keeping the armies in the field and away from Paris also appealed to the personal security of French civilian politicians. Thus, what had begun as a limited attempt to suppress the dubious threat posed by émigré bases of support turned into an expansionary conquest of the rest of western Europe, thanks to a heady mixture of revolutionary zeal and confidence and periods of spectacular military success.[17]

A French movement in the direction of Belgium and then the Netherlands by late 1792 finally convinced the reluctant British of the necessity of intervening in 1793. Once involved in the coalitionary wars against France, Britain was the one major power that consistently remained at war with France (except for a few months) until 1814–15. What had begun as a war between two states with both sides confidently expecting a quick resolution evolved into a global war that lasted for nearly twenty-four years.

A third development traceable to the outbreak of war was the end of the short-lived period of constitutional monarchy some six months after war was declared. Intermixed with the increasing radicalization of the revolution, some sort of liberal compromise among the monarchy, the aristocracy, and the non-noble elite was precluded in the early 1790s (Skocpol, 1979: 183; Price, 1993: 106). Wartime emergencies also meant the likelihood of a reconcentration of political power as opposed to the power deconcentration normally associated with liberal democracy. The simultaneous impact of external and internal war also led, temporarily, to the terror that involved the brutal elimination of not only people suspected of treason but also political opponents in a general atmosphere of a war mobilization crisis, continuing economic deterioration, and widespread revolt. By the end of the 1790s, a succession of civilian ruling groups were supplanted ultimately by Napoleon's military coup and, in due time, imperial rule. A number of analysts argue that, given the circumstances, military dictatorship was practically inevitable.[18]

Whether or not Napoleon was inevitable, his ascension underscores the interaction between domestic political regime and foreign policy expansion. The bickering civilian politicians were in the way of meeting effectively

renewed external threats and the opportunities to fully establish French hegemony in Europe. By this time, the expansion of France had become more important than abstract questions of popular sovereignty.

[By 1799] war had become a way of life, even a matter of necessity for the Directory. Only war could keep ambitious generals and disruptive soldiers out of France, only war could keep the armies supplied and paid, only war could justify the repeated abuse of the constitution, only war could bring the regime some badly needed prestige (Blanning, 1986: 196).

In the end analysis, J.F. Bosher (1988: 242) is correct when he notes that the national aspirations of France, which were highly appreciated by a majority of the population, proved to be stronger than the liberal aspirations entertained by a small group of politicians and intellectuals.

The North American Story

The border between the United States and Canada has long been an easy one to cross. The possibility of war between these two states seems inconceivable. But that was not always the case. The early history of U.S.–Canadian relations, moreover had more to do with Anglo-American relations than they did with U.S.–Canadian interactions. Although the invocation of the term Anglo-American relations is apt to stimulate an image of a "special relationship," this also was not always the case. Or, perhaps more accurately, the nature of the special relationship has undergone significant transformation over the last two centuries.

The absence of interstate warfare in North America after 1815 is as remarkable as the more gradual de-escalation of warfare between Scandinavian states. It was not due to a lack of interest in regional expansion on the part of U.S. decision-makers. For the most part, however, U.S. expansion moved to the south and west, and not to the Canadian north. If North America had been more heavily populated and multipolar, as in western Europe, it is hard to imagine the United States surviving international competition without being forced to create a strong military early on and without suffering the domestic impacts of frequent warfare. In this alternative history, U.S. democratization tendencies would have no doubt been affected and perhaps significantly diverted. If the United States and Britain had fought frequently over possession of Canada, it is possible that some of the more negative effects of war participation would have been realized. They did not fight frequently. Why they did not fight frequently is particularly interesting because it cannot be attributed solely to dyadic regime type. Both countries became more democratic in the nineteenth century, and especially in the second half of that century. However, their history of conflict and potential warfare stretches from 1783 to around 1895. Regime type may play some role but, clearly, something else must have been happening as well.

In the century or more following the American War of Independence, the United States and Britain increased the likelihood of their going to war

against one another some nine or ten times. Table 3.1 lists these occasions. These crises, militarized disputes, and war scares were not all of equal significance. Nor did they always involve Canada directly. In some cases, there is also good reason to believe one or both sides were bluffing. Yet, in each case, decision-makers on one or both sides perceived an increased probability of a war breaking out. Only one war, the War of 1812, did break out and this exception may be the one that proves the rule.

A number of miscalculations figure into the outbreak of war in 1812. Ian R. Christie (1982: 308, 319) suggests that the British assumed incorrectly that New England commercial interests, preferring to maintain trade ties with Britain, could restrain the prowar factions. Melvin Small (1980: 58–59) notes that British decision-makers had begun to move toward a compromise with American demands at the last minute but that the negotiations had been complicated by: (1) the assassination of the British prime minister and the slowness involved in naming a successor, (2) American

Table 3.1 U.S.–British crises

Year	*Issues*	*Comments*
1793–94	Canadian frontier posts, shipping restrictions	Britain conciliatory due to war with France and desire to keep North American supplies flowing, United States feared economic repercussions and also angry at French shipping
1807–08	Attack on *Chesapeake*	Britain initially conciliatory but United States imposed trade embargo that turned out to be too costly to U.S. economy
1812–15	Maritime restrictions, impressments, Canada, U.S. trade embargo	British concessions came too late to avert U.S. war declaration at a time when Britain was involved heavily in the Napoleonic Wars
1837–38	U.S. private assistance to Canadian rebels	U.S. government attempted to punish neutrality violations, neither side wished to fight
1838–41	Maine–Canada boundaries, Canadian official on trial in U.S., slave trade tensions	Canadian official acquitted, Britain at war elsewhere, negotiations successful
1845–46	Control of Oregon and Texas	Britain did not value Oregon highly, poor economic conditions discouraged war in Britain, both sides made concessions
1854–56	Central American canal, fishing rights	Britain involved in Crimean War and chose not to press grievances
1861	U.S. boarding of *Trent*	U.S. conciliatory during early stages of civil war
1862–63	Intervention in American civil war	British decision hinged on assessment of whether the civil war was likely to continue indefinitely, need for intervention made less likely as Northern victory becomes more likely
1895	Venezuela–British Guiana boundaries	U.S. securities values dived, British Guiana not valued highly, Germany showing support for Boers at same time, Britain conciliatory toward United States

Sources: Bailey (1958), Burt (1961), Bourne (1967), Campbell (1974), Wright (1975), Jones (1980), Brauer (1984), Field (1984), Chamberlain (1988), and Jones (1992).

unawareness of Napoleon's defeat in Russia (thereby decreasing British vulnerability), and (3) the extreme slowness of communications across the Atlantic. However, Reginald Horsman (1962: 264) maintains that the general problem was that the British preferred the risk of war with the United States to the risks associated with altering its stance on neutral trade rules and a possible victory for Napoleon. Nevertheless, and this exception notwithstanding, the reasons for the relative absence of war between Britain and the United States are fairly clear in retrospect, although, naturally, they are subject to variable interpretation.

The general pattern of nineteenth-century Anglo-American crisis behavior can be summarized in the following way. Both sides were well aware of their high degree of economic interdependence. The United States was dependent on Britain for a sizable proportion of its trade and financial investment. Britain was dependent on American raw materials. The evidence suggests that economic interdependence was always fairly high and tended to increase throughout the first century after American independence. Nevertheless, while economic considerations were always in the background, it was never clear, except perhaps prior to 1812, that economic interdependence was a major barrier to the onset of war. At times it may have even contributed to increasing the probability of war. On more than one occasion, decision-makers used interdependence as an argument for anticipating, incorrectly, that the other side would back down during crisis maneuvering.

Strategic considerations were a more important factor. British decision-makers were always aware of their military vulnerability in Canada and the West Indies. The United States could attack in either direction without encountering too much opposition. American privateers had earlier demonstrated the vulnerability of British shipping. British decision-makers had also learned the hard way that it was difficult to contemplate a successful conquest of the United States. The territory was too large and the political economy too decentralized. On the American side of the equation, U.S. decision-makers were always cognizant of their own lack of preparedness either to launch an attack or to defend themselves against somebody else's attack. In particular, they recognized the difficulties of defending an extremely long coastline against the world's leading maritime power.[19]

Equally if not more crucial, British decision-makers consistently seem to have regarded the issues in contention in crises with the United States as having less than vital importance to their own perceptions of British national/imperial interest. Britain may have been the most obvious foreign threat from the American perspective, but the perception was not reciprocated. Throughout the nineteenth century, British decision-makers were more concerned about European, Middle Eastern, and Asian problems than they were about North, Central, and South American issues. There was always the fear that military involvement in the Americas would encourage the activities of European rivals (at different times, France, Russia, or

Germany), just as the 1775–83 fighting had encouraged French, Spanish, and Dutch assistance to the American rebels. Quite often, Britain was already involved in other, more important conflicts when American crises developed. A good example was the 1838–41 interval of compounded tensions that did directly involve Canada in terms of disputed boundary lines and American assistance to Canadian rebels. While British decision-makers contemplated war with the United States over the fate of a Canadian official on trial in New York, they were already fighting wars in Syria, Afghanistan, and China. As it happened, the official in jeopardy was acquitted and the official U.S. stance had been consistently conciliatory thereby ending the crisis, but it is hard to imagine the British enthusiastically committing themselves to a fourth war.

There is no need to ignore totally the possible mediating role of shared culture, language, institutions, and values. Yet these same presumably integrative attributes were also sources of friction. In the first half of the nineteenth century, British decision-makers feared the domestically destabilizing consequences of what was regarded as an excessively democratic republic across the water. U.S. decision-makers, for their part, stressed the inegalitarian and oligarchic nature of the British political system. John Bull and Cousin Jonathan may have spoken the same language but they did not always have much use or respect for one another. Anglophobia was a persistent feature of the American political landscape throughout the nineteenth century.

A final factor worth considering was the opportunities for relatively easy expansion that were open to the United States in the nineteenth century. Britain could thwart northern expansion but its opposition was much less significant, both as a matter of choice and military logistics, in the west. If, in the 1840s, Texas had become a British dependency or if Britain had accepted Mexico's offer of California, the circumstances might have been much different. Neither of these possibilities did occur even though they were contemplated by British decision-makers. Instead, United States western expansion came primarily at the expense of a relatively weak southern neighbor, Mexico. Britain only had to surrender part of the Oregon territory that it did not think it could defend very easily and that it did not care all that much about in the first place.

The point to be stressed in this telling of the early U.S.–British relationship story is that for about one hundred years after U.S. independence, Anglo-American crises were characterized by a variety of geopolitical factors (British preoccupations elsewhere, British/Canadian vulnerability to U.S. attacks, U.S. lack of preparedness, and so forth) that help to clarify why these crises often had negotiated outcomes. An alternative explanation predicated on changes in the form of government is possible. Michael Doyle (1986: 1156), for example, suggests

> . . . During the nineteenth century, the United States and Great Britain engaged in nearly continual strife; however, after the Reform

> Act of 1832 defined actual representation as the formal source of the sovereignty of the British parliament, Britain and the United States negotiated their disputes.

We are not in a particularly good position to assess how much of the variance explained is attributable to geopolitics or form of government, but it should be clear that republican institutions cannot claim all the credit. Anglo-American differences were negotiated both before and after 1832. Yet the geopolitical reasons that help to account for the outcomes appear to have been maintained from 1783 to at least 1895. If we had to choose between the two types of explanation—and it is not argued here that we must—the geopolitical factors (regional politics constrained by the threat of external force) seem relatively more persuasive.

But what about after the United States had narrowed the capability gap between itself and Britain? At the end of the nineteenth century, a Britain in decline was confronted with two principal challengers, Germany and the United States. Britain chose to fight the former and ally with the latter. Why did it not fight both threats to its global position at the same time or in sequence?

Randall Schweller (1992: 251) argues that democratic leaders never seriously consider attacking a democratic challenger:

> Posing no threat to each other, democratic states tend to view their relationships with other democratic states in positive-sum, rather than zero-sum, terms. This is not to say that a faltering democratic hegemon graciously concedes its leadership to a democratic aspirant; rather, it says that preventive war is never seriously considered, because both states realize that their competition can be mutually beneficial as long as it remains peaceful. Hence, despite the eclipse of its relative power, the declining democratic state is satisfied with an increase in its absolute gains through accommodation with the democratic challenger.

The problem with this argument is that it demands evidence on decision-maker perceptions in a declining, democratic hegemon. Even though there is only one genuinely suitable case so far, Britain and an ascending United States, Schweller offers no evidence of positive-sum perspectives on the part of the British in the period leading up to 1914. Instead, he provides a cross-tabulation of some 30 cases—most of which do not involve hegemons, however defined, in confrontation with challengers.

Positive-sum perspectives on relative decline and upstart challengers with democratic political systems may be asking a bit much. A simpler approach can be related to figure 3.1's emphasis on regional primacy as well as the nature of the Anglo-American nineteenth-century relationship. If faced with a choice between two or more challengers, declining system leaders will choose to oppose the most proximate threat. Between the sixteenth

century and 1945, declining global leaders located on the fringe of Europe periodically fought ascending European regional leaders.[20] The most dangerous threat was the one closest to home. This is why the late seventeenth-century Dutch moved away from their initial confrontation with the English to face the adjacent French threat.[21]

It also helped that the English goal had been to reduce the Dutch lead in trade while the French sought to supplant it completely. Somewhat parallel, the late nineteenth-century German threat was not only closest to home in the geographical and military senses, it had also been manifested in the Middle East, Africa, and Asia—all areas considered to be of primary interest to Britain. The U.S. threat was seen by British decision-makers as less extensive and, therefore, less acute.

The Taisho Japan Story

The Taisho adjective refers to a decade or two (roughly 1910s–20s) in Japan's political history that is considered an era of movement toward liberalization and democratization. Yet the extent to which Taisho democracy had progressed by the early 1930s was still not sufficient to satisfy the minimal threshold for most definitions of liberal republic or democracy. Thus, the Japanese case does not represent the same sort of case that revolutionary France did. However, it does delineate an extremely interesting case of a liberalizing, major power "regime" that pursued cooperative strategies in its foreign policy but gave way to an increasingly authoritarian "regime" that pursued anything but cooperative strategies and initiated the Pacific War.

In the 1920s, Japan participated in the League of Nations, concluded agreements with other powers on disarmament, introduced universal male suffrage, started administration by major party rule, and was intellectually open to Western political thought and ideology. In the 1930s she withdrew from the League of Nations, launched military aggression on China, and finally, dissolved political parties and trade unions and organized a military garrison state that attacked Pearl Harbor (Kato, 1970: 217).

Not only does Japan in the 1920s and 1930s present us with an almost perfect case of one type of foreign policy behavior giving way to the exact opposite behavior, it also is a case in which the formal regime never really changed. Japan's constitution in 1941 was the same Meiji one it had had since 1889. But a number of other things did undergo change, with some significance for our principal question concerning the direction of the link between domestic regimes and foreign policy. In brief, the Taisho Japanese case illustrates how dependent domestic liberalizing movements are on facilitative geopolitical environments. Once the environment changed, the Taisho experimentation was impelled toward much more brutal domestic political strategies as well as more aggressive external maneuvers.

Japan was forced to end two-and-a-half centuries of isolation in the mid-nineteenth century by Western sea power. Unlike most other Asian states,

Japanese decision-makers chose to respond by competing with the West. To do so required a period of economic and military development to create a "Rich Nation, Strong Army" that would be capable of competing on equal terms with the great powers.[22]

The Meiji Restoration regime that succeeded the discredited Tokagawa regime was managed by a genro oligarchy that, among other domestic reforms, created a state format that resembled at least superficially those of Japan's Western rivals. From 1889, Japanese political institutions included a constitution, a bicameral parliament, and a prime minister/cabinet head of government system. The initial inspiration for these institutional innovations was twofold. One, great power strength was equated in part with the governmental formats most commonly found in Western political systems. Two, disputes within the oligarchy required some type of institutional forum for expression if they were to be controlled effectively. In addition, new political groups with policy preferences were beginning to emerge that also needed to be organized and constrained in some fashion.

A multiparty parliament offered one way to achieve these goals, as long as its powers were strictly limited. Accordingly, prime ministers were selected by the genro from among their own group. Cabinet ministers derived their authority from the Emperor and had nothing to do with majorities in the House of Representatives. Indeed, the cabinet had the power to dissolve the Diet rather then the other way around. The parties, in turn, were linked only weakly to popular constituencies with approximately 1 percent of the population eligible to vote in elections (Reischauer, 1964).

The gradual expansion of relative legislative powers beyond what was initially designed is a well-known phenomenon in Western political history. The process worked fairly quickly in Japan thanks in large part to a series of wars with China, Russia, and Germany between 1894 and 1918. Wars require increased expenditures that lead to the need for increased revenues and taxes. One power that was granted to the House of Representatives was the responsibility to approve governmental budgets. A recalcitrant house would mean increased resistance and less capability to expand and finance military resources. Thus, after 1895, the ruling oligarchy and the governmental bureaucracy increasingly sought to create ties of affiliation with the political parties, as opposed to regarding them as intractable opponents. It helped that party tactics had changed from intense confrontation with the government to accommodation. By 1918, the first party-based cabinet was constructed and in 1924, the first cabinet government to lose power in a general election due to a shift in party support occurred. At least some of the foundations for democratic constitutional government were in place (Duus, 1968: 235).

The institutional changes were facilitated by other domestic changes. The Meiji genro were losing their political control due to the dwindling number of surviving oligarchs. Industrialization, especially during World War I, led to increased urbanization, an expanding middle class, and greater

industrial concentration (*zaibatsu*). As the ability to pay taxes expanded and the war-driven need for more taxes increased the political logic of exchanging some form of political participation for material support led to expansions in the right of suffrage. The size of the population eligible to vote doubled after the Sino-Japanese War and almost tripled after World War I before quadrupling in 1925 with universal suffrage (Berger, 1977: 14).

At the same time, the increasing significance of the *zaibatsu* provided an important source of financial support for party activities. In return, the political parties increasingly promoted the types of policies desired by *zaibatsu* interests. In the aftermath of World War I, these interests were focused on lower taxes, economic stability, and the avoidance of conflict and expansion on the Asian continent that might interfere with more peaceful approaches to expansions of market control. Such an approach also implied fewer resources for the army and navy.

For the most part, governmental policies in the 1920s came to reflect these policies, with particular emphasis placed on attempting to cooperate with Western great powers. The Siberian expeditionary force was withdrawn. World War I gains in the control of Chinese territory were surrendered. Limitations on the size of the navy were accepted. The size of the army was reduced.[23]

The interaction of three factors derailed this "experiment" in evolutionary democratization.[24] One factor involved a consistent commitment to a strategy of expansion in East Asia. Another factor was represented by the primary agent of this strategy, the military. The third element was the world depression of the early 1930s and other changes in the external environment that were not conducive to the survival of cooperative foreign policies.

The initial impulse to create competing political parties emerged from elite disagreements over the appropriate strategy to pursue in East Asia as early as 1872 (Scalapino, 1962: 42). There was never much dissent over whether the Japanese should expand their territorial control on the continent.[25] The principal question was when and where. The "when" question had two basic answers. One approach cautioned patience and caution with an emphasis on developing Japanese capabilities first so that any external resistance to expansion could be managed. The other basic approach argued for seizing or making opportunities for expansion as they became available and preferably before other parties developed even greater capabilities to oppose Japanese expansion. An early expression of this strategic debate broke out in the arguments over whether to invade Korea in the early 1870s. The cautious school of thought won, war was not declared on Korea on this occasion, but the debate was hardly resolved.

The debate continued in part because there was little opposition to the idea of hegemony over East Asia. There were only differences of opinion over specific strategies and timing. As a consequence, the primary agents of coercive expansion were consistently oriented toward East Asian expansion as an accepted, principal strategic objective. When government restraints posed obstacles to pursuing this mission, the military, especially the army,

were increasingly inclined to lobby strongly at home both for more resources with which to expand and more forceful action by the government. Eventually, military extremists began to go beyond mere lobbying and resorted to terrorism and attempted coups to remove politicians who stood in their way. The army also began to act independently abroad to force the government's hand in pursuing more aggressive policies. The 1930s manufacture of incidents by the army in Manchuria is well known. Less well known is that similar attempts had been made as early as 1912 and 1915 (Hata, 1988: 279). The point is not simply that there was persistent opposition to more cooperative foreign policies. There was but a problem in dealing with this opposition—it was so difficult to completely reject their ultimate objectives.

The debate continued as well because the geographic focus of the argument shifted over time due to the growth of the Japanese Empire. As Peattie observes, Japanese conceptions of strategic interest were based on drawing concentric circles around first the home islands and then imperial outposts as they were absorbed by the empire (Peattie, 1988: 219–20). The focus in the late nineteenth century was Korea. After Japanese control of Korea was accomplished, attention shifted to Manchuria and Mongolia in the 1910s through the early 1930s. After Manchuria was acquired, the rest of China became more important as a source of threat. In the early 1940s, circumstances and still more debate on the superiority of northern versus southern strategies led to southeast Asia becoming a core strategic problem.[26] The strategic debate was unlikely to be resolved as long as the nature and location of the problem kept shifting. Expanding imperial boundaries almost guaranteed "turbulent frontiers" (see Thompson and Zuk, 1986) and the urge by some to settle perceived defense problems by further expansion of territorial control.

Perhaps most important to the outcome of the strategic debate were the multiple changes in the external environment that contributed to the domestic ascension of the more risk-acceptant approach to regional expansion. Berger identifies four or five changes that were most critical in the late 1920s and early 1930s (Berger, 1988: 102).[27] Chiang Kai-shek had reinvigorated the nationalist Chinese movement and threatened Japanese control in Manchuria. The Soviet Union had been strengthening its Far Eastern military capability. There was little Western governmental sympathy for what was being perceived as an increasingly threatened continental position by Japanese decision-makers. Moreover, it appeared after the 1930 London Conference that Britain and the United States had aligned against Japan in order to restrict its ability to defend its Asian and Pacific interests.[28] The corollary to this perception was that the incumbent government that signed and ratified the London Conference restrictions on Japanese naval size appeared to be unable to defend the country's national security interests. Finally, the world depression made international economic cooperation much less likely in general. In Japan, the effects of the depression were more intensely felt in the agrarian part of the economy that had always been a

source of support for more authoritarian political approaches. Economic hardships only intensified rural alienation and discontent—an issue of great importance to many junior officers recruited from the countryside. More generally, the ability of party governments to appear effective in managing domestic economic problems was undermined.

A sixth factor that interacted with some of those cited by Berger was another consequence of World War I. The Japanese military had been impressed by the way in which Germany had been defeated and its implications for how the next war would be fought. In the future, war would entail a total mobilization of human and material resources. If one's national stock of resources were limited, it was all the more imperative that access to the most critical resources be assured. This expectation of total war, then, increased the sense of urgency many military officers felt in the need to secure control of Manchurian coal and iron. To survive the next war, some level of self sufficiency had to be obtained.[29]

The external environment had become much more threatening at the same time that a quasi-liberal party system had begun to appear quite incapable of dealing with Japan's various internal and external problems. It did not help that the two leading parties found it impossible to cooperate in a time of crisis. None of the parties had developed effective linkages to the masses, especially in rural parts of the country, despite the still highly agrarian nature of the Japanese economy. They had no supporters to mobilize in response to threats by non-liberal attackers. It is unlikely that many people even believed in the desirability of a liberal democratic political system.

In 1931, segments of the Japanese army independently initiated a war of expansion in Manchuria. Rapid success at little cost meant that the war could not be disavowed or even criticized by the government. A year later, the assassination of the prime minister and army demands for a suspension of party cabinets created a political crisis that was only exacerbated by the continued threats of extremist terrorism and factional infighting within and between the major parties. The political solution was to take governmental control away from the party politicians at least temporarily, for the genro still continued to appoint prime ministers.[30] Once the parties had lost control of the cabinet, their political fortunes began a rapid downward spiral. Moderate party leaders risked assassination. The inability to control governmental policies meant a loss of control over government patronage. The *zaibatsu* withdrew financial support. It became increasingly difficult to recruit new party members since there was no longer much to be gained from participating in party activities. By 1941, the parties had voted themselves out of business.

Some analysts have taken the Taisho parties to task for their earlier disinclination to challenge the constitutional status quo.[31] Since they had not radically transformed the institutional structures in which they operated or managed to reduce the political clout of rivals for power, it is certainly true that it was far less difficult than it might have been otherwise for other groups to push them aside. But if the parties had attacked the Meiji

constitution in the Taisho era, it seems unlikely that there would have been a relatively liberal interlude in prewar Japanese political history.[32]

Another approach suggests that it was the movement toward liberalization that triggered the counter-movement toward greater authoritarianism. Depending on how one elaborates the argument, this thesis has some attractiveness. Duus (1976: 194) points out that foreign policy had once been a genro monopoly but by the time of the Taisho era, foreign policy had become "a complicated struggle among a welter of forces [involving] the cabinet, the foreign office, the military services, the Diet, and surviving genro." In a period of institutional transition from genro oligarchy toward Diet-cabinet political leadership, the situation was highly vulnerable to the intervention of civil and military bureaucrats. This was all the more the case if neither the remaining genro nor the quarreling parties appeared to be in control or pursuing the appropriate strategies in the midst of what was perceived to be a time of national emergency.[33] To obtain external expansion, therefore, it was necessary to attack the fledgling democratic institutions that stood in the way (Reischauer, 1962: 164). Moreover, in a time of economic and military crisis, party politicians were increasingly seen as lacking the expertise needed to make decisions about economic centralization and military mobilization.

As Taichiro Mitani (1988: 55) notes, a set of conditions developed that facilitated the rise of party cabinets in spite of the intentions of the designers of the Meiji constitution. But none of the conditions were irreversible. If enough of the facilitative factors were altered substantially, a quite fragile political system would be faced with a serious crisis. "Taisho democracy" faced such a crisis in the early 1930s largely due to external developments that were beyond the control of Japanese politicians. Admittedly, other, earlier, external developments had been important in facilitating the experiment with party controlled government. However, the external changes of the late 1920s and early 1930s, in conjunction with the persistent commitment to East Asian hegemony, worked together to conclude the Taisho liberalization phase.

The Timing and Impact of Diminished Conflict Probabilities

The Nordic states in Scandinavia were forced to relinquish their regional hegemony aspirations well in advance of substantial progress toward democratization in the mid-nineteenth century. In North America, geopolitical restraints on the probability of an outbreak of war characterized the Canadian–U.S. border as early as 1815, and especially after the 1840s—prior to the development of a Anglo-American dyad characterized by much democratization.

In each case, orientations toward coercive regional primacy were critical facilitators of subsequent gains in democratization. War and regional expansion in Europe and Asia, respectively, clearly doomed the experimentation

with greater democratization in 1790s France and 1920s–1930s Japan. Demotion from the ranks of major powers helped transform political, social, and economic inequalities within the Scandinavian states. The British–U.S. standoff in the nineteenth century contributed greatly to the unusually demilitarized nature of both international relations and state making in North America. This is not meant to imply that warfare has had no significant impact on North American state-building and popular participation. That is hardly the case. Rather, the impact of warfare, and preparations for warfare, was strikingly intermittent (the Civil War and World War I) at least prior to 1945.

More to the point, the *antecedent and subsequent* pacific relations among this democratic sample can be accounted for in terms other than their form of government. The implication is not that democratic peace arguments are necessarily wholly spurious but it is doubtful that the lion's share of the historical credit for the relative absence of combat between democratic states can be traced directly to the emergence of new domestic regimes. The case is consistently stronger for the reverse argument. Diminished conflict propensities or geopolitical constraints on conflict escalation preceded, and in many cases actually facilitated, democratization processes in the early waves of liberalization.

Conclusion

Does this mean that the multiple arguments about regime type have been proven to be wrong? The answer must be no. The geopolitical stories cannot "prove" that liberal institutions are not the main reason democratic states have not gone to war with each other. However, a rather strong argument can be made that different types of geopolitical constraints, associated with orientations toward regional primacy issues, emerged independently of, and prior to, the onset of liberal institutions. Democratic peace arguments may not be entirely spurious. The plausibility of some reciprocal relationship between regime type and war probability is readily acknowledged. But, at the very least, by looking only at regime type attributes we have exaggerated their influence and explanatory potential at the expense of other historical factors such as geopolitical context.

Democratic peace arguments emphasizing regime type may well be more parsimonious in explanatory form but that hardly precludes the possibility of their explanatory powers being illusory. It is also curious that nothing resembling a consensus has emerged as to just what is it about democratic regimes that makes them less likely to go to war with other democratic regimes. The foci range from institutional constraints through attitudes to crisis signaling and proclivities for third party mediation. In other words, proponents of regime type explanations are still groping for the key to their puzzle. The argument that has been presented here is simply that the key may well lie elsewhere than within the parameters of regime type. Of course, the four cases scrutinized in this examination cannot

determine conclusively the merits of the argument. But they do appear to be highly supportive. At the very least, further exploration of this geopolitical interpretation appears to be warranted.

We think chapters two and three have at least established a basic foundation for the claim that peace can facilitate the development of democracy. No doubt, there is more that can be done and should be done to explore this particular puzzle of the democratic peace phenomena. In chapter four, however, we turn to questions about the relative strength of regime type, which, presumably, can constrain more forceful activities in external spheres, versus other considerations, especially ones such as rivalry that encourage more coercive approaches to resolving international disputes. Chapter four is particularly concerned with the lingering strength of various types of path dependency—of which the quest for regional primacy reviewed in this chapter is a good example.

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PART THREE

The Relative Strength of Regime Types in Coercive Contexts

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CHAPTER FOUR

Path-Dependencies and Foreign Policy

There has been a tendency to pursue regime type explanations as if arguments about democracy and autocracy are likely to be necessary and sufficient. Democracies do not fight other democracies while autocracies fight both democracies and autocracies. Yet, not only is it fair to say that we do not know for sure what it is about regime type that might restrain conflict within some dyads, we also do not know how much relative explanatory credit to give to regime type. Let us assume for the sake of argument that dyadic regime type does restrain war making propensities within democratic dyads. Surely, few would contend that regime type is the only variable of interest or influence. Only if all democratic states were equally likely to go to war with any nondemocratic state at any time could one insist that explanations involving democratic dyads were sufficient to account for why states go to war. No analyst is likely to be comfortable with such a claim.

The question, therefore, should not be solely one of why democratic states do not fight one another. We also need to ask how regime type considerations interact with other influences in order to account for differential war-proneness tendencies. The argument that is pursued here is that interstate warfare is in some part attributable to a combination of external pressures and internal path-dependencies. The probability of one state fighting another hinges in part on considerations such as geographical location, rivalries, and power distributions between the states in question and within the region or neighborhood in which the states are located. Moreover, any state may possess certain internal characteristics, other than regime type, that make war with certain opponents at some time more probable. If these characteristics are genuinely idiosyncratic, they will not yield much explanatory power. If they tend to reoccur in different circumstances and different places and some level of generalization becomes conceivable, the explanatory utility of these factors becomes more attractive. More specifically, it will be argued that path-dependencies, such as irredentism, the perceived need to break out of a containment system, or something resembling a collective inferiority complex, make some

generalizable difference in accounting for the paucity of warfare between democratic states, as well as between other combinations of regime types. Furthermore, internal path-dependencies are often themselves causally related to antecedent war experiences. Advances in democratization, too, may be traceable to war participation. At the very least, the domestic political institutions → war behavior link should be viewed as a more historically contingent and contextually sensitive, war → institutions and internal path-dependencies → war linkage.

To pursue these issues further, we first need some preliminary sense of the extent to which regime type is useful in accounting for war decisions. Most empirical studies have relied upon highly aggregated correspondences between regime type and war participation. In this examination, an unusual purposive sample is proposed. Twenty-four states have moved in and out of the relatively democratic category through the early 1990s. The question is whether these states have behaved differently when the democratic "switch" has been on, as opposed to periods when it was off, and especially within the contexts of internal path-dependencies and fluctuations in relative power distributions. Have these states been more or less likely to initiate war (in contrast to simply participating in them) when their political systems are more or less democratic? If war initiations are associated exclusively with one type of regime, other possible influences may not matter all that much. If, on the other hand, war initiations are found on both sides of the regime ledger, we may assume that other attributes and processes besides regime type deserve attention. Since the level of analysis is clearly not dyadic, no direct challenge of the findings on, and arguments about, the pacific nature of democratic dyads is intended. A more general question is at stake. Assuming that the general predictive power of regime type is less than perfect, what other sorts of influences seem to influence states' inclinations to go to war? Yet external pressures and internal path-dependencies are no more easily measured than are democratic norms, institutional constraints, or signaling. This awkwardness suggests the need to fall back on more traditional techniques to explore their possible causal significance. But there is simply too much material to do justice to all 24 cases at one time. Of the 24 cases, only 4 states (France, Spain, Greece, and Turkey) initiated wars in and out of the relatively democratic regime category and, therefore, constitute the most interesting cases upon which to focus in this examination. Since we cannot do justice to all four cases simultaneously, we have selected the French and Greek cases for primary attention.

Intermittent Democracies and Their War Initiation Behavior

Do states that move back and forth from more to less authoritarian status demonstrate any noticeable proclivity for war initiation when they are in the more authoritarian category? In order to discuss this question, some

preliminary caveats are inescapable. For instance, there is simply no way of circumventing the inherent awkwardness of the concept of war initiation. Who is responsible for actually starting a war is always a difficult and highly subjective question. Even when it is clear that one side attacks another first, there is always the question of whether or to what extent the attack was provoked by the behavior of the attack's target. Some room for perceptions of shared responsibility frequently exists. Nevertheless, it is usually possible to discern which side in a confrontation moves its military forces against the other side first. Small and Singer (1982: 196–97) provide one useful schedule of who has attacked whom in interstate warfare since 1816.[1]

This Correlates of War conception is a minimalist approach to the question of war initiation. Whether or not the "initiator" deserves all of the blame for starting a war is a separate question. Whether states subsequently join wars already in progress must also be put aside as a separate question as well. Yet the record of who moved their military forces first suffices for addressing the question of inhibitions associated with regime type. If less authoritarian states are more inhibited than more authoritarian states, less authoritarian states should demonstrate less of a tendency to initiate warfare.

Equally contentious is the question of differentiating less and more authoritarian states. Defining the essence of democracy, and justifying relevant indicators, is a very old exercise about which no strong consensus has yet to emerge. Rather than entering into that debate anew, we fall back on the Polity III approach used in most of the other chapters.[2]

At the monadic level, though, should we expect war initiations of the intermittent democracies to be concentrated in democratic phases, nondemocratic phases, or equally distributed across both phases? Different schools of thought on why democratic dyads are relatively pacific would answer this question differently. Those analysts who stress normative or behavioral interaction within the dyad might say that no prediction is possible about monadic behavior. Others who stress the constraining influences of democratic institutions might predict fewer war initiations in the more democratic column. Yet if democratic and nondemocratic states are equally prone to initiate wars, controlling for the varying distribution of different regime types, we might expect intermittent war initiations to be equally distributed.

Table 4.1 lists and tallies the 21 war initiations of the 24 intermittent democracies. Of the 24 states, 8 (France, Prussia/Germany, Austria, Spain, Colombia, Chile, Greece, and Turkey) were responsible for all of the intermittent democracy war initiations and they, in turn, were credited with slightly more than a fourth of the 76 interstate wars of the 1816–1997 period.[3] This overachievement alone should make them an interesting group to examine. Not coincidentally, the list also includes two traditional great powers.

The outcome is asymmetrical. In the aggregate, more than twice as many wars were initiated (15 versus 6) by intermittent democracies when they were nondemocracies than when they were not. Of the eight states that initiated wars, the four Prussian/German as well as the four Austrian,

Table 4.1 War initiations by intermittent regime type states

War	*Democratic initiator*	*Autocratic initiator*
Franco-Spanish (1823)		France
First Schelswig-Holstein (1848–49)		Prussia
Roman Republic (1849)	France	
Crimean (1853)		Turkey
Spanish-Moroccan (1859)		Spain
Italian Unification (1859)		Austria-Hungary
Franco-Mexican (1862–67)		France
Ecuadorian-Colombian (1862–67)		Colombia
Second Schleswig-Holstein (1864)		Prussia
Spanish-Chilean (1865)		Spain
Seven Weeks (1866)		Prussia
Franco-Prussian (1870–71)		France
Pacific (1879–83)		Chile
Sino-French (1884–85)	France	
Greco-Turkish (1897)	Greece	
Boxer Rebellion (1900)	France	
Spanish-Moroccan (1909)		Spain
World War I (1914)		Austria-Hungary
Greco-Turkish (1919–22)		Greece
World War II (1939–45)		Germany
Turco-Cypriot (1974)	Turkey	

Colombian, and Chilean initiations took place in nondemocratic circumstances. The Spanish, Greece, and Turkish cases are split. The six French cases were equally split between the two regime type categories. Thus, from a national perspective, there appear to be a number of different stories concealed by the aggregate data. Yet, from an aggregate perspective, the warfare is not evenly distributed across the two types of regime type phases. Seventy-one percent of the war initiations (15 of 21 cases) began in more authoritarian phases.

Accordingly, we cannot dismiss regime type as possibly possessing some explanatory value. At the same time, regime type does not quite determine who fights whom. Are there other factors that might contribute to explanatory power? There are certainly alternative ways of explaining the initiations recorded in table 4.1. For instance, we might describe the story(ies) behind each case in a highly descriptive mode. Unfortunately, such an approach is only likely to yield a number of stories that might not appear to have much in common. What is needed are other generalizable factors that are capable of complementing and/or competing with the asserted regime type association.

Two factors with some promise are internal path-dependencies and external pressures. The concept of internal path-dependency (David, 1993) refers to the inability of political systems to shake off the effects of past events. These past events, or their persisting influences, act as restraints on the range of probable choices open to decision-makers. A good example is irredentism. Decision-makers in states that emerge with some portion of

their perceived "natural" population or nation still under the control of other states will find it difficult to ignore opportunities to unify the nation. The domestic political costs are simply too great for it is an easy issue, due to the popularity of nationalistic appeals, for rivals and opponents to coopt. Path-dependencies do not determine which choices are made but they do make some choices more probable and others unlikely. It is certainly possible to break free of their influence, but overriding path-dependencies has not proved to be either a very easy or frequently successful process.

One interesting characteristic of path-dependencies is that they are apt to persist regardless of changes in regime, even though those same regime changes may have been designed, at least in part, to break free of certain sensitivities to past events. In this respect, path-dependencies have some potential for explaining why a particular state may be equally likely to initiate certain kinds of warfare or go to war in certain circumstances, regardless of its regime type.

External pressures represent more familiar terrain for explaining war propensities. Perhaps the most well-known type of external pressure is the geopolitical variety. For example, states are situated within regional settings and power distributions. If one's neighbors to the west and north are too powerful to contemplate attacking, there is some increased probability that any expansionary activities that are undertaken will be oriented toward the east and/or the south. Decision-makers in all states do not spend a great deal of time contemplating expansion and attacking neighbors but those that do will find themselves encouraged by circumstances to move along paths of least geopolitical resistance. One facet of the present argument is that the paths of least geopolitical resistance for expansionist states have tended to coincide with areas populated by more authoritarian regimes. As a rule, less authoritarian targets have been less tempting targets, either because they were too powerful themselves, too difficult to attack readily, or protected by strong allies. However, none of these characteristics have been constants.

External pressures are not restricted to the proximity of powerful neighbors. Transnational economic depressions constitute another important source of pressure. Yet these external pressures do not exist in vacuums any more than do regime types. Path-dependencies, external pressures, and, no doubt, regime types interact. Some of the products of their interaction are found in table 4.1's partial list of war initiations. At this point, we need to shift analytical modes—moving from generalizations to specific cases—in order to illustrate how these factors may have interacted. There is no need (or space) to tell comprehensive and detailed stories about the 25 wars initiated by the intermittent democracies. Instead, the focus is placed first on France and then Greece, as two representative states (one great power and one non-great power) that have experienced significant variation on both the regime type and war initiation dimensions, in order to assess the interaction of path-dependencies, external pressures, and regime types. These national "stories" will not be any more comprehensive than stories about

25 wars might have been. Rather, they represent brief distillations of what seems to have transpired and what influences seemed to have been most important. Obviously, other analysts might prefer different interpretations. That is why this form of analysis must be regarded as both tentative and exploratory. The question readers should ask is not so much one of whether the story is completely nuanced, but whether the interaction of the highlighted influences makes a plausible case for the significance of internal path-dependencies and external pressures in explaining foreign policy. That is our basic hypothesis. We do not argue that internal path-dependencies and external pressures are so significant that they will always overwhelm regime type. We do argue that they can do so.

Strategic rivalries provide one example of a combination of external path-dependency and external pressure in this context.[4] Strategic rivalries are relationships in which the states involved view each other essentially as threatening enemies. In table 4.1, the Greek and Turkish initiation cases occurred in both democratic and more authoritarian regime circumstances. They also were directed against each other as long-time rivals, even though it should be acknowledged that the 1974 case, technically, only indirectly involved the Greeks and Turks in a war. We might well ask what happens to strategic rivalries when regime types change from authoritarian to democratic? Do they disappear or do they persist? Table 4.2 lists the states with regime type intermittency and strategic rivalries. These 15 states have been involved in a high proportion of the rivalries in existence over the past two centuries ($N = 173$ between 1816 and 1999). The total rivalry N of 67, however, somewhat exaggerates this overrepresentation since some of the

Table 4.2 The persistence of rivalries through periods of regime type intermittence

State	*Number of rivalries*	*Rivalries ending prior to intermittence*	*Number of rivalries that peristed through intermittence*	*Number of rivalries restricted to one regime type*
Argentina	4	1	3	0
Austria	6	6	0	0
Chile	4	2	2	0
Colombia	4	0	3	1
France	8	0	8	0
Germany	10	2	2	6
Greece	4	0	4	0
Haiti	1	1	0	0
Korea (Republic of)	1	0	1	0
Nigeria	2	0	1	1
Pakistan	2	0	2	0
Poland	3	0	3	0
Spain	2	1	1	0
Sudan	8	0	4	4
Turkey	8	5	3	0
Total	67	18	37	12

rivalries (those between states with regime type intermittency) are double counted. Still, the most important thing to note is that of the 49 rivalries that coexisted with the regime type intermittency (subtracting 18 from 67), as many as 37 (75.5 percent) persisted despite the fluctuation in regime type. This proclivity for rivalries to persist (although not necessarily indefinitely) suggests, in turn, that adopting a democratic regime does not necessarily alter the strategic environment. Enemies have had a good probability of remaining enemies in the past. That may change as states move away from regime type intermittency, but that remains a different question.

Pakistan and India and Turkey and Greece have continued to be strategic rivals in spite of regime changes. The two Koreas continue to be rivals in spite of regime changes in one half of the dyad. This is our basic point. Other factors besides democratization can make some difference in whether states are confronted with benign or malign environments. In some cases, the other factors, such as the strategic rivalries enumerated in table 4.2, the types of foreign policy ambitions exhibited in chapter three, or the irredentism to be discussed later in this chapter, may prove to be more important to explaining the aggressiveness of external behavior than regime type. Thus, our central hypothesis in this chapter is

H8: *Certain path dependencies (for instance, foreign policy ambitions, irredentism, strategic rivalries) encourage more aggressive foreign policies in spite of domestic constraints on aggressive behavior.*

Table 4.2 is suggestive but it does not make an empirical case for rivalries "trumping" regime type. This is a question pursued in chapter five. For now, it is appropriate to both illustrate and elaborate the contexts in which various sorts of path dependencies and pressures promote aggressive behavior. To continue our focus on regime type intermittency, we limit our examples to the foreign policy histories of three states—two of which are listed in table 4.2's list of intermittent democracies (France and Greece) and one that is not (Italy) to serve as a point of comparison. Italy may not have been an intermittent democracy but it also underwent several changes in regime without altering its strategic environment. One could argue, indeed, that the unaltering strategic environment made some of those regime changes more likely. Evidence for this same type of reversed causal arrow, a variation on the one examined in chapters 2 and 3, can be found in the French and Greek cases as well.

France

One of the keys to French foreign policy since the early eighteenth century has been the goal of resisting the implications of a declining trajectory in relative power.[5] Gildea (1994: 1120) notes that the phrase "The role of France is to retain its rank" might have been uttered by any number of

prominent French decision-makers from Napoleon to DeGaulle but was actually proclaimed as recently as 1989 by President Mitterrand. Ever since the defeat of Louis XIV, the European preeminence of France has been increasingly questioned as its absolute leads in regional wealth and population have eroded.[6] The Napoleonic Empire may have represented a brief and temporary exception to this generalization but its defeat in 1814 and 1815 accentuated the political problems associated with declining regional preeminence.

The Allied occupation of France in 1814 and 1815 created a two-headed policy problem for French decision-makers. Externally, the Vienna settlement designed to check further French expansion within Europe rankled as an unwarranted, alien imposition on French sovereignty and policy ambitions. Foreign troops remained in France for several years. Reparations were paid. Territory was lost. Hostile armies and fortifications were found at all of France's land frontiers. The British navy was prepared to act on the maritime borders. Moreover, the principal great powers were united throughout the first half of the nineteenth century on the necessity of suppressing any renewal of French expansion within Europe. France continued to be the primary continental power but most of its ambitions of improving its regional position were thwarted by effective or anticipated external opposition.[7]

All contending parties within France reacted negatively to the containment aspects of the 1815 settlement but they disagreed on the appropriate strategies to do so. Conservatives preferred rebuilding French power while avoiding a great power attack. Republicans preferred a more aggressive foreign policy and the export of liberal ideas and revolution. The contending parties also disagreed on the form of government most suitable for France. From at least 1789 on, domestic politics revolved around attempts to liberalize the French political system in the face of strong conservative opposition. The 1815 settlement had reimposed the Bourbon monarchy and restored the traditional aristocracy but it could not completely turn the societal clock back to before 1789. Nor could it eliminate the societal changes that had been created by the French Revolution, the Napoleonic Empire, 24 years of global warfare, and long-term economic changes. As a consequence, nineteenth- and twentieth-century France moved back and forth between more authoritarian and more liberal governmental formats, depending on external shocks and the relative strengths and strategies of the contending domestic political forces, in a search for a successful formula.

One aspect of this societal tug-of-war was the vulnerability of the government in power at any point to appearing too weak in foreign policy. All political systems presumably face this threat to variable degrees, but the French problem was made more acute by the fact that the monarchy initially had to be reimposed by external force. In 1815, the French population had to accept the Bourbons. Yet enthusiastic support for, and loyalty to, the regime was an entirely different matter. One way to lose political

popularity at home was to appear too complaisant in European politics. However, an overly aggressive foreign policy was sure to provoke a unified great power retaliation. Successive regimes and governments, therefore, had to seek a course of policy that managed to avoid external and internal attack. The subsequent changes in regimes in 1830, 1848, 1852, and 1871 suggest that developing such policies proved to be a difficult task.

A partial solution to this problem involved seeking French glory outside of Europe. The conquest of Algeria, beginning in 1830, was one of the byproducts of what might be termed "geopolitical deflection."[8] The tendency to focus expansionist energies in areas that were least likely to incur the opposition of other great powers meant that deflected states such as France were less likely to be involved in European warfare and more likely to be embroiled in colonial warfare. By deflecting energies to parts of the world where one was extremely unlikely to encounter native regimes of a democratic nature, the probability of France, a sometime liberal republic, fighting another liberal republic was consequently diminished. The caveat to this generalization, of course, is that while colonial expansion may have decreased the probability of French warfare within Europe, it presumably increased the probability of conflict with other states that had ambitious colonial policies—some of which were also liberal republics.

In this context, the rise of Napoleon III definitely represented a more authoritarian turn for the French political system.[9] His reign also involved much greater French foreign policy activism and revisionism (Mcmillan, 1991). The two facets were not independent. Imperial control, no doubt, gave Napoleon III greater leeway in foreign policy maneuvering and his penchant for secret diplomacy. Foreign policy success, on the other hand, was important for creating support for the new dynasty. But therein lies an important clue. French foreign policy success would be popular because revision of the 1815 settlement and its implications for France's position in Europe and the world was not a goal on which domestic camps disagreed. Napoleon III's Empire also replaced the more liberal Orleans constitutional monarchy that had suffered domestically from the impression that its foreign policy efforts were too restrained. The Bonaparte heir was expected to try harder.

Napoleon III did work harder on the foreign policy front than the Orleans regime. But he also enjoyed a significant advantage, other than a more authoritarian regime, over his immediate predecessors. Namely, the European great powers of the 1850s and 1860s were much less likely to present a unified front against French foreign policy activism than they had been between 1815 and 1848. France actively pursued accommodation with Britain as evidenced in the Crimean War, the 1860s lowering of tariffs, and the possibility of a joint intervention in the American Civil War. Britain, in any event, was even less interested in continental intervention than it had been before. Russia needed to rebuild after the Crimean War. The ascendancy of Prussia made Prussian–Austrian cooperation less of a sure thing. Austria was attempting to cope with the emergence of an Italian

state. All of these developments provided France with external opportunities that it had not enjoyed for some time.

Nor did Napoleon III revert to the first Napoleon's tactics for revising the French position within Europe. The Second Empire engaged selectively in European warfare and even accomplished some modest territorial gains in the late 1850s but French hegemony was not being sought primarily on the basis of military coercion. Interestingly, Napoleon III's own preferred strategy seems to have been negotiation and international conferences to resolve disputed issues. Even the Mexican adventure may have reflected as much the decreasing degrees of foreign policy freedom within Europe as anything else. Seeking ostensibly easier successes outside Europe was no more unique to the Second Empire than the general goal of revising the 1815 outcome was a novelty for French foreign policy in the 1850s and 1860s. Similarly, the demise of the Second Empire was very much a consequence of developments in Germany and the ascendance of a new contender for the leading position in Europe.

Still another dimension of nineteenth-century French foreign policy, as hinted above, was the search for a great power ally to both weaken the containment of France and to improve its ability to deal with the other great powers. Although rarely popular at home, Britain was the most likely candidate.[10] The problem, especially, in the first half of the nineteenth century was that there were too many points of friction, potential and realized, and both within and outside of western Europe, for an Anglo-French entente to succeed for any length of time.[11] Prior to the first decade of the twentieth century and certain ensuing changes in international relations, Anglo-French cooperation was only intermittent.

The possibility of an outbreak of war between the two liberal republics, arguably, was greatest in 1831 (however Britain is not coded as liberal until 1832), 1840, and 1898. If war had broken out on each of these occasions, the less authoritarian initiation of war against another less authoritarian state cell in table 4.2 would not have been empty and the generalization that democracies do not fight other democracies would have lost some of its appeal. The question for our immediate purposes is whether the avoidance of war in these three cases should be attributed to chance, regime type, or something else. The answer is that in each case, France backed down because its decision-makers expected to lose a militarized conflict with Britain. One may wish to argue that different outcomes might have occurred if different regime types had been involved, but, in actuality, the French monarchy proved to be a restraining factor in the 1840 crisis.

The first of the three crises took place in Europe and had important implications for the 1815 settlement. Belgium sought to break free from the Dutch monarchy in 1831. An independent Belgium constituted a challenge to the 1815 settlement in several respects. The Netherlands had obtained Belgium, formerly an Austrian province, at Vienna. A number of fortifications on the Belgian border with France were part of the French containment network. A breakaway and liberal Belgium also constituted

a challenge to the legitimacy of conservative monarchical rule—and thereby a threat to Prussia, Austria, and Russia. It also provided an opportunity for French intervention on behalf of Belgian independence and its own interests.

In the end, Britain at sea and France on land intervened with military forces to compel a retreat by Dutch forces bent on reconquering Belgium. Once the Dutch threat had been eliminated, France insisted on a partition of Belgium favorable to France and appeared reluctant to withdraw its land forces. It required a British ultimatum threatening war, in conjunction with the desire of the conservative monarchies to restore royal control over the secessionists, and the transferal of five Belgian border forts (to France) to ensure French withdrawal.[12]

The next crisis focused on control of Syria. The French supported Mohammed Ali who, in 1839–40, was threatening to bring down the Ottoman Empire from his base in Egypt. His occupation of Syria not only threatened the dissolution of the Ottoman Empire, it also threatened the British position in India. Successful expansion on the part of Mohammed Ali would mean Egyptian control over the Suez and other overland routes between the Mediterranean and the Persian Gulf and Indian Ocean. Ali was closely aligned with France, which had provided military training for the Egyptian forces. France's expanding position in Algeria, coupled with the overthrow of the status quo threatened not only the routes to India but also control in North Africa and the Mediterranean. The fall of the Ottoman Empire would in addition have serious destabilizing implications for the Austrian and especially Russian positions in southeastern Europe and the eastern Mediterranean.

Britain organized Russian, Austrian, and Prussian opposition to Mohammed Ali remaining in control of Syria. France, led by Adolphe Thiers as head of government, resisted the pressure on Ali, underestimating the extent of great power agreement and resolve to maintain the status quo by force if necessary and Mohammed Ali's ability to maintain military control of Syria. The Allies offered Mohammed Ali several choices with increasing penalties, including removal from power if a series of deadlines were not met in withdrawing from Syria. Ali ignored the deadlines. France threatened war if Ali were actually deposed. British–Austrian military intervention subsequently led to the forced eviction of Mohammed Ali's forces from Syria.

While Ali ultimately was allowed to remain in control of Egypt, the French threat to fight was accompanied by several steps to prepare for war with Britain. Reserves were mobilized. Extraordinary credits for the army and navy were approved. Parisian fortification efforts were accelerated. Popular support for war was clearly demonstrated. British decision-makers, although not entirely in agreement, thought the French were bluffing but were prepared to go to war if it became necessary to teach the French a Palmerstonian lesson (Jardin and Tudesq, 1983: 155).

Whether bluffing or stalling, the prospect of facing all of the great powers simultaneously in a war that might have even greater costs than those

imposed in 1815 persuaded some leading governmental elites in France to rethink their stance. There were also reasons for believing that war would increase the probability of revolution or insurrection and the overthrow of the Orleanist monarchy.[13] After a confrontation with the constitutional monarch, Louis-Philippe, Thiers resigned after it was made clear that the continuing risk of war was no longer considered tolerable. He was replaced by the more willing-to-accommodate ambassador to Britain. The crisis was defused although Collingham and Alexander (1988: 237) argue that the external defeat in 1840 contributed to the 1848 overthrow of the monarchy by increasing the number of Frenchmen who saw Louis-Philippe as too much of an internal restraint on French foreign policy.[14]

Almost sixty years later, the last serious Anglo-French crisis also had to do with control of Egypt. In the early 1890s, France had initiated several exploratory attempts in East Africa seeking control of the upper Nile and a link with colonial possessions in West Africa without much opposition. By the late 1890s, however, Britain was involved in suppressing the Mahdist Revolt and moving into southern Sudan. Fashoda was the point at which a small French force and a much greater British military force met. Continued French occupation of Fashoda posed the threat of the capability of controlling the flow of water into the Nile tributary, with its associated implications for Egyptian agriculture and British control of Egypt.

French decision-makers hoped to use the possession of the Fashoda bargaining chip to bring about an international conference on Egypt with the intended outcome of securing a British withdrawal or, alternatively, an improved French position within Egypt. They believed that at the very least the British would be willing to negotiate. The problem was that Britain enjoyed a clear military superiority at Fashoda—to such an extent that the meager French force was unable to even communicate with its government. Not coincidentally, the British government also perceived no need to negotiate the question of who would control the Nile. The British were no more eager to go to war than the French but they were willing to contemplate the possibility. "Unlike France, in the last analysis Britain could and would go to war for a few square miles of swamp" (Wright, 1972: 188). As long as a military clash could be avoided, the French government could withdraw if it chose to do so. One indication of the mood among French decision-makers is the foreign minister's prediction that should war break out, the French fleet would be at the bottom of the sea within two weeks (Bates, 1984: 158). Lacking adequate military forces anywhere near Fashoda, possessing a navy that could not compete with Britain's and a Russian ally that was unwilling to provide assistance, beset at home by the Dreyfus Scandal, and confronted by an unyielding Britain, the French government had little rational choice but to back down in 1898. Bates (1984: 186) argues that there is no archival evidence that either France or Britain "seriously considered going to war over Fashoda." But an inadvertent military clash in southern Sudan might have narrowed the options.[15] In any event, a year later and with a mutual eye on the German threat in

Europe, Britain, and France were able to resolve their territorial disputes in both West and East Africa. The foreign policy environment had suddenly become more conducive to more or less continuous Anglo-French cooperation.

Greece

Positional considerations and ambitions are often thought to be confined to the foreign policies of great powers. While it is certainly true that great powers can entertain loftier ambitions than minor powers, path-dependent positional concerns can also emerge in situations involving weaker powers. The Megali theme in Greece, the desire to unify people of Greek nationality resident in different parts of the Ottoman Empire within a single state, offers a strong illustration of the persisting effects of path-dependency and external pressures, despite changes in domestic regimes.

The achievement of independence for Greece was highly dependent upon the military intervention of Britain, France, and Russia. The subsequent nature of Greek foreign and domestic politics continued to be conditioned by the constraints imposed by these external benefactors. The initial Greek state was small in territorial size and encompassed less than half of the Greek population and none of the major commercial centers within the Ottoman Empire. This starting point is attributable in part to British and French fears that a larger, newly independent Greek state might become a client state of Russia (Legg, 1969: 64). Yet, whatever the motivations involved, the Greek state began its contemporary existence from a position of weakness that was accentuated by its form of government. Other things being equal, the Greek political system that began as a liberal republic would probably have continued in that formal format subject to the rise and fall of local dictators, operating within a system characterized by a variety of intense cleavages. The great power preference for greater stability in the eastern Mediterranean, however, led to the adoption of a monarchy that first came from Bavaria and later from Denmark.

The external expectation was that the monarch could be relied upon to suppress popular attempts to expand Greek borders to encompass a greater proportion of Greek nationals (Petropulos, 1968: 52). Internationally destabilizing attacks on the Ottoman Empire were not welcomed. It was also expected that the monarchy would ensure efforts to meet any international financial obligations incurred despite the inherent shortcomings of the impoverished Greek economy and provide some central management of a multipolarized and conflict-prone domestic political system.

However, the internal expectations were that the adoption of a monarchy ensured continued great power patronage and financial assistance. The king was also expected to work toward expanding the size of the Greek state to the north and east at the expense of the Ottomans. In addition to the pervasive nationalistic allure, irredentism appealed to some as a quick fix solution to Greek economic weakness. If more wealthy and productive

centers remained outside the Greek borders, why not simply acquire them by force (Clogg, 1979: 76)? The failure to live up to these various expectations would lead to repeated efforts to overthrow the monarchy and the intensifying cleavage between proponents of a political system with and without a role for royalty.

When the internal constraints of poverty and conservative monarchies did not work to check tendencies toward expansionistic foreign policies vis-à-vis the Ottomans and later, Bulgaria and Serbia, some form of great power intervention to maintain the status quo could be anticipated. Prior to the multiple interventions associated with World War II and the Greek Civil War of the 1940s, external interventions had occurred in 1840–41, 1854–57, 1878–81, 1886, 1897, 1912–13, and 1916 to prevent the Greeks from pursuing inconvenient foreign policies.[16] Not unlike the French case (and the Italian case to come), Greek foreign policy was severely constrained by external pressures and circumstances.

The fulfillment of Greek foreign ambitions could come only when all the powers agreed or were indifferent, or, if one of the major powers favorably disposed to Greek aims exercised a paramount influence in international affairs (Legg, 1969: 67). Perhaps the most expansionistic period in contemporary Greek history occurred immediately before (the Balkan Wars) and during and after World War I when the Greeks were encouraged initially to land forces in Turkey in the ultimate pursuit of the Megali notion. The defeat of this expedition by the Turks led to a significant diminishment of the appeal of a Greater Greece (except vis-à-vis Cyprus) and fundamentally altered the nature of domestic Greek politics. Over one million Greek refugees from Asia Minor made their way to Greece in the early 1920s, constituting one-fourth of the total population in Greece (Legg, 1969: 57), with major repercussions in terms of urbanization, land redistribution, anti-monarchical sentiments, and left versus right cleavages (Clogg, 1979). Defeat in war led to the abdication of the reigning king, a sequence of purges within the military depending upon who was in power, increased political instability, and, with help from the interwar depression, the abandonment of a liberal republican format.

Invasion by the Italians, the German occupation, and a bitter civil war until 1949, the outcome of which hinged on external intervention, did not transform the Greek political situation. The 1967 military coup and its downfall in 1974, precipitated by the prospect of another Greek–Turkish war, continued patterns established long before. Regime type and foreign policy behavior became increasingly intertwined in the Greek experience, but, much of the time, the causal arrow from foreign policy behavior to regime type has seemed stronger than the other way around.

Are these lagged influences and path-dependencies unique to countries that have fluctuating experiences with their domestic political systems? The most probable answer is no. The Italian experience is an example of another type of case of lingering foreign policy influences without the democratic experimentation prior to being defeated in World War II.

Italy

If one of the keys to interpreting French foreign policy is a long-term decline trajectory and the need to preserve or reclaim France's rank, the history of Italian foreign policy exhibited another side of the same coin. As "the least of the great powers," Italy throughout the nineteenth century and beyond was too weak and economically underdeveloped to assume the role of a major power (Bosworth, 1979; Shorrock, 1988). Italy had depended a great deal on external help to achieve unification.[17] It also had little luck in European warfare. Piedmont, the core state of the Italian unification movement, was defeated in 1848 by Austria and might have lost its autonomy if not for the fear of French intervention (Woolf, 1979: 432). Piedmont participated in the Crimean War on the side of Britain and France in the unrealized hope that Austria would ally with Russia and that in an expanded war, Piedmont's allies would help to weaken Austria's hold on Italian territory (Woolf, 1979: 417). Control of Venice was accomplished only because the Prussians, allied with Italy, defeated the Austrians in 1866. Italy, itself, had been badly embarrassed by the 1866 defeats at Custozza and Lissa. Rome was regained only because the French, who were providing military protection for the Papacy, were forced to withdraw due to the Franco-Prussian War in 1870. At the same time, Italian decision-makers never contemplated, and probably could not contemplate, the option of reverting to minor power status. As a conesquence, Italian foreign policy, in trying to live up to a role frequently beyond its capabilities, became unusually sensitive to the variable degrees of maneuver permitted it by external circumstances. Yet foreign policy remained important as an avenue of compensation for weakness and failure on other fronts and in the past even though the foreign policy record was equally less than successful.

Another part of the Italian problem was location. Its traditional enemy, Austria-Hungary, lay immediately to the north. As long as Austria-Hungary was perceived to control territory and populations rightly belonging to Italy, it could hardly be ignored. Yet Austria-Hungary was also perceived to be too strong to challenge with impunity. Allies were needed to help equalize the playing field and to provide protection from Austro-Hungarian attack. France sometimes played this role while Germany did so at other times. Neither ally was particularly altruistic when it came to advancing Italian goals; both alliances had their price. For its assistance in the 1859 war to liberate Italian territory from Austrian rule, France received Nice and Savoy as its reward. Later German protection from the possibility of a French attack via the Triple Alliance was purchased at the cost of relinquishing the possibility of attacking Austria-Hungary, the other alliance member.

With a relatively powerful Austria-Hungary to the north and a too powerful France to the west, Italian expansionist ambitions were deflected to the east and the south. Expansion to the east, however, was complicated by the presence of the Austro-Hungarian Empire in the northeast. Expansion

to the southeast would incur the displeasure of Britain. The only path of least resistance lay to the south.[18] But even here there were also restrictions. The closest southern territory, Tunisia, had been taken by the French in 1881 despite the presence there of a sizable Italian community. The consequent rift between Italy and France over the Tunisian issue led directly to the need for German protection in the Triple Alliance. As an Ottoman province, Libya was initially protected by the great power disinclination to accelerate the dismemberment of the Ottoman Empire—one of the factors that precluded Italian movement into southeastern Europe. The British were already in Egypt and the Sudan. The French were in Algeria and would not tolerate an attack on Morocco. The only conceivable opening in northern Africa in the late nineteenth century was Ethiopia. However, the 1896 defeat at Adowa only added another embarrassment to a string of foreign policy humiliations, thereby fueling perceived needs to try harder at the imperial expansion game.

A major opportunity arose in 1911. Italian–French relations had improved significantly as early as 1897–98 and immediately after the fall of Crispi. By 1902, the two states had reached formal agreement on their mutual interests in Libya and Morocco. In 1887 and 1902, the renewals of the Triple Alliance with Germany and Austria-Hungary indicated that Italy's allies would not interfere should Italy choose to act in North Africa. In 1909, an Italian–Russian agreement exchanged support for an Italian move into Libya for greater freedom for Russian shipping through the Dardanelles. When the Franco-German Agadir crisis broke out over French advances on Morocco, Italian decision-makers perceived a window of opportunity to make their own advance in Libya. With the major European powers polarized and distracted by the Moroccan crisis, what appeared to be approval by the British of Italian action in Libya removed all apparent restraints on Italian action. Despite a lack of military preparedness, Italian decision-makers felt that if they did not act in September 1911, another opportunity might never come (Bosworth, 1979: 127–64). In the near future, the other great powers would be more likely to restrain any Italian expansion against Turkey. Tripoli and Cyrenaica were attacked by Italian bombardment six days after the French moved their military forces into Morocco.

One of the generalizations in the democracy-war literature is that liberal republics tend to band together to fight authoritarian foes.[19] Italy's 1915 entry into World War I on the side of Britain and France, even though Italy was formally allied with Germany and Austria-Hungary, would seem to represent a dramatic illustration of this tendency. However, the calculations of the Italian decision-makers paint a different picture.

Italy had allied with Germany and Austria-Hungary to gain protection from France over the possible escalation of disputes about the control of North African territory. An Austro-Hungarian attack on Serbia was a different matter. Italy had absolutely no incentive to assist Austria-Hungary in extending its influence across the Adriatic Sea from Italy, a sphere coveted

by Italian foreign policy makers. But Italy was also felt to be too weak to resist Austria-Hungary on its own. Austria-Hungary, for its part, had little that it could afford to offer in exchange for Italian cooperation. The Italian price included the transfer to Italy of Trentino, Alto Adige, Trieste, and parts of the Dalmatian Coast. In marked contrast, Britain and France were quite prepared to see these territories go to Italy as a reward for joining the war effort on their side. After waiting to see whether the Germans would win quickly and with some hope that both Austria and France would somehow be beaten badly, Italian decision-makers sensed that the war would end soon with an Allied victory (Bosworth, 1979: 397, 401). While they might have been tempted by an opportunity to settle past grievances with France, British naval power was regarded as too potent to withstand given Italy's long, largely undefended coastline. An Allied victory would also have implications for the disposition of territory in the Middle East and Africa.

After negotiating with both sides, Italian decision-makers opted for what appeared to be the most profitable course of action. They miscalculated on the war being almost over. They also miscalculated that the promised territorial rewards would be delivered in full at war's end. Some of the promised Austrian territory was transferred. Other promised territory became part of an independent Yugoslavia. No territory was acquired in Turkey and while Britain and France had taken over the former German colonies in Africa, no compensation in the Ethiopian area was offered to Italy. As a consequence, the impression of a "mutilated victory" became popular in postwar Italy. After making extensive sacrifices and perceived contributions to the war effort, Italy had once again been humiliated by being denied its just rewards.

The "mutilated victory" idea was to become a central motif of the nationalist movement after World War I (Lyttleton, 1987: 19). Italian territorial expansion, justified by domestic population growth and the need to find room for immigration, had been thwarted by "more decadent" states, such as France, whose populations were no longer growing. One of the explanations for the foreign policy failure was the inherent incompetency of democratic regimes to defend Italy's share of the spoils of war. These images also fed into Fascist ideologies justifying the need to redistribute power and privileges among the newly ascending, have-not states in the system and the related need for latecomers to possess states with high degrees of power centralization.

One reason for allocating space to the Italian case is its claim to early liberal republic status (1848 for Piedmont and 1860 for Italy). It was also one of the earliest defectors from the liberal republic camp (1922). Both developments had much to do with war. Italy's (and Piedmont's) claim to liberal republic status had a decidedly flukish character. In 1848, the Piedmont monarch, Charles Albert, reluctantly surrendered some of his royal power to establish a parliamentary political system with a constitutional monarch. It was done hurriedly and the motivations for taking this step are not entirely clear. But it is clear that Charles Albert was hardly an enthusiastic

democrat. One very likely hypothesis is that the liberalization of the Piedmont political system was carried out initially in reaction to other liberalization moves that same year elsewhere in Italy. The idea being that if Charles Albert hoped to seize the leadership of the drive toward Italian unification, he would have to appear as representing something other than the head of just another aristocratic, expansionist dynasty. There was also the possibility that revolting republicans outside Piedmont would be successful and secure control of the drive for unification (Woolf, 1979: 381). The year 1848 was also a dangerous year for monarchs that may suggest an element of preventive action at home in Piedmont. Some measure of restricted democracy was better than the emergence of a more radical form of democracy.

Ironically, the Piedmont parliament was able to expand its political role in the 1850s beyond what was initially intended because Charles Albert's successor, Victor Emmanuel II, was regarded as dangerously ineffectual and because elites feared the threat of more radical republicanism. The military failures of the monarch, a tradition that contrasted markedly with the Prussian experience and one that continued through the first few decades of the Italian state, also helped ensure that the crown did not retain or regain strong executive powers (Lyttleton, 1987: 5; Smith, 1989: 4).[20] Another formative pressure may have come from France. Napoleon III is credited with discouraging Victor Emmanuel II's desires to bring down the Piedmont republican structure by military coup (Woolf, 1979: 439). It was feared that a more conservative government might align itself with Austria.

Lyttleton (1987: 7) observes that after universal suffrage was enacted in 1912, followed almost immediately by the crisis of World War I, the "limited and artificial" type of democracy practiced in Italy that had depended on limited participation could only be succeeded by one of three alternatives: more democratization, social revolution, or dictatorship. That Italy was the first democratic political system to turn authoritarian in the interwar years is usually explained in terms of the traumatic consequences of the world war, the strong presence of frustrated expansionist drives, the domestic polarization of left and right, and the incapacity of relatively weak political institutions to manage political, economic, and social problems in a context of expanded enfranchisement.[21] This is not the place to explore the factors facilitating the rise of Mussolini.[22] But that rise did alter the nature of the political system. The obvious question for our purposes, then, is whether Mussolini's foreign policy was all that much different from the foreign policy of the liberal era and, if so, how much credit for the change(s) should be attributed to the change in regime form?

Several commentators on Italian foreign policy have argued for strong elements of continuity between 1860 and 1943. Bosworth (1979: 419), for instance, contends that the

> foreign policy of Liberal Italy was more covert, more hesitant, more verbally restrained than that of Fascist Italy, but it was not different in

kind; instead from the Risorgimento to the fall of fascism, Italy pursued the foreign policy of the least of the great powers.

The fundamental continuity was that Italy's relative capability meant that it could only hope to satisfy its foreign policy ambitions when external circumstances created opportunities that could be exploited. Some differences were inevitable and one was that the Fascists were probably more proactive in encouraging situations that enhanced Italy's degrees of foreign policy freedom (Lyttleton, 1986: 429).[23] Yet the foreign policy ambitions, themselves, remained fairly constant: irredentism to the north and northeast and the revisionist expansion of its influence and control to the east and south. The creation of a new Roman Empire in Africa and the Mediterranean was not an invention of the fascist era. The general goal had been created decades before in a liberal era. Nor were the interwar Italian maneuvers in the northern Adriatic, Albania, Greece, and Spain all that innovational. They were more reactions to new opportunities that had emerged in the aftermath of World War I and rationalized by an Italian nationalism that had been shaped by a half-century of international failures.

Additionally, the decision-making behind Italy's participation in World War II resembled the situation preceding World War I. Italy was slow to join the war as it waited to see who was most likely to win. Once it seemed apparent that Germany would win, it was time to join the bandwagon before it was too late to be rewarded for Italian war participation in the subsequent peace settlement (Cunsolo, 1990: 159). One author goes so far as to suggest that if France had offered in 1938 to transfer territory considered to rightly belong to Italy, at least by Italians (Nice, Tunisia, Savoy, and Corsica), Mussolini might have been seduced away from the German alliance (Barclay, 1973: 161). Such an offer seems an improbable counterfactual, but the point remains that prior to the mid-to-late-1930s, it would have been difficult to predict exactly what fascist Italy might do once a major power war had commenced. With the remarkable accuracy of hindsight, perhaps the safest prediction would have been that fascist Italy would most likely behave along lines similar to liberal Italy, regardless of regime format, and bandwagon with the apparent winners after it was evident who that might be. One difference, of course, was that Italian decision-makers guessed correctly in 1915 and incorrectly in 1940. Nevertheless, it is difficult to link this difference in outcome to the inherent differences in regime.

Conclusion

Few analysts argue that foreign policy decision-makers enjoy complete free will in choosing their options. At times, however, the emphasis on the role(s) of democratic institutions in restraining foreign policy behavior gives the impression that other factors besides regime type, whether in the monadic or dyadic mode, have little consequence. The argument here is

that external pressures and internal path-dependencies do not dictate foreign policy decisions, including those relating to war initiations, any more than political institutions and normative sentiments do. Nevertheless, a plausible case can be constructed that they probably have both long- and short-term impacts on the options that are pursued. The more general point is that decision-makers are apt to be confronted with a range of variable constraints—including external pressures, internal path-dependencies, institutions, and public opinion—on their ability to formulate preferences and to make choices. We need to be careful not to give too much of the credit or blame for outcomes of interest to only one of several possible sources of influence until or unless we are in a position to sort out their relative effects.

Moreover, it needs to be kept in mind that a strong argument can be made for war influencing the likelihood of democracy—as we already saw in chapter three. Once again, we should not rule out elements of reciprocal influence. Moreover, the role of warfare in establishing path-dependencies seems particularly salient. The defeats of Napoleon in 1814 and 1815 established the emphasis on French containment to which French decision-makers were required to react between 1815 and 1848. The Greek war for independence led to a state smaller than the imagined Greek nation. Only more warfare was likely to correct that discrepancy. The nationalistic embarrassments associated with the wars of Italian unification influenced foreign policy efforts for years that followed. Subsequent war experiences did little to overcome the initial effect.

It should be noted as well that wars played extremely important roles in bringing about regime changes in France, Greece, and Italy. Warfare contributed to the demise of the First and Second French Empires and the Third and Fourth Republics. It reimposed the Bourbons in 1814 and 1815. Later, the Paris Commune was suppressed in part because Bismarck released French prisoners of war to fight against it.[24] Warfare was essential to the creation of a unified Italian state and to the balance of political power between the monarchy and legislature. Warfare was equally essential to both the rise and the fall of Mussolini's fascist regime.

Of course, a cursory and highly selective overview of the foreign policy experiences of a few states cannot be expected to determine this question of relative impacts once and for all. At best, it can merely suggest that external pressures and internal path-dependencies may have some general explanatory significance. Still, the reasons for selecting these cases should be kept in mind. France and Greece were not selected randomly. They represent only one-twelfth of the 24 intermittent democracies, yet they initiated 8 of the 21 wars begun by this group.

There is no basis in this analysis upon which to insist that regime type made no difference. Authoritarian regimes no doubt facilitated the foreign policy activism of Napoleon III, the Greek junta of the 1960s–70s, and Mussolini. However, regime type played much less important roles in developing the goals pursued that tended to stem from situations developed

in the past. For France, it was maintaining its once commanding regional position in the face of change and relative decline. For Greece, it was expanding the state to encompass the location of Greeks throughout the eastern Mediterranean. For Italy, it was the effort to act as a great power without the capabilities to do so. In none of these cases did the foreign policies pursued during the more authoritarian interludes constitute marked discontinuities with what had taken place earlier. But geopolitical pressures and opportunities did change providing French, Greek, and Italian decision-makers more or at least different degrees of freedom.

In all three cases, expansionist foreign policies were highly dependent upon facilitative external environments. France and Greece had to back down when confronted with unified great power opposition. France consistently backed down in crises with a more powerful Britain. Italy could not act alone in Europe. When the external environment was not facilitative, energies and attention tended to be deflected away from possible and nearby targets (Britain and Belgium for France and France for Italy). France conquered Algeria, tried to control Mexico, and sought a colonial empire in Africa and Southeast Asia, especially after its war defeat in 1871, to compensate to some degree for its relative decline and the too-powerful resistance encountered in Europe. Unable to fight Austria or France, Italy pursued empire in Ethiopia and Libya. One implication is that the inclinations to go to war were suppressed not so much by legislative resistance, the lack of public support, or normative constraints. Instead, rational, if reluctant, calculations of capability inferiority seemed to have prevailed in crisis circumstances.

In the end analysis, domestic regimes have no doubt influenced foreign policy decision-making. So, too, have geopolitical and economic external pressures and internal path-dependencies, which have also influenced mightily domestic regimes.[25] Nor need we ignore the activity of contending political factions who argue over whether and when to go to war and how much democracy is appropriate. Ultimately, they are the leading targets of the conditioning influences of regime constraints, external pressures, and path-dependencies. But given these complex situations, what grounds do we have currently to emphasize one source of influence over others?

Such a conclusion does not imply that we are forced to fall back on traditional story-telling by default. What is needed are more balanced, more ambitious, and yet more historically sensitive theories that seek to integrate the multiple sources of influence on foreign policy decision-making. Much of the literature on the democratic peace takes the following form: political regime characteristics → foreign policy decision-making → external context. To put things in their proper perspective, what we need are theoretical arguments that approximate the following form: external context → intermediation of path-dependencies, regime characteristics, and other constraints → foreign policy decision-making → external context.

Regime type may give us strong clues about whom democratic states are unlikely to go to war with but regime type does not appear to be able to

tell us when these same states are likely to target nondemocratic states. Nor does regime type tell us much about the war initiation propensities of more authoritarian states. To assess the probability of war initiation more generally, we need information on other variables that, unfortunately, are often more difficult to measure than regime type. Once we overcome these hurdles, it should be possible to assess the relative effects of these factors on encouraging and discouraging war. Ultimately, we may even find that dyadic regime type explains less than many analysts currently believe.[26] Until then, we need to keep in perspective the limited scope of democratic peace explanations and the necessity of introducing other variables into the war and peace equation.

In chapter five, we do precisely that—add another path-dependency variable to the democracy–peace equation. Rivalry is an important indicator of situations favoring the continued use of force and political–military solutions to international problems. How does the presence or absence of democratic regime types stack up against the effect of the presence or absence of rivalries in predicting whether states resort to the use of force? We do not presume that rivalry will "wash out" the effects of regime type, but we do expect that rivalry will prove to be a stronger predictor of international conflict than the hypothesized constraints of democracy.

CHAPTER FIVE

Do Rivalries Trump Regime Type?

While democratic peace questions have generated a large number of empirical findings, the ratio of questions still unresolved to those that have attained some degree of resolution remains high. Many of these questions are quite interesting but they are also contingent to varying degrees on the disagreements about why there might be a democratic peace. As long as the theoretical debate persists, ancillary questions about the role of regime type will continue to proliferate. One possibility is that we are simply forcing democracy to shoulder too much of the causal burden in accounting for pacific propensities. More peaceful communities may be emerging but it may not be solely, or even primarily, attributable to the types of regimes they possess.

Our position is a modest one. We are open to the possibility of transformations in world politics. We can imagine an explanatory role for regime type in accounting for conflict and cooperation but dyadic regime type seems an unlikely variable to support the heavy burden of transforming single-handedly the very nature of world politics. Such a perspective need not imply that the democratic–nondemocratic distinction has no explanatory value. Evidently, it will take some time to sort out the alternative explanations for the apparent distinctiveness of democratic dyads. In the interim, the question should be one of assessing how much explanatory weight should be bestowed on the regime type variable relative to other variables that also have some potential for explanatory significance. Why states cooperate and interact more peacefully are big questions. It is doubtful that they lend themselves to monovariate answers any more than do alternative questions about why states engage in conflict. Yet democratic peace answers have tended to don monovariate cloaks even when that was not the author's intention. Still another way of expressing this same sentiment is to argue that it is time that we move ahead in our examination of democratic peace implications. We need to begin integrating regime type variables into broader frameworks rather than constricting ourselves to issues confined narrowly to the notion of democratic peace.[1]

Our immediate focus is to ask what are the comparative explanatory powers of rivalry and regime type, with no expectation that one variable is likely to push aside the other completely. Rather, our expectation is that both variables working together, but focusing on different sides of the same conflict "street," with one a facilitator and the other a constraint, should be more powerful than either one alone. Yet we do anticipate that rivalry relationships are more fundamental and central to conflict relationships and, therefore, will prove to be the more powerful of the two variables. But, there is no reason to assume that the relative explanatory values of the two variables are constant over time. It is quite conceivable that the explanatory power of regime type has become stronger and that of rivalry less so. Indeed, the empirical evidence suggests that this is in fact the case.

Assessing the relative weights of regime type and rivalry is one of our tasks. However, another way to look at their interrelationship is to ask a conditionality question. Does the regime type variable work differently in the presence as opposed to the absence of rivalry? Does the rivalry variable work differently in the presence or absence of democratic regimes? It turns out that the answers to these questions are unambiguous. The presence or absence of rivalry conditions the effect of regime type and not the other way around.

These findings also suggest possible solutions to some levels-of-analysis puzzles related to democratic peace arguments. If democracies rarely fight other democracies, are democracies more or less likely to become involved in conflict on the monadic level? Why are dyads pairing autocracies and democracies more conflict-prone than either autocratic or democratic dyads? The systemic puzzle is how should we expect increasing democratization to influence aggregate conflict? Is any increase in the number of democratic dyads likely to decrease aggregate conflict? Or, must the entire system be democratic before one can anticipate the realization of a system-wide democratic peace? We think that the monadic, dyadic, and systemic puzzles can be addressed successfully in the context of rivalry. Space considerations caution against pursuing the monadic puzzle in the present undertaking but we suspect that democratic and autocratic states exhibit similar conflict propensities in the presence of rivalry. In the absence of rivalry, democracies are less conflict-prone than autocracies. In the major power subsystem at least, and no doubt, thanks to ideological conflicts, mixed dyads are more rivalry-prone than autocratic dyads. Finally, we argue that information on the extent of democratization is simply insufficient to know whether conflict will be reduced or increased at the systemic level. If the effect of regime type is conditioned strongly by the presence or absence of rivalry, we need at least information on both variables to describe trends in systemic transformation. At the same time, all rivalries are not equally significant or dangerous. It only takes one rivalry between two major powers to generate more systemic conflict than the conflict generated by all of the other major powers. Some indeterminancy in moving from a world of autocracy and rivalry toward a world of democracy and the

absence of rivalry—if that is the trajectory on which we are—should therefore be anticipated.

Rivalry and the Democratic Peace

The analysis of democratic peace phenomena has moved well beyond the cottage industry stage. Of course, major questions concerning why there should be a democratic peace, how much credit should go to regime type, and what other phenomena might be encompassed by democratic virtues remain to be answered. But, for the most part, the core finding that democratic states tend not to go to war with other democratic states has emerged from countless tests relatively unscathed.

Yet, for all the empirical analysis, there is at least one outstanding problem with the democratic peace phenomenon that has not received sufficient attention. Because there is a virtually perfect correlation between democratic dyads and the absence of war, we infer a pacifying effect for joint democracies. But other types of dyads—Latin American, West African, Islamic, Warsaw Pact, North American, South Pacific, and, lest we forget, states with McDonalds franchises prior to Kosovo—seem to exhibit some tendencies to avoid war as well.[2] The implication is that there are factors other than regime type that exert constraints on conflict propensities. Moreover, regime type has always seemed a fragile reed upon which to place the heavy explanatory burden of perpetual peace, or even the absence of war. Norms and institutions can certainly matter, but should they matter so much that the future of the planet hinges to a great degree on the universal convergence of domestic institutions around a single format? Some observers might answer in the affirmative but we have our doubts.[3]

As a consequence, there remains some nagging doubt that the phenomenon to be explained—the avoidance of intense hostility and physical conflict by states—may be larger than we suspect. That is, there may well be an emerging proclivity toward peace among some types of states but all or even most of the credit may not belong to regime type alone. We appreciate that advocates of democratic peace explanations may protest that no one claims that the joint possession of democratic regimes is sufficient for dyadic peace. While that may be technically true, much of the literature appears to advance on the premise that the expansion of democratization will lead to expanded zones of peace. This belief—whether implicit or explicit—hovers perilously near not only the necessary but also the sufficient causal categorization.

One reason for the strong beliefs in the democratic source of the peace, other than the powerful empirical results, is the absence of a formidable and successful competitor. If not regime type, what else can explain not only the apparent relationship between joint democracy and nonwar, but also the absence of war between some states? Various analysts have examined all sorts of control variables without challenging successfully—at least so far—the relationship between dyadic regime type and conflict (e.g., Maoz and

Russett, 1991). The absence of a successful challenge to date, however, does not preclude the possibility of a successful challenge at some point, as long as we have yet to exhaust all of the possibilities.

One possibility that has not been pursued adequately is the role of strategic rivalry and its relationship to conflict. Our conceptualization of strategic rivalries differs from the more familiar concept of "enduring rivalries" based on the temporal density of militarized dispute behavior (Diehl and Goertz, 2000). Strategic rivalries begin when the decision-makers of two states perceive each other as threatening competitors. Competition between states in all sorts of venues is reasonably common and all competitors are not necessarily perceived as rivals. Competition can and does proceed without any expectations that the competition is so intense that a competitor may be prepared to do physical harm in order to win the contest. Perceived threats, on the other hand, are probably fairly rare but rivals do not monopolize the sources of threat. For instance, in cases of extreme capability asymmetry, the weaker party may perceive a threat but also realize that it is in little position to compete with, or to resist, the threatener, without the assistance of stronger allies. Alternatively, the stronger party may simply see the weaker party as a nuisance, minor policy problem, or a low-ranking actor serving as proxy for some stronger competitor.

Strategic rivals combine the interactive effect of a perceived threat from an actor considered to be a competitor—that is, an adversary considered to possess sufficient capability on their own to oppose or thwart the attainment of important foreign policy goals, and thereby qualify as playing in the same "league." For example, Haiti and the former Soviet Union, at various times, have posed problems for U.S. foreign policy. The latter was perceived to be a competitor while the former never was. One way to defuse a strategic rivalry, accordingly, is for one side to convince the other side that they are no longer as competitive as they once were. But as long as the rivalry remains active, these dyads represent actors who have selected one another as their most likely opponents. Hence, they are also the actors who are most likely to come to blows.

To be sure, misperception can facilitate conflict but it is assumed here that the most serious conflicts stem from genuine conflicts of interest. Actors are well aware of these conflicts and, on occasion, are prepared to defend or advance their interests in militarized fashions. Our argument is that rivalry should be more central to conflict propensities than is dyadic regime type. We do not see this contention as necessarily a mutually exclusive proposition. This is not an argument that only rivalry can have significance or that dyadic regime type is likely to have no explanatory significance. Rather, we see this conceptual pairing as one comparing a dyadic category for actors who are most likely to fight one another versus another dyadic category for actors who, for some reason, appear unlikely to fight one another. The question is which impulse is stronger and/or to what extent are the two impulses complementary?

It is conceivable that the two categorical distinctions could have equal predictive value. But, we see rivalry as the less ambiguous categorization of the two. Strategic rivals are prepared by definition to compete intensively and, perhaps if necessary, to fight. Democratic dyads have not yet fought wars and may be unlikely to compete as intensively with their democratic counterparts as they are with nondemocratic counterparts. The ambiguity that we see lies not so much with how to categorize dyads but rather what is being categorized. Rivalry zeroes in on dyads with some probability of combat. Democratic dyads focus on pairs of states that are less likely to escalate into extreme conflict. Put another way, rivalries are already or almost "there" (with there equaling high conflict) while many democratic dyads may not have had much reason for conflict in the first place. Two democratic states may not have much reason to be conflictual for a variety of reasons. They may be too distant to compete over territory or too wealthy to have strong incentives to upset the status quo from which they benefit. They may be too asymmetrical in capability for the weaker party to expect to survive a confrontation. They may regard each other as "natural allies" ideologically and trust each other to fulfill commitments. They may be confronted by similar threats from an autocratic and revisionist state. They may realize that public opinion will constrain overly ambitious foreign policies. They may be organized in a coalition by a liberal system leader to oppose radical challenges. The point is that any or all of these conditions may apply in various contingent combinations—with no single factor sufficiently strong to emerge statistically significant—and it may be, as a result, that we are bestowing the net credit for the pacific outcome on the dyadic regime type distinction.

The strategic rivalry construct is less ambiguous, therefore, because we know what it signifies—two states with a higher probability of conflict than two nonrivals, since they have genuine conflicts of interest, regard each other as competitors, and perceive the other side as threatening their security. In contrast, we may agree about which dyads are democratic but we are less sure why that condition leads apparently to pacific outcomes. Of course, the causal ambiguity associated with democratic dyads does not mean that dyadic regime type can have no independent effect. That should remain a theoretical and empirical question.

As explanatory variables, rivalry and regime type parallel one another in interesting ways. They are both inherently dyadic in structure. It takes two states to form a democratic dyad just as it does to from a rivalry. Strong explanatory claims are also made for both variables. Partisans for the notion of a democratic peace have lionized the pacifying virtues of democracies (at least when paired with another state of the same regime type). Only when autocracies are eliminated and all dyads are democratic will peace between states be the norm—or so the most ambitious versions go. Partisans for the rivalry route to conflict explanations claim that knowing who is a rival to whom is crucial for eliminating the noise in the vast majority of interstate interactions. Compared to the immense number of

dyads in the international system, only an extremely small number of rivalries possess genuine potential for conflict. Nonrivals are unlikely to fight one another because they are less likely to have sufficiently intense and genuine conflicts of interest. The history of previous disputes, current threat perceptions, intense levels of suspicion, and fears of the shadow of the future combine to make conflict more probable for rivals who do have genuine conflicts of interest. As a consequence, about 80 percent of the wars since 1816 can be traced to specific rivalry roots. Presumably then, only when all rivalries have been eliminated once and for all can we anticipate very low propensities toward extreme levels of conflict.

Another common denominator is that even though we have high expectations for their explanatory value, we do not possess very strong understanding of precisely how they work. Rivalry processes have only recently become the focus of analysis. As a consequence, the rivalry construct is less ambiguous than our understanding of rivalry processes. Even though the idea of rivalry is quite ancient, explicit attempts to decipher their tendencies toward escalation and deescalation are emerging only now. In contrast, the arguments for a democratic peace are much older than the recent turn toward rivalry analysis. One can go back at least to Kant in the late eighteenth century, if not earlier. Yet in comparison to the relative dearth of theoretical material for explaining rivalry processes, we enjoy a number of alternative explanations for the pacific behavior of democratic dyads. Various sorts of institutional restraints, norms, signaling tendencies, and the triumph of liberalism compete in overlapping ways for explanatory primacy. But they have not proven easy to pin down empirically. Thus, there is considerable evidence for a democratic peace effect but debate proceeds on exactly why that might be the case.

Given these parallels, rivalry and regime type offer an attractive match. The rivalry variable should provide powerful explanatory competition for the dyadic regime type variable. We think that rivalry should be regarded as more central to war and peace questions than regime type considerations. But that does not mean that controlling for rivalries will or must negate the customary explanations for the democratic peace. There is no reason to assume that this is a zero-sum contest with only one winner conceivable. Both variables can have explanatory value. Rather the question should be: what happens to the relationship between joint democracy and conflict when one controls for the presence or absence of rivalries (and vice versa)?

Our basic hypotheses read as follows. The first one we regard as already well-established (e.g., Bremer, 1993) but we include it for the purposes of comparison with the other hypotheses.

H9: *Democratic dyads are less prone to militarized disputes and wars than are nondemocratic dyads.*

If, by definition, strategic rivalry information differentiates states that have concrete reasons to engage in conflict and that see their rivals as threatening competitors from dyads that are not engaged in such relationships, it

seems reasonable to anticipate that rivals are more likely to become involved in disputes and attempts at coercion than are nonrivals.

H10: *Strategic rivalry dyads are more prone to militarized disputes and wars than are dyads not engaged in strategic rivalry.*

In addition, we have argued above that the rivalry relationship is more directly linked to conflict than is the shared regime type relationship. Rivalries are very much about protracted conflict that has already been manifested while regime type, at best, is a constraint on potential and manifested conflict. The beginning of a rivalry suggests that conflict intensity has increased. Rivalry termination suggests that either conflict intensity has abated considerably or else the dyad members no longer view each other as competitors. In contrast, two states can possess the same regime type without having much, if any, contact with one another. Two states can also have considerable contact without developing goal incompatibilities, and either share regime types or not. One implication is that the connection between regime type and conflict is apt to be looser than the connection between rivalry and conflict.

H11: *The effect of strategic rivalry on militarized dispute and war behavior is greater than is the effect of dyadic regime type.*

However, we do not rule out the possibility that the relative strengths of these variables has changed over time, if for no other reason than that in the nineteenth century, there was little variance in regime type. Moreover, one school of thought argues that what we are perceiving to be a democratic peace is actually limited only to the post-1945 era and is a function of other processes such as Cold War alliance constraints.[4] While the evidence for this point of view is disputed it is not an interpretation that can be dismissed out of hand.[5] This is all the more true since it is probable that some readers may have problems differentiating our emphasis on strategic rivalries from Farber and Gowa's claims about common interests providing the key to greater and lesser disputatiousness.

Gowa (1999) and others argue that states with common interests are less likely to become involved in disputes and wars than states that lack common interests. They find that democratic dyads only become relatively pacific after 1945 and not before 1914 and they attribute this finding to the common interests that democratic states developed in the Cold War after 1945. Lacking any direct measure of common interests, they fall back on alliance measures as an admittedly crude proxy. Essentially, they find that democratic states were more likely to become allied with other democratic states only in the Cold War era and not prior to 1914. This finding is taken to bolster their common interest interpretation, even though it falls far short of anything resembling a direct test of their argument.

We suspect that there are major problems in viewing formal alliances as indicators of common interests in general. The OAS, the OAU, the Arab League, NATO, and the Warsaw Pact have all qualified as formal alliances

with respectable durations but it is difficult to generalize about the extent to which they all indicate common interests in even a roughly similar fashion. It is also possible to argue that the nineteenth-and twentieth-century alliance propensities of democratic states may have more to do with shifts in British and U.S. leadership strategies. When Britain was the system's liberal leader, it preferred to avoid alliances, as did the United States until it ascended to the liberal lead. Do rivalries offer a better indicator of interests than alliances? Obviously, they do not tap into common interests very well. They do isolate more intense conflicts of interest that might be viewed as the converse of common interests. But this only means that rivalries and common interests may be on the same conceptual continuum. What we cannot assume is that more or fewer rivalries means fewer or more common interests. Imagine a continuum with intense conflicts of interest at one end and special relationships or strong cooperative arrangements (common interests) at the other end. Most interstate dyads are at some midpoint on this continuum with their relationships being characterized by a mixture of moderate conflict and cooperation. Some dyads move away from the midpoint toward one or the other extreme. Once dyads are at the extreme endpoints, they tend to move back toward the midpoints at some point. Rivalries de-escalate and special relationships deteriorate. What is even more rare is for dyads to move rapidly from one extreme endpoint to the opposite endpoint. The implication is that changes at one or both ends of the continuum need not be very informative about what is taking place at the opposite endpoint.

Whether or not common and severe conflicts of interest are thought to be on the same continuum, it should be kept in mind that Farber and Gowa have not yet established that the pacific nature of post-1945 democratic dyads is due to more common interests after 1945 than before. That is simply their inference from indirect analyses about the relationships between dyadic regime type and dispute behavior, and dyadic regime type and alliance behavior. Even if there were more common interests among democratic states after 1945 than before, it is not altogether clear that this development would violate democratic peace expectations. Democratic peace arguments could be said to be about moving the midpoint of the conflict/cooperation continuum in the direction of the highly cooperative endpoint. Such a movement need not take place overnight. If such transformations take time, we are back to the possibility that the effect of the dyadic regime type variable may grow stronger over time.

Indeed, the most simple explanation is that the democratic peace phenomenon is an emergent property of the world system. We would not have expected it to be fully manifested in the sixteenth, seventeenth, or eighteenth centuries, although we may see some hints of the phenomenon in the mid- and late-seventeenth-century behavior of the English and Dutch republics. The liberal–autocratic alignments of the twentieth-century world wars can be viewed as following in this tradition.[6] Moreover, it is only in the nineteenth century that democratic dyads began to emerge in any

number. Accordingly, if the pacifying effect of dyadic regime type is an emergent property, we should expect its effect to become stronger over time.

We would expect the effects of strategic rivalry, on the other hand, to be fully operative throughout the nineteenth and through at least the first half of the twentieth century. Various and reasonably well-known arguments exist about the restraining effects of bipolarity and nuclear weapons (see, among others, Waltz, 1993) that could be seen as constraining the impact of major power strategic rivalries in the second half of the twentieth century. Thus, we have still another interesting parallel. There are reasons to anticipate one variable (dyadic regime type) to be gaining in explanatory strength over time while the other variable's (strategic rivalry) explanatory strength may be weakening.

H12: *The relative explanatory value of strategic rivalry and regime type are unlikely to be constant. The relative contribution of dyadic regime type vis-à-vis strategic rivalry should be expected to improve over time.*

Moreover, we propose to further complicate/enrich the analysis by breaking down nondemocratic dyads into autocratic and mixed dyads. The argument is that mixed dyads, that is, dyads combining democracies and nondemocracies, are even more conflict-prone than either autocratic or democratic dyads (Gleditsch and Hegre, 1997). If this is the case, we need to move away from dichotomizing dyads into democratic versus nondemocratic categories. However, the obvious substantive question is why this rank order prevails? Other things being equal, one might expect mixed dyads to fall in between what are often taken to be the opposite ends of the regime type continuum. As mixtures, the dyads might combine some of the pacificity and bellicosity attributed to democratic and autocratic regimes. They might then be somewhat less bellicose than autocratic dyads and somewhat less pacific than democratic dyads. But another possibility is that these three types of dyads are not equally prone to fostering rivalries. If mixed dyads are more rivalry-prone than autocratic dyads, other things being equal, we might expect mixed dyads to be more conflictual than autocratic dyads. It could also be that the conflicts of mixed dyads are more likely to be ideological, and therefore inherently more intractable in nature. It is one thing to surrender territory or even relative influence in a pecking order. It is quite another to acknowledge the defeat of a belief system. Should the tripartite distinction be upheld, we will have to see whether that means hypothesis 3 requires revision.

H13: *Mixed regime type dyads are more prone to militarized dispute and war behavior than are autocratic dyads, both of which, in turn, are more prone to militarized dispute and war behavior than are democratic dyads.*

In addition to the overt dyadic nature of our hypotheses, the analysis focuses exclusively on the major power subsystem. We do this for four substantive reasons. First, war and conflict are not generated solely by major powers, but they have generated more than their fair share of it. If strategic

rivalry and dyadic regime type are significant variables in accounting for conflict, they should be significant for this elite and historically highly conflictual subsystem. A second reason is that immediately before the democratic peace idea became so fashionable, there was considerable analytical interest in a different kind of peace—the long peace (Gaddis, 1987, 1991). Major powers had not fought each other since at least the Korean War. While a long stretch of nonwar among major powers is not unique to the post–World War II era, it is something worth explaining. It is also a phenomenon that seems similar in nature to the democratic peace but one that cannot be explained solely by the democratization of the major power subsystem—something that has yet to occur. If peace is our objective, we need to be careful about compartmentalizing different types of peace, and the explanations that go along with them, prematurely.

The major power subsystem seems all the more appropriate a place to begin because, not only has it been an unusually dangerous arena subject to intermittent pauses, it also has been characterized by increasing democratization and decreasing tendencies toward rivalry. At the end of the Napoleonic Wars, five major powers were linked by six major power rivalries. During the 1990s, no major power rivalries remained in existence—an entirely unprecedented situation. By Polity III (Jaggers and Gurr, 1995) standards, it was not until some 65 years after Waterloo that the major power subsystem finally developed its first major power democratic dyad. As we enter the twenty-first century, the only definite democratic holdout among the major powers is China. Thus, democratization and rivalry developments have demonstrated a classic scissors pattern with democratization on the ascent and strategic rivalry on the descent. We are asking why one blade of the scissors should receive all or most of the credit. Until we look at both blades simultaneously, it will be difficult to apportion credit and blame appropriately.

Finally, we feel most comfortable beginning our analyses with a small group of states rather than plunging immediately into a large *N* and even larger dyadic *N* encompassing all states that have been independent since 1816. Obviously, our findings will be limited at least initially to the sample examined, but we will have also avoided some of the empirical noise associated with very large dyadic data sets in which many of the dyads are not only inactive but also largely irrelevant to one another.[7] We may also be focusing initially on the single most important sample or grouping of states conceivable given our interest in explaining conflict behavior.

Testing Considerations

To reduce possible sources of criticism about the validity of our results, we adopt as catholic an approach to identifying major powers as possible by accepting the Correlates of War definition (see the appendix). From a rivalry perspective, these temporal conventions are not always convenient and we have some misgivings about their Eurocentricity. For instance, the

Table 5.1 Major power status and rivalry

Dyad	*Major power*	*Rivalry*
Austria–Britain	1816–1918	Absent
Austria–China	NA	Absent
Austria–France	1816–1918	p1816–1918
Austria–Germany	1816–1918	p1816–70
Austria–Italy	1860–1918	1848–1918
Austria–Japan	1895–1918	Absent
Austria–Russia	1816–1918	1816–1918
Austria–United States	1899–1918	Absent
Britain–China	1950–92	1831–1900
Britain–France	1816–1992	p1816–1904
Britain–Germany	1816–1945	1896–1918; 1934–45
Britain–Italy	1860–1943	1934–43
Britain–Japan	1895–1945	1932–45★
Britain–Russia/USSR	1816–1992	p1816–1956
Britain–United States	1899–1945	p1816–1904★
China–France	1950–92	1844–1900
China–Germany	NA	1897–1900
China–Italy	NA	Absent
China–Japan	NA	1873–1945
China–Russia	1950–92	1816–1949; 1958–89★
China–United States	1950–92	1949–78★
France–Germany	1816–1945	p1816–1955
France–Italy	1816–1943	1881–1940
France–Japan	1895–1945	Absent
France–Russia	1816–1992	p1816–94
France–United States	1899–1992	1830–71
Germany–Italy	1860–1943	Absent
Germany–Japan	1895–1945	Absent
Germany–Russia	1816–1945	1890–1945
Germany–United States	1899–1945	1889–1918; 1939–45★
Italy–Japan	1895–1943	Absent
Italy–Russia	1860–1943	1936–43
Italy–United States	1899–1943	Absent
Japan–Russia	1895–1945	1853–1945
Japan–United States	1899–1945	1900–45
Russia–United States	1899–1992	1945–89

Notes

1. p indicates that the rivalry began before 1816.
2. ★ indicates that a rivalry relationship predates the attainment of major power status by one or both members of the dyad.
3. Rivalries that ended before both states attain major power status are not shown unless the dyad was engaged in a rivalry relationship after both states acquired major power status.

United States engaged in rivalry with Britain from 1783 on and with France from 1831 to 1871 (table 5.1). Both Britain and France sought to contain the expansion of the United States in the Americas. U.S. expansiveness alone could not make the United States a major player in Europe but it did make it one of the leading actors in North and South America

long before 1898. Italy in the form of Piedmont was engaged in rivalry with Austria prior to 1860 and the Russo-Japanese, Sino-Russian, and Sino-Japanese rivalries all predate 1895. Yet something similar could be said about the 1816 starting point since a number of major power rivalries predate the end of the Napoleonic Wars. The general point, presumably, is that there is no completely ideal way to approach the analysis without introducing some form of bias. Our preference in this analysis is to accept the major power conventions and biases most commonly encountered in the literature.

Most of our other data (militarized interstate disputes and wars) are equally conventional. Polity III data are used for analyses that compare democratic, autocratic, and mixed (democracies on only one side) dyads. Rivalry data, as before, are taken from the strategic rivalry data set (Thompson, 2001). For this analysis, while we draw upon a data bank on rivalry identifications based on actor perceptions that encompass the 1816–1999 period, the focus on rivalries is also restricted to rivalries *between* major powers. Such a focus requires us to censor behavior on rivalry that takes place when actors are not considered major powers, even though they may become major powers at a later date. It also means that we pay no attention in this examination to rivalries between major and nonmajor powers.

We estimate the dispute aspect of our hypotheses with time-series, cross-sectional data and with a binary dependent variable using ordinary logit.[8] Given the relatively few wars between major powers and the nature of their distribution vis-à-vis the regime type variable, the war aspect of the hypotheses is analyzed separately using cross-tabulations accompanied by the usual tests of significance.

Analysis

Table 5.2 shows that regime type and rivalry are statistically significant and related to militarized dispute behavior while controlling for the number of years between disputes. As expected (hypothesis H9) democratic dyads are less likely to be associated with disputes than nondemocratic dyads. The odds of democratic dyads being involved in disputes decreases by 54 percent in comparison to other types of dyads. Also as anticipated (hypothesis H10), rivalry increased the odds of disputes by 146 percent. As for the general effect that regime type has on dispute behavior relative to rivalry (hypothesis H11), the inverse of the negative odds ratio coefficient for regime type is 2.17 and quite close to the rivalry coefficient of 2.46.

On the other hand, when we estimate the conditional probabilities of this model, the results (reported in the bottom half of table 5.2) show that the combination of nondemocratic dyads with rivalry is associated with a 0.60 probability of dispute occurrence. When rivalry is absent, the probability of disputes for these types of dyads decreases to 0.38. The pattern is similar for democratic dyads. Without rivalry, democratic dyads are associated with a 0.22 probability of becoming involved in a dispute; with rivalry, the probability increases to 0.41.

Table 5.2 Logit estimates for militarized disputes, 1816–1992

Variables	*B*	*Robust S.E.*	*z-value*	*Odds ratio coefficient*
Regime	−0.79**	0.31	−2.54**	0.46
Rivalry	0.90**	0.15	6.11**	2.46
Constant	−0.48**	0.15	−3.55**	–
Peace Years	−1.05**	0.09	−11.65**	0.35
Spline(1)	−0.03**	0.00	−8.61**	0.97
Spline(2)	0.01**	0.00	7.43**	1.01
Spline(3)	−0.00**	0.00	−3.55**	1.00
Log likelihood	−745.31			
Chi-square	464.75**			
Significance	0.00			
Sample size	2,618			

Notes

1. Regime is coded as 0 and 1; 0 = nondemocratic dyads, 1 = democratic dyads.
2. Rivalry is coded as 0 and 1; 0 = absence of rivalry, 1 = presence of rivalry.
3. Robust S.E. refers to Huber/White estimates.
4. Asterisks denote statistical significance at 0.05 level or lower.
5. Spline coefficients are based on duration of Peace Years variable.

Probabilities of disputes (1816–1992) based on the estimated coefficients in upper table

	P
Regime dyads are nondemocratic; rivalry is absent	0.38
Regime dyads are nondemocratic; rivalry is present	0.60
Regime dyads are democratic; rivalry is absent	0.22
Regime dyads are democratic; rivalry is present.	0.41

In order to compare the influence of various regime combinations on disputes, we estimate three logit models regressing disputes on autocratic and democratic dyads, democratic and mixed dyads, and autocratic and mixed dyads. The results summarized in table 5.3 indicate that rivalry continues to demonstrate a significant influence regardless of the regime combination. Democratic dyads in comparison to autocratic dyads are significantly less likely to be associated with disputes (although democratic dyads are not significantly different than autocratic dyads). Moreover, mixed dyads are significantly more likely than autocratic dyads to be involved in disputes. These results support hypothesis H13: autocratic and mixed dyads are indeed more disputatious than democratic dyads while mixed dyads are more disputatious than autocratic dyads.

Rivalry, however, continues to have a strong positive impact across all three types of dyads. The conditional probabilities (at the bottom of table 5.3) show that the probability of dispute behavior for each regime combination is higher when rivalry is present than when it is absent. These probabilities range from 0.38 to 0.69 in comparison to the probabilities of 0.18 to 0.44 for non-rival regime combinations.

Table 5.3 Logit estimates of militarized interstate disputes on three types of regime dyads and rivalry, 1816–1992

Variables		*Autocratic and democratic*	*Democratic and mixed*	*Autocratic and mixed*
Regime	B	−0.53	1.07**	0.56**
	S.E.	0.34	0.32	0.14
Rivalry	B	0.76**	1.24**	0.90**
	S.E.	0.22	0.19	0.15
Constant	B	−0.72**	−1.52**	−0.80**
	S.E.	0.24	0.33	0.18
Peace Years	B	−1.00**	−0.99**	−1.05**
	S.E.	0.13	0.12	0.09
Spline(1)	B	−0.03**	−0.02**	−0.03**
	S.E.	0.00	0.00	0.00
Spline(2)	B	0.01**	0.01**	0.01**
	S.E.	0.00	0.00	0.00
Spline(3)	B	−0.00**	−0.00	−0.00**
	S.E.	0.00	0.00	0.00
Log likelihood		−371.35	−402.99	−696.63
Chi-square		219.72**	299.48**	412.58**
Significance		0.00	0.00	0.00
Sample size		1,565	1,390	2,281

Notes

1. Regime is coded as: autocratic = 0 & democratic dyads = 1; democratic = 0 & mixed dyads = 1; and autocratic = 0 & mixed dyads = 1.
2. Rivalry = 0 for absence; 1 = presence.
3. Standard errors are Huber/White estimates.
4. Asterisks denote statistical significance at 0.05 level or lower.
5. Spline coefficients are based on Peace Years duration variable.

Probabilities of disputes (1816–1992) based on the estimated coefficients in table 5.3

	P
Based on eq.1 (autocratic and democratic)	
Regime dyads are autocratic; rivalry is absent	0.33
Regime dyads are autocratic; rivalry is present	0.51
Regime dyads are democratic; rivalry is absent	0.22
Regime dyads are democratic; rivalry is present	0.38
Based on eq.2 (democratic and mixed)	
Regime dyads are democratic; rivalry is absent	0.18
Regime dyads are democratic; rivalry is present	0.43
Regime dyads are mixed; rivalry is absent	0.39
Regime dyads are mixed; rivalry is present	0.69
Based on eq.3 (autocratic and mixed)	
Regime dyads are autocratic; rivalry is absent	0.31
Regime dyads are autocratic; rivalry is present	0.53
Regime dyads are mixed; rivalry is absent	0.44
Regime dyads are mixed; rivalry is present	0.66

We infer from these results that regime type is an important predictor of dispute involvement but rivalry conditions the relationship significantly. Democratic dyads are less likely to be involved in disputes but that probability declines substantially in the presence of rivalry. Autocratic and mixed dyads, or nondemocratic dyads in general, are more likely than democratic dyads to be involved in disputes but the probability of dispute involvement increases dramatically when combined with the presence of a rivalry. We view these findings as support for hypothesis H3's prediction that rivalry is a more important factor than dyadic regime type.

Our hypotheses refer to both militarized disputes and war. Tables 5.2 and 5.3 have focused exclusively on disputes. Tables 5.4 and 5.5 focus on interstate war and there are no surprises in either table. Table 5.4 assumes that democratic and nondemocratic dyads are related significantly to the onset of war. If we control for the presence or absence of rivalry, the relationship between dyadic regime type and war remains statistically significant. Note, though, that war is almost three times as likely to take place when rivalry is present as when it is absent.

Table 5.5 repeats the analysis conducted in table 5.4 but this time utilizes the tripartite distinction among dyadic regime types. The outcome in

Table 5.4 Cross-tabulation of war on regime dyads, contingent on the absence/presence of rivalry, 1816–1992

	Rivalry absent		*Total*
	Nondemocratic dyads	*Democratic dyads*	
War	31	0	31
	(2.7)	(0.00)	(2.2)
No War	1,117	292	1,409
	(97.3)	(100.0)	(97.8)
Total	1,148	292	1,440
	(100.0)	(100.0)	(100.0)

Chi-square 8.06**; $p = 0.01$.
Fisher's exact test = 0.00.

	Rivalry present		*Total*
	Nondemocratic dyads	*Democratic dyads*	
War	86	0	86
	(7.6)	(0.00)	(7.3)
No War	1,047	45	1,092
	(92.4)	(100.0)	(92.7)
Total	1,133	45	1,178
	(100.0)	(100.0)	(100.0)

Chi-square 3.68**; $p = 0.05$.
Fisher's exact test = 0.07.

Table 5.5 Cross-tabulation of war on regime types, contingent on absence/presence of rivalry, 1816–1992

	Rivalry absent			*Total*
	Autocratic dyads	*Democratic dyads*	*Mixed dyads*	
War	14	0	17	31
	(2.5)	(0.00)	(2.9)	(2.2)
No War	550	292	567	1409
	(97.5)	(100.0)	(97.9)	(97.8)
Total	564	292	584	1440
	(100.0)	(100.0)	(100.0)	(100.0)

Chi-square for full table: 8.3**; Fisher's exact test, $p = 0.00$.
Chi-square for autocratic vs. democratic dyads: 7.4**; Fisher's exact test, $p = 0.00$.
Chi-square for autocratic vs. mixed dyads: 0.20; Fisher's exact test, $p = 0.72$.
Chi-square for democratic vs. mixed dyads: 8.7**; Fisher's exact test, $p = 0.00$.

	Rivalry present			*Total*
	Autocratic dyads	*Democratic dyads*	*Mixed dyads*	
War	37	0	49	86
	(5.6)	(0.00)	(10.5)	(7.3)
No War	627	45	420	1092
	(94.4)	(100.0)	(89.6)	(92.7)
Total	664	45	469	1178
	(100.0)	(100.0)	(100.0)	(100.0)

Chi-square for full table: 13.3**; Fisher's exact test, $p = 0.00$.
Chi-square for autocratic vs. democratic dyads: 2.6*; Fisher's exact test, $p = 0.16$.
Chi-square for autocratic vs. mixed dyads: 9.3**; Fisher's exact test, $p = 0.00$.
Chi-square for democratic vs. mixed dyads: 5.2**; Fisher's exact test, $p = 0.02$.

table 5.5 is very similar to the outcome in table 5.4 and resembles the outcome reported in table 5.2 for militarized disputes. Dyadic regime type retains its statistically significant relationship with war, regardless of the presence or absence of rivalry. Perhaps the most interesting feature, though, is not only the very low probability of warfare in the absence of rivalry found in both tables 5.4 and 5.5, but also the little difference between autocratic and mixed dyads' war propensities in the absence of rivalry (see the insignificant chi-square test reported in table 5.5). Adding rivalry to the mix, however, alters the situation considerably. The gap between autocratic and mixed dyads widens (5.6 percent versus 10.5 percent) as does the contrast between the two types of nondemocratic dyads and democratic dyads, as predicted by hypothesis H11. The analysis of war behavior, then, yields findings that are quite similar to the findings for militarized disputes. The findings summarized in tables 5.4 and 5.5 also provide more empirical support for hypotheses H9–H11 and H13.

For purposes of presentation convenience, we have postponed testing hypothesis H12, which predicts that the relative effects of rivalry and regime type are not constant. Table 5.6 contrasts the relative explanatory contributions of the two variables, in the context of militarized disputes, for two time periods: 1816–1945 and 1816–1992. We might have contrasted the pre- and post-1945 periods more directly but this approach allows us to avoid an asymmetrical *N* situation. If the effect of rivalry is diminishing over time relative to regime type, we should expect to find that the relative effect of regime type is stronger in the full 1816–1992 period than it is in the 1816–1945 period. As long as the effect of rivalry either diminishes or stays about the same, the relative effect of regime type should grow stronger. The outcomes summarized in table 5.6 are quite supportive of hypothesis H12, especially when democratic dyads are compared to either nondemocratic or autocratic dyads. The log odds ratio coefficient is 2.17 and statistically significant in the 1816–1992 period as compared to the statistically insignificant coefficient of 1.46 in the 1816–1945 period. Furthermore, the effects of regime type and rivalry are about the same in the full period (2.17 versus 2.46), whereas rivalry has the stronger effect (2.28 versus 1.46) in the pre-1945 era.

The regime comparison of democratic and mixed dyads is significantly related to dispute behavior in both time periods, albeit at the 0.10 level.

Table 5.6 Log odds ratio coefficients of militarized interstate disputes on four types of regime dyads and rivalry, 1816–1992 and 1816–1945

Variables	*Nondemocratic and democratic*	*Autocratic and democratic*	*Democratic and mixed*	*Autocratic and mixed*
1816–1992				
Regime	2.17**[a]	1.72[a]	2.93**	1.75**
	(−2.54)	(−1.56)	(3.33)	(3.91)
Rivalry	2.46**	2.13**	3.45**	2.46**
	(6.11)	(3.39)	(6.23)	(5.94)
Sample size	2,618	1,565	1,390	2,281
1816–1945				
Regime	1.46[a]	1.22[a]	1.82*	1.21**
	(−1.16)	(−.60)	(1.78)	(2.39)
Rivalry	2.28**	1.82**	3.41**	2.15**
	(5.01)	(2.72)	(4.98)	(4.43)
Sample size	2164	1377	977	1974

Notes

a These coefficients are derived from taking the inverse of negative log odds ratio coefficients in order to facilitate direct comparisons between positive and negative effects. Positive and negative odds ratio coefficients are not measured on the same scale (Long, 1997: 82).

b T-statistics are reported below the odds ratio coefficients and are based on robust standard errors. Double asterisks indicate significance at 0.05 level or lower; single asterisk indicates significance at 0.10 level or lower.

c All eight models are estimated with the Peace Years duration variable and its three cubic spline segments.

However, the log odds ratio coefficients for these dyads do not differ substantially: 2.93 in the 1816–1992 period and 1.82 in the 1816–1945 period. Comparing autocratic and mixed dyads, table 5.6 shows that the effect of regime type is significant in both time frames. Nevertheless, the log odds ratio coefficients show that rivalry has a much stronger effect than any regime combinations in both the full and the pre-1945 periods.[9]

Some Implications

We now know that rivalry and regime type provide important information about militarized dispute and war behavior in the major power subsystem. Both variables are significantly related to conflict, but rivalry appears as a somewhat more influential predictor, especially in the case of disputes. What should we do with this knowledge? One implication of our findings that is obvious is that information on rivalries is likely to prove very useful in explaining other types of conflict propensities. Rivalry information may also have further implications for the various processes hitherto attributed primarily or in part to regime type characteristics. To say more without further analysis and further extension of the rivalry data set, however, would not be prudent.

Our findings are suggestive of the controversies over extending dyadic peace arguments to monadic and systemic levels of analysis. If democratic dyads are relatively pacific, what should we anticipate for the conflict behavior of democratic states in general? Some analyses argue that we have no reason to expect democratic states to act any differently than other types of regimes. Others argue that the institutional variant of democratic peace explanations suggests that democratic states should be less bellicose than other types of regimes. Empirical findings are available currently that support both positions.[10] We suspect that the introduction of rivalry data, in conjunction with directed dyads, may help clarify this problem. For instance, if rivalry can counter the pacific effects of democratic regimes, it is conceivable that monadic differences in behavior by regime type depend on the intervening variable of rivalry. However, that is a question that we leave to another analysis.

We think that the systemic controversy is somewhat easier to address but not to resolve within the context of our present findings.[11] In this case, the argument is over what we should expect to find at the systemic level if it is demonstrated that one intermittently expanding cluster of states (democratic dyads) is more pacific than other states in general. One interpretation is that the more democracies and democratic dyads, the less conflict one should anticipate.[12] Another view, however, argues that the transition from few to many democracies is vulnerable to increased conflict between mixed regime type dyads that should also increase in number in the middle of the transition. If true, a curvilinear relationship between systemic democratization and conflict should be anticipated (Gleditsch and Hegre, 1997). A third view (Ray and Wang, 1998) is that it is fallacious to expect any similarity in

relationships between variables at different levels of analysis. Still a fourth view is that it is hard to say what might happen in the transition from no democracies to a situation in which all states in the system are democratic. It may only take a small number of nondemocracies to cause a great deal of trouble. One powerful nondemocracy may be sufficient to generate a third world war. Moreover, the democratization transition of the system will require many years to complete and there is no reason to expect that the process will be any more linear and continuous than it has been in the past.

We are most sympathetic to this fourth position. Our argument all along is that too much emphasis should not be placed on the singular virtues of regime type. Its relationship to militarized disputes, if not war, is strongly conditioned by the presence or absence of rivalries. Extrapolating to either the systemic level of analysis or future system periods on the basis of a single variable is a dangerous analytical practice. It is reminiscent, in some respects, of forecasting future limits to growth on the basis of current energy consumption rates and known reserves while at the same time ignoring the possibilities of variations in demand, expansion of supply through new discoveries, or the innovation of new technologies with different fuel requirements. There is nothing wrong with modeling or projecting univariate realities as long as we keep in mind just how simple the models really are.

For various reasons, there are no overt major power strategic rivalries at the present. But, we have little reason to expect these circumstances to continue indefinitely into the future. European rivalries seem unlikely to reemerge with the same fervor of the 1494–1945 era, but the same cannot be said about former Asian rivalries that do appear to have some potential for reemergence within the next few decades.[13] If they should reemerge in an intense form, the institutional and/or normative constraints associated with democracy, whether they are present there or elsewhere, may not suffice to head off serious conflicts. It is even possible that major power rivals with democratic political systems will reemerge in the future. If rivalry has been able to overwhelm the pacifying effects of regime type in the past, can we dismiss the possibility that it will do so again in the future?

Whether major power rivalries are likely to reemerge or not is not the most pertinent question to raise at this point. What is most clear is that major power rivalry de-escalation has been occurring in rough parallel fashion with increased major power democratization. These trends are depicted in figure 5.1. One series calculates the annual mean level of democracy attributed to the major power set by Polity III standards.[14] The major power subsystem mean democracy score begins around a low −4 and rises to a high of 6.0 for the years 1991–92. The second series represents the ratio of the number of major power rivalries in existence divided by the maximum number of major power rivalries possible in each year. There are certainly other ways to count rivalries but this approach has the benefit of normalizing for the varying number of major powers. This score begins at 0.7 in 1816 and drifts up and down to a low of 0.0 in 1992.

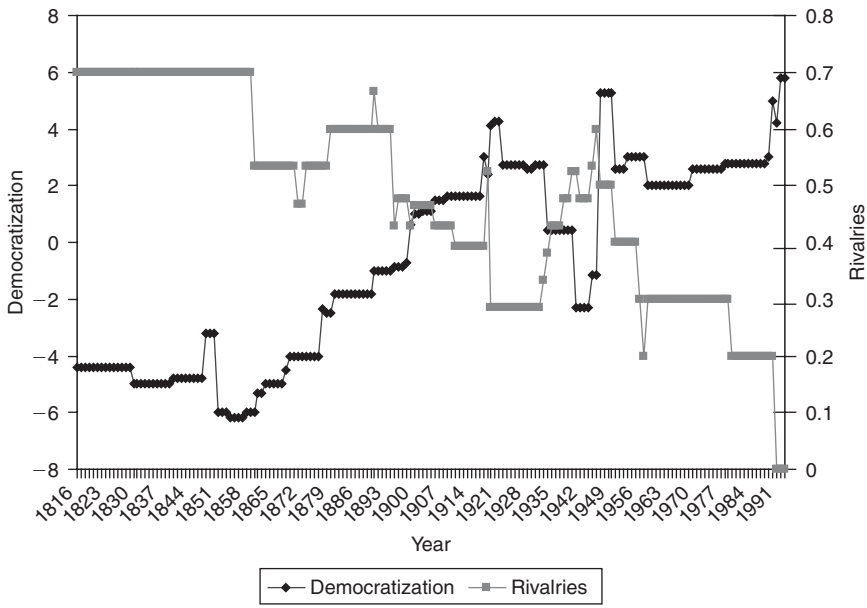

Figure 5.1 Democratization and rivalries in the major power subsystem

A visual examination of the series suggests that there may be some correlation between the two series. Two series with strong trends, even if oppositely signed, are likely to be correlated even if there is no substantive relationship. But at various points, as in the 1850s, the 1930s, or the 1980s, the two series do seem to move further apart in some synchronization. These systemic movements may be worth further analysis in terms of examining antecedence but that is neither our immediate question or purpose in plotting the values. Rather, we wish to draw attention to what is being done by aggregating these data at the systemic level.

One approach to moving up the level of analysis empirically is to aggregate the monadic or dyadic attributes as characteristics of the system. Figure 5.1, as it happens, aggregates monadic attributes to the systemic level for the sake of calculation convenience but we could have aggregated the dyadic attributes as well. As a general practice, we do not wish to quarrel about the utility of aggregation. But, sometimes, attributes do not aggregate awkwardly. Rivalry is a case in point.

Aggregating rivalries has the same problem that aggregating other international relations phenomena, such as democracies, alliances, or wars, possess. All rivalries, democracies, alliances, and wars are not equal in their significance. When we aggregate, we assume that each unit is roughly comparable.[15] But would anyone be willing to argue that the Franco-Italian

rivalry was as critical to European developments as the Franco-German rivalry? Were the Sino-U.S., Soviet-U.S., and Sino-Soviet rivalries all equally important to Cold War processes? The obvious retort is that some weighting scheme would be useful and that may be true, but on what basis do we weight? Intuition, population, wealth, and military capability are all possibilities and all have problematic implications. The point remains that we do not have at this point in time a very compelling formula for weighting.

Perhaps even more telling, however, is that aggregating rivalry processes distorts the way rivalry processes presumably have worked in the major power subsystem. We do not claim to know as much as we might like to know about these processes. Yet some dimensions of the history of major power rivalries are reasonably well known. For example, compare the prelude to World Wars I and II as depicted in figure 5.1. The rivalry index declined in the years immediately before World War I while it increased immediately prior to World War II. If we stopped the analysis in the early 1930s, the impression that is given is that the number of rivalries in the system is negatively linked to the onset of global war. If we were to start the analysis in the 1920s, though, the opposite relationship would probably emerge—the number of rivalries is positively related to the probability of global war. Interestingly, much the same observation might be made about the relationship between the level of democracy and global wars, as long as we reverse the signs (a positive relationship prior to World War I and negative for World War II).

Now it may be that we can generate some post hoc explanations for this seeming anomaly. One could argue that the extent of subsystemic democratization between 1816 and 1913 was formidable, albeit unevenly manifested in different major powers—yet it fell short of attaining very high mean levels (1.38 in 1913). Nor was the negative democratization experienced in the interwar years entirely coincidental to the ideological alignment of major powers in the 1939–45 showdown. One could also point out that the reason why the rivalry count was declining prior to World War I was due to British negotiations with Japan, France, Russia, and the United States to de-escalate its rivalries with those countries in order to cope more adequately with a new principal rivalry with Germany. Prior to World War II, alignment processes worked "differently" with new rivalries emerging, or old ones being rekindled, as countries made their choices as to which side they preferred.

In reality, the alignment processes did not actually function differently. In both cases, decision-makers chose to align or de-align. In some cases, old rivalries were de-emphasized while in others, new ones were given emphasis. The difference was that the World War I alignments reflected processes in the nineteenth and early twentieth centuries in which some rivalries had had time to more or less play themselves out (the Anglo-American and Anglo-French ones in particular) or, temporarily, to lose some of their threat potential (the Anglo-Russian rivalry). At the same time, some one-time allies (Germany and Austria-Hungary vis-à-vis Russia) had had time to develop

new conflict cleavages and others formed anew (the Anglo-German rivalry) or increased in intensity (the Franco-German rivalry).

World War II, on the other hand, reflected many of the exact same rivalries that were important to World War I. Rivalries between Germany and Russia, Britain and Germany, and Germany and France were probably the most significant ones in the European region. The origins of all three predated 1914. But the combat in World War I had reduced the aggregate number of major power rivalries. Austria-Hungary no longer existed. Other rivalries seemed resolved in the 1920s, only to reemerge in the 1930s and thereby increasing the aggregate number of rivalries in the subsystem.

Thus, we can conceivably explain after the fact what figure 5.1 seems to suggest and/or why different relationships seem to pertain at different times. What we cannot do is to expect aggregated rivalry counts (and perhaps aggregated democracy counts as well) to consistently measure the processes that we are trying to tap into empirically. The exceptions to this statement are at the two ends of figure 5.1. At the beginning of the figure (in 1816), the subsystem is portrayed as characterized by a relatively high number of rivalries and a very low level of democracy. Toward the end (1994–95), the major power subsystem is described as possessing no ongoing rivalries and moving toward a fairly high mean level of democracy. The interpretative problem lies in making sense of the many years of fluctuations between 1816 and 1995, or as someone else has noted, the devil is in the details. Trying to interpret year-by-year changes in the scores as fully and consistently meaningful is only asking for analytical problems. Aggregating monadic or dyadic attributes to index systemic attributes is not something that should be attempted automatically. Sometimes, as in the case of rivalries, it is a procedure that needs to be very carefully contemplated before attempting because some rivalries are more important than others.

At the dyadic level, we would expect a rivalry to be more conflictual than a non-rivalry and a democratic dyad to be less conflictual than a non-democratic dyad, et ceteris paribus. At the systemic level, we would feel comfortable in arguing that a system with no rivalries should be less conflictual than a system with rivalries. Similarly, a system of only democratic states should be less conflictual than a system with states of different regime types. However, we would need to quickly qualify this last statement by adding that the safest statement, given our current findings, is that a system of exclusively democratic states *with* no rivalries should be less conflictual than a system with a mix of regimes and rivalries. Whether we are destined to arrive at a fully democratized system without any strategic rivalries, and how we might arrive at such an end point, remains unclear. Yet if we are moving toward such an end point, we would expect the path to be more rocky than smooth.

However rocky that path may prove to be, it may be heartening to find that the effect of regime type appears to be increasing. We cannot totally eliminate the possibility that the post-1945 regime type outcome is an

artifact of the Cold War era as some authors have suggested. This interpretation remains one way to view the pre-1945 explanatory weakness of the regime type variable. But it seems just as likely, if not more so, that what our findings are revealing is that the effect of the regime type variable is not fixed. The democratic peace appears to be an emerging process. In the nineteenth century, there were precious few democratic dyads to test its strength. In the twentieth century, there have been more dyads of the democratic persuasion and the statistical impact of regime type has registered that development. Even more such dyads, assuming more democratic dyads are in the offing, may ultimately enhance the probability of reduced conflict in the future. We remain pessimistic not so much about the trend line but rather whether the trend line is likely to be linear and, for that matter, steeply pitched. Only time will tell.

Conclusion

One way to look at our findings about the major power subsystem is to view them as a cautionary tale about relying too heavily on a single variable, such as dyadic regime type, in explaining conflict. Dyadic regime type does make a contribution in explaining militarized dispute and war behavior. Yet the effects of regimes on their decision-makers and populations hardly take place in vacuums. Domestic institutions are situated within more or less complex environments and can be expected to interact with, and to be conditioned by, changes in the environment. One of the important features evident of that external environment is the presence or absence of strategic rivalries between states over positions and space. Not much serious conflict takes place in the absence of rivalries. A great deal of conflict takes place in their presence. For militarized dispute behavior in the major power susbystem, at least, we can say that rivalries appear to have been a more significant factor than dyadic regime type, even though that explanatory edge appears to be vanishing. Yet there should be no impulse to supplant one monovariate explanation with another. Our analytical position is better with information on both rivalries and regime types—as opposed to possessing information on only one or the other.

Of course, there is much more to rivalry processes than mere presence and absence.[16] For instance, the presence or absence of rivalry cannot explain why some rivalries escalate to bloodshed and others do not. The absence of strategic rivalries in western Europe after the mid-1950s tells us something about the absence of war in that region. It does not tell us why the United States and the Soviet Union did not go to war over their rivalry that involved, among other things, control of western Europe.[17] Nor does possessing information on rivalries between major powers necessarily tell us anything about how major power rivalries become entangled with minor power rivalries. But simply knowing something about where rivalries exist and where they do not is quite informative about the propensities for conflict and cooperation in general. That this is the case should not be

surprising, for the existence of rivalry provides a rather strong clue about who is most likely to become involved in conflict and who is not. Unfortunately, it has taken us some time to begin to appreciate this fact systematically. It will probably also take more time to fully incorporate a sensitivity to the role of rivalries in our thinking about the other processes of world politics—one of which appears to be the emerging democratic peace that, we think, deserves a broader label than it currently has.

Rivalries, of course, are not the only factors that outweigh the constraints of a democratic political system. In chapter six, we turn to the different but related question of the role of democracy in winning wars. This is a variation on the more fundamental question of how much of an explanatory burden is regime type capable of carrying. It will come as no surprise, now that we are skeptical of the claims put forward about the linkages between democracy and war outcomes. The older, more conventional emphasis on relative capability as the primary clue to who is most likely to win or lose wars seems to have a stronger claim to primary consideration. Nevertheless, the utility of relative capability need not possess an exclusive monopoly on accounting for war outcomes. We need not rule out some role for democracy. The more appropriate question is just how significant regime type is in making sense of who wins and loses interstate warfare.

CHAPTER SIX

Do the Gods of War Really Favor Democracies?

As long as there have been gods and wars, combatants and observers alike have asked themselves whether their god(s) favored their side in an upcoming or ongoing confrontation. Usually the answer was affirmative although sometimes the oracles could be perverse in their verdicts. Of course, once the confrontation was over and one side had triumphed, a post hoc answer could always be inferred: the gods must have favored the winning side.

This ancient question has never gone away. But now we have at least developed new techniques to assess who the gods of war favor and why. Social science theory helps us to determine which factors should play a role in successful outcomes. Empirical data enables us to test how useful the theories really are. Yet while most of us may prefer theory, data, and empirical testing to reading sheep entrails or chicken bones, the more recent approaches are not without their liabilities. One liability that is not really all that novel is that the empirical outcome may be characterized by various types of ambiguity. An interesting case in point is that collectively we have assembled evidence that tells us that:

(1) war initiators are more likely to win than non-initiators,[1]
(2) democracies are more likely to win than nondemocracies,[2] and
(3) stronger states are more likely to win than weaker states.[3]

Each statement taken in isolation is both explicable and predictable, and also reasonably unambiguous. It is when the three statements are considered simultaneously that problems of assessment and interpretation emerge. Do all three contribute equally and significantly to the winning of wars? Should we anticipate that regime type, is more or less important than capability in the winning of wars? Can we assume that initiation, regime type, and capability are independent of one another? The problem is that studies have looked at one or two of these relationships but it is quite rare for anyone to examine all three simultaneously in the specific context of war outcomes. In the very few cases when they have done so, the outcomes

have diverged. One study found that only two of the three were significant (initiation and regime type) while another found that all three were significant. Yet it remains unclear how to rank their relative contribution to the victory equation. Unlike the childrens' game of rock, paper, and scissors, then, we still do not know which of the three variables is the most important contributor to victory. And, given the nearly inevitable disagreements about conceivably critical research design decisions, it remains to be seen, we argue, whether indeed all three of these variables do retain their explanatory importance when examined simultaneously. Our own finding is that they do not. In general, regime type fares least well in our simultaneity tests. If we look only at the three possible predictors of war outcome, and depending on how we measure it, the regime type variable usually fails to attain conventional statistical significance levels. The explanatory value of initiation seems greater than regime type but it tends to vary over time. Of the three, only capability consistently offers explanatory value.

We conclude that capability is more important than initiation to the explanation of war outcomes, and that both capability and initiation are, or have been, more important than regime type. We do not regard these findings as particularly threatening to the expanding literature on the democratic peace—much of which is focused on the relative absence of conflict between democracies. Nor are they necessarily calamitous for efforts to inject more domestic politics variables into the study of interstate processes. However, the findings do serve as a warning that we need to be careful in putting too many theoretical eggs in the regime type basket. That particular "basket" may not be capable of bearing all the explanatory weight that some analysts would like to impose on it.[4]

To arrive at these conclusions, we first review briefly two recently developed theories that not only highlight regime type in the context of capability and war initiation to account for victory in war, but that have also been tested empirically. We then advance a few generic criticisms pertaining to this type of theorizing and several doubts about some of the operational assumptions that are often associated with this type of analysis. New tests are performed on 1816–1988 war outcome data that are intended to correct the problems we raise. Our tests lead to findings that fail to support either theory vis-à-vis their emphases on democratic advantages in winning wars. Nor do our findings buttress the current enthusiasm for regime type interpretations of conflict and cooperation processes.

The Theoretical Questions at Stake

One of the more prominent inter-paradigmatic debates in the recent study of international relations has revolved around whether more or less theoretical emphasis should be given to domestic or external sources of political behavior. The two variables that have been most likely to serve as lightning rods in this debate are relative capability and regime type. Realists

have promoted the former as the key realpolitik consideration. Stalin's World War II question about how many battalions the Vatican could be expected to field serves as the leitmotif for this point of view. In contrast, liberals have argued that much more attention should be bestowed on attributes of domestic political systems, such as the differential foreign policy implications associated with the operations of democratic and autocratic institutions. The basic belief in this instance is the more liberal the world becomes, other things being equal, the less war-prone it should also become.

Within this larger debate much attention has been focused on various dimensions of democratic peace questions. The core questions have been whether democracies are less belligerent in general or, more selectively, only with other democracies. Ancillary questions concerning such topics as alliance behavior and war outcomes have also emerged as analysts have extended the purview of insights garnered from the study of one behavioral dimension to other, possibly related phenomena. This process has also benefited from progress in theory construction that has attempted to synthesize and make more coherent the numerous research findings that have spilled forth. Two provocative examples are Lake's (1992) microeconomic theory of differential rent seeking and Reiter and Stam's (1998a) cost–benefit model of war costs. Both approaches offer explicit theories that emphasize strongly the role of regime type in foreign policy behaviors. By doing so, they deserve attention in their own right but they also serve as worthy representatives of the new and vigorous thrust toward bringing domestic politics factors into the study of interstate relationships.

We propose to make use of them in this chapter as representatives of larger tendencies in the field. Our contention is that the examination of the explanatory value of domestic factors is most welcome but that we need to be cautious in not exaggerating the likely substantive significance of variables such as regime type. Analytical bandwagoning around the domestic politics banner runs the risk of slighting old verities unless we also make some effort to assess the relative explanatory contribution of internal versus external factors. The question should not be simply whether democracies behave differently than nondemocracies. Rather, we need to ask whether regime type information gives us more, less, and/or complementary explanatory leverage vis-à-vis older emphases on relative power and capability. Assessing the relative contributions of initiation, capability, and regime type on war outcomes offers one possible avenue for exploring this comparative explanatory value question.

Two Theoretical Approaches to Democratic War-Making Advantages

Lake's (1992) theory is based on microeconomic assumptions, with the state treated as if it were a firm exchanging services such as protection in return for tax revenues. The state will seek to maximize its rents or profit (basically

revenues minus production costs) and the extent to which rents can be increased will depend on whether citizens can constrain the rent-seeking impulse. In turn, whether there are societal constraints that hinge on the costs of monitoring governmental activities, political participation, and migration. All three, but especially political participation, are less costly in democratic regimes. Therefore, autocratic regimes will have greater abilities to earn rents than will their democratic counterparts.

States with greater rent-seeking ability will be more predisposed toward external expansion than states with lesser rent-seeking ability for three reasons. High rent-seeking units will prefer to eliminate nearby low rent-seeking actors because that will make migration more costly, higher external tension will enable greater revenues to be extorted from the populace, and territorial expansion will increase revenues and the optimal size of the successfully expanding state.

Lake is able to derive a number of interesting hypotheses from this theory but the ones that are of most immediate concern are about war outcomes. One prediction is that political system constraints will restrict the expansion of democratic states to relatively low cost efforts or ones fairly easy to win. A second cluster of predictions suggests that democratic states are likely to have more efficient economies, more wealth and resources for national security, greater societal support, greater extractive capability, and the ability to create overwhelming countercoalitions. In short, they should have a superior capacity, in comparison to nondemocratic states, for generating the resources needed to win wars.

Reiter and Stam's (1998b; see also Stam, 1996) theory is built around the assumptions that decision-makers weigh costs and benefits, that they choose to go to war when the perceived probability of winning is sufficiently high, and that they choose to concede defeat when the inflicted costs exceed the anticipated benefits associated with winning. While the assumptions are different, the implications are similar to those associated with the Lake theory. Democracies are more selective in the wars they choose to initiate because of the greater threat of electoral costs if the war should go badly and because democratic decision-makers have access to better information with which to make their cost–benefit calculations. If we further assume some positive relationship between perceived probabilities of winning and objective probabilities of victory, it follows that initiators, and especially democratic initiators, should be likely to win.

If war outcomes hinge on relative capacities for inflicting and enduring costs and democracies are in a better position to mobilize societal support than nondemocracies due to the legitimizing effects of political participation and representation, democracies should have an edge in waging wars over nondemocratic adversaries. An edge in martial effectiveness, other things being equal, should translate into a greater probability of victory.

We view these theory construction efforts as admirable but not irreproachable. The most important liability might be called the immaculate conception of regime type. By the "immaculate conception" of regime

type, we mean that no attention is paid to the timing of the emergence of differences in regime type. The existence of differentiated democracies and autocracies is taken as an ahistorical given. Why is this problematic? The theories are centered on rent-seeking propensities and cost–benefit calculations that are heavily influenced by regime type. But which came first? If regime type genuinely preceded (or even if it was simultaneous) with the development of rent-seeking propensities and perceived cost calculations, there is no causality problem. However, it is difficult to argue against the historical evidence that variations in rent-seeking and cost–benefit calculations long preceded regime type differentiation.

One implication is that certain conditions, attributes, or processes were necessary to produce regime differentiation. If this is the case, and we choose to ignore them, it is quite possible that the behavior we attribute to regime types will be, at least in part, spurious. Consider for example the Lake deduction that democratic regimes will create fewer governmental distortions in their economies and thereby generate greater economic wealth. What if relatively wealthy and distortion-free economies were a prerequisite to democratization? Instead of democracy producing wealth and war-winning capability, perhaps it is wealth and relatively efficient economies that are likely to generate democracy and war-winning capability.

Another example is the deduction that democratic regimes have greater extractive capability than autocratic regimes because their citizenry are more likely to be supportive of governmental policies. One historical problem is that prior to the more extensive differentiation of regime types, the advantage in mobilizing the financial sinews of war went to states such as the United Provinces of the Netherlands and its successor as system leader, Britain, in their struggles with Spain and France. We may agree that the Dutch and the British states were less rent seeking and less autocratic than the Spanish and French states. But they were other things as well. The Netherlands and Britain were smaller and wealthier states with less interest in local territorial expansion. They paid more attention to international trade and less to agrarian pursuits and could do so because their political economies were more focused on a single center. Different types of political economies do not lead necessarily to different forms of cost–benefit calculation but what is regarded as costly and beneficial may be likely to vary by political economy type.

Moreover, the leading European maritime powers were more capable of mobilizing financial and material resources in part thanks to representative institutions but also because their revenue gathering techniques were more pioneering and because their economies were also more innovative and could be expected to grow. Hence, they were more likely to continue paying interest on loans and thus were regarded as good credit risks. Most importantly, the good credit risks only gradually evolved into possessing increasingly democratic political systems; they did not begin that way.

The more general problem, and one that extends well beyond any specific theory to much of the democratic peace literature, is the widespread

tendency to give a great deal of credit to the regime type variable without considering the history of the evolution of political regimes. One starts with the question of *whether* regime type makes a difference and, almost immediately, the question is transformed into what effect can we attribute to the recently observed variety in regime type. But if the variety in regime type is itself the outcome of complicated processes that are for the most part ignored, it becomes difficult to tell how much regime type really matters, or should be expected to matter. The problem is compounded further if little effort is made to introduce alternative interpretations.

These criticisms, and others that might be raised, do not imply that Lake's or Reiter and Stam's theories are invalid or without value. The criticism merely implies that the derived relationships between democratic war behavior and the likelihood of victory are not as compelling as they might otherwise be. The theories are constructed too ahistorically to tell us whether high/low rent-seeking or cost–benefit calculations preceded regime type differentiation, whether it was the other way around, or whether some mixed causal patterns prevailed.

As a result, it is difficult to distinguish among the consequences, prerequisites, and subsequent behaviors of democratic political systems when so much weight is attributed theoretically to the prevalence of variance in regime types. This problem is not so much a problem of poor theory construction. It is more a matter of constructing theories that explicitly emphasize one of the conceivably weaker sources of influence within the field of possible influences. It is not implausible that regime type makes some difference. What seems less plausible is that regime type should make as much difference as some of our theories seem to imply.

Nonetheless, the Lake and Reiter and Stam analyses are particularly noteworthy not solely for their theoretical contribution. They also proceed to test their theories and, in doing so, find support for their initiation and regime type hypotheses. Initiators, democratic initiators, and democracies in general tend to win the wars in which they participate. They both also introduce capability variables to better assess the strength of the regime type explanation. Lake found that capability made no statistically significant difference; Reiter and Stam found that capability did make a difference. The discrepancy is puzzling but rather than dwell on why different answers to similar questions emerged, our position is that the nature of certain research design choices made in previous studies have left the ultimate relationships among initiation, capability, regime type, and war outcome open to further questioning.

Operational Assumptions

It almost goes without saying that many, if not all, empirical findings are highly sensitive to assumptions made about how best to measure pertinent variables. Since the "best" approach to measurement is rarely obvious, a great deal of room for disagreement emerges. If there are substantial

disagreements about design decisions that may have a critical impact on the analysis, then, it follows that all existing research findings cannot be taken at face value. Findings are invariably linked to, among other things, the perceived attractiveness of specific measurement procedures. While we are not particularly interested in engaging in step-by-step critiques of previous studies on war outcomes, two predominant assumptions related to work on regime type indicators and questions seem highly debatable, if not dubious. Both are found in the Lake and Reiter and Stam studies, as well as in a number of other studies. One assumption pertains to interval versus nominal measurements of regime type; the other assumption is about how we should conceptualize and measure war participation.

A common practice in measuring regime type is to combine information on two, 0–10, institutional scales—one of which focuses on autocratic traits while the other focuses on democratic traits (see, for instance, Jaggers and Gurr, 1995 and Gleditsch and Ward, 1997). If one subtracts the autocratic score from the democratic score, a net democratic score emerges that is usually interpreted in such a fashion that high positive scores indicate the attainment of democratic status. Scores lower than 7, 6, or 5 (the preferred thresholds vary) are assigned to anocratic and autocratic categories. We have no problem with this practice. The problem that we have relates to what one does with the subsequent category assignments. The prevailing practice is to treat the net scores intervally. However, that leads to war coding situations in which both sides may be autocratic with one side less autocratic than the other. If our regime type question is about democracies versus nondemocracies, why would we want less autocratic victories over more autocratic opponents to contribute to the statistical outcome in much the same way that democratic victories over less democratic opponents do? Granted, democratic scores are higher in value than nondemocratic scores by definition and, therefore, are given more weight in the quantitative analysis. But the principal concern remains whether, say, "7's" beat "2's" or "−3's." We are not necessarily equally interested in whether "4's" are likely to defeat "3's" or "−6's," or if we are, it would be preferable to treat these separate questions separately.

This observation raises the related question of scale monotonicity. We are more comfortable in treating high positive scores as capable of capturing some level of democratic attainment than we are in assuming that movement to and from each and every point on the scale demarcates a precise interval of difference. Another way of expressing this point of view is to say that we are also more comfortable in treating political systems that score, say, 6 through 10 as more democratic than systems that score 5 or less and that when it is time to examine the regime type–war outcome relationship, we do not care to assign more weight to "10's" defeating "2's" than we accord to "6's" defeating "2's." The preference betrays some skepticism about the accuracy of the measurement instrument but it also remains a preference. We do not say that it is wrong or right to treat regime type intervally, ordinally, or nominally. Our preference for examining war outcomes is

to treat regime type information as a nominal indicator for the admittedly conservative reasons outlined above.[5]

The second practice that we choose to criticize is less a matter of preference and probably more important from a substantive point of view. The prevailing approach to the appropriate unit of analysis is to focus on dyads. As long as wars are restricted to one state fighting one other state, there is no interpretation problem. When there are more than one states on one or both sides, the dyadic approach creates assessment problems. We may adopt the 1919 Hungarian-Allies War as an illustrative example. Hungary, pursuing territorial expansion ambitions, attacked Czechoslovakia. Romania counterattacked Hungary leading to Hungary's defeat. The dyadic approach yields two dyads, Czechoslovakia versus Hungary and Romania versus Hungary, both of which are coded as Hungary losing. However, the allied war effort was highly asymmetrical. Romania provided most of the allied troops and did most of the fighting. Czechoslovakia's coded win is therefore misleading. The problem is intensified when the allied side, as it does in this case, consists of one democracy and one nondemocracy defeating another nondemocracy. Not only is the recorded Czech win over Hungary an artifact of the dyadic approach, it also generates another case of a democracy defeating a nondemocracy even though it is difficult to attribute any concrete role for the Czech political system per se in bringing about the Hungarian defeat.

The alternative approach to the dyadic unit of analysis is to treat each side in a war as a coalition and the side or coalition then becomes the unit of analysis. For questions concerning war outcomes, as opposed to war onsets, the war coalition unit seems preferable. Otherwise the interpretation problem, and the subsequent statistical noise created by misleading dyadic outcomes is difficult to control. One may counter by saying that some wars have large coalitions of participants whose contributions tend to be grossly unequal. Measurement problems can be created by aggregating attributes across these coalitions. That may be true but, we contend, one runs an even greater risk of mismeasurement if the existence of a coalition is ignored. In the Hungarian-Allies case, it is possible to say that one dyad was more important to the war outcome than the other. In most other $N > 2$ war participant cases, the relative contribution of each war participant is not always so clear. Nor is it always clear that victories by war coalitions are not genuinely shared. For instance, in World War II, the dyadic approach would have us code Britain, the Soviet Union, the United States, New Zealand and Brazil, among others, each defeating Germany (and others) in separate dyadic encounters. While we may have reason to be skeptical of the Brazilian contribution, it is highly debatable whether Britain, the Soviet Union, or the United States could or would have been able to defeat Germany single-handedly. It is far less controversial to stipulate that the three states, operating as a war coalition, did defeat Germany.

We argue that in most cases in the study of war outcomes it is more justified to adopt coalitions rather than dyads as the unit of analysis.

This assumption is especially important if capability is one of the variables of interest. Britain's 1938 capability vis-à-vis Germany is not very illuminating as to why Britain ended on the winning side. The combined capability of Britain, the Soviet Union, and the United States, among others, vis-à-vis the combined capability of Germany, Italy, and Japan, among others is more telling as to why one side won and the other lost.

But coalitions are important to regime type questions as well. If, as in World War II, a mixed coalition of democracies and nondemocracies defeated a coalition composed overwhelmingly of nondemocracies, how are we to link regime type to war outcome? In the dyadic approach, one would have a number of democratic versus nondemocratic and nondemocratic versus nondemocratic lineups. From a coalitional perspective, however, all one can really say is that the mixed coalition defeated a coalition that was composed primarily of nondemocratic political systems. Hence, members of a dyad can be either democratic or nondemocratic but a coalition's regime type can be one of three types: democratic, nondemocratic, or mixed. Since there are a number of prominent mixed war coalitions, the point is not trivial in appropriately linking regime type to war outcome.

Given our theoretical and operational criticisms, we propose to reexamine the relationships among initiation, regime type, capability, and war outcomes. Our principal question is whether relative capabilities in general are more or less important to determining war outcomes than is regime type and the asserted war-making edge of democracies over nondemocracies. We do not view this question as a mutually exclusive one. It is quite conceivable that democracies do possess certain war-making advantages but the advantages associated with possessing a superior capability position are so entrenched in the study of war—violent events in which two sides attempt to determine who is stronger and, as a consequence, whose policy preferences will prevail—that it seems most unlikely that capabilities make no difference to war outcomes. On the contrary, we would expect relative capabilities and perhaps the initiating side to be the two best single predictors of who wins and loses. The stronger side does not always win but superior capability does seem to prevail on the battlefield more often than not. Stalemates due to roughly balanced sides are not unknown but they are not the rule either. In addition, all theoretical arguments, as far as we can tell, assume that initiators believe that they have strong prospects for victory. As long as serious misperceptions about which side is stronger do not cancel the advantages of attacking first in situations often characterized by relative asymmetries in power, we would expect initiators to win more wars than they lose.

Regime type, we think, should at best be the weakest predictor of war outcomes. Whether democracies as a category possess superior war-making capability or a superior ability to endure the costs of war is debatable. Historically, some democracies have generated extraordinary war-making capability while others have surrendered without a fight in the face of overwhelming odds. One could just as well argue that an autocratic ability to

suppress dissent can be useful in making and enduring wars. But even more likely is the probability that any capability advantages based on regime type will depend to a variable extent on access to other types of resources. This possibility is certainly implicit to the Reiter–Stam argument and is actually quite explicit in the Lake argument, if not his findings. If the problem reduces to one of inflicting and enduring costs, all sorts of capabilities can be expected to count. The question is which capabilities count most. But, as a general rule, one cannot rely exclusively on popular enthusiasm (a factor that has hardly been monopolized by democracies) or even better information if the other side possesses overwhelming material superiority and is prepared to use it. Thus, we would expect the impact of regime type, if the theoretical emphases on democratic war-making advantages are correct, to be complementary with, and subordinate to, material capabilities.

H14: *Capability and initiation are more important to war outcomes than is regime type.*

Still, we are not convinced that these relationships are likely to remain fixed over time. Prior to 1945, the annals of warfare were dominated by major powers for whom relative capability was apt to be have predictable implications. After 1945, major powers have been less prominent in making war. Newer states with large but not always effective armies have been quite active, if not always successful. The advantages of capability and incentives to initiate based on confidence of victory may not work as well as it presumably did in the nineteenth century. In addition, nineteenth-century democracies were neither very common nor well established. If democracy implies that decision-makers must be more careful than autocrats in picking their wars because of the domestic political costs of defeat, it is not inconceivable that these effects would be more noticeable in the later part of twentieth century than earlier.[6]

H15: *The relative effects of capability, initiation, and regime type are changing over time, with regime type becoming a stronger influence over war outcomes and capability and initiation becoming weaker influences.*

Testing Considerations

In this chapter we examine wars that were fought and concluded between 1816 and 1988 as defined by the Correlates of War (COW) Project.[7] The opposing war coalitions consist of the states listed as participating on each side. Therefore, coalition membership is not dependent on war contribution or the duration of participation. War outcomes and the identity of the initiator raise highly subjective issues. The COW Project codes winners and losers according to the "consensus among the acknowledged specialists in deciding which side 'won' the war" (Small and Singer, 1982: 182). The few wars that do not have a definitive outcome are excluded from the subsequent analysis.[8] The identity of the war initiator hinges on "whose battalions

made the first attack in strength on their opponent's armies or territories" (Small and Singer, 1982: 194).[9] Given our approach, we have no choice but to assign initiation to the coalition of which the initiator is or becomes a member, even though we realize that coalitions as collectivities do not usually initiate wars.[10]

Regime type is treated nominally as a matter of nondemocracies fighting other nondemocracies or democracies. As is quite common, we utilize the Polity III scales for democratic and autocratic scales, subtract the autocratic scores from the democratic scores, and treat net scores of 6 or greater as indicating relatively democratic states. States with scores of 5 or less are relatively nondemocratic states. However, we also examine the interval regime type scores to see whether they are critical to the outcome. War coalitions that are composed of democracies and nondemocracies are excluded from the subsequent analysis.[11]

There are, to be sure, a variety of points of view about how best to measure relative capabilities. The arguments tend to reduce to diverse preferences for weighting various capabilities differently and problems of data availability. For present purposes, we seek measures of capability that capitalize on multiple dimensions of capacities for war making and that are available for nearly the full run of the nineteenth and twentieth centuries. We can imagine questions that would require us to differentiate among different types of capability—for example, economic versus military or army versus navy—but those are not the questions that are currently at issue. Therefore, we employ the COW Project's six-indicator composite measure (Singer, Bremer, and Stuckey, 1972) and normalize the measurement by first aggregating the total capabilities possessed by all war participants and then calculating the proportional shares controlled by each side.[12] We lose information on the absolute level of capabilities involved in exchange for greater comparability from war to war as to which side has the capability advantage.

This relative capability calculation is made only once in the year immediately prior to the outbreak of war. One might argue that we should examine instead the aggregate of each war year, or perhaps the last year of war, since prewar capabilities are not as important as wartime capabilities and there is apt to be some differences in certain dimensions (such as the military component) in moving from prewar to wartime footings. As far as we know, no one has yet taken a close look at this question and it might well be worth pursuing. For our purposes, though, introducing expanded wartime capabilities runs the risk of contaminating the distinction between relative capability and the capability mobilization advantages associated with democratic regimes. We could not be sure whether to attribute dramatic changes in wartime capability directly to the process of converting potential capability into actual capability, to the intervention of regime type, or some complex mix of both possibilities. To avoid this problem of interpretation as much as possible, we look only at the year immediately preceding the war onset.

There are other factors pertinent to the outcome of war that could be examined, which we choose not to look at at this time. We ignore battle deaths, contiguity, allied capabilities, land versus sea power distinctions, and tactical considerations. We do not claim that these factors are irrelevant but only that they are of lesser importance to the capability–regime type question upon which we are focusing. Thus we freely admit that our approach to examining war outcomes is less than comprehensive. In this case, we are exchanging comprehensiveness for the ability to focus more precisely on a specific theoretical–empirical puzzle.

We introduce controls for the possibility of differences in major versus minor power behavior and for the possibility that the relationships in question have changed over time. The first control is quite conventional practice while the second is not, but should be if we are to assess fully the robustness of our relationships. Our particular question is quite likely to be sensitive to change over time. Before 1945, the world was characterized by a relatively small number of nation-states—a few of which had gradually become more democratic in their domestic politics—and dominated by European major powers with progressively eroding influence over much of the world that they had briefly conquered. After 1945, the European major powers faded from the system's elite as the international capability structure came to be perceived as bipolar and highly bipolarized for a while and focused on the United States and Soviet Union's Cold War rivalry. The number of states in the system expanded dramatically. Some new democracies emerged just as some older states became more democratic.

Within the larger contextual changes, we might also suspect that the influence of our principal variables might vary over time. After 1945, as compared to earlier, there are probably more states with large armies, populations, and economies that are less capable than the usual indicators might suggest. One might add that there are also several small, "over achieving" states that do better than one might expect looking only at conventional capability indicators. If war initiation is predicated to some extent on confidence in winning and capability indicators have become less reliable predictors, we might anticipate that the relationships among capability, initiation, and war outcomes will weaken over time.[13] In contrast, very few states were long-time and well-entrenched democracies during the nineteenth century. If it takes time for democratization to evolve, it may also take time for its impact on other behaviors to evolve. Thus, if democratic institutions do have some effect either on constraining decision-makers from starting wars that they are likely to lose or in enhancing war-making resources once one is underway, we might anticipate that these effects should be more discernible in more recent times than earlier.

We address the selectivity question by coding separately whether different regime types (democracies and nondemocracies) initiate wars against opponents with more or less capability than the initiator and win. Democratic selectivity should be demonstrated by a finding that democracies are more likely to initiate victorious wars against weaker opponents

than are nondemocracies. If we find that there is no difference according to the regime type of initiators or that nondemocracies are more likely to attack and defeat weaker opponents, the evidence will suggest that democratic decision-makers are no more selective than anyone else in choosing ostensibly easy-to-win wars.

In sum, then, we expect to find that the "harder" variables—capability and initiation—will prove to be more important to war outcomes than the "softer" regime type variable. But, we can also envision these relationships changing over time with the former pair growing somewhat weaker and the latter possibly growing stronger as we approach the end of the twentieth century. We remain agnostic about the selectivity argument. It seems quite conceivable to us that decision-makers of any regime stripe would prefer to attack weaker targets or not to attack at all. But this question remains one worth checking empirically.

Analysis

Table 6.1 reports the statistical outcomes for five models. The first three examine the singular relationship between each of the three main variables and war outcomes. As one might expect, all three are statistically significant and characterized by the appropriate signs. Taken in isolation from one another, states or coalitions with superior capabilities, states or coalitions that initiate, and/or states or coalitions with democratic political systems are likely to win the wars in which they participate. The fourth model combines all three variables.[14] Superior capability and initiation retain their statistical significance while the democratic advantage does not. Model 5 reexamines model 4 without the regime type variable and demonstrates that virtually nothing is lost by eliminating the regime type variable from the equation. Presumably, then, we have a quick answer to our first question. Not only are capabilities and initiation clearly more important than

Table 6.1 War outcomes and capability, initiation, and regime type

Models	*1*	*2*	*3*	*4*	*5*
Variables					
Constant	−1.34**	−0.69**	−0.26	−1.70**	−1.61**
	(0.394)	(0.267)	(0.200)	(0.438)	(0.427)
Capability	2.55**			2.09**	2.15**
	(0.689)			(0.730)	(0.721)
Initiation		1.24**		0.90**	0.94**
		(0.378)		(0.404)	(0.400)
Regime type			1.02*	0.78[a]	
			(0.500)	(0.540)	
Log likelihood	−77.52	−79.53	−82.93	−73.67	−74.75

$N = 123$.

[a] denotes statistical significance at the 0.10 level or better (one-tailed test).

* denotes statistical significance at the 0.10 level or better (two-tailed test).

** denotes statistical significance at the 0.05 level or better (two-tailed test).

regime type in accounting for war outcomes, regime type appears to be unimportant.

Table 6.2 examines the first four models in table 6.1 before and after 1945. The end of World War II is arbitrary but it is adopted because there are few democracies at risk in the nineteenth century (and even fewer involved in interstate warfare). The end of World War II is not only approximately half way through the twentieth century, it also serves as a watershed in the subsequent emergence of a large number of new states and new war participants. The findings pertaining to models 1, 2, and 4 of table 6.1 are replicated before World War II with capability and initiation significant. Regime type is insignificant when examined alone or in conjunction with the other variables. However, after World War II, none of the four models generate coefficients that are significant at the 0.05 level. Capability is significant at the 0.10 level in models 1 and 4 and regime type also approximates this threshold in model 3.

Table 6.2 provides rather striking support for the contention that these relationships with war outcomes are not particularly stable over time. After 1945, only capability retains a level of statistical significance in the multivariate

Table 6.2 Explaining war outcomes before and after 1945

Model	*1*	*2*	*3*	*4*
Variables				
1816–1945				
($N = 90$)				
Constant	−1.65**	−1.04**	−0.22	−2.39**
	(0.507)	(0.336)	(0.224)	(0.621)
Capability	3.03**			2.74**
	(0.893)			(0.982)
Initiation		1.80**		1.63**
		(0.466)		(0.494)
Regime type			0.92	0.61
			(0.741)	(0.834)
Log likelihood	−55.54	−53.92	−61.37	−48.12
1946–90				
($N = 33$)				
Constant	−0.80	0.12	−0.40	−0.93
	(0.636)	(0.486)	(0.456)	(0.702)
Capability	1.78*aa			2.34*aa
	(1.09)			(1.40)
Initiation		−0.12		−1.18
		(0.697)		(0.948)
Regime type			1.22aa	1.12a
			(0.755)	(0.818)
Log likelihood	−21.44	−22.84	−21.48	−19.76

* denotes statistical significance at the 0.10 level or better (two-tailed test).
** denotes statistical significance at the 0.05 level or better (two-tailed test).
a denotes statistical significance at the 0.10 level or better (one-tailed test).
aa denotes statistical significance at the 0.05 level or better (one-tailed test).

examination in model 4. We presume that this instability is due primarily to the earlier noted heterogeneity in who fights wars before and after 1945. But it is at least clear that war outcomes have become less predictable over time in the sense that capability and especially initiation have become less useful in forecasting who wins and who loses. Regime type can be said to have improved its explanatory contribution over time but not sufficiently to make much meaningful difference.

If, instead of controlling for the possibility of behavioral changes over time, we introduce a control for major versus minor power behavior, a much different 1816–1988 outcome emerges. In table 6.3's model 1, capabilities, regime type, and the major power control are all significant while initiation is not. Comparisons of models 1, 2, 3, and 4 that vary as to which variable is excluded from the analysis, show that the introduction of a control for rank strongly influences which variables are significant. Without the control for major/minor powers, regime type is insignificant (model 3); with it, initiation is insignificant (models 1 and 2). By examining the log–likelihood test comparing model 1 with model 2, we can also say that when we eliminate the regime type variable from model 2, there is a significant loss of predictive power. When we eliminate the major power control in model 3, we also lose a significant amount of predictive power. But, when we eliminate initiation from model 4, the log–likelihood ratio test yields a statistically insignificant chi-square statistic indicating that we do not lose any predictive power in model 4 (compared to model 1). Therefore, the outcome summarized in table 6.3 suggests that it is initiation that is the least important of the three main variables.

If our outcomes depend on which controls are applied, the obvious solution is to combine them and examine the relationships controlling for rank and

Table 6.3 War outcomes controlling for major powers

Model	*1*	*2*	*3*	*4*
Variables				
Constant	−1.74**	−1.63**	−1.70**	−1.59**
	(0.448)	(0.433)	(0.438)	(0.427)
Capability	1.63**	1.84**	2.09**	1.79**
	(0.773)	(0.748)	(0.730)	(0.755)
Initiator	0.65[a]	0.77[aa]	0.90**	
	(0.425)	(0.414)	(0.462)	
Regime type	1.12**		0.78[a]	1.23**
	(0.560)		(0.540)	(0.549)
Major power	1.11**	0.83[aa]		1.30**
	(0.488)	(0.462)		(0.471)
Log likelihood	−71.02	−73.09	−73.67	−72.19

$N = 123$.

[a] denotes statistical significance at the 0.10 level or better (one-tailed test).

[aa] denotes statistical significance at the 0.05 level or better (one-tailed test).

** denotes statistical significance at the 0.05 level or better (two-tailed test).

temporal changes. But we can do this only partially because major powers fought only four times after 1945 and always won, at least according to the COW coding rules.[15] Lacking suitable variance after 1945, we are restricted to the pre-1946 evidence summarized in table 6.4. Nevertheless, the outcome is quite informative. With or without the inclusion of the rank and regime type variables, capabilities and initiation are statistically significant predictors of war victory. The regime type variable and the control for major powers are either insignificant or, when examined more closely utilizing log-likelihood tests, fail to add significantly to the predictive power of model 1. This outcome tells us that the strength of the rank or status control in table 6.3 is deceptive. It is almost entirely dependent on the four post-1945 cases, interestingly, three of which involve former patrons punishing or attempting to punish one-time clients that were no longer adhering to the preferred line (e.g., the Soviet Union and Hungary, Britain and Egypt, China and Vietnam). The common denominator suggests that the outcome may hinge less on the involvement of major powers per se and more on the motivations for major power involvement. At the same time, the coding of these cases is not totally beyond dispute. For instance, did the Soviet intervention in 1956 differ greatly from Soviet interventions in East Germany and Czechoslovakia that killed fewer people and were not coded as wars? Did the Israelis, French, and British really win the Sinai war when the United States forced their retreat before removing Nasser? Ultimately, the point is that the outcome for the major power control is a thin reed on which to make generalizations.

The insignificance of regime type with a major power control before 1945 is also revealing. In contrast to the outcome reported in table 6.3, it does not appear to be a matter of appropriate controls yielding the anticipated

Table 6.4 War outcomes controlling for major powers and regime type prior to 1945

Models	*1*	*2*	*3*	*4*
Variables				
Constant	−2.43**	−2.38**	−2.39**	−1.89**
	(0.629)	(0.622)	(0.621)	(0.546)
Capability	2.35**	2.47**	2.74**	2.35**
	(1.028)	(1.013)	(0.982)	(0.959)
Initiator	1.43**	1.51**	1.63**	
	(0.512)	(0.505)	(0.494)	
Regime type	0.89		0.61	1.20[a]
	(0.853)		(0.834)	(0.808)
Major power	0.80[a]	0.66		1.19**
	(0.552)	(0.531)		(0.513)
Log likelihood	−48.12	−48.68	−49.18	−52.18

$N = 90$.

[a] denotes statistical significance at the 0.10 level or better (one-tailed test).

** denotes statistical significance at the 0.05 level or better (two-tailed test).

outcome that had not emerged in its absence (tables 6.1 and 6.2). Regime type is insignificant with (model 1) or without (model 3) the status control before 1945. We know from table 6.2 that regime type is only very marginally significant in the post-1945 period when no major power control is applied. Therefore, the significant outcome for regime type in table 6.3 is highly dependent on those same four cases of major power warfare after 1945. We conclude, therefore, that tables 6.1 and 6.2 provide more reliable information than does table 6.3. Of the three main variables, superior capabilities is the most useful in attaining victory in war. Initiating warfare appears to be of declining utility. Possessing a democratic political system is the least helpful attribute.

Nevertheless, we have not yet dealt fully with the relative lack of predictive power of all three variables after 1945 that was revealed (in table 6.2) prior to the introduction of the major power control. There is also another possible threat to the validity of our results that remains unexamined—namely, are our regime type results dependent on how we have measured the difference between democracies and nondemocracies? We examine both issues in table 6.5. Models 1 through 3 in table 6.5 reexamine some of the analysis reported in table 6.2. The difference is that we have substituted an interval measure of democratic political systems (based on the net score obtained by subtracting the Polity III score for autocratic elements from the score for democratic elements) in exchange for the nominal regime type indicator. When we look at the problem this way, the 1816–1988 and 1816–1945 results again suggest the existence of significant relationships between both capability and initiation, and war outcome. The interval measure for regime type, referred to here as "democracy," is significant in the full time period but not in the pre-1946 interval. After 1945, democracy is significant while initiation is insignificant.

Table 6.5 War outcomes and more examination of the democratic effect

Models	*1*	*2*	*3*	*4*
	1816–1990 ($N = 123$)	1816–1945 ($N = 90$)	1946–90 ($N = 33$)	1946–90 ($N = 23$)
Variables				
Constant	−1.41**	−2.13**	−0.58	−2.72
	(0.447)	(0.639)	(0.724)	(1.452)
Capability	2.48**	3.08**	2.86	16.61
	(0.776)	(1.038)	(1.504)	(9.236)
Initiation	0.84**	1.56**	−1.29	−10.37
	(0.412)	(0.499)	(0.991)	(6.305)
Democracy	0.09**	0.08[a]	0.12**	−0.14
	(0.038)	(0.056)	(0.058)	(0.112)
Log likelihood	−71.33	−48.39	−18.38	−8.03

Note: In model 4, Israeli wars have been removed from the sample.

[a] denotes statistical significance at the 0.10 level or better (one-tailed test).

** denotes statistical significance at the 0.05 level or better (two-tailed test).

If we were to stop the analysis at this point and renounce our earlier preference for a nominal regime type indicator, some of the puzzling earlier results might seem clarified. For the most part, capability is consistently significant while democracy becomes significant only after World War II. Initiation not only loses its initial significance, the sign of its coefficient switches from positive to negative in the most recent time period. None of these findings would be hard to explain. Yet we still have some nagging doubts about the reliability of our capability indicators over time to inform us about which side had more resources prior to the war onset. There is also the extreme heterogeneity in who fights wars before and after 1945. Model 4 in table 6.5 represents one last test of war victories and losses that attempts to address these issues.

Model 4's alteration of the test associated with model 3's outcome is deceptively simple. We eliminate the wars of one state, Israel, from the analysis. We do this for several reasons. One is that, it has been shown before that Israeli conflict can have a disproportionate impact on statistical analyses of conflict and cooperation patterns (Duval and Thompson, 1980).[16] The Arab–Israeli wars usually are characterized by various Arab capability advantages that have turned out to be markedly illusory on the battlefield. Israel's political system, is clearly more democratic than any of its Arab opponents, but since the wars tend to be of extremely short duration, it is not clear which of the attributes asserted to flow from democratic political system, pertain in these cases. In fact, from the Israeli perspective, the wars must be completed quickly in order to avoid the prospects of economic exhaustion. For the same reason, Israeli military capabilities tend to be undercounted in quantitative examinations by focusing exclusively on peacetime indicators. Israeli military strength depends on rapid mobilization much more so than does the strength of its Arab neighbors.

The Arab–Israeli wars are good examples of our earlier observation about pre- and post-1945 war behavior. Israel and some of the Arab war participants did not exist as independent states prior to the late 1940s. Nor is counting relative capabilities in the Middle East a straightforward proposition. Large populations and armies do not always mean what they might signify elsewhere. Our question, then, is how robust are our findings when we delete the wars of one seemingly, highly atypical state operating in a regional subsystem in which capability standings can be tricky to delineate accurately? Model 4 in table 6.5 generates only one statistically significant relationship (at the 0.07 level) and that one is for the capability–war victory relationship. Initiation and democracy are insignificant and incorrectly signed.

We are forced to conclude that while it clearly does make some difference how one measures regime type, the more critical factor is the shift in warfare away from the traditional Eurocentric core to other, often more newly independent states with capabilities (and political systems?) that are in general comparatively more difficult to assess. If we remove Israel from the sample, the performance of the interval regime type indicator is more

Table 6.6 War outcomes and democratic initiators

Models	*1*	*2*	*3*
	1816–1990 ($N = 41$)	1816–1945 ($N = 29$)	1946–90 ($N = 8$)
Variables			
Constant	−0.33	−0.024	−0.69
	(0.364)	(0.403)	(1.225)
Democratic selectivity	−0.08	−0.86	1.10
	(0.741)	(1.223)	(1.528)
Log likelihood	−27.81	−19.39	−5.27

similar (i.e., insignificant) to the nominal indicator employed in tables 6.1 and 6.2. Regardless of measurement preferences, then, the findings persist in giving more explanatory weight to capability than to regime type, with both capability and initiation more or less declining in explanatory value as we approach the most contemporary period.

That leaves only our secondary question on the selectivity of democratic war initiators unaddressed. However, if democracies do not possess a clear advantage in fighting wars, the selectivity question loses much of its inherent appeal in the sense that there does not appear to be anything left to explain. Still, the empirical question can be addressed quickly. Table 6.6 reports the outcome when the question is whether democratic initiators that attack weaker targets are any more or less likely to win than nondemocracies that also attack weaker targets. The answer is no, democratic initiators are no more likely to win than are nondemocratic war initiators. That does not completely rule out the possibility that democratic decision-makers are more selective than their nondemocratic counterparts, but it does suggest that if selectivity is operative, it is not working the way the decision-makers thought it would.[17]

Conclusion

There are two ways to interpret our findings. The most direct way is that our expectations were supported in large part, but not perfectly. In contrast to the current analytical gusto for the explanatory significance of regime type, we find little in the way of support for the notion that democracies have advantages over autocracies in winning wars. Capability has counted most but either that relationship is changing or our ability to capture it with conventional indicators of military, economic, and demographic strength has eroded. Initiation has also waned in explanatory power. Before 1945, it was more important than regime type but not afterwards.

A second way to look at the data outcomes is to stress the evident lack of robustness of the findings. The analysis of war outcomes is highly sensitive to variations in nominal versus interval indexing, temporal heterogeneity,

and the introduction of standard controls such as the one for rank. The lack of robustness suggests that we need to be cautious in interpreting empirical findings as supportive of theories highlighting the role of variables such as regime type or, for that matter, capability, to the relative exclusion of other factors.

More generally, the findings suggest that we should not look for easy tests of competing paradigms. Paradigms cannot be reduced to single variables. The explanatory powers of capability or regime type in predicting war outcomes certainly cannot tell us in any definitive way whether realists or idealists are "right" or "wrong." Nor can they tell us that rent-seeking theories of the state or cost–benefit theories of war decisions are valid or invalid. However, the outcomes associated with the correlates of victory that we examine suggest that we should not insist too strongly on the importance of any single variable. Theorists constructing arguments designed to highlight the role of specific variables should anticipate problems in obtaining unequivocal support for their arguments. Warfare patterns apparently are too complex for regime type, or any other monovariate explanation, to take us as far as we wish to go in predicting who is likely to defeat whom. Even so, it is not at all clear from the present findings that regime type is likely to play much, if any, prominent role in the multivariate analysis of war outcomes.

Finally, it should also be apparent that we need to develop a stronger appreciation for historical change and evolution in warfare patterns, the development of regime types, and the meaning of different types of capability across time. Things change. We need to develop theories that are sensitive to the important changes in international relations behavior and context. One way to start is to relax the search for laws relating the unvarying relationship of variable *X* to variable *Y*. It is not clear that we are likely to find many such unvarying relationships. To search for them may lead us farther astray than will the counter assumption that relationships in international politics are likely to covary with "time" and other considerations. At the very least, we need to develop a greater appreciation for the probability of predictable fluctuations in the strength of relationships between and among those variables that we find most interesting in the study of international relations.

Still, it seems fairly clear that the predictive powers of the presence and absence of democratic political systems is somewhat less than is frequently claimed. Indeed, the strong claims put forward by analysts about the explanatory superiority of domestic processes (such as democratization) have encouraged the neglect of systemic considerations. That is most unfortunate because even if one insists on the significance of democratization as a selective pacifier of international relations, it is most difficult to focus solely on internal considerations. Democratization is as much a systemic process as it is a domestic one. Some might claim, moreover, that it is difficult to comprehend how democratization matters without viewing it from a systemic perspective. Chapters seven, eight, and nine explore this claim

empirically. Chapter seven focuses on what is called the monadic puzzle of the democratic peace. Namely, if democratic dyads are peaceful, why aren't democracies more peaceful than autocracies? One answer could be that the survival of democracy as an organizing principle for political systems has had to beat alternative approaches such as aristocracy, fascism, and communism. The successive contests between and among these abstract principles could not help but be reflected in the pattern of interstate conflict. Is it possible to harness this contest among political ordering schemes to help account for the monadic puzzle? We think the answer lies in the affirmative.

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PART FOUR

The Systemic Context in Which Democracies Compete

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CHAPTER SEVEN

Do Democratic–Autocratic Rivalries Muddy the Monadic Waters?

There are a number of puzzles associated with the democratic peace phenomenon. The most central one, the dyadic puzzle, raises the question of why is there such a phenomenon in the first place? Why are two democratic states less likely to engage in, or escalate, conflict with one another in contrast to how the same two states might deal with non-democracies? There are, of course, a number of possible answers but since we are not sure, the explanation for the dyadic behavior remains a puzzle. Yet this same puzzle also implies a second one, the monadic puzzle. If two democratic states are less likely to be conflictual with one another, why is it that this pacific trait does not extend to all foreign policy behavior in which democratic states engage?

We make no claim in this analysis of being able to answer the dyadic puzzle. However, we do advance one partial solution to the monadic puzzle. Whatever the precise roots of the democratic peace phenomenon, democratic political systems are of fairly recent origin. As novel strategies for organizing domestic political systems, they encountered resistance from both earlier formulas and others that were also relatively novel. As a consequence, the late nineteenth and almost the entire twentieth century has been characterized by a struggle between an emerging democratic community and its various ideological opponents. Two world wars, a cold war, and large number of crises and disputes have taken place not so much between democracies and autocracies per se. Rather, the fight has been mainly between the leaders of the emerging democratic community and leaders of the various rival strategies (aristocracy, fascism, and communism). We test this argument primarily by examining the differential tendencies for democratic major powers to conflict with autocratic rivals more so than autocratic major powers conflict with democratic major powers rivals. We view these findings as support for the idea that the greater-than-anticipated (by some) monadic belligerence of democratic states, and especially major powers, is due to the nature of democratization as an emergent property in world politics. As such, the monadic belligerence of democratic states should be a temporary phenomena.

The Monadic Puzzle and an Evolutionary Solution

If we knew the answer to the dyadic puzzle, the monadic question might be less interesting. For instance, if the primary explanation for the democratic peace is that it is strictly a dyadic phenomenon and requires the reciprocal interaction of two of democracy's political cultures, both of whose elites are operating on the assumption that the other side will follow the types of norms exercised in their own domestic politics, then we would have less reason to expect the same influence to work at the monadic level. If, on the other hand, the primary explanation involved institutional restraints on democratic foreign policy or the greater transparency of democratic foreign policy, we should be able to observe some constraints operating on the behavior of individual democratic states.

We might call this the theoretical dimension of the monadic puzzle. How does one account for the foreign policy behavior of democratic states in general if we believe they operate distinctively in pairs? But there is also an empirical dimension to this puzzle. While most of the analysts who have examined democratic peace related questions seem to operate on the premise that democratic states, in general, are no more or no less pacific than nondemocratic states, the relevant empirical literature actually remains divided on this question. Without stretching the point too much, the question has been debated for over half a century without attaining any real resolution. As noted in table 7.1, a number of analysts argue for, and find, that democracies are not more pacific or less conflict-prone than nondemocracies. A smaller number argue for, and find, that democracies, in various ways, are more pacific, or, at least, less conflict-prone than nondemocracies. In some cases, the same authors find support for both positions, just as they sometimes switch their positions over time.

In the early 1990s, the literature consensus sided with the no difference position. Less than a decade later, Russett and Starr (2000: 97) argue that the majority view has swung to the opposite position—that there is some discernible difference between democracies and autocracies in both monadic and dyadic conflict behavior. Whether the earlier consensus or current "majority view" possess sufficient justification for their claim to a representative stance remains unclear. Presumably, the division in opinion revolves around distinctions between overall frequencies of conflict that most analysts find to be no different and particular forms of behavior in which some less conflictual trait, such as the escalation of conflict once initiated, is manifested. Still, the issue would appear to remain a matter of some contention.

There are a number of conceivable reasons why there is disagreement on this empirical question. Analysts have posed different questions. For instance, one analyst may ask whether democracies are more or less war-prone while another asks whether they are simply more or less conflict-prone than nondemocracies? These are two different questions that may yield different answers. Then there are a forbidding host of methodological

Table 7.1 Disagreement in the literature on the monadic relationship between regime type and general foreign policy belligerence

No difference between democratic and autocratic belligerence	*Some difference between democratic and autocratic belligerence*
Wright (1942/65)	Haas (1965)
Richardson (1960)	East and Gregg (1967)
Rummel (1968)	Salmore and Hermann (1969)
Salmore and Hermann (1969)	Zinnes and Wilkenfeld (1971)
Weede (1970, 1984, 1992)	East and Hermann (1974)
McGowan and Shapiro (1974)	Sullivan (1976)
Russett and Monsen (1975)	Rummel (1979, 1983, 1985, 1995, 1997)
Small and Singer (1976)	Geller (1985)
Doyle (1983a, 1983b, 1986)	Domke (1988)
Chan (1984, 1993)	Bremer (1992, 1993)
Garnham (1986)	Bueno de Mesquita and Lalman (1992)
Domke (1988)	Schweller (1992)
Levy (1988, 1989)	Carment and James (1995)
Dixon (1993, 1994)	Ray (1995)
Maoz and Abdolai (1989)	Benoit (1996)
Merritt and Zinnes (1991)	Hewitt and Wilkenfeld (1996)
Morgan (1993)	Rousseau, Gelpi, Reiter, and Huth (1996)
Gleditsch (1992)	Brecher and Wilkenfeld (1997)
Morgan and Schwebach (1992)	Chan (1997)
Starr (1992a, 1992b)	Rioux (1998)
Bueno de Mesquita and Lalman (1992)	Russett and Starr (2000)
Maoz and Russett (1993)	
Cashman (1993, 1999)	
Russett (1993)	
Hagan (1994)	
Kegley and Hermann (1995)	
Gates, Knutsen, and Moses (1996)	
Gaubatz (1996)	
Huntley (1996)	
Maoz (1996)	
Geller and Singer (1998)	

differences in how analysts have framed their questions. It can make a difference if one looks at one or a few unrepresentative years, as opposed to a respectably lengthy series. It can make a difference if one looks at the entire world or just one region. In addition to temporal and spatial considerations, different indicators (and levels) of conflict (wars, crises, militarized disputes, foreign policy event interactions, interventions, agreements, and so forth) may well lead to different conclusions about the relative peacefulness of democratic states. How one decides which states are democratic can also make some difference to the analytical outcome.

We cannot dismiss any of these possibilities as traceable sources of disagreement.[1] One way to resolve the puzzle might be to try unraveling the labyrinth of alternative research design decisions. However, the large number of studies and the wide range of permutations in assumptions does not

encourage one to contemplate what would be a truly herculean task. On the other hand, there is yet one possible, and fairly simple, answer to the monadic puzzle that seems to have been overlooked.[2] Rather than focus exclusively on the putative behavioral constraints associated with democracy, another possibility is that democracies, because they are democracies, are more likely to become involved in conflict with some types of nondemocracies. In this sense, democracy encourages (or has encouraged) some types of dyadic conflict while it may restrain (or has restrained) other types of dyadic conflict. Adopting this point of view assumes that democratic attributes cannot be viewed in isolation from the environment in which democracies are located. If the whole world is democratic, that is one type of environment. If the world is one in which democracies are a decided minority, that is a markedly different environment. Still another type of environment is one in which democracies begin as a small minority and move toward a majority position. The difference between the exclusively democratic world and the democratic minority world is that in the latter democracies can anticipate repeated challenges over their very existence and survival. In the world evolving from a democratic minority to a democratic majority, one can also assume that not only are democracies beating off their challengers but that they are defeating them as well.

It is precisely this type of evolutionary environment that has characterized the twentieth century. The nineteenth century was overwhelmingly autocratic in terms of the most prevalent modes of domestic political order. Fissures in this uniformity, it is true, had begun to appear in the late eighteenth century. The United States emerged relatively democratic by the standards of the time while the French Revolutionary experimentation led ultimately to Napoleon. Slowly and gradually throughout the nineteenth century democratic reforms in a few states expanded the number of democracies in the world. Yet the overall number remained small. World Wars I and II proved to be critical trials by fire for democracy. In World War I, one side had democratic leadership while the other side was almost exclusively autocratic. If the democratic side had lost in 1918, there would have been fewer democracies in existence after 1920 than before. Without speculating about the counterfactual fate of the states that could be considered democratic in 1913, the defeat of the Central Powers led to the dissolution of the Austro-Hungarian Empire and the emergence of a number of new states, many of which at least began with democratic institutions. Yet the interwar period was not favorable for the spread of democracy. A number of the new European states reverted to various forms of authoritarianism. World War II, even more so than World War I, took on the appearance of an ideological showdown between democracy and fascism, the leading authoritarian variant of the 1919–39 era. The victories of the Axis powers during World War II demonstrated quite vividly that democratic institutions would not survive in countries occupied by fascist conquerors.

Fascism, ultimately, was of course defeated in World War II. Prewar democracies that had been occupied were able to resurrect their former

institutions. A number of the defeated authoritarian states were now occupied by forces from the victorious democratic coalition. As a consequence, their political constitutions were rewritten in a more democratic vein. Still, the winning coalition in World War II had not been exclusively composed of democratic states any more than the winning coalition of World War I had been. The post–World War II Cold War represented a split in the winning coalition and another form of intensive ideological struggle—this time between democracy and communism. Both sides viewed the struggle as zero-sum in nature. The victory of one side over the other would mean the ideals and institutions of the losing side would disappear. Certainly, there were other issues at stake as well. The Cold War was hardly entirely ideological in nature. But the democratic versus autocratic dimension was hard to miss. By 1989, the democratic coalition had prevailed once again leading, among other outcomes, to another imperial disintegration, an expansion in the number of newly independent states, and another increase in the number of democracies in the system.

Thus, between 1914 and 1989, democratic states survived three bouts of intensive competition between coalitions espousing markedly different solutions to organizing political and economic order. A coalition of democratic and autocratic states first defeated a coalition led by nineteenth-century-style autocrats in 1918. In 1945, a coalition of democratic and autocratic states next defeated a coalition of states led by a form of authoritarianism that emerged in the aftermath of World War I. By 1989, another coalition of democratic and autocratic states (counting Cold War allies often in the Third World) had defeated still another form of authoritarianism with strong World War I roots. These repeated triumphs of liberal democracy over its principal competitors, fascism and communism, has led one commentator to describe the end of the Cold War as the "end of history." Fukuyama (1992), of course, did not mean literally that history had ceased to exist but that one ideology had finally triumphed over its competitors and that henceforth history, as a manifestation of a struggle over the relative virtues of political–economic ideas, would no longer be the same.[3]

Whether we have ceased to fight about political–economic ordering ideas remains to be seen. Less ambiguous, though, is the history of the twentieth century as an ideological crucible in which three variants of approaches to political organization competed quite intensively. Whatever the powers of foreign policy restraint that are associated with democratic institutions and belief systems, the environment in which democracies have emerged, survived, and finally triumphed as the premiere form of political ideals, has been anything but friendly. Democracies were seen as subversive and threatening by nineteenth-century autocrats and as serious obstacles to survival and expansion by twentieth-century fascists and communists. The emergence of democratic states as a minority in a hostile world therefore suggests that at least until democratic states formed a majority of states in the system, one should expect an increasing probability of conflict between democracies and some types of autocracies.[4]

Why qualify this prediction by the phrase "some types of autocracies"? The problem is that the universe of autocracies is much more heterogeneous than is the universe of democracies. The autocratic pool includes old-fashioned monarchies, fascist and communist single party dominant regimes, military regimes, theological dictatorships, and states controlled by small numbers of people and families that may or may not overlap with the other forms of autocracy and/or sometimes superficial forms of democracy.[5] Not all of these autocratic variants have been perceived as equally threatening by democratic decision-makers. In the early nineteenth century, a United States could make temporary accommodations with a Britain or a Russia as long as their mutual interests dovetailed. In the twentieth century, Czarist Russia and the Stalinist Soviet Union were critical members of the democratic-led coalitions of World Wars I and II. During the Cold War, a number of military regimes and countries controlled virtually by a small number of families felt more threatened by Marxists than by democrats and were welcomed to the anticommunist coalition. The historical record suggests that democracies per se are not destined to struggle with autocracies per se. Yet some autocracies are viewed as more malign than others and the same can be said about autocratic views about democracies. Therefore, it would be an error to posit a dyadic war between democracies and autocracies that is the counterpart of the dyadic peace between democracies. But there is a decided likelihood that autocracies that are seen as expansionist in nature will be viewed as mortal enemies by democracies, and autocracies that possess expansionist ideologies and foreign policy ambitions will view the more powerful democracies as thwarting their foreign policy goals and thus mortal enemies as well. These observations are contingent on an environment with substantial variety in domestic political forms. Once that variety ceases to exist, this particular form of ideological animosity should also cease to exist, a la Fukuyama's "end of history" notion.

This interpretation overlaps and unifies several other arguments that have already appeared. One is that status quo–oriented actors are less likely to fight other status quo–oriented actors. They have much less to fight about than they do with status quo–challengers.[6] The classical examples of status quo–challengers in contemporary IR history were first the Central Powers in World War I, the Axis Powers in World War II, and the Soviet Union in the Cold War. All autocracies have certainly not opposed the international system's status quo but fights over preserving or overturning the status quo have tended to pit democracies versus autocracies.

A second thread is the idea that neighborhood context matters. Democracies in "nice" neighborhoods where there is little opposition should fare better than democracies in "nasty" neighborhoods where they must fight for their very survival.[7] Conceivably, monadic propensities toward conflict will vary by neighborhood. The major power subsystem would have to be considered a nasty or tough neighborhood for democracies throughout most of the last 200 years in which they have been a wealthy minority prior to 1945.

Finally, there is also the political distance argument. Russett and Starr (2000: 96) contend that when one state is democratic, the farther apart the two states are in political distance, greater will be the probability of violence. Others have also emphasized the antipathies that separate democracies and autocracies.[8] But Cold War practice also suggests that some democracies have a high tolerance for autocracies that were on their side, and vice versa. The question remains whether democracies and autocracies fight because of domestic institutional distance or for other reasons that encourage *some* autocracies to clash with *some* democracies.

The more basic point is that it is conceivable that democratic institutions, transparency, status quo–orientations, or norms constrain foreign policy behavior both toward other democracies in particular and toward other states in general. But these constraints do not operate in a vacuum. The international environment in which democracies have emerged has until quite recently been largely hostile, albeit subject to different waves of reasons for the animosity (e.g., aristocratic, fascist, communist). The very existence of democracies has attracted attacks from other countries with antagonistic types of political belief systems, just as democracies have attacked other countries with antagonistic types of political belief systems. This process reflects what has been an ongoing experimentation with different forms of political order in which the main variants have had to resist the coercive claims to superiority of their competitors. In this respect, democracies have been no different than other political creeds. They have fought to sustain the "democratic way" and also to expand its domain. If they had not, we probably would not be attempting now to decipher what the democratic peace is about. Other variants would have been triumphant instead.

Does this mean that democracies are just like all other types of political systems when it comes to dealing with nondemocracies? That is one possible conclusion. In this case, the "end of history" story would offer one explanation for why democracies are pacific toward other democracies but are no less belligerent than nondemocracies when dealing with nondemocracies. But we should be able to take this argument one step further and, in the process, be more specific about differences between democratic and autocratic foreign policy conflict behavior. Consider an external environment in which democracies are confronted by three types of actors. There are other democracies that we assume are categorized as friends, if for no other reason, that they share enemies antagonistic toward democracies. There are autocracies that are regarded as benign because they appear to be nonthreatening. Then there are the malign autocracies that either do pose serious threats or that are perceived as if they did. With which of the three groups are democracies most likely to develop categorically conflictual relationships? The answer is malign autocracies that are perceived as expansionist threats.[9] With which of the three groups are democracies most likely to develop categorically nonconflictual relationships? Democratic peace advocates would suggest the answer is other democracies. Presumably, this

gives us a conflict priority table with benign autocracies falling somewhere in between malign autocracies and other democracies. Quite possibly, the in-between category of benign autocracy will fall closer to the other democracy end of the schedule than it will approximate the malign end of the continuum.

What can we say about autocracies in this respect? Since they are a more heterogeneous group than democracies, the priorities are less clear-cut. Like democracies, however, autocracies should be most likely to conflict with states with intentions that are viewed as malign and least likely to conflict with states with intentions that are viewed as benign. But autocracies have enemies in both the democratic and autocratic camps. Thus, their conflict priority schedule is relatively short. Autocratic or democratic enemies head the list while autocratic and democratic non-enemies constitute the least conflict-prone group. Table 7.2 summarizes these distinctions for democracies and autocracies.

While the distinctions remain somewhat broad, they suggest, if substantiated empirically, that it would not be entirely accurate to say that democracies are just as belligerent as nondemocracies in their foreign policy conflict with nondemocracies. The level of conflict (or belligerence) might be the same but its orientation is not the same. Since the differential orientations reflect interaction with a hostile environment that is not constant, the fundamental disagreement manifested in table 7.1 may simply be traced to overlooking the evolutionary nature of democratization and its implications for foreign policy conflict behavior.

Note, however, that we are not attempting to account for the monadic puzzle in monadic terms. Our answer to why democratic major powers are more involved in conflict than might otherwise be expected is that they represented the vanguard of a particular strategy to organizing political systems. As they emerged they met resistance from old and new alternative strategies. To survive and expand, liberal political systems have clashed repeatedly with aristocratic, fascist, and communist political systems. To the extent that the liberal systems have defeated their opponents after more than a century of intermittent combat, it may be more possible for democratic systems to demonstrate lesser propensities toward interstate conflict. One can argue that this is a dyadic explanation. Others might characterize

Table 7.2 Conflict schedules for democracies and autocracies

For democracies	*For autocracies*
1. Autocratic enemies	1. Autocratic or democratic enemies
2. Autocratic non-enemies	2. Autocratic or democratic non-enemies
3. Democracies	

it as a systemic interpretation, an attribution with which we would not disagree. In our tests, we examine both monadic and dyadic propensities of certain sorts explicitly. But, we see nothing unusual in this respect. If we knew and agreed on what drove the democratic peace, for instance, it could inform us what to expect at the monadic level. Thus, if the answer was a matter of shared political culture—a dyadic explanation—we might not have any reason to anticipate monadic behavior. If the democratic peace rested primarily on constrained democratic institutions—a monadic explanation—we should anticipate some sort of conforming monadic behavior. But if the democratic peace was predicated somehow on the hegemony of Britain and the United States either in terms of hegemonic restraints on violence or less dissatisfaction with the status quo among the older, wealthier, and increasingly democratic states—systemic explanations—we might still anticipate monadic behavior that distinguished between democracies and autocracies. The point remains that solutions to the monadic puzzle are not confined to arguments about monadic attributes and behavior.

Five hypotheses are suggested by the discussion above. We start with the old consensus focused on the overall frequency of conflict and then proceed to qualify its implications significantly by considering who is likely to conflict with whom:

H16: *In general, autocracies and democracies seem to be equally belligerent in the frequency of their overall conflict behavior.*
H17: *Both autocracies and democracies are equally likely to distinguish between enemies and non-enemies in their conflict behavior.*
H18: *Democracies are likely to distinguish between democracies and autocracies in their conflict behavior while autocracies are less likely to do so.*
H19: *Democracies are most likely to engage in conflict with autocratic enemies and least likely to do so with democracies. Autocratic non-enemies, as opponents, fall somewhere in between.*
H20: *Autocracies are most likely to engage in conflict with enemies, whether democratic or autocratic and least likely to do so with non-enemies, whether democratic or autocratic.*

In the next section we develop ways in which to measure regime type, enemies versus non-enemies, and conflict behavior so that we can test the five hypotheses in the section that follows the measurement discussion.

Testing Considerations

We measure regime type and conflict (militarized interstate disputes) as in earlier chapters. We might have focused solely on war activities instead. Yet the major power war *N* for the nineteenth and twentieth centuries is small and the numbers become even smaller when distinctions are made about the participation of different regime types. The strategic rivalry data utilized in earlier chapters are used to tap the enemy dimension. Still, the strategic rivalry data set does not seek to identify all enemy categorizations, but only those of a certain type—those roughly between perceived equals

("competitors") even though that sometimes yields mismatched competitors on the order of the United States and Cuba. One way to evade the problem of incomplete data is to focus solely on the major powers. Such a strategy does not completely eliminate the missing information problem but it should help to minimize it. Major powers are likely to regard one another as competitors. Therefore, we are less likely to overlook any enemy categorizations within the major power subsystem than in the entire world. Major powers certainly do have foreign policy problems with smaller powers but usually these problems fall short of rivalry/enemy categorizations.

The major power subsystem is attractive for other reasons as well. Major powers do not monopolize world conflict but they participate in it at levels greatly disproportionate to their numbers. Even more compelling is the linkage between the evolution of democracy and the major powers. As argued earlier, the long-term fate of democratization has been at least partially a functioning of major power clashes in World War I, World War II, and the Cold War. These affairs were primarily the outcomes of intersecting, major power rivalries, and were strongly flavored by a democracy versus autocracy dimension. The evolutionary pattern we are ascribing to the environment in which democratization has emerged should be most acutely manifested at the major power level. The empirical answers that emerge for the major power subsystem will not necessarily correspond to world-level analyses but at the same time there should also be much less "noise" introduced by conflict patterns in corners of the world in which the democracy versus autocracy dimension remains less significant.

The rivalries between the major powers are identified in table 7.3. We take this opportunity to also highlight the evolutionary nature of major power rivalries. Of most immediate interest are the three "generations" of rivalry that are discernible. The first wave represents, for the most part, a carryover from the eighteenth-century conflicts that climaxed in the Napoleonic Wars but whose enemy categorizations continued into the nineteenth century in part because the rivalry alignments were centered on the containment of France through the mid-nineteenth century. At the same time, some of the older autocracies became more democratic, thereby altering the ideological complexion of the alignments. The containment of Germany became an increasingly central issue toward the end of the nineteenth century and through the mid-twentieth century. It is certainly no stretch to describe the Cold War, from a Western perspective, as centered on the containment of the Soviet Union.[10] In this fashion, the successive generations of major power rivalry, with a few exceptions, have paralleled the ideological conflict between first aristocracy and democracy, then fascism and democracy, and then communism and democracy.

Analysis

Hypothesis H16 asserts that, overall, democracies and autocracies appear to be equally belligerent in conflict frequency. Table 7.4 does not fully support

Table 7.3 Generational waves in the history of major power rivalries

	Democratic–democratic rivalries	*Autocratic–democratic rivalries*	*Autocratic–autocratic rivalries*
	◄---------- ----------	---------- ----------	Britain–France (1816–1904)
	◄---------- ----------	Britain–United States (1816–1904)	
Eighteenth–nineteenth century		◄---------- ----------	Britain–Russia (1816–1956)
		◄---------- ----------	France–Germany (1816–1955)
		◄---------- ----------	France–Russia (1816–90)
		◄---------- ----------	Austria–France (1816–1918)
			China–Russia (1816–1949, 1958–89)
			Austria–Prussia (1816–70)
			Austria–Russia (1816–1918)
			Austria–Italy (1848–1918)
		France–Italy (1879–1940)	
		Britain–Germany (1896–1918, 1934–45)	Germany–Russia (1890–1945)
		Britain–Italy (1934–43)	Japan–Russia (1873–1945)
Late nineteenth–mid twentieth century		Britain–Japan (1932–45)	Italy–Russia (1937–43)
		Germany–United States (1889–1918, 1939–45)	
		Japan–United States (1900–45)	
late twentieth century		Russia–United States (1945–89)	
		China–United States (1949–78)	

this proposition. The mean dispute behavior of democracies and autocracies is not much different. Both average about one dispute per year. But major power democracies have been slightly more prone to disputes than autocracies have and the difference is statistically significant. This finding contradicts most of the findings found by many of the entries in table 7.1, especially those listed on the left-hand side of the table. Presumably, the outcome may have something to do with a major power sample—as opposed to the larger *N*s often relied upon in table 7.1's studies. Yet as our first finding for major powers, it does suggest one answer to the monadic puzzle—namely, that there is no monadic effect if we find that democracies are slightly more belligerent in general—but, as we have argued, it would be premature to stop the query at this point.

Table 7.5 summarizes the evidence for the questions about differential propensities to discriminate between regime types and friends and foes. As hypothesized (H17), neither autocracies nor democracies have any problem in distinguishing between rivals and nonrivals. About 75 percent of democratic

Table 7.4 Dispute propensity by regime type

	Democracies	*Autocracies*	*Difference of means test*
Frequency	352	688	
Mean	1.093	0.964	$t = 32.25^{**}$

** denotes statistical significance at the 0.05 level or better.

Table 7.5 Dispute frequencies for democracies and autocracies by opponent type

	Rival opponents	*Non-rival opponents*	*Difference of mean tests*
For democracies			
Frequency	265	87	
Mean	0.823	0.270	$t = 11.52^{**}$
For autocracies			
Frequency	519	169	
Mean	0.727	0.237	$t = 12.25^{**}$
	Democratic opponents	*Autocratic opponents*	
For democracies			
Frequency	40	312	
Mean	0.124	0.969	$t = 16.89^{**}$
For autocracies			
Frequency	312	376	
Mean	0.437	0.527	$t = 2.25^{**}$

** denotes statistical significance at the 0.05 level or better.

major power militarized disputes have been with rivals. Nearly 80 percent of the autocratic major power militarized disputes have been with their rivals. Major power nonrivals, accordingly, have been much less likely to become engaged in militarized disputes with one another.

While democracies and autocracies converge on their ability to focus on foes, hypothesis H18 predicts that the two regime types will diverge on discriminating between political system types in their conflict behavior. Table 7.5 reports that democratic dispute behavior has been focused on autocratic opponents (87.3 percent) while autocratic disputes are about equally split between democratic (48.1 percent) and autocratic (51.8 percent) opponents. Thus, autocracies do not appear to differentiate on the basis of regime type but this finding, it is argued, is not due to any inherent attributes of autocracy other than the heterogeneity of the regime type.

If democracies differentiate between rivals and nonrivals, as well as between democracies and autocracies, we can combine this information and make some more specific predictions about whom democracies are most/least likely to engage in conflict with. Hypothesis H19 places autocratic rivals at the top of the conflict schedule and democracies at the bottom. Democratic peace arguments preclude the likelihood of many democratic states being strategic rivals and enemies for very long and that would suggest that there should be no behavioral differences exhibited toward democratic dispute opponents who are either rivals or nonrivals. But such an expectation ignores the evolutionary character of these conflict processes. If democratic peace arguments are correct, strategic rivalry between two democracies should be rare or nonexistent. Yet, what if rivalry preceded democratization, as in the cases of the Anglo-French and Anglo-American rivalries? From an evolutionary perspective, we would expect the absence of strategic rivalry among democracies to be at best a condition toward which actors' behavior moved.[11] In both of the democratic strategic rivalries in the major power subsystem, the rivalries ended very early in the twentieth century in order to better deal with the autocratic threat emanating from Germany. In the period after World War I, there was some movement back toward rivalry among Britain, France, and the United States that was cut short by the threats posed first by the fascists and then by the communists. From this perspective, it is rather difficult to credit democratization with ending the Anglo-French and Anglo-American rivalries for it is quite clear that mutual threats from ideological antagonists played an important role.

The historical existence of two democratic strategic rivalries muddies the analytical waters a bit but they would pose a real threat to the democratic peace arguments if they persisted (or perhaps reemerged in a world composed exclusively of democracies). Since they did not persist (and we have not yet attained an exclusively democratic world or major power subset), they are not much of a threat to the predictive logic. Empirically, it also turns out that they are no threat at all. Table 7.6 reports an empirical outcome that mirrors exactly the predicted schedule. Autocratic rivals head the dispute frequency record encompassing some two-thirds of all democratic

militarized disputes among the major powers. The second most frequent category is registered by autocratic nonrival disputants, but the frequency in this category is only about a fourth (27.8 percent) as large as the leading frequency category. The two lowest categories are associated with democratic rivals (9.7 percent) and democratic nonrivals (4.5 percent). If we combined the two democratic categories, the frequency total would still be less than the one recorded for autocratic nonrivals.

The outcome in table 7.7 conforms well to the prediction of hypothesis H20 as well. Hypothesis H15 predicts that the autocratic record should distinguish only between rivals and nonrivals. Table 7.7 shows some difference in the frequency of disputes with democratic rivals (35.9 percent) and autocratic rivals (44.6 percent), but the difference is not great. The frequency associated with democratic nonrivals (12.2 percent) is greater than the one linked to autocratic nonrivals (7.3 percent), but again the difference is not impressive. Autocratic major powers thus have enemies in both regime type camps. As a categorical group, though, they have been unable to focus exclusively on adversaries with different domestic regimes. But this characteristic also fits the autocracies that were the ones most intensively

Table 7.6 Dispute frequencies for democratic major powers

	Autocratic rivals	*Autocratic non-rivals*	*Democratic rivals*	*Democratic non-rivals*
Frequency	233	79	32	8
Mean	0.724	0.245	0.099	0.025
Difference of means tests				
Autocratic rivals		8.12**	8.33**	5.83**
Autocratic nonrivals			5.21**	0.75
Democratic rivals				0.195

** denotes statistical significance at the 0.05 level or better.

Table 7.7 Dispute frequencies for autocratic major powers

	Autocratic rivals	*Democratic rivals*	*Democratic non-rivals*	*Autocratic non-rivals*
Frequency	286	233	79	90
Mean	0.401	0.326	0.111	0.126
Difference of means tests				
Autocratic rivals		1.938	3.45**	3.44**
Democratic rivals			2.07**	2.22**
Democratic non-rivals				0.11

** denotes statistical significance at the 0.05 level or better.

aligned against democracies as ideological adversaries. Germany and Austria-Hungary had their Russian problems, as did Japan. Once the Soviet Union's problems with Germany and Japan were ameliorated, the old Sino-Russian rivalry reemerged less than two decades after 1945. One of the ironies of this twentieth-century story is that the ultimate triumph of liberal democracy depended on these animosities among autocracies.

Conclusion

The monadic democratic peace puzzle concerns the problems we have in theoretically and empirically identifying pacific tendencies on the part of democracies, as opposed to democratic dyads. The issue remains the subject of considerable disagreement despite a widespread, although apparently waning, acceptance of the idea that there is no pacific effect at the monadic level that corresponds to the dyadic level between democracies. There is no puzzle if the democratic peace is predicated entirely on some reciprocal interaction between two democratic states. But if the peace is traced to attributes of the democratic regime type, one might have good reason to expect some monadic effects in addition to the dyadic effects.

We suggest that some proportion of the theoretical and empirical disagreement can be traced to asking the wrong question. The empirical facet of the puzzle has frequently hinged on contrasting democratic behavior toward all states with autocratic behavior toward all other states. But the query should not stop at this juncture. If there is something about democracy and democratization that influences foreign policy behavior, it need not be solely a matter of regime type attributes. We also need to take into consideration the environment in which democracies have attempted to survive and expand. That environment has been largely hostile to democracies but the nature of the animosities have evolved over time as competing visions of order have struggled for acceptance and dominance.

Reflecting upon the historical evolution of the international environment in the past two centuries suggests that a corresponding monadic effect should be manifested most noticeably in terms of who is most likely to conflict with whom. Democracies should prioritize their conflict so that autocratic rivals/enemies receive the lion's share of attention, followed by other autocracies, and, then, other democracies (both rivals and otherwise). This is precisely what we find in the militarized dispute record for major powers. Autocracies, in contrast, should be expected to differentiate only between rivals and nonrivals because of the sheer heterogeneity of the autocratic pool. This also is exactly what we find in the major power record.

We have also failed to reaffirm the contention that democratic major powers engage in as much conflict as do autocratic major powers. But the question should not be how much, but with whom. Democracies have had good reasons to engage in conflict with their autocratic rivals. If they had not done so, there could not have been any dyadic (monadic) democratic

peace phenomenon to quarrel about. At the same time, the nature of the findings suggest that the historically high conflict levels of the democracies are contingent on the evolution of the international environment. If democracy has defeated once and for all its main ideational challengers, as some commentators have suggested, it is only then that we might anticipate observing significant monadic effects of the democratic peace.

Of course, this last generalization also assumes that whatever drives the dyadic democratic peace phenomenon does not rely exclusively on reciprocal interactions between democratic states. Otherwise, we have no reason to expect monadic effects. But, since we do find strong monadic effects as hypothesized, one implication is that the democratic peace is not predicated solely on the reciprocal interactions of democracies. Indeed, it may well be that some proportion of the dyadic peace between democracies is due to the very high levels of hostility encountered by democratization in the past two centuries. Mutual enemies can generate strong incentives for moderating conflict between states attempting to cope with a threatening environment. If so, we may not have much success in delineating the sources of the dyadic puzzle until we have greater variation in the level of external hostility to democratization.

Even so, the limitations of our findings need to be emphasized. The explanation has been crafted with major powers in mind. The empirical examinations, therefore, are restricted to major powers. We cannot say that our argument applies to all democracies and all autocracies. We suspect it does but not necessarily as strongly as it applies to major powers. The pressures facing minor powers, as well as the opportunities available to them, are not exactly the same as the pressures and opportunities confronted by major powers. The ideological wars of the twentieth century were most intensely contested by major powers confronting other major powers. The decidedly more local environments in which minor powers operate are also apt to be much more heterogeneous than the common playing field of the major powers.

We have placed considerable emphasis on evolutionary trends in the nineteenth and twentieth centuries. Will our findings hold equally well into the twenty-first century? The answer to that question is not clear. If our argument is contingent on an environment in which democracies must fight to survive the hostility of other major powers and that hostility dissipates, it may be that the incentives for inter-democratic cooperation may also dissipate. But it is also possible that democracies are different from autocracies in other ways than in their ability to discriminate among rivals and regime types. If they are genuinely less belligerent due to institutions, culture, transparency, or status quo–orientation even in the absence of concerted hostility, the international system of the twenty-first century is unlikely to resemble the nineteenth and twentieth centuries all that closely. Yet without knowing precisely what drives the dyadic democratic peace phenomenon—the answer to the dyadic puzzle—it is hard to be definitive about the expected half-life of our partial answer to the monadic puzzle.

International contests among competing political organizing schemes for primacy is one way in which the system matters to democratic peace phenomena. Yet it is hardly the only way in which the system plays some role. Confidence in the explanatory power of regime type and other domestic variables has led some analysts to suggest that we can dispense altogether with systemic perspectives. Our response is that the abandonment of systemic perspectives is premature. In chapter eight we demonstrate two more ways in which systemic perspectives can help illuminate the interaction between regime types and the nature of interstate conflict. One way has to do with the argument that democratization actually increases for a time the number of democratic–autocratic antagonisms. In many respects, this is another take on the subject already explored in this chapter. The second systemic path explored in chapter eight is the relative explanatory contributions of our interpretation of structural change (at the global and regional levels) versus, or in conjunction with, increased democratization. Both systemic paths yield definite contributions to a further elucidation of what appears to be going on in international politics.

CHAPTER EIGHT

Long-Term Structural Change and Regime Type

In the past decade or so, the empirical study of world politics has gravitated overwhelmingly toward dyadic analyses. In what might be termed a macro-Noah's Ark perspective, international relations is viewed as consisting of the aggregation of thousands of pairs of interacting states. The main reason for this shift in focus is that it makes considerable sense for a number of questions. At the core of such traditional phenomena as war, crisis, and arms races, there are often two states in confrontation. More recently emerged foci on militarized disputes, democratic peace, and enduring rivalry all seemingly lend themselves readily to a dyadic interpretation. While the adoption of a dyadic perspective makes great sense and has contributed to a number of advances in our understanding of international relations processes, there is a cost to pay if the one level of analysis is permitted to crowd out other levels of analysis. This is all the more the case if dyadic arguments and findings generate puzzles that can only be dealt with at some level of analysis other than the dyadic.

A case in point is the democratic peace. If we accept the finding that two democratic states are less likely to engage in conflict than a dyad involving one or more nondemocratic states, what should we make of the probability that a democratic state will become involved in conflict (in comparison to an autocratic state)? Are democracies monadically less conflictual as they are when paired with another state of the same regime type? What should we make of an international system that is increasingly populated by democratic dyads? Is a democratizing international system less conflictual than one that is predominantly autocratic? These are interesting questions in their own right, but they are also important to the dyadic analyses. An answer to the monadic puzzle should help answer not whether but why there is democratic peace at the dyadic level. If the reason is that democracies are more institutionally constrained or more transparent, we should anticipate less conflict behavior at both the monadic and, selectively, the dyadic level. If the reason for the democratic peace requires some type of cultural interaction effect, we should not anticipate a monadic effect of regime type

on conflict behavior. The implication is that an exclusive focus on a single level of analysis will not necessarily suffice in making sense of the topics in which we are interested.

A new consensus seems to be emerging that the democratic peace works at both the monadic and the dyadic levels (Ray, 2000; Russett and Starr, 2000). A similar generalization cannot be applied to the system level of analysis. Some authors extrapolate linearly that more democracies means less conflict in the aggregate. Others say that the relationship is curvilinear. Up to a point, more democracies also means more antagonistic, mixed dyads. If autocratic–democratic dyads are especially prone to conflict, an increase in their number means more conflict even as the number of democracies is initially increasing. Only after democracies exceed 50 percent of the system will the number of mixed dyads begin to decline. So, too, presumably, will the conflict that they produce. But, still other authors insist that there is no reason to expect dyadic tendencies to translate predictably at the systemic level. So, one systemic puzzle is precisely how does democratization affect systemic levels of conflict—if, in fact, it does?

A second systemic puzzle presumes some systematic relationship between democratization and conflict. If democratization does influence conflict at the systemic level, how does its effect compare with other influences? We found in chapter five that regime type's influence is weaker than other, less liberal, types of influences such as rivalry while others (Oneal and Russett, 1999) find that structural variables such as hegemony have much less influence than dyadic regime type in predicting conflict levels. Thus, assuming some relationship between democratization and conflict at the systemic level, how does the strength of this relationship compare with other possible influences? More to the point, are structural variables largely irrelevant to democratization effects?

We examine both systemic puzzles with the assistance of 1816–1992 information on militarized interstate disputes between major powers as our index of conflict. The major power subsystem serves two purposes simultaneously. As a recognizable subsystem, we can assess whether various arguments are useful in explaining its evolution over time. Systems change unevenly and there is no reason to expect democratic peace phenomena to emerge equally potent in all parts of the world at the same time. As the elite subsystem of the larger world, we should also expect it to encompass some of the key motors of systemic change. To explain systemic change, we also update an interpretation of global–regional dissynchronization formerly applied to the coevolution of the global political economy and regional European international relations.[1] In place of the eclipsed European region after 1945, the regional focus shifts to the broader Eurasian grouping bounded by the Arctic in the north and the Atlantic, Pacific, and Indian Oceans. The basic argument is that one must examine both global and regional concentration tendencies simultaneously—as opposed to the tendency to do one or the other exclusively, or, more commonly, to examine some fused version of the two different domains. Global concentration and

regional deconcentration predicts less conflict. Global deconcentration and regional concentration suggests more conflict is likely.

In addition to structural change to or away from concentration in the two domains, an index of major power democratization is constructed and compared to the explanatory power of global and regional concentration. We find that global and regional structural change, as well as democratization, help account for fluctuations in conflict at the systemic level. Democratization operates as a consistent constraint on militarized disputes while regional concentration encourages conflict activity. Global concentration restrained conflict prior to 1945 but encouraged it in the post–World War II era. Thus, our answer to the two systemic puzzles is that the effects of major power democratization on conflict appear to be reasonably linear. The more democratization, the less likely is major power conflict. Nor need we dismiss the efficacy of macrostructural change in explaining this conflict. Regional and global concentration patterns appear to be just as important as democratization in the movement toward a more peaceful major power subsystem.

The Systemic Puzzles: Whether and How Much?

The two systemic puzzles associated with the democratic peace are (1) whether democratization has a pacifying effect on the system writ large and (2) if there is an effect, does its strength preclude the need for structural explanatory variables? Since the first puzzle logically takes precedence, we address it first.

There are three views on how best to translate the implications of the dyadic democratic peace into systemic terms. The linear argument is the most straightforward one. If two democratic states are unlikely to engage each other in conflict, the more numerous such pairs, the less overall conflict one might expect. Therefore, the more democracies in the system, the greater the number of democratic dyads, and the less the systemic tendency to conflict.[2]

Gleditsch and Hegre (1997) dissent from the linear perspective and move the focus from the pacific tendencies of democratic dyads to the bellicose tendencies of democratic–autocratic dyads. When democracies first emerged in the system, both the total number of democratic dyads and the number of democratic–autocratic dyads increased. Moreover, the number of the latter increased more quickly than the number of the former. Thus, systemic democratization initially and somewhat ironically generates more antagonistic dyads than it does pacific dyads.[3] The net effect should be toward greater conflict until the point is reached at 50 percent democratization at which the number of more antagonistic dyads begins to decline.

Table 8.1 provides an illustration. Imagine a 10-actor system. At the beginning, there are 1 democracy and 9 autocracies. This configuration

Table 8.1 An illustrative schedule of rates of change in the dyadic mix

System N	*Number of dyads*	*Number of democracies*	*Number of democratic dyads*	*Number of mixed dyads*	*Number of autocratic dyads*
10	45	1	0	9	36
10	45	2	1	16	28
10	45	3	3	21	21
10	45	4	6	24	15
10	45	5	10	25	10
10	45	6	15	24	6
10	45	7	21	21	3
10	45	8	28	16	1
10	45	9	36	9	0
10	45	10	45	0	0

creates 0 democratic dyads, 9 democratic–autocratic dyads, and 36 autocratic dyads. With 3 democracies, there are only 3 democratic dyads but 21 democratic–autocratic dyads. It is not until the sixth democracy emerges that the number of democratic–autocratic dyads begins to diminish from the number attained with 5 democracies. Note as well that while the number of mixed dyads continues to decline after the half-way mark, it is not until the eighth democracy emerges that the number of democratic dyads outnumbers the number of democratic–autocratic dyads. If mixed dyads are as bellicose as democratic dyads are pacific, it may be that a system has to go well beyond the 50 percent democratization threshold to begin experiencing reductions in conflict propensities.

Ray (2000: 311–12) has suggested that this perspective is entirely mathematical in conceptualization. That is, a given number of democracies and autocracies will produce *X* democratic dyads, *Y* mixed dyads, and *Z* autocratic dyads. If our theory tells us that we should be primarily concerned with the number of democratic dyads, the mix of different dyad types may not matter. Yet Gleditsch and Hegre's (1997) point is that it is the mixed dyads that are particularly problematic when it comes to assessing conflict propensities. Thus, it is not simply a mathematical argument but one based on substantive information about differential tendencies toward conflict. However, Gledtisch and Hegre's conclusion that the relationship between democratization and systemic conflict will be curvilinear (positive to the 50 percent point and then negative from there on) is more mathematical in nature. The curvilinearity conclusion is certainly logical but, empirically, it also depends on how the system develops. In table 8.1's illustration, the movement is gradual starting with 1 democracy and adding 1 more democracy at each successive interval. The assumption is one of gradual but irreversible democratization. The illustration and the Gleditsch–Hegre argument also assume that all democratic–autocratic dyads are as equally bellicose as all democratic dyads are equally pacific. Moreover, it is assumed that all states or dyads are equally powerful or equally weak. Otherwise, we

would not be assuming that one mixed dyad was as significant for trouble-making as one democratic dyad was for avoiding trouble.

None of these assumptions is likely to have been manifested in the recent history of world politics. Democratization has made advances just as it has regressed. While it is believed that all democratic dyads are no longer prone to war, only some mixed dyads have been highly conflictual. We also know that democratic-led coalitions defeated coalitions of autocratic challengers on two occasions in the twentieth century, in part because the democratic coalitions possessed superior capability positions. On the other hand, as already noted, it is also possible that the system has to become much more democratized than 50 percent for democratic dyads to begin outnumbering the mixed dyads. All of these observations suggest some caution in anticipating an observed curvilinear relationship. We should certainly keep an eye open to a nonlinear possibility but it is also quite possible that any observed relationship will prove to be linear.

The third interpretation (Ray and Wang, 1998; Ray, 2000) of how democratization might affect systemic conflict is one of skepticism that findings for one level of analysis necessarily have any meaning for other levels. Knowing something about dyadic tendencies need not tell an analyst anything about how the overall system will operate. For instance, as long as a highly democratized system encompasses one autocratic or mixed dyad, considerable conflict could still be generated. A third world war might not be excluded, especially if the surviving autocracies are extremely powerful.[4] While we are sympathetic to this stance, it downplays the analytical propensity to look for behavioral patterns. Certainly one very powerful autocracy is capable of generating a high level of conflict for the system. But the question is whether democratization encourages or discourages conflict—not whether it is capable of suppressing it altogether. If democratization makes conflict somewhat less probable at the monadic and dyadic levels, it does not seem unreasonable to anticipate a similar effect at the systemic level. The strength of the effect need not be identical (or statistically significant) at all levels but we can see no specific reason why the signs of the relationship might vary depending solely on the aggregation principle utilized.

H24: *Democratization and systemic conflict are positively related.*

Thus, our answer to the "whether" question is that we should expect a generally positive relationship between democratization and systemic conflict. The possibility of a nonlinear relationship should not be ruled out; nor can we rule out the possibility that the relationship may be weaker at the systemic level than at other levels. Statistical significance partially addresses the "how much" question but there is much more substantively at stake here.

Much of the empirical work on the democratic peace has focused on the question of whether there is a relationship between regime type and conflict. Much more rare are studies that move on to the next question: if there is a relationship, how does it compare to other influences propelling

or moderating conflict? A noteworthy exception is Oneal and Russett (1999). In an ambitious Kantian model, they compare the influence of regime type at the dyadic level with economic interdependence and international organization membership. Even more interesting for our present purposes, they also include a comparison of their Kantian variables with three "realist" variables: hegemonic strength, satisfaction with the systemic status quo, and hegemonic threat perceptions. Of the three, the status quo indicator was insignificant and the threat perception indicator (hegemon's defense burden) was positive and significant. The most curious outcome was manifested by the hegemonic power indicator. The initial outcome was negative and described as "nearly significant" (at the 0.06 level). However, when the analysts removed the first year of each world war, the coefficient reversed its sign from negative to positive (and became statistically significant). Their conclusion was that a strong hegemon did not reduce conflict during the years between the world wars. On the contrary, hegemonic strength seems to encourage conflict in general, while other factors such as democratic and economically interdependent dyads worked to discourage conflict selectively within the appropriate dyads.

This empirical outcome is quite puzzling, at least for analysts who have devoted some time to exploring structural explanations. It does not say that regime type and economic interdependence are more important predictors of conflict than hegemonic strength. That was not exactly the question addressed by Oneal and Russett. It does imply rather strongly, however, that what a number of people thought they knew about structural variables, such as power concentration, is simply wrong. The evidence that power concentration in the system reduces conflict apparently turns out to hinge on the inability of Britain to forestall World Wars I and II. Furthermore, these findings suggest that if we are interested in explaining conflict, dyadic analyses are vastly superior to systemic analyses. There may be a systemic relationship between democratization and conflict, but why bother?[5]

Still, all empirical findings are not equally persuasive. Whether they are persuasive often hinges on such factors as index construction in particular and, more generally, research design considerations. Our reading of the Oneal and Russett analysis is that the jury is still out on the relative contribution of system structure and dyadic variables to explaining conflict. We reach this conclusion for several reasons. First, the Oneal and Russett analysis is fundamentally a dyadic analysis with some systemic variables, such as hegemonic strength and defense burdens, included. It is not a systemic analysis per se. One might expect dyadic variables to fare better in a dyadic examination than might non-dyadic variables. Second, the Oneal and Russett analysis focuses on the late nineteenth century and most of the twentieth century because of data availability problems with dyadic trade information. That means that most of the "hegemonic strength" era for the nineteenth century has been excluded from the analysis. Oneal and Russett are basically looking at 65 years of British weakness and less than 50 years of U.S. strength, albeit subject to relative decline. It may be that

fusing eras in this fashion distorts the effect of systemic leadership in some way that led to the weak findings.

Third, conflict is measured in terms of militarized disputes and not war. Systemic variables have been used primarily to explain the most serious and widespread warfare. It is certainly not inappropriate to look instead at militarized disputes but one needs to adjust expectations accordingly.[6] The 1816–1945 period can be described as a long period of global war avoidance (1816–1913) and a period of intensive, if intermittent, global war (1914–45). This period thus conforms to our customary expectations about structural change. A new system leader emerged after the global warfare of 1792–1815 and was not seriously challenged until toward the end of the nineteenth century. The question of succession was not resolved until 1945. It is after 1945 that systemic history deviates from structural change expectations. While there has been no new global war in the half-century plus since the last one, a serious challenger emerged almost immediately in the postwar period. That challenge, manifested in the U.S.–Soviet Cold War, is very much represented in the annals of militarized interstate disputes. The fact that the post-1945 period was more conflictual than the post-1816 era is also not a reason to avoid examining militarized interstate disputes. But it does help explain why Oneal and Russett find a positive relationship between "hegemonic strength" and conflict. Their maximal period of "hegemonic strength" occurs during the disputatious Cold War period. The question, then, that needs to be resolved is not whether militarized disputes should be used to represent conflict, but whether the expectation that "hegemonic strength" should be expected to reduce conflict in all instances. We suggest that that is not a reasonable expectation. The hegemony–conflict relationship requires significant theoretical qualification.

Finally, Oneal and Russett measure hegemonic strength in terms of the standard Correlates of War capability index. This index combines information on population size, urban density, armed forces size, military expenditures, iron/steel production, and energy consumption. But size is not always the most important factor in delineating periods of systemic leadership.[7] Nor does the "size" of the system leader tell us very precisely where the effects of systemic leadership are most likely to be felt. As Stein (1984) once noted, all periods of leadership are partial. Some parts of the world are less likely to be affected by the presence or absence of a strong leader than are others. Yet the hegemonic concept often seems to imply universal dominance—hence the suppression of conflict is also expected to be equally universal. If systemic leadership has been most concerned with ordering interregional transactions, regional disorders may be a separate question. Or, to put it another way, there are differences of opinion about how best to identify systemic leadership and the structural circumstances in which it is likely to matter most. Before we jettison structural considerations altogether in favor of dyadic attributes, it might be wise to pursue some different approaches to this question of explaining conflict in world politics.

A Systemic Approach to Explaining Conflict

As the heading hints, Oneal and Russett tried one approach; we propose to try another one. Building on earlier work, we contend that the past 500 years of international conflict have been influenced by the emergence and dissychronization of two structural patterns.[8] Measuring polarity in terms of one overarching structure misinterprets the divergence between the global system and principal regional system that became more pronounced after 1494. The global system is focused on long-distance, interregional issues, including trade and order while the principal regional system (increasingly western Europe between 1494 and 1945) fixated on questions of territorial expansion and hegemonic dominance (Hapsburg/Spain, France, and Germany). Each system or subsystem was characterized by longitudinal fluctuations in power concentration but with differential effects. Concentration in the global system was based on commercial and technological innovation, sea power and leadership of a global war-winning coalition. In the global system, power concentration reduced conflict. In the primary regional system, power concentration was based on population size, wealth, and large and effective armies. Bids for regional hegemony increased conflict within the region. Bids for regional hegemony also led to the intermittent fusion of global and regional subsystems in the sense that a bid for regional European dominance was viewed as threatening for the stability of the global system and therefore something to be thwarted. Global wars occurred when global powers organized themselves and others to resist a bid for European hegemony.

The global and primary regional subsystems were related in another way as well. Power concentration in the global system discouraged regional power concentration. Likewise, the decline of the global system leader encouraged regional ambitions and the effort to resist the fulfillment of those regional ambitions encouraged reconcentration of power at the global level. Thus, structural change in the two subsystems were linked but often dissynchronized. The outcome of World War II changed that by downgrading the importance of western Europe to the world system. That might have meant an end to an unusual pattern that began to emerge only in 1494 if it were not for the existence of an adjacent region. Earlier, we had expressed some reluctance to stipulate whether the global–regional dissynchonization pattern ended in 1945 or shifted its focus to some other region such as east Asia that possessed some of the necessary ingredients (i.e., large and powerful territorial empires with continental grand strategies competing with offshore sea powers). Our error was in not thinking on a grand enough scale. Eastern Asia was a possibility that encompassed several actors with considerable significance for the twenty-first century (Russia, China, Japan). But what had happened in the 1494–1945 era was that the world outside western Europe grew (or returned to being) too big for Eurocentric control. The United States and Russia eclipsed the traditional European major powers in a variety of dimensions that led commentators

to call the most powerful two states superpowers. The competition between the two superpowers seemed increasingly global in scale. Other actors simply could not afford to play in this league. In this context, eastern Asia seems to be too small of a theater to replace western Europe.

But another way of looking at what happened after 1945 is that the superpower competition only seemed global in scale. In reality, it was primarily centered on control of the Eurasian region. With exceptions for the Cuban Missile crisis and a few crises focused on Egypt (at the very edge of Eurasia), all the wars and most of the crises took place in the eastern or western ends of the Eurasian land mass. The preoccupation of the NATO alliance was to prevent Soviet control expanding further west in Eurasia. Korea and Vietnam were to prevent Soviet "proxies" from expanding further in the east after the loss of China. In some respects and not coincidentally, the U.S.–Soviet Cold War took on many of the features of the British–Russian Great Game of the nineteenth century. The main idea was to contain Russian expansion in Eurasia in both the nineteenth and twentieth centuries. Thus, with the successive exhaustion of Spain, France, and Germany in the west, the focus moved east but not to an entirely novel phenomenon. The nineteenth-century system leader had been challenged by the threat of Russian expansion and was in fact slow to shift its attention away from Russia (and France) to the primacy of the German threat only toward the end of the century. One of the reasons British decision-makers worked poorly with their Russian-Soviet counterparts in World War I, in the interwar period, and in World War II was that both parties continued to see the other state as a principal rival—not for control of western Europe per se but for larger Eurasian implications. After 1945, the United States succeeded to the role of chief container of Russian expansion throughout Eurasia.

What was relatively novel about the post-1945 era was the degree to which the Soviet Union was prepared to challenge the American order. From a historical–structural perspective, the opponents of the main global war victor are usually thought to be too exhausted or not yet ascendant to contest the postwar order. This is one source for the idea that hegemonic strength reduces conflict. Hegemonic strength tends to be greatest immediately after the war that creates a new systemic leader if for no other reason than that all of its opponents (and some of its closest allies including the former system leader) are flat on their backs. But we now think this is an erroneous generalization. It fits the post-Napoleonic era well but less so in any other comparable period.

Just which postwar periods are comparable for historical–structural comparison are contested. However, if we go back to 1494, the rise of Portugal as the first global system leader in the early sixteenth century did not appear to have much impact on Valois–Hapsburg conflict in the European region. It continued and eventually merged into the Spanish-centered contest of the second half of the sixteenth and first half of the seventeenth centuries. The ascendancy of the United Provinces of the Netherlands as global

system leader after 1608 did not prevent the resumption of Dutch-Spanish hostilities in the Thirty Years War—a conflict centered primarily on local or regional issues as opposed to global issues. The emergence of Britain after the wars of Louis XIV did lead to some short-lived suspension of conflict over the regional hegemony issue and even a shorter-lived rapprochement between Britain and France but by the 1740s, the Anglo-French conflict was renewed and not resolved by 1748, 1763, 1783, or even 1815. But after 1815, there was a long peace of major power sorts. After 1918, no system leader could or was willing to emerge. After 1945, the United States was finally willing but also confronted with an immediate Soviet challenge.

From this angle, the post-1815 long peace was exceptional and not the rule. It may have been due more to the unanimity of opinion on the need to contain France and French reluctance to test the concert than it was traceable to British industrial and naval predominance. Whether a system leader is challenged immediately after the global war seems to depend on several related variables: the maturation of new processes in international relations, the relative strength of the new system leader, whether the new system leader has decisively defeated its rivals in the preceding global war, and whether a member of the global war-winning coalition defects into opposition immediately or at some later point in time. One must also add the extent of potential challenger ambitions. There is no need to assume that these ambitions are constant. While the ambitions no doubt expand in response to systemic opportunities, they still must emerge and be articulated by distinctive domestic leaders.[9]

Early in the emergence of the dissynchronization process, we should expect the global system leader to have less impact on the behavior of others than they might several hundreds of years later (e.g., Portugal versus the United States). We should expect weak leads (Portugal again and Britain I after 1713) to have less impact than strong leads (Britain II after 1815 and the United States after 1945). The short-lived absence of conflict after 1608 reflected a temporary truce between the Dutch and the Spanish. Intensive conflict was resumed in 1621, although the Spanish were no longer in the powerful position they occupied in the 1580s. Finally, it is not unknown for members of the winning coalition to defect into the opposition but, in world politics, they do not usually do so as soon as the global fighting ends, as in the Soviet case.

The above qualification of historical–structural perspectives does not mean that the structural dissynchronization model is not useful. High concentration at the regional level and low concentration at the global level sets up systemic conditions ripe for an attempt to overthrow the systemic status quo. But global and regional concentration levels do not necessarily predict how much conflict short of global war will be experienced. Still, we would expect regional concentration to be positively correlated with systemic conflict. Other things being equal, we would expect global concentration to be negatively correlated with systemic conflict. Unfortunately, other factors cannot always be assumed to be constant. In the context of an

immediate postwar challenge, as in the U.S.–Soviet Cold War, the sign of the relationship may well switch to positive. Conflict was greatest when the U.S. lead was great. The U.S.–Soviet feud only became more manageable and less disputatious as the United States experienced relative decline in its lead. That certainly does not mean that the collapse of the Soviet challenge can be explained in terms of continued U.S. relative decline. Far from it, the ultimate explanation for the collapse of the Soviet Union will probably have some role for the renewal of the U.S. lead thanks to innovation in information technology. Rather, the point is that while a strong U.S. lead after 1945 may have reduced the probability of a new global war, the strong lead did not suppress significant resistance to the U.S. preferences for world order. It may even have galvanized some of the challenge.

Within this macrostructural context, the possible transformational effects of democratization are not ruled out as either irrelevant or marginal. Democratization also represents a type of structural change in this case at the domestic level. Democratization appears to generate "nicer" foreign policy strategies or at least reduce the probability of "predatory" strategies on the part of expansionary powers. Why exactly that may be the case is not something that we feel the need to resolve at this point. Yet it is unlikely to be coincidental that the last two system leaders have been leading democracies in their own right, protective of other democracies, and have also encouraged other states to adopt democratic constitutions. The system leaders have not always been successful in their protection or encouragement efforts, but their victories in the global warfare of the twentieth century (hot and cold) made more rather than less democratization more probable. To the extent that system leaders attempt to shape the world in their own image (and others attempt to imitate the success of the leaders), democratization has become very much a part of that material and ideational package. If the system leaders' autocratic (World War I), fascist (World War II), or communist (U.S.–Soviet Cold War) foes had triumphed, we would most likely be pursuing some analytical questions other than systemic puzzles of the democratic peace.

H25: *Macrostructural change (global and regional concentration), even controlling for democratization, remains a significant explanatory factor in accounting for systemic conflict.*

Testing Considerations

In earlier analyses we have examined global–regional dissynchronization over the 1494–1945 period in attempts to explain systemic warfare.[10] To deal with more contemporary democratization developments, we need a finer-tuned and shorter index of conflict other than a series focusing on the relatively rare systemic war. Following Oneal and Russett (1999), we measure conflict in terms of the annual frequency of militarized interstate disputes. Unlike Oneal and Russett (1999), we aggregate the disputes for all

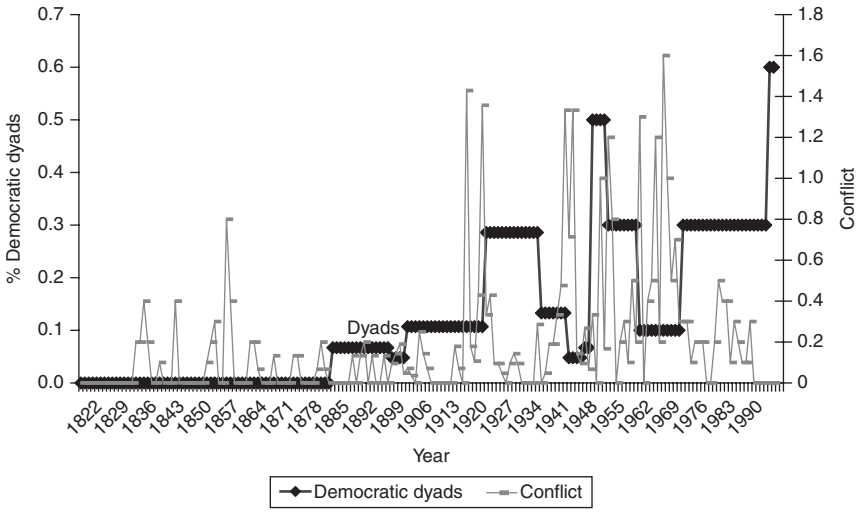

Figure 8.1 Democratization and conflict

major powers in each year. Dispute data are available for 1816–1992; therefore, we focus on this period.[11] These data are plotted below in figure 8.1.

Dissychronization is captured, mostly as before, by constructing measurements of concentration in global and regional capability distributions. We have argued that the primary region was western Europe up to 1945. After 1945, we now contend that the primary region focus shifted to Eurasia. Regional powers are states that develop their resources primarily to advance territorial, economic, and security interests in their immediate neighborhood. Armies, therefore, have tended to be the privileged coercive instrument. Accordingly, we first identify the major regional actors as those great powers resident within the appropriate regions (France, Prussia/Germany, Italy [1860–1943], Austria [to 1918] for western Europe between 1816 and 1945 and Russia, China [from 1950 on], France, and Britain for Eurasia after 1945).[12] Lacking annual army data for the 1816–1992 period, we first aggregated the armed forces sizes of these actors (using Correlates of War data once available at http://www.polisci.lsa.umich.edu), calculated the annual shares of each actor, and then utilized this information to compute annual concentration indices, employing a concentration formula developed by Ray and Singer (1973).[13]

Global powers are states that develop their resources primarily to advance their economic and security interests within the transcontinental, global sphere. The need to project force at long distance places a premium on sea power as the privileged coercive instrument. In order to qualify as a global power, minimal capability and activity prerequisites must be met.[14] Qualifying global powers in the 1816–1992 periods are Britain (to 1945), the United States, France (to 1945), Germany (1870–1945), Japan (1875–1945), and Russia or the Soviet Union. To measure the concentration

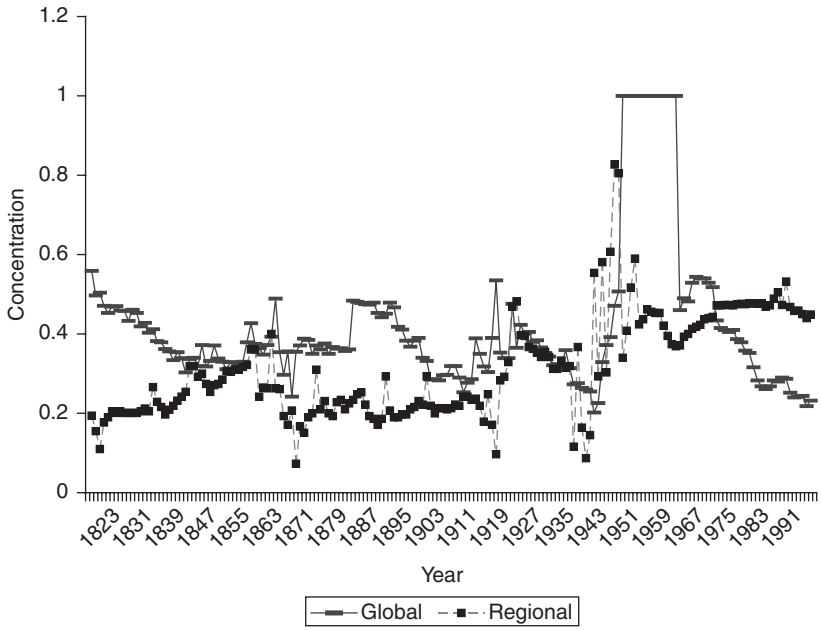

Figure 8.2 Global and regional concentration

of global naval power, we aggregate indices of sea power developed by Modelski and Thompson (1988), calculate the shares of each of the actors, and compute the same type of concentration indices as is used in the regional context.[15] Figure 8.2 plots the regional and global concentration series that emerge from these computations.

The long-term pattern is for global concentration to increase after a period of global war and to then erode. Regional concentration is usually low after a period of global war and gradually increases, depending in part on how ambitious regional hegemony schemes are and when they are pursued. The long nineteenth century deviates from this pattern in some respects. Global concentration was high in 1816 and eroded to a more moderate level but one that was still higher on average than had been the case in the past. From the 1840s on, global concentration oscillated, with lows in the 1860s and highs in the 1880s, before declining once again into the World War I period. After World War I, a very brief spurt of reconcentration occurred only to erode to a fairly low position immediately prior to World War II. After World War II, global reconcentration was quite impressive through the 1950s, only to erode considerably after 1960.

Regional concentration was initially low after the Napoleonic Wars and gradually rose through the first two-thirds of the century. After the Franco-Prussian War (1870–71), regional concentration first dipped briefly and then fluctuated mildly until World War I. The fact that there was no discernible buildup of the German army size prior to 1914 that outpaced

other military expansions of the period speaks against a fully premeditated plan for German hegemony.[16] After the conclusion of World War I, as in the global concentration, there is a movement toward reconcentration at the regional level. However, this movement is somewhat misleading since it is organized around a short-lived French lead in armed forces size. That lead also eroded into World War II, a war in which the Germans quickly gained a lead in armed forces size and retained it vis-à-vis other west European states throughout the war.[17] In 1946, the Eurasian region's level of concentration was much lower than the west European level in 1945. But it reconcentrated fairly quickly, although not to the same extent as global reconcentration. After some initial fluctuation, the regional series deconcentrated in the 1950s—thanks to Chinese ascendancy to major power status—before the level of regional concentration began to rise again in the 1960s. Aside from a short increase in the 1980s, regional concentration remained fairly steady from the late 1960s on through the early 1990s.[18]

A systemic measure of democratization is constructed by first calculating the number of democratic major powers, as determined by subtracting the Polity III autocracy scale score from the democracy scale score and using a 6 value as the threshold for democracy.[19] The number of democratic dyads is then calculated (N ($N-1$) divided by 2). This number is then divided by the number of dyads in the major power subsystem each year to obtain an index of the proportion of democratic dyads present. Figure 8.2 plots this series in conjunction with the aggregated militarized dispute data.

Major power democratization has resembled an up-and-down staircase that may yet have some way to go. Democratization trends upward into the interwar period before descending into the World War II era. After World War II, the number of major power was reduced while the proportion of democratic major powers reached a new high (three of four). The Chinese entry into the major power ranks and the Gaullist interlude in France reversed the brief postwar positive gains until the late 1960s. After 1969, the movement has been upward unless Russia is no longer counted as a democracy. While examining figure 8.2, the reader should also note that the U.S.–Soviet Cold War appears to attain the same levels of conflict as experienced in World Wars I and II. This is more a disadvantage of treating all disputes as equivalent than it is an accurate reading of longitudinal conflict tendencies. Still, there is no denying that the post-1945 era was highly conflictual. The problem is thus simply that in some Cold War years there were more militarized disputes than during the years of World Wars I and II. We might choose to offset this by giving "bonus points" to the years 1914–18 and 1939–45 but such an intervention into the data would be awkward, difficult to justify in very precise terms, and, as we show, not really necessary for our current purposes.

Moreover, we control for possible "outlier" effects introduced by World Wars I and II by creating separate dummy variables for each conflict, in which the years 1914–18 and 1939–45, respectively, are coded as 1 while all other years are coded as 0. We also check for the possibility that behavior

after World War II differed from that observed before 1939–45 by contrasting results for two periods: 1816–1992 and 1816–1945. If, for example, global concentration worked to constrain conflict before 1945 but galvanized it after World War II, we should expect to find a switch in the signs of the global concentration coefficients (negative before 1945 and either much weaker or positive for the entire period).

Analysis

We are interested in two theoretical and empirical questions. Is there a systemic and linear relationship between democratization and conflict?[20] If so, is there additional explanatory value added by also considering the impact of global and regional concentration patterns? The first question hinges in part on the pacifying effect of an expansion of the number of democratic dyads. Following Gleditsch and Hegre, it also hinges on the possibly offsetting increase in democratic–autocratic dyads that are presumed to increase conflict propensities. Table 8.2 and figure 8.3 address this possibility.

Table 8.2 records the observed progression of democratic and democratic–autocratic dyads in the major power subsystem. Nothing changed before 1877. After 1877, the change was quite abrupt. While the movement toward a large number of democratic dyads is slow and intermittent, the expansion in democratic–autocratic dyads is quick. Roughly half of the dyadic system is composed of such mixed dyads as early as 1880. That situation more or less prevailed until 1991. We see this transformation as suggesting that ideological conflict among the major powers became more pronounced toward the end of the nineteenth century and remained relatively constant, albeit changing its normative coloration every so often, through most of

Table 8.2 Democratization in the major power subsystem

Year	*Number of major powers*	*Dyads*	*Number of democracies*	*%*	*Democratic dyads*	*%*	*Autocratic–democratic dyads*	*%*
1816–76	5–6	10–15	0	0.000	0	0.000	0	0.000
1877	6	15	1	0.167	0	0.000	5	0.333
1880	6	15	2	0.333	1	0.067	8	0.533
1895	7	21	2	0.286	1	0.048	10	0.476
1899	8	28	3	0.375	3	0.107	15	0.536
1919	7	21	4	0.571	6	0.286	12	0.571
1933	7	21	3	0.429	3	0.143	12	0.571
1940	7	21	2	0.286	1	0.048	10	0.476
1944	6	15	1	0.067	1	0.067	8	0.400
1946	4	6	3	0.750	3	0.500	3	0.500
1950	5	10	3	0.600	3	0.300	6	0.600
1958	5	10	2	0.400	1	0.100	6	0.600
1969	5	10	3	0.600	3	0.300	6	0.600
1991	5	10	4	0.800	6	0.600	4	0.400

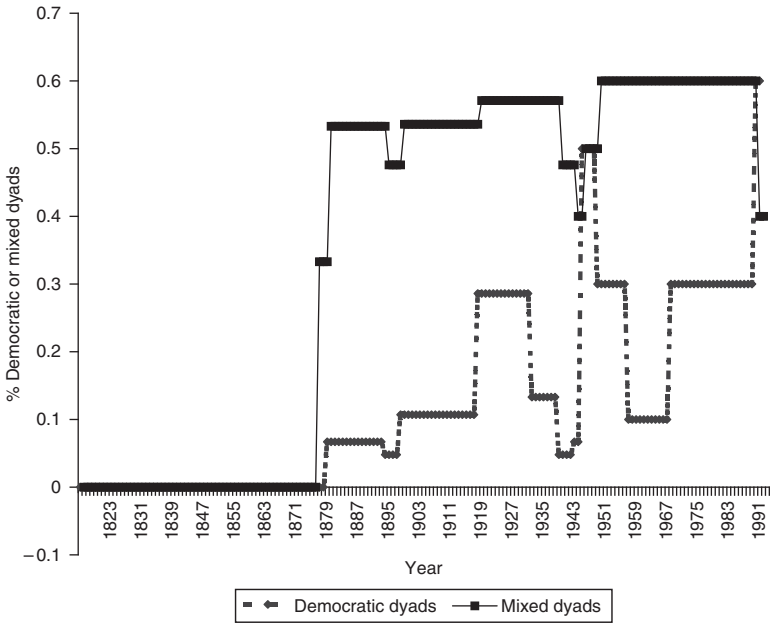

Figure 8.3 Democratization and mixed dyads

the twentieth century. Empirically, then, the major power subsystem did not evolve at the linear pace described in table 8.1's illustration. The number of democratic dyads fluctuated. The number of major powers also fluctuated. The circumstances predicted by Gleditsch and Hegre's astute observation that mixed dyads would expand more quickly than democratic dyads up to a point certainly took place but the expansion in the two types of dyads was not equally protracted. For some 110 years mixed dyads were a relatively invariant quantity in the major power subsystem. Therefore, we should have less reason to anticipate a curvilinear relationship between democratization and conflict. The pacific nature of democratic dyads may indeed be offset by the bellicose nature of autocratic–democratic dyads, but since the latter fluctuated little, the effect of the number of democratic dyads is less likely to be offset in a nonlinear manner.

Table 8.3 addresses both of our principal questions by summarizing the outcomes associated with regressing systemic conflict on democratization, regional concentration, and global concentration.[21] Democratization is a negative and linear predictor of conflict. The coefficient is statistically significant in both the 1816–1945 and 1816–1992 eras. Utilizing a two-tailed test, however, the democratization coefficient is insignificant for the longer period (1816–1992) in comparison to a significant coefficient for the shorter time period (1816–1945). These results suggest the influence of democratization has become stronger over time. Such a finding conforms to earlier findings at other levels of analysis.[22] Moreover, an examination of

Table 8.3 Time-series regression of major power militarized disputes, 1816–1992

Independent variables	*Coefficients*	
	1816–1992	*1816–1945*
Democratization$_t$	−0.99**	−0.54**
	(−3.01)	(−1.65)
Regional concentration$_t$	0.52**	0.62**
	(1.86)	(2.34)
Global concentration$_t$	0.70**	−0.70**
	(4.44)	(−2.09)
World War I	0.68**	0.85**
	(4.71)	(7.00)
World War II	0.22	0.34**
	(1.48)	(2.38)
Trend	0.003**	—
	(3.33)	
AR(1)	0.13*	0.33**
	(1.70)	(3.29)
AR(2)	0.22**	—
	(2.63)	
AR(3)	−0.12*	−0.15*
	(−1.55)	(−1.55)
Constant	−0.42**	0.21
	(−4.43)	(1.53)
Residual Diagnostics		
Adjusted *R*-square	0.38	0.43
Log likelihood	−1.87	29.43
Ljung-Box *Q*-statistic ($df = 36$)	33.58	23.74
Breusch-Godfrey *F*-statistic ($df = 2$)	0.19	0.72
Sample size	174	130

Note: T-statistics are reported below coefficients. Double asterisks indicate significance at 0.05 probability level or lower; single asterisks indicate significance at 0.10 level or lower (One-tailed tests). The dependent variable is normalized by the number of major power dyads in each year.

the bivariate scatterplot for democratization and conflict yields no hint of a curvilinear relationship.

Also confirmed by table 8.3's outcome is our expectation that structural change has some explanatory value. This was hardly a foregone conclusion. Earlier tests of the dissynchronization argument examined 1494–1945 changes on less than a dozen major wars. By switching the focus to militarized disputes in the 1816–1992 period, we have transformed the dependent variable while at the same time extending the interpretation of regional concentration through the post-1945 era. Even so, regional concentration works as expected (and as before), encouraging increased conflict both before and after 1945—even though the strength of this relationship declined in the post-1945 period. Global concentration acted as a constraint on conflict up to 1945. After 1945, it became a target for the Soviet-led

challenge. Thus, in spite of controls for the effects of World Wars I and II, the explanatory value of macrostructural change remains quite respectable—no more or no less so than democratization. At the same time, none of these influences, singularly or in combination, can be said to determine the conflict outcome. The adjusted *R*-squares for the 1816–1992 and 1816–1945 regressions indicate that considerable variance remains to be explained.[23]

Conclusion

We all have well-honed instincts to restrict our analyses to a single level of analysis. If the democratic peace idea works well at the dyadic level, that will suffice for many students of conflict. Why should we try to translate this effect into monadic and systemic interpretations? One answer is that it would be a very peculiar phenomenon if we could not find some traces of its existence at other levels once we find it strongly present at any specific level. In the case of democratization, there are several sub-issues. If there is an effect at the systemic level, does it operate linearly or nonlinearly? There are good reasons to suspect a curvilinear outcome due to the generation of a number of autocratic–democratic dyads. Yet in the major power subsystem, it did not work that way. The creation of autocratic–democratic dyads definitely outpaced the emergence of democratic dyads but it did it in such a way that the mixed dyads rather abruptly became a constant for over a century. If some critical democratization threshold needed to be attained before some negative effect on conflict was realized, there would probably have been no effect manifested. In the major power subsystem, the democracies have been preponderant in terms of numbers only infrequently and only in recent years at that. Nonetheless, the democratization coefficient is reasonably robust in both the 1816–1992 and 1816–1945 equations reported in table 8.3. We must conclude, therefore, that democratization has a systemic and linear effect on conflict. The more democracies in the major power subsystem, the more peaceful is the subsystem. Whether it works the same way in the full international system remains to be seen but given the critical significance of the major power subsystem to total conflict, if for no other reason, the current findings suggest there is some reason to anticipate a similar outcome when all states are examined.

Some of the debate over the effects of democratization has seemed to suggest that altering regime types is capable of eliminating conflict, as if nothing else mattered. But it is a dubious proposition that democratization is so powerful and autonomous a process that it can influence conflict outcomes without assistance from other processes. From a systemic point of view, structural change appears to be one of the influences that matter in addition to democratization. Oneal and Russett found that "hegemonic change," as construed in terms of general capability leads, did not appear to matter. But there are different interpretations of structural change and how systemic leadership manifests itself. One alternative is that it is not simply how much capability advantage a lead state has over its rivals that makes a

difference when challengers assess their chances. Rather, the structural circumstances are more complicated. The destabilizing potential of global deconcentration depends on concentration in the system's primary region. There is nothing mystical about this dissynchronization scenario. Global deconcentration implies that the incumbent system leader is fading and that other global powers are catching up. Concentration in the primary region means that one state is pulling ahead of its continental rivals. Moreover, if we stress coercive capabilities when the level of concentration is assessed, regional military concentration hints rather strongly at the possibility of a bid for regional domination. Thus, the most dangerous macrostructural circumstances since the late 1490s have been declining concentration at the global level and increasing concentration at the primary regional level.

Does this interpretation of structural change still matter in the face of democratization? Or does it also fade into insignificant obsolescence in the context of contemporary changes? The answer is that for the major power subsystem, macrostructural change still matters. It also seems to matter just as much as democratization. This outcome should not really be surprising. Macrostructural change, as we conceive it, and democratization are not inherently in conflict as in a realist versus liberal type contest. Democratization has strongly influenced the basic nature of macrostructural conflict with increasingly democratic global powers taking on systemic challenges by regional level autocracies. In this respect, the two types of explanation are quite compatible and complementary. Combined they do better in explaining nineteenth- and twentieth-century major power conflict than either emphasis does on its own. Yet even in combination, they leave a fair amount of variance unexplained. Further expansion of the explanatory framework is obviously warranted.

One reason for further exploration of the systemic problem is that the twenty-first century is likely to see more macrostructural changes. Global concentration tendencies increased in the aftermath of the U.S.–Soviet Cold War but that condition seems unlikely to persist. Regional concentration in Eurasia appears to be increasing in favor of Chinese relative power. We are not suggesting that a Chinese bid for regional hegemony must come but, on the other hand, there does not seem to be much reason to rule out this future possibility. In the past, what has mattered most is not overt attempts to establish regional hegemony. Rather, it has usually been more critical whether other major powers perceive such an attempt as likely or unfolding. Intense conflict over the perceived implications of regional concentration can be just as deadly as full-fledged bids for domination. One does not have to engage in China-bashing to recognize this possible problem.

Nor can we assume that all major power dyads will become democratic. If all major powers are transformed into democracies, and they remain that way, it is conceivable that global–regional dissynchronization will become less dangerous. But as long as there is one (or more) powerful Eurasian holdout, some possibility of another century of dissynchronization problems

remains plausible. We do not claim to know exactly why democratization seems to pacify interstate dyads. We cannot even say, historically, that regional concentration and democratization never occur in tandem. The French Revolutionary Wars offer an important exception to such a generalization. But we can claim that the world system has evolved into a phase in which autocratic–democratic major power dyads have been central to the most intensive systemic conflict. This phase had a false start in 1792 but emerged with increasing salience in the late nineteenth century. The types of autocracy mutated (old-fashioned aristocracies and monarchies, fascists, and communists) over the years. Inter-autocratic conflicts (e.g., Russia–Germany or Russia–Japan) did not disappear; nor were they insignificant in causing trouble. Yet the coalitional combat of World War I, World War II, and the more recent Cold War revolved, to some extent, around autocratic–democratic, dyadic identities. That does not mean that this ideological phase will continue indefinitely into the future. If we truly have experienced the "end of history," it will continue indefinitely. Unfortunately, though, it does not appear that we are quite out of the woods just yet. Yet recognizing the interaction between macrostructural change and democratization might actually help accelerate the full transformation of the world system and its major power elite vanguard, without suffering the conflictual implications of a less-than-complete transformation.

In chapter nine, we focus on two more systemic issues related to democratization and peace. One is the crucial role of systemic leadership in establishing a context for democratization. Another relates to the question of whether democracies are more likely to ally with other democracies than are autocratic dyads. We think systemic leadership is indispensable in explaining the asserted effects of democratization in a number of ways. We also show, specifically, that systemic leadership appears to be responsible for the apparent alliance propensities of democratic states.

CHAPTER NINE

Democratic Alliance Joining or Changing System Leader Containment Strategies?

Following the reaffirmation of the utility of top-down interpretations demonstrated in chapter eight, this chapter focuses on one of the more neglected explanatory dimensions of the democratic peace—the linkages between it and the variable presence or absence of systemic leadership. Several arguments are pursued. Following arguments put forward by Quincy Wright (1942/65), George Modelski (Modelski and Gardner, 1991, 2002) and others, systemic leadership is advanced as one of the major sources of democratic peace. An economically vibrant, democratic, naval power with insularity, or global system leader, is a necessary factor in expanding the size of the democratic state pool and encouraging the relative absence of intense conflict within that community. While Wright made this argument over a half century ago, it dovetails nicely with more contemporary, leadership long cycle arguments.

The first empirical question examined here is whether the emergence of, and defections from, democratic political systems appear to be correlated with Wright's schedule of periods of general peace punctuated by general wars and the relative presence or absence of a system leader. It turns out that Wright's argument works better in predicting the emergence of liberal states than it does in predicting defections. But how one interprets this finding depends, in turn, on whether all defections are treated as if they are generically alike. Wright's predictive powers are improved when defections due in part to external coercion are distinguished from defections due largely to the pulling and hauling of domestic politics.

The linkage of democratic peace to systemic pax is important in its own right. But some of the arguments associated with this perspective also suggest different ways of looking at one of the auxiliary puzzles linked to analyses of democratic peace phenomena. The "birds of a feather" findings of Randolph Siverson and Juliann Emmons (1991) concerning the affinities of democratic states for democratic alliance partners, relate to one element of Wright's argument, namely that the inherent nature of foreign policy in democratic states ultimately encourages autocratic expansionists

because democracies are reluctant to commit resources to international defensive organizations. Whether Wright was right or wrong on this score, it offers an opportunity to put a different twist on democratic alliance affinities—and one that is clearly linked to systemic leadership patterns.

If one examines democratic alliance formation between 1816 and 1992, the findings of Siverson and Emmons for the 1920–65 period can be replicated but also couched in a longer context. Democratic states have been increasingly likely to show an affinity for allying with other democratic states only up to a point. But, the extent to which democratic states demonstrate an alliance affinity for one another varies by time period and, it is argued, by the nature and preferences of systemic leadership, the ambiguity of perceived external threat, and, the self-reinforcing trend toward democratization itself. It also tends to be highly regionalized behavior. Without a North Atlantic Treaty Organization (NATO), it is dubious that any contemporary affinity would be noticeable.

Quincy Wright's Integration of Regime Type and Long Cycle Considerations

In chapter one, we reviewed Quincy Wright's (1942/65) thoughts on how regime types interacted and how system leaders were critical to the outcome. While neither democracies nor autocracies were thought to be relatively more or less war-prone, democracies were thought to possess institutional handicaps when it came to competing with autocracies in international politics. Democratic decision-making was slow and transparent. Democratic decision-makers tended to be conciliatory welfare-maximizers by profession. The ups and downs of party fortunes and public opinion make it very difficult to devise and execute long-term strategies in democracies. Only after wars begin, do democratic advantages come to the fore. Their advantages in societal cohesion and economic wealth give them better chances of outlasting autocracies in long wars.[1]

Thus, democracies suffered from a number of disadvantages that made it difficult to develop mechanisms to suppress autocratic threats before war began. The nature of democratic domestic politics made it very difficult to play balance of power strategies. It was also difficult to surrender sovereignty to international institutions that might work as multilateral bulwarks against autocratic aggression. In these respects, Wright thought that democracies were essentially "sitting ducks" or "sheep ripe for the plucking" by autocratic wolves. The major offsetting factor, in addition to wartime advantages in cohesion and wealth, was the post-Napoleonic Wars emergence of Britain as a "balancer." As the liberal leader in sea power, industry, and finance, Britain and its Pax Britannica could protect new and old democracies to some extent. It could also act as a model for institutional emulation. Hence, the most peaceful/prosperous eras between general wars that were synonymous with the high water marks of what we now call

systemic leadership were also likely to be most favorable for the incubation and fostering of democratization. Periods of weak leadership and intense conflict, on the other hand, encouraged autocratization and instability.

Wright did not elaborate much on the ways in which systemic peace could be expected to "produce" democracy and there is certainly more to the process than the relatively passive modeling role that he envisioned. But, it is fairly easy to elaborate this point of view from the leadership long cycle perspective. At least nine generalizations on the relationships between the world power as the "center of democratic gravity" can be suggested. In each case, the impact of the structure is accomplished through the agency of the system leader's actions. Wright's work may have predated our current fascination with problems of structure and agency, as well as the development of long cycle theory, but he was very clear to link systemic peace to the presence of Britain—the system leader with which he would have been most familiar from the vantage point of the interwar years. It seems unlikely that he would have objected to most, if any, of these generalizations.

(1) The defeat of autocratic states in a global war opens opportunities for the emergence of new states. The defeated states may be dismembered. Their absence or relative weakness permits the emergence of new political systems in areas where the defeated states previously had exercised dominant influence. All of these new states will not be democratic but some are likely to emulate the victors of the global war, even if only to curry their favor and aid.
(2) System leaders are likely to encourage more liberal regimes to emerge within the states they have defeated in global war. Recently defeated states have few, if any, defenses against external political intervention. To the extent that global wars can be viewed as autocratic–democratic ideological struggle, the victorious system leader will have strong incentives to reshape the political systems of its defeated foes. System leaders also generally prefer states to have decentralized political regimes that favor abstaining from interfering with open trade relations.[2]
(3) Newly victorious system leaders press for greater opening of the world economy by encouraging the reduction of protectionist barriers. These barriers have included colonial ties to a metropole. Formerly dependent territory and closed markets are thus more likely to emerge as independent states.[3]
(4) Not coincidentally, periods of vigorous systemic leadership tend also to be periods of relative prosperity. As a consequence, economic conditions tend to be conducive to the emergence and survival of democratic regimes.[4]
(5) The very preeminence of system leaders when they are strongest also increases the probability that their political systems will be emulated by decision-makers in emerging political systems. In some cases, the inference will be made that political–economic success is equated

with the attributes, including a democratic political system, possessed by the system leader. In other cases, it may be assumed that a newly independent state will be treated better by the major powers if there is greater similarity in political systems (see generalization one).

(6) System leaders may encourage liberal, democratic regimes in client states. There is no reason to assume that they will do so consistently or effectively, but only that one can anticipate at least intermittent pressure for greater democratization from system leaders.

(7) System leaders have become increasingly likely to organize their spheres of influence against outside threats of expansion. The preceding generalization suggests that these spheres of influence are most likely to be subject to the at least intermittent liberalizing pressures of system leaders. Trade and aid are likely to be manipulated toward this end. At the same time, the states within the system leader's sphere of influence that possess liberal political systems are the ones most likely to cooperate with organizational attempts to build defenses against the expansion of autocratic enemies located in and outside the sphere. They have more to lose and may also be more likely to be selected as a target for expansion.

(8) The presence, the status quo preference for stability, and the explicit containment policies of system leaders may deter or at least impose constraints on the territorial expansion of autocratic powers.

(9) In the last resort, system leaders organize specific balancing coalitions against expansionist, autocratic states. Other liberal regimes generally are more likely to join these coalitions because their political and economic ways of life are most threatened by the prospects of successful autocratic expansion. Liberal states that were not given timely protection may be liberated at a later point because the liberal side, while slow to organize, tends to enjoy geopolitical and resource advantages over the autocratic coalition.

It may seem as if these generalizations encompass several different arguments about system leader behavior. In fact, though, all lead to the expectation that system leaders are increasingly likely, although not always in a consistent fashion because other priorities may intervene, to favor, or help bring about, the spread and defense of liberal political systems. The nine generalizations focus on different time periods in the life cycle of system leadership. Generalizations 1 through 3 are fixed on the period immediately following a global war. The next five generalizations concern probable behavior at the peak of a system leader's influence—at some point between global wars. The last generalization returns to a focus on global war circumstances and, in the latest iteration, to the postwar NATO phenomenon.

It should also be emphasized that these generalizations refer to behavioral tendencies that are only tendencies and not strict rules. Examples of behavior that violate many of the generalizations abound in the annals of diplomatic history. The question is whether the deviations from the rules

are more frequent than the number of times system leader behavior has adhered to the discussed propensities. It is argued here, without opportunity for demonstration, that the latter on balance are more frequent than the former. And just as democratic regimes have become more numerous and more democratic over time, we should also expect the nine generalizations to find more empirical support in more recent times than in earlier decades of say the past 200 years. Yet, at the same time, the influences on democratization are very likely intermingled with a large number of other factors, operating at various levels. Accordingly, our main hypothesis on this facet of systemic effects on democratization is relatively modest:

H26: *Global war and systemic leadership are related to the pace of democratization*

Wright saw democracies and autocracies as equally war-prone if considered as monads, despite the attribution of different strategic advantages and liabilities. From the dyadic perspective, autocracies were thought to be more likely to attack democracies (than democracies were to attack autocracies) if the circumstances were facilitative. Systemically, the circumstances are least facilitative for autocratic attacks when the system is led by an economically strong, democratic, naval power. For a variety of reasons spelled out above, these same periods of strong systemic leadership are optimal for the expansion of the number of democratic states. Periods of systemic leadership decline and general war are most facilitative for autocratic expansion in both the military and institutional senses.

One empirical question, then, is whether the evidence supports this argument that democratization is linked to macrostructural change and, specifically, to the strength of systemic leadership and the intensity of systemic conflict. A second question that is explicit in Wright's interpretation relates to his claim that democracies are unable to organize prior to an actual autocratic attack. Therefore, they do not tend to be in positions to utilize balance of power politics or international organizations as a defense mechanism against the possibility of an attack. Writing in 1942, it is not difficult to see how Wright might have arrived at this conclusion. Writing in the early part of the twenty-first century, we suspect that his generalization on this disadvantage of democracies is dated. It probably made sense prior to 1945. After 1945, democracies changed some of their earlier behavior and created a defensive alliance mechanism, the NATO, with considerable staying power. NATO does not represent a classical balance of power mechanism but it is an international institution that involves some surrender of sovereignty on the part of its members. It was also developed, at least in part, to preclude Soviet expansion into western Europe. The second question, therefore, is whether democracies have exhibited consistent propensities in their ability to organize defensively. We will return to this question in a second section of this chapter and link it to the contemporary question of whether democracies have a propensity to ally with other democracies. Briefly, though, our position is that Wright's pessimism about democratic abilities to organize defensively is understandable from a 1942

perspective. After 1945, though, democracies demonstrated little problem in organizing against the Cold War threat. In doing so, we think that democratic propensities to ally depend more on variable systemic leadership strategies than on inherent characteristics of regime type.

H27: *Systemic leadership is a major influence on democratic states' alliance formation propensities.*

Was Wright Right?

To assess the degree to which Wright's interpretation, which overlaps considerably in this instance with leadership long cycle arguments about democratization, contributes to our understanding of democratic peace phenomena, we need information on who attacks whom and when. We also need to have a sense for the timing of expansions of, and defections from, the liberal, democratic camp. Does the timing jibe with expectations linked to the presence/absence of general wars and systemic leadership?

Taking Wright's argument very literally, one of the key elements is wrong. Whatever the circumstances, autocratic states simply do not attack democratic states in the very specific sense of going to war against them all that often. We looked at an aspect of this question earlier in chapter five. The rarest dyadic pairing of a war initiator and war target in the Small and Singer interstate war inventory, of course, is a democratic state attacking another democratic state ($N = 0$). The next rarest pairing is an autocratic state attacking a democratic state. Only four instances (Russia versus Poland in 1920, China versus India in 1962, Egypt versus Israel in 1973, Argentina versus Britain in 1982) are recorded in the period between 1816 and 1997. In contrast, democracies attacked autocracies 20 times in the same period. Granted, it is always easy to dispute which side "really" initiated a war and it is certainly possible to quarrel about the circumstances in some of these cases. But the point remains that of 76 cases in which it was deemed possible to designate an initiator, only 4, slightly more than 5 percent, qualified as an autocratic initiation against a democratic target.[5]

Autocracies attacked autocracies in 68 percent of the 1816–1997 cases ($N = 52$). In view of the disproportionate number of autocratic states in the system, two-thirds of war initiations in the autocrat versus autocrat column may seem a bit high, but before 1945, there really were not that many democracies to attack. In some respects, though, this generalization is linked to the way in which war initiations are coded. World War I began with Austria-Hungary attacking Serbia. World War II began with Germany attacking Poland. In both cases, an autocratic state initiated war against another autocratic state. The subsequent coalition warfare, pitting largely democratic states against autocratic states, is ignored entirely by the dyadic emphasis.

It is clear that Wright was interested mainly in world or general wars as the appropriate context for his arguments about political system types and the probability of war. He actually summarized much of his argument by

referring to systemic phases of general peace with tendencies toward democratization punctuated by phases of general war and movement away from democracy. Restricting his list of general wars to the nineteenth and twentieth centuries would provide the following sequence: the Napoleonic Wars (1792–1815), the Crimean War (1853–56), World War I (1914–18), and World War II (1939–45). One approach then might be to compare the years of general war with those of general peace (years of non-general peace). The problem with such an approach is threefold. One, there are very few years of war and many years of general peace. Two, general wars are preceded by periods of increasing tension that can have destabilizing effects on domestic political systems, as was well demonstrated in eastern Europe in the interwar years. At the same time, the outcome of general wars opens opportunities for some defeated autocracies to be transformed into democracies. But does that mean we need to extend the years allotted to the general war column and to what extent? How should we handle the double-edged effect of general wars—that is, their effect in weakening established democracies while also later facilitating the emergence of new democracies? A third problem is that Wright's argument is not simply about general wars. It is also about systemic leadership that is exhibited during general wars (e.g., in terms of building war coalitions) and that is likely to be maximal during only some years of peace time. Unfortunately, the presence or absence of systemic leadership can constitute a tricky measurement problem if we are required to demonstrate actual exhibitions of leadership.

If instead, we fall back on critical attributes, Wright talked about the leader's need for a strong economy, a powerful navy, and a democratic political system. Britain is coded as democratic after 1880. It retained naval leadership from 1816 through 1913, subject to variable challenges from the French and the Germans (Modelski and Thompson, 1988). Britain's leadership in technological innovation had peaked around the middle of the nineteenth century and was in relative economic decline after the 1870s. Combining these factors suggests that the heyday of Britain's democratic leadership should have been roughly the period between the 1830s and early 1870s, except that Britain is not considered to have been sufficiently democratic during that same time period.[6]

The comparable period for U.S. systemic leadership is some portion of the post-1946 period. The United States has retained its democratic status and its naval leadership up to the present. The question mark concerns its economic leadership. The lead in technological innovation was retained at least through 1973. After 1973, U.S. relative economic decline accelerated as its leadership was increasingly contested by other contenders in western Europe and East Asia. By the mid-1990s, U.S. economic leadership seemed on the upswing again, riding as it was the increasing emphasis on information technology.[7]

The bottom measurement line seems to be that the most appropriate circumstances for testing Wright's argument are a bit murky. Systemic leadership should be strongest in the 1830s–70s (but less relevant if Britain must

also qualify as democratic) and late 1940s–early 1970s, and perhaps again in the 1990s. Yet no one is claiming that systemic leadership determines the timing of democratization. That means that, ideally, we would also be able to control for other types of factors that influence the probability of democracies coming and going. All of these caveats suggests that our inspection of the evidence should be highly provisional. Does the evidence suggest some room for a partial systemic leadership explanation?

Figure 9.1 provides an overview of the historical pace of democratization. The figure focuses on the proportion of states in the system that qualify as democratic. The growth in both democracies and the number of states in the system was more modest throughout the nineteenth century than figure 9.1 suggests. For instance, numerical membership in the system was roughly constant between 1850 and 1890 although the number of democracies more than doubled (5 to 12). World War I brought about some significant changes. The number of states in the system increased from 46 in 1910 to 61 in 1920, and the number of democracies almost doubled again (15 to 27). During the interwar years, the number of states remained about the same but the number of democracies was reduced substantially (back to 14 in 1940). World War II was then partially responsible for adding another 15 members to the state system. Yet the number of democracies again doubled (14 in 1940 to 29 in 1950). After World War II, the number of states continued to increase (109 in 1960, 146 in 1970, 168 in 1980). The real explosion came as the Cold War ended in the late 1980s. By 2000, as many as 203 states were counted as system members.[8] This recent growth in the state system initially outpaced the number of democracies, with not too much net change in the absolute number of democracies between 1960 and 1980 (an increase of 5). Yet between 1990 and 2000, the number of

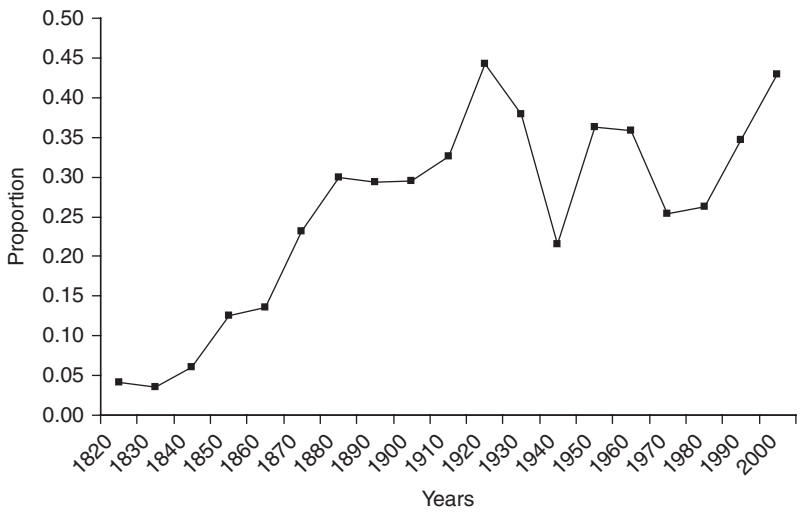

Figure 9.1 Proportion of democratic states

democratic states increased by almost 50 percent (60 to 87)—one democracy short of doubling the number of democracies in 1980 (44).

Thus, the pace of democratization has not been one of steady accretion. It started very slowly, picking up discernible momentum after World War I. In various ways, the democratization of Denmark (1915), Finland (1917), Sweden (1917), the Netherlands (1917), Czechoslovakia (1918), Estonia (1917, 1920), Poland (1918), Germany (1919), Austria (1920), Latvia (1920), and Ireland (1922) owed something to World War I. The pace then reversed itself in the interwar/World War II years. The outcome of World War II was again clearly beneficial for democratization efforts, just as the immediate postwar years were conducive to a doubling of the democratic population. The states liberated from German occupation were able to resume their democratic practices (Finland [1944], Belgium [1945], Czechoslovakia [1945], Denmark [1945], Luxembourg [1945], Netherlands [1945], Norway [1945], and France [1946]). Austria (1946), Italy (1948), Germany (1949), and Japan (1952) joined or rejoined the democratic camp as a consequence of World War II. Several other cases like Brazil (1946), Turkey (1946), Sri Lanka (1948), Israel (1949), India (1950) were at least indirectly related to the war outcome. The end of the Cold War was also beneficial to the democratic count, although certainly not all of the new democracies after 1990 can be attributed to this sea change in global geopolitics. However, Bulgaria (1990), Hungary (1990), Armenia (1991), Estonia (1991), Latvia (1991), Lithuania (1991), Russia (1991), Ukraine (1991), Czechoslovakia (1993) and its subsequent split into two countries, Mongolia (1993) at a minimum can be credited directly to the end of the Cold War account.

War, especially global war and the nature of its outcome, then, clearly has made some difference to democratization. This generalization is also supported by looking at the decadal rate of joining and defecting from the democratic camp.[9] After 1909, there were at least eight new democracies per decade, with the exception of the 1930s.[10] The 1990s, although data only for the first half of the decade are available, leads the list with twice as many new democracies as any other decade. The 1910s and 1940s were also active decades, although not particularly more active than the 1950s and 1960s. The defections have been fewer in number but tend to cluster in the 1930s–40s and 1960s–70s. Thus, it should be clear that there are other factors at work besides global shocks and cushions. Still, the external environment looms large as a factor in the history of democratization.

Yet there is an important difference between the pre- and post-1945 entry/defection behavior. In the one period on which most, if not all, observers would be sure to characterize as lacking systemic leadership, the years between World Wars I and II, most of the defections were due to a mixture of internal political pressures, externally imposed economic distress, and external coercion within regions associated with increasing levels of interstate tension. The defections of Austria, Czechoslovakia, Belgium, the Netherlands, Denmark, Norway, and France might well have been

avoided if these states could have counted on more effective protection from a strong system leader. The defections after 1945 are more strictly the outcome of internal political combat in states in which the political–economic environment for sustaining liberal political systems has been something less than ideal.

Thus, a number of the newly democratized states after 1945 have cycled back and forth between more and less liberal–democratic constitutions. Some of this cycling might have been discouraged more effectively by the incumbent system leader but it is difficult to attribute it to a lack of protection from an expansionist, external opponent. Moreover, as the number of democratic states has expanded dramatically, the probability of a number of states cycling back and forth between democracy and autocracy has also expanded. To the extent that this cycling behavior is driven primarily by local considerations, systemic leadership appears to be less involved. Although, when it suits a system leader's purposes, a high tolerance for autocratic clients has also been exhibited. Thus, the lack of concern on the part of a system leader—and even its occasional strong encouragement of autocracy—paradoxically can be a factor inhibiting democratization. In this respect, the geopolitical strategies of system leaders are the Jekyll and Hyde of political liberalization. Democratization probably could not have been sustained in western Europe without the effective protection of system leaders, but that does not mean that system leaders always encourage democratization everywhere and at all times.

In general, a combination of war, peace, and systemic leadership considerations—in conjunction with domestic structures and processes—is useful in accounting for adherence to, and defections from, the democratic camp. By no means can these systemic variables explain all that we might like to know about the ups and downs of democratization—much of which take place within individual political systems. However, the point remains that the subsystemic pulling and hauling among domestic contenders for power does not take place within a systemic vacuum. External factors can facilitate or constrain the likelihood of democratic victories and defeats. Periods of systemic leadership and peace, in particular, facilitate subsystemic democratic victories just as it discourages some types of defeats. In this respect, Wright's emphasis on the significance of systemic leaders was entirely justified, as long as we do not treat it as a monovariate explanation of democratization. It is clearly one of several factors that appear to have played and continue to play important roles in the expansion of the number of democratic political systems.[11]

The Birds of a Democratic Feather Problem

Wright's interpretation also stressed that some of the constraints on democratic states' foreign policy behavior contributed to the probability of autocratic expansion. One of the things democratic states did poorly was to take preventive action by organizing against the possibility of expansionist

threats. Presumably, he had the League of Nations uppermost in his mind (when he initially was writing in the run-up to World War II) but the observation could also apply to alliance formation.

A blocking coalition has always been a critical element of balance of power models. An expansionist threat should be countered by a coalition of threatened states, thereby recreating the initially disrupted balance. Of course, the problem is that the balancing coalition often came too late to prevent an intense war between the defenders and attackers of the status quo. Wright's point would have been that the slowness of the coalition to form is due partially to certain attributes of the defenders' political systems. The threat has to become very acute before democracies are likely to mobilize. In the absence of a standing blocking coalition, autocratic expansionists are thought to be encouraged by the relative weakness of their opposition.

These considerations have some bearing on the "birds of a feather" puzzle encountered by Siverson and Emmons (1991). Examining alliance behavior between 1920–39 and 1946–65, they found that alliances between democratic states were about half as likely as expected in the 1920–39 interval but nearly twice as likely as one might have reason to expect in the 1946–65 period. They also found that alliances between democratic states were more likely to form early in the periods as opposed to later. However, the propensity to ally eroded in the first period but did not erode in the second period.[12]

The question is why do democratic alliances behave in these ways? Siverson and Emmons suggested and tested several possible explanations. Their initial assumption that democratic states have much in common and that there is relatively little distance between their foreign policy preferences may help to account for the basic democratic alliance affinity. If true, as they point out, it would also help explain why democratic dyads are less likely to become involved in militarized disputes, dispute escalation, or wars with one another. They have less to fight about and when they do come into conflict, it is much easier to negotiate the narrow gaps between their respective positions than it might be for other types of dyads.

The basic affinity is thus explained by assumption rather than the testing of rival hypotheses. The distance between foreign policy positions is captured by alliance choices. If there is little distance between democratic states, they should be more likely to ally with one another than with nondemocratic states. But the auxiliary characteristics of early formation and variable proneness tendencies as well as variable persistence of "dem–dem" alliances were subjected to analysis. The authors managed to partially rule out the possible influences of geographical proximity, U.S. hegemony, the legacy of wartime coalitions, and the alliance-proneness of democratic major powers. Some of these factors might have limited explanatory power but none were sufficient to account for the twentieth-century alliance choice findings.

One hypothesis was that the increasing alliance-proneness of democratic states was simply capturing the tendency for democratic states to be located

close to one another and might, therefore, be more likely to ally. However, geographical proximity proved to be statistically insignificant in analyzing proneness patterns. U.S. hegemony might have some relationship to post-1945 alliance behavior but Siverson and Emmons could not see how it could explain 1920–39 behavior. Alternatively, it was found that members of winning wartime coalitions were unlikely to be allies five years into the postwar era so that democratic alliance affinities could not in general be traced to the alliance lineup in World Wars I and II.

A fourth hypothesis pertained to the possibility that the findings could be traceable to the alliance-proneness of a few major powers that happened to be democratic. It turned out instead that democratic major powers were not unusually prone to ally with each other. Nor were democratic minor powers all that likely to ally with other democratic minor powers. However, democratic major powers did show a marked affinity in their alliance choices for democratic minor powers (or vice versa).

Yet knowing that democratic alliance affinities possess an asymmetrical characteristic does not explain why the affinity persisted in the second period and decayed in the first period. Siverson and Emmons introduced a fifth hypothesis that focused on external threat and leadership. After 1945, democratic states were exposed to a persistent threat from an autocratic state, the Soviet Union, and were organized by a leader, the United States, prepared to overlook free ride and essentially overpay the costs of alliance.[13] In the 1920–39 period, the autocratic threat was present but it was not met by a leader willing to overpay. Britain had declined the honor.

Wright, Siverson and Emmons, in different ways, have drawn attention to an interesting phenomenon.[14] Wright argued that democratic states were disadvantaged in organizing against autocratic threats. Although it is not discussed in Siverson and Emmons, it is worth noting that much of the post–World War II alliance interaction between democratic states is traceable to the NATO. NATO was organized under U.S. leadership and has a membership that is predominately minor powers. Knowing this, we can read the findings of Siverson and Emmons as suggesting that Wright may have been right before World War II but that something changed after 1945. For Siverson and Emmons, the something was the presence of systemic leadership (even if they did not conceptualize it precisely in these terms).

The Siverson–Emmons answer to the "birds of a feather" puzzle is certainly plausible. Systemic leadership clearly was more evident in the post-1945 era than in the interwar period. But what about before World War I? Britain did demonstrate some leadership in safeguarding democracies against autocratic expansion, as Quincy Wright argued, in the nineteenth century. For instance, the chances for Belgian and Swiss survival and democratization certainly were enhanced by British protection. This would suggest an on–off–on pattern for leadership that would require an expansion of the analysis to three time periods as opposed to two time periods of Siverson and Emmons.

Siverson and Emmons contemplated examining pre-1920 alliance affinities but chose not to do so for two reasons. First, the number of democracies was small and alliances between democratic states were infrequent. Second, voting rights were much expanded after 1920. Widespread popular participation in the political decision-making of democracies, therefore, was more likely after 1920 than before. Yet while the accuracy of these observations is undeniable, it is possible to quarrel with the design logic implications. The small number of democracies and democratic alliances is not a hindrance. As long as there was some alliance activity, we can calculate observed and expected choice propensities. The suffrage observation, on the other hand, appears to introduce an additional threshold attribute for attaining democratic status and explicitly invokes what they refer to as "Kant's 'consent of the citizens'." But the logic of their initial argument focused on distances between foreign policy preferences and not the constraints of mass participation or the Kantian argument about republican populations rationally avoiding the economic costs of war. In sum, their reasoning does not appear to be a sufficient obstacle to constructing an extension of their analysis backwards in time.

But is there sufficient reason to extend the analysis back in time? While Siverson and Emmons demonstrate that Wright was probably wrong on this question of democratic organizational reluctance, their findings do not necessarily rule out other interpretations. An emphasis on leadership per se would be supported by pre-1914 behavior that more resembled post-1945 behavior than it did interwar behavior. But we also know that alliance behavior, especially on the part of democratic states and Britain, was not all that common in the nineteenth century.[15] Something else may be at work than leadership alone.

For instance, leadership may have to be combined with unambiguous threat in order to create functioning defensive organizations. Leadership was relatively absent in the interwar period but major power decision-makers also disagreed about the seriousness of the threats posed by Germany, Japan, and Italy. Alternatively, the findings of Siverson and Emmons may reflect some type of learning process. Something like the Munich lesson may be responsible for the greater "dem–dem" alliance behavior of the post-1945 period. Then again, states have become more democratic and more states have become democratic in the second half of the twentieth century as compared to the first half. Greater democratic affinity may be a function of the quantity and quality of the pool of democratic states.

Nor are we limited to going only backwards in time. Siverson and Emmons stopped their analysis in 1965 due to a lack of data. We can now extend the alliance series another 27 years beyond their stopping point. This extension of the series should be most useful in two respects. One, part of the Siverson and Emmons puzzle is that intra-democratic alliance-making eroded in the interwar years but not in the post–World War II years. Yet this difference in behavior may be due not so much to learning but, more simply, to too short of a series. If we find erosion in the intra-democratic

alliance proclivity in the 1946–95 period, the post-1945 era will appear to be less distinctive. A second incentive for examining the post-1965 era is that we know a large number of new democracies appeared. If there is any democratic proclivity to ally with other democracies as a reflection of common interests predicated on domestic regime type, we should expect a corresponding expansion of democratic alliance commitments. If this is not forthcoming, we need to look elsewhere for explanations of democratic alliance-making.

In any event, examination of longer time period should be helpful in picking and choosing among the several alternatives—not all of which are likely to be mutually exclusive. A leadership alone emphasis should require, if other things are equal, that democratic alliance affinities fluctuate up–down–up across three consecutive periods with leadership present in the first and third period. A leadership–unambiguous threat interpretation would predict that the earlier period should more closely resemble the second than the third period. Such a prediction, of course, assumes that it is reasonable to characterize the nature of pre-1914 autocratic threat as ambiguous.

Evidence in support of some sort of a learning curve should probably take the form of a trend of increasing affinity across the three time periods. A positive trend alone would not help us to differentiate between the Munich/League of Nations lesson versus the expansion of the democratic state subsystem type of learning. However, a short "trend," encompassing the second and third periods only, might point us toward the Munich syndrome interpretation—barring some coincidental threshold phenomena for the quantity–quality interpretation.

But interpreting trends across aggregated periods can be misleading, especially if we also know that alliances between democratic states tend to be formed early in the time period and then either persist (after 1945) or decay (pre-1945). Neither pattern fits nicely with the learning–evolutionary imagery that would presumably anticipate positive trends within, as well as across, the time periods. A longer time period would provide more opportunity to observe whether democratic alliance behavior is concentrated consistently at the beginning of the time period, or whether the timing may be idiosyncratic to the periods preceding and following World War II.

Figure 9.2 demonstrates alliance affinities on a year by year basis by first calculating an expected number of allied democratic dyads based on the proportional number of democratic dyads. The expected number for our immediate purposes can be seen as equal to the proportional number as in Siverson and Emmons's study. By plotting the observed number of allied democratic dyads against the expected number, we can visually see when and whether the observed number corresponds to the expected number.

Figure 9.2 is actually composed of four separate figures. The two subfigures on the left-hand side (A and C) compare expected and observed numbers for all alliances (upper left-hand corner) and defense pacts only (lower left-hand corner). The figures on the right-hand side (B and D) use the

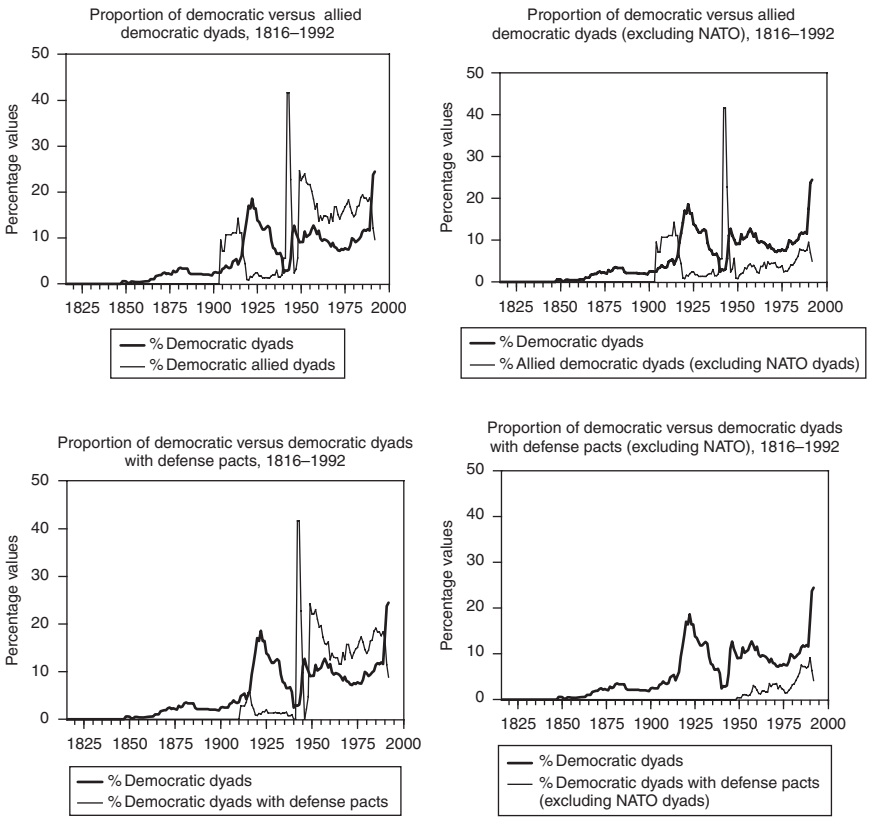

Figure 9.2 Observed versus expected numbers of alliance

same information as those on the left-hand side but have had the NATO linkages removed. The reasons for displaying four separate figures is to: (1) demonstrate the outcome's strong dependence on one alliance (NATO) and (2) to ensure that our findings are also not dependent on whether we focus on all types of alliance (ententes, neutrality pacts, and defense pacts) or only the more significant commitments associated with defense pacts.

Looking first at sub-figure A, we find a greater-than-expected upsurge in observed democratic alliances immediately prior to World War I. Democratic alliance-making is noticeably absent in the interwar era. During (the strong spike in democratic alliances) and after World War II, the observed democratic alliance-making again exceeds the expected until the very end of the series in the early 1990s when the positions of observed and expected series change position. After the end of the Cold War, democratic alliance-making fell below what might be expected, primarily because the number of new states expanded faster than democratic alliance-making. While some additional states adhered to NATO after 1992 (and more may be expected to join), it appears unlikely that more data on

post-1992 alliance-making would alter the observation that the expansion of the number of democratic states is currently growing faster than are alliances among them.

Using the three other figures to check this interpretation, sub-figure C suggests that the pre–World War I upsurge in democratic alliance-making (but not the post–World War II behavior) was dependent on alliances that fell short of the commitments found in defense pacts. Sub-figures B and D, however, suggest that if we were to ignore NATO activity, there is far less evidence of a democratic state affinity for making alliances with other democracies. We conclude, therefore, that any appearance of a democratic dyad alliance affinity is largely based on NATO connections and the change in systemic leadership behavior. Only after World War II is there a concerted and successful effort to organize a standing and very explicit coalition of states to resist the expansion of the principal threat to the Western postwar order.[16] This behavior was not anticipated by Wright writing in 1942 and marks something of a change in system leader grand strategy. The historical norm has been to wait until the source of the hegemonic threat in Europe was preparing to strike before attempting to organize a blocking coalition. Of course, western decision-makers thought the Soviet threat was greater in the 1940s and 1950s than, again with the advantage of hindsight, it probably was—hence the mutation of grand strategy is not quite as great as it might otherwise seem. Still, the bottom line is that the support for democratic dyadic alliance affinity appears to be due almost exclusively to U.S.–European Cold War balancing behavior.

Raymond Cohen (1994) contends that the geographical circumscription of the "pacific union of democratic states" (the North American–European concentration) suggests that notions of democratic peace are neither universal in scope nor necessarily accurate in attributing consequent behavior to subsystemic regime types or norms. His alternative explanation is that a cultural preference for peaceful dispute resolution may have emerged in the North Atlantic area prior to the development of much democratization. This idea is a rival hypothesis to the most popular explanations for democratic peace that is difficult to test. Yet while the alliance data corroborate the regional circumscription thesis, the exercise of regional and systemic leadership that led to the formation of NATO is too recent to attribute in large part to an older (pre-democratization) regional culture.

Instead, the geographical limitations on democratic alliance formation inherent to NATO underscore the significance of system leadership, which, in turn, has always been geographically restricted. To give credit to a constant regional culture also misses the tendency for there to be variable admissions to, and defections from, the liberal camp that are correlated with (but not determined by) the strength of systemic leadership. If systemic leadership is crucial, the larger question of the universal potential of the democratic peace could hinge on the expansion of those communities currently organized around the incumbent system leader. NATO is expanding into eastern Europe in a significant way.[17] The point remains that these

expansions are not spontaneous nor are they random. They are orchestrated by actors in attempts to resolve short-term security problems. System leaders may indeed attempt to suppress conflict within their own backyards but they could also end up doing more by promoting democratization directly and indirectly. In the process, the NATO security community may well evolve into something else in the long term. Or, NATO may simply become obsolete as European decision-makers increasingly chafe at U.S. political–military overlordship established in the late 1940s. It is too soon to tell which future outcome is more likely.

Do these developments mean that Wright was wrong about the liabilities of democracies in organizing to prevent autocratic expansion? They probably do signify that his arguments on this question are at least dated. One of the staples of the early Cold War years was that dictatorships had a decided advantage over democracies in being able to develop a strategic plan and sticking to it. Democracies were forced by the nature of their domestic political systems, at best, to react to their opponent's initiatives. Whether or not regime type confers advantages and liabilities in foreign policy, it is not clear that autocracies necessarily do develop strategic plans and adhere to them. Nor is it clear that autocracies are any more or less reactive in practice to changing circumstances than are democracies. But, most important, the Cold War demonstrated that it is possible for democracies to organize in security communities to protect themselves against autocratic threat without waiting until the last minute before war breaks out.

But this violation of Wright's generalization presumably required certain facilitative factors. Unlike the end of World War I, the end of World War II was characterized by a sharp cleavage among the victors and by a wealthy and militarily strong system leader prepared (after some initial faltering) to play an active role in world politics. From a democratic perspective, the source of the greatest external threat was clear very early. Given these major differences, it is rather difficult to assess the independent role of institutional factors. A better test may have to await the emergence of an entirely new source of threat that would more closely resemble the situations prior to the twentieth century's two world wars. New sources of threat tend to be ambiguous or at least it usually is possible to disagree about the ultimate extent of the threat, or what to do about it—as in the case of terrorism. Differences in threat perception discourage coordinated action and tend to accentuate differences in preferred strategies and interests. A new, twenty-first century, source of threat might also arise in an environment in which the incumbent system leader was no longer the world's predominant economic and military power. Then we might be better able to see which history repeats itself, the pre–Cold War history emphasized by Wright or the Cold War experience that seems to have introduced new behaviors into the foreign policy repertoires of democracies.

A new test might also allow us to assess the extent to which the novel development of liberal/democratic security communities has genuinely transformed the nature of world politics. And, of course, the best test of

liberal thought on this question would be the nonappearance of new autocratic security threats on the scale of a Nazi Germany or the Soviet Union. It will take more time to fully assess the accuracy of Wright's observations. Nevertheless, a strong case can continue to be made for the critical nature of system leadership in contributing to the world's progressive liberalization—and in explaining the appearance of democratic alliance affinities.

Is system leadership both necessary and sufficient to the spread of liberal, democratic regimes? Wright seemed to suggest that this was the case when he observed that systemic peace preceded democracy and not the other way around. But there is no need to be that intolerant of other interpretations and factors. Democratization and liberalization are multifaceted processes. They are more likely to thrive and expand when environmental factors are conducive to their growth, just as they are apt to contract when factors are less conducive. System leadership is "merely" one of the more important conducive factors. It may even be a necessary factor. But it is certainly not the only conducive factor—as testified to by the arguments and evidence of the preceding eight chapters—and, therefore, cannot be regarded as a sufficient factor in the cultivation of something called the "democratic peace."[18]

We have now struggled through a number of issues pertaining to democratic peace phenomena. Chapter one put forward a modified "trading state" theory as an alternative explanation for the "democratic peace." Chapters two and three explored some of the issues associated with reversing the causal arrow between democracy and peace. Chapters four, five, and six wrestled with questions of relative explanatory weight. Just how much does information on democratic constraints contribute to our attempts to explain interstate conflict in contrast to other factors that work to override said constraints? Chapters seven, eight, and nine focused on several questions related to the utility of systemic perspectives. The basic issue is not one of putting all one's explanatory chips in the domestic or external realms. Rather, how can we best harness interpretations of such topics as systemic structure and systemic leadership to help explain domestic processes. It turns out that it need not be a zero-sum game. Along the way, 24 hypotheses have been examined and tested. It is now time to return to bigger picture questions and contemplate how what we have learned sheds lights on puzzles of the democratic peace. That is the mission of chapter ten.

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PART FIVE

Conclusion

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CHAPTER TEN

History and Selective Transformations of World Politics

In chapter one, we advanced a theory focused on the alternative attractions of war (conflict) and trade (cooperation). The basic argument is that, at least for some actors, war/conflict is becoming (or has become) a less attractive strategy and trade/cooperation is becoming (or has become) a more attractive strategy. Why that should be the case is not easily traced to any single variable, let alone regime type. Democratization is part of the story—but only a part.

One important facet of these phenomena is the indisputable contention that war has grown into a very costly enterprise. Only a small number of states can contemplate playing in the military major leagues. An even smaller number have actually taken the next step and mobilized the resources to pay for major league military establishments. Over the past several hundred years, though, most of these contenders have been eliminated as major players. We are now down to only a handful that are capable of engaging in major power warfare. That process of elimination has not yet extinguished the possibility of major power warfare, but it has lowered its probability immensely—at least for the immediate future.

Warfare certainly persists but it tends to be a practice engaged in primarily by relatively weak states. As a consequence, the effects can be devastating for the participants but the geographical scope of these affairs tend to be restricted. The casualties depend to some extent on the durations that are often quite short. They also depend on the weaponry employed that tend not to involve the higher order weapons of mass destruction. Only those affairs involving highly asymmetrical pairings of major powers against minor powers (e.g., the Persian Gulf War, Kosovo, and the latest round of fighting in Afghanistan) bring into play massive aerial bombing and missile attacks that have the potential for escalating the costs of war very quickly.

Yet the escalating costs of warfare are only part of the evolutionary processes at work. Economic development has intensified—again selectively. The number of states engaged in long-distance trade has expanded impressively over the last 500 years. What was once a distinctive specialization

of a few maritime states has become a routine strategy for enhancing wealth. Moreover, a number of states have become industrialized. As their economies have become more complex, trade has become more attractive. For a number of wealthy states, the international trade sector has become an important part of the national economy. The transportation costs of trade have also declined thereby encouraging an even greater expansion of worldwide trade. For less wealthy states, trade may represent the only viable way to bring in external resources. Attempts to remain aloof from the world economy have failed to deliver competitive economic development. The alternative—participation in the world economy—will not necessarily deliver competitive economic development either but at least it can hold out some prospect of short-run gain. Thus, more states are enmeshed in international trade networks than ever before, whether they like it or not.

A crucial intermediary in these changes in the relative attractions of war and trade has been and continues to be systemic leadership. System leaders organize winning coalitions in most major wars. They specialize in the development of military global reach instruments. They develop some semblance of world order by generating rules for the postwar era of long-distance trade. They also generate the radical innovations that fuel expansions in world commerce and industry. In many respects, they have underwritten the ways in which war and trade have been organized and practiced in the world system for the past 500 years.

System leaders have also been important to the emergence of democratic political systems. Competition among domestic groups have been important to the development of leading sector innovations in commerce and industry. As a consequence, it has not been a coincidence that the most successful "trading" states (Venice, the United Provinces of the Netherlands, England, the United States) have also possessed the most democratic constitutions of their respective eras.[1] The ways in which this democratization has been manifested has not always resembled the democratic ideal of the period from late twentieth to early twenty-first century. Therefore, many have missed the historical linkages connecting the earlier oligarchic republics to the more contemporary democracies with more extensive electoral enfranchisement and participation. But as leading "democracies" of their respective eras, they have fought successfully external opponents favoring less democratic domestic constitutions. They have led the fight against alternative principles of domestic political organization (e.g., aristocracy–monarchy, fascism, communism) and helped considerably to defeat the likelihood of these alternatives prevailing as principle political strategies.

War, trade, systemic leadership, and democratization are thus intertwined processes of evolutionary change. It should not be surprising if we have problems sorting out which thread deserves the most credit. However, the argument in this book has been that democracy and democratization has not been the strongest link. To refer to contemporary reductions in conflict as "the democratic peace" bestows too much credit on one variable that is

itself a partial product of other complex forces. Accordingly, we began our analysis with three overarching hypotheses:

H1: *The influence of democratic regime type is only one of a number of factors encouraging less conflict in contemporary world politics.*
H2: *The emergence and survival of democracies is embedded within a larger context of structures and processes that share responsibility for consequent reductions in conflict.*
H3: *Evolutionary tendencies in world politics work against the over time stability of relationships between conflict and its antecedents.*

Yet these are big statements that allude to big processes and "big pictures." How can we best tackle them? Our approach has been to examine an additional 24 hypotheses that are related to the overarching claims. Support for any single hypothesis is unlikely to make our case. But support for two dozen hypotheses should begin to suggest that the overarching propositions need to be taken seriously. This is precisely where the "provisional" nature of this chapter enters. No matter how many hypotheses are proposed and tested and no matter what their outcome might be, it is unlikely that we can produce a totally definitive product that, once and for all, establishes the ultimate superiority of our theory over competing arguments. We think our approach is an improvement over the democratic peace arguments with which we are familiar because we can explain many of their empirical findings and, at the same time, go beyond what democratic peace arguments are capable of addressing. Still, one can only do so much of this enterprise in one book. It is and needs to be an ongoing process of theory elaboration and empirical verification.

What is it that we believe we have addressed in the 24 hypotheses examined in this book? There is some overlap in coverage that needs to be made explicit at the outset. Hypotheses 4–7 (peace → democracy and regional hegemonic ambitions), 9–13 (rivalry and democracy's effects on militarized dispute behavior), 14–18 (the identities of democratic and autocratic rivals), and 24–27 (the effects of systemic considerations on conflict and democratization) address both of the first two overarching hypotheses. That is, they illustrate some of the ways in which democratic regime type is not the only antecedent of conflict reduction (H1). They also demonstrate some of the ways in which democratization is embedded in larger structures and processes (H2).

That leaves hypotheses 8 (path dependencies → aggression) and 19–23 (the relative effects of various influences on war initiation) addressing the first overarching proposition on multiple paths to reduced conflict. Hypotheses 12 (rivalry versus regime type), 20 (capability/initiation versus regime type), 23 (internal and external constraints on warfare and war initiations), and 24 (the relationship between democratization and systemic conflict) speak primarily to the third overarching proposition that evolutionary tendencies work against the stability of conflict relationships. Generally, realpolitik relationships that held prior to 1945 are no longer as

easily observable as they once were. But, then, all of the analysis undertaken in this book addresses this question at least indirectly.

Given our chapter structure, the hypotheses and related findings cluster most easily into six groups. The first group encompasses chapters three and four and hypotheses H4–H7.

H4: *The greater is the perceived external threat, the more likely is a state to develop and maintain high levels of domestic power concentration.*
H5: *The greater is the level of domestic power concentration, the less likely a state is to democratize.*
H6: *Controlling for external threat and domestic resource concentration, the greater the level of democratization, the more likely it is that a state's foreign conflict behavior will be constrained, especially with other democratic states.*
H7: *States heavily involved in the pursuit of regional primacy and coercive expansion have been the least likely to develop democratic forms of government.*

No one claims democracies emerge in vacuums. Yet many analysts seem content to take democratic regime type as a given without questioning whether there are any circumstances associated with its emergence that might be linked to dyadic conflict reduction. Of course, one problem in discussing the emergence of democratization is that the process comes in waves. Whatever generalizations might be advanced about one wave do not necessarily pertain to the circumstances associated with other waves. If we focus on the first wave of democracies (roughly pre–World War I vintage) as the one that established the system's democratic hard core, however, one argument about their early emergence is that they often possessed geostrategic positional advantages (maritime insulation in the cases of Britain and the United States, mountains in the case of Switzerland) that made them less vulnerable to external attack. States with high vulnerability to external attacks tended to develop domestic structures designed to deal with persisting threat (autocracy, bureaucratization, large armies). A corollary of the interactions among these domestic processes is a concentration of the resources thought useful in creating conducive settings for democratization (wealth, military power, information). Less external vulnerability facilitated avoiding the concentration of domestic resources and, thereby, encouraged the emergence of democratic institutions and culture.

This argument is an example of a "reversed causal arrow" claim. Rather than focus exclusively on the democracy → peace proposition, the question raised in the reversed causal arrow puzzle is whether peace → democracy? The empirical support found for hypotheses H4 and H5 suggests strongly that peace does promote democratization. But does that mean that we are misattributing the peaceful dyadic outcome associated with the democracy → peace relationship to democracy when we should be linking it instead to the earlier antecedent—the lower vulnerability to external attack? The causal linkage would then be a form of neighborhood peace → dyadic peace. The findings that stem from testing hypothesis H6 suggest that this is not the case. It is not simply peace → peace. Rather, the causal

structure is neighborhood peace → democratization → dyadic peace. Hypothesis H7 makes this general point more emphatically by focusing on the special case of states involved in external aggression in tough neighborhoods. Domestic processes within such states may or may not push toward democratization, but ambitious foreign policies of territorial expansion against stiff opposition are apt to discourage further democratization.

In a larger context, it is the states that find war strategies most appealing and/or essential to survival that have been the least likely to democratize successfully. Only after grandiose foreign policy ambitions have been extinguished or the regional neighborhood rendered more pacific do domestic liberalization efforts stand much chance. Although trading states have not been able to evade war altogether, their wars with aspiring regional hegemons tended to be defensive. Their other wars tended to be more distant from the home front. By making early choices for trade over war, democratization did not come automatically but it came more easily. Yet this is not simply a story about early democratizers. The same sort of process can be found in eastern Europe between World Wars I and II. Most of the democracies that emerged after World War I were no longer democratic by 1939. Domestic liberalization efforts had succumbed to an increasingly threatening external environment.

These observations about the reciprocal nature of external and domestic environments are manifested in hypothesis H8 as well. It is not just foreign policy ambitions of territorial conquest or even constant external threat that cause problems for democratization processes. The legacy of irredentism or the myth of a divided nation that needs to be reunited within one territorial unit is another type of path dependency that can "override" democratization processes. A strategic rivalry with another state can also distort domestic processes, much along the lines of early-modern European absolutism (e.g., autocratization and militarization to meet the external threat). The point here is that democratization is not so powerful a force that it can overcome all obstacles. It must compete with other processes and factors in influencing outcomes. In some cases, certain path-dependencies have proven to be more powerful than intermittent democratic institutions and practices. That is not really a startling message. What is more important is the attempt to explicitly delineate, vis-à-vis democratization, which processes are as powerful or more powerful in affecting foreign policy outcomes. What should we expect when states are exposed to multiple influences as is usually the case?

H8: *Certain path-dependencies (e.g., foreign policy ambitions, irredentism, and strategic rivalries) encourage more aggressive foreign policies in spite of domestic political constraints on aggressive behavior.*

Hypotheses H9–H13 continue this line of thought. If we accept the idea that both democratic dyads and rivalry dyads have affinities toward or away from engaging in conflict (hypotheses H9 and H10), which type of relationship is more powerful? Can rivalry relationships trump regime type

relationships, or it the other way around? Should we anticipate that the answer is constant over time? Our answer is that rivalry relationships should exert more influence on the probability of conflict than can the constraints of regime types (hypothesis H11). We find empirical support for this argument, just as we find support for our related argument that the power of information about rivalry structures to predict dyadic conflict is weakening over time while the power of information about democracy structures to predict dyadic conflict is strengthening (hypothesis H12).

H9: *Democratic dyads are less prone to militarized disputes and wars than are nondemocratic dyads.*
H10: *Strategic rivalry dyads are more prone to militarized disputes and wars than are dyads not engaged in strategic rivalry.*
H11: *The effect of strategic rivalry on militarized dispute and war behavior is greater than that of the effect of dyadic regime type.*
H12: *The relative explanatory value of strategic rivalry and regime type are unlikely to be constant. The relative contribution of dyadic regime type vis-à-vis strategic rivalry should be expected to improve over time.*

Nevertheless, we also found that the situation was more complicated than simply one of comparing democratic dyads with nondemocratic dyads. The most conflict-prone dyads are those that pair a democracy with an autocracy. Both autocratic and democratic dyads are relatively more peaceful than these mixed regime duos. Corroboration for that argument sets up inquiry into the next cluster of hypotheses linking rivalries and regime types.

H13: *Mixed regime type dyads are more prone to militarized dispute and war behavior than are autocratic dyads. Both of which, in turn, are more prone to militarized dispute and war behavior than are democratic dyads.*

Chapter six responded to arguments that democracies have a propensity to win their wars because they are democracies. We find that is not exactly the case. The side that has the capability edge and chooses to initiate the war tends to win (hypothesis H14). Yet that generalization is not carved in stone. The advantages of capability and initiation are less clear-cut in post-1945 wars, while the influence of regime type appears to be becoming more significant over time (hypothesis H20). Given the strong roles of Israel and India in the post-1945 annals, it is hard to tell to what extent this may be an artifact of some strong outliers.

H14: *Capability and initiation are more important to war outcomes than is regime type.*
H15: *The relative effects of capability, initiation, and regime type are changing over time, with regime type becoming a stronger influence on war outcomes.*

The H16–H20 hypothesis cluster begins with the monadic observation that, other things being equal, democracies appear to be just as belligerent as autocracies (hypothesis H16). This observation is the foundation for what

has been called the monadic puzzle of the democratic peace. How is that democratic dyads can be more pacific but that same pacificity does not extend to democracies per se? Our answer to this puzzle is that the empirical record is biased by largely twentieth-century events that are closely linked to the expansion of democracy. Whatever the merits of the arguments that democracies are more pacific or more constrained in their foreign policy behavior, the expansion of democratization has not gone uncontested. In order to survive, democracies have had to fight various types of autocratic regimes (monarchies, empires, fascists, communists). The very process of democratization is sufficient to generate rivals for democracies (hypothesis H17). As a consequence, democratic rivals, especially at the major power level, have tended to be autocratic opponents (hypothesis H18) with an interest in seeing fewer rather than more democracies. Autocratic rivals, on the other hand, tend to be more heterogeneous. Some are democracies. Some are other autocracies. It follows, then, that we should see ample democratic conflict with autocratic rivals and ample autocratic conflict with their rivals (hypotheses H19 and H20). These tendencies are clearly manifested in the empirical record. It may be reasonable to speculate, therefore, that as long as democracies have autocratic rivalries, the monadic tendency for democracies and autocracies to exhibit similar conflict propensities should persist. The monadic puzzle then may simply be an artifact of democratization and its tendency to increase conflict between mixed dyads.

H16: *In general, autocracies and democracies appear equally belligerent in the frequency of their overall conflict behavior.*
H17: *Both autocracies and democracies are equally likely to distinguish between enemies and non-enemies in their conflict behavior.*
H18: *Democracies are likely to distinguish between democracies and autocracies in their conflict behavior while autocracies are less likely to do so.*
H19: *Democracies are most likely to engage in conflict with autocratic enemies and least likely to do so with democracies. Autocratic non-enemies, as opponents, fall somewhere in between.*
H20: *Autocracies are most likely to engage in conflict with enemies, whether democratic or autocratic, and least likely to do so with non-enemies whether democratic or autocratic.*

Finally, hypotheses H21 through H24 continue the focus on systemic processes begun with hypotheses H16 through H20. In many respects, hypothesis H21 is linked to the emphasis on mixed regime dyadic conflict highlighted in hypothesis H13 in which mixed regime dyads were identified as the dyads most prone to militarized dispute and war. The systemic version of that question focuses on what happens when the mix of mixed regime dyads changes over time. Is the democratization–conflict relationship curvilinear, peaking in the middle of the democracy expansion when the number of mixed regime type dyads is greatest? Is the relationship positive because it is not so much the number of mixed regime type dyads

but rather the intensity of their antagonism that is more critical? If the intensity of antagonism remains high as the number of mixed dyads decline, conflict propensities could also remain high. Or, is there no discernible relationship as others have maintained because one autocratic trouble-maker can be as problematic as several. We find that the democratization–conflict relationship has been positive within the crucial major power subsystem.

H21: *Democratization and systemic conflict are positively related.*

The last three hypotheses return our attention directly to the role of systemic leadership. This is a critical variable in our modification of the original trading state theory. Systemic leadership conditions the way most major wars are fought; it also exerts a strong influence on the expansion of trade. But most democratic peace arguments are couched in strictly dyadic terms. Systemic considerations thus are often consigned explicitly or not to residual influences. One exception is the findings that "hegemonic" structure is irrelevant to the effects of democratization on pacifying conflict (Oneal and Russett, 1999; Russett and Oneal, 2001). Such a conclusion depends very much on how one conceptualizes hegemonic structure. In hypothesis H22, the effects of changing global and regional power concentration structures are expected to remain significant, despite controlling for democratization. That expectation is strongly supported by the empirical evidence. We need not throw out systemic structure as irrelevant as we attempt to unravel the ongoing transformation of world politics. It still matters.

H22: *Global concentration is negatively correlated and regional concentration is positively correlated with systemic conflict, even when the influence of democratization is controlled.*

Another way in which systemic structural change matters is the link between systemic leadership and the pace of democratization. This area highlights another facet of democratization's embeddedness in larger processes. The argument is that democratization has depended on systemic leadership for its very survival, especially in showdown wars between autocracies and democracies, and that the strength of systemic leadership is closely related to the successful expansion of the number of democracies in the system (hypothesis H23). Both of these generalizations are supported by the timing of fluctuations in the number of democratic political systems.

H23: *Global war and systemic leadership are related to the pace of democratization.*

Still another role for systemic leadership is found in alliance behavior. Contrary to the argument that democracies have a special affinity for allying with other democracies, hypothesis H24 attributes the appearance of this relationship to changes in systemic leadership strategies. Defensive alliances were not that important in the Pax Britannica, or in earlier periods of systemic leadership. Only after 1945 did defensive alliance-making in the Cold War era become prominent. This marks something of a sea change in the

traditional autocratic advantage of attacking democracies reluctant to organize in their own self-defense, as manifested in the preludes to World Wars I and II. Still, the democratic dyadic alliance affinity appears to be a function of the NATO. If one controls for that particular alliance, no special affinity is discernible. Therefore, it is systemic leadership that is most important to this aspect of contemporary alliance behavior, and not regime type.

H24: *Systemic leadership is a major influence on democratic states' alliance formation propensities.*

Going Beyond the Democratic Peace Idea

It is, of course, easy to suggest that there are major limitations to democratic peace explanations. A more formidable task is to put forward an alternative that offers a more general interpretation of transformations in world politics and that is also susceptible to empirical testing. The point to be stressed is not that democratic peace arguments have their relationships all wrong, but rather that their explanations of the relationships that have been uncovered are too narrowly confined to regime type as the primary source of modifications in behavior. Many of the bivariate empirical relationships between regime type and conflict are quite likely to survive repeated testing. The real question, though, is what should we make of these findings. We claim that there is a "bigger picture" at stake. Democracy does not make the world go around but it is a component, and perhaps even a highly significant component, within a larger context.

The larger context that we have tried to elaborate in our modified trading state theory pits increasing costs of coercion and warfare against increasing benefits of trade and cooperation, mediated by systemic leadership. This is not exactly the same argument as advanced by Rosecrance (1986) but we like to think of it as primarily an elaboration of Rosecrance's ideas. Are these ideas completely alien to the discourse on the transformation of world politics and the associated role of democracy in the ongoing transformation? We think not. There are other arguments that seem highly compatible with our own view even if they come packaged differently. We conclude with a demonstration of this assertion by comparing our general argument with the arguments advanced by Weede (1999), Mousseau (2000), Russett and Oneal (2001), and Jervis (2002)—although not exactly in this order.

Kant Versus Hobbes

Bruce Russett and John Oneal's (2001) overall argument can be summarized as one distinguishing between Hobbesian and Kantian system dynamics. Hobbesian systems are governed by conventional realpolitik principles. Anarchy or the absence of centralized government places a premium on power and self-help to achieve national security. Yet attempts

by any single state to improve its own security can be interpreted as a threat to the security of other states. Negative spirals of increasing conflict and war can result and lead to donnybrooks or attempts to establish hegemonic dominance.

Kantian systems are Hobbesian systems that are increasingly tamed or constrained by the development of attributes and processes that work to make conflict less likely. From Russett and Oneal's perspective, the three main constraining forces are democratization, economic interdependence, and international organizations. The operations of these processes are characterized by reciprocal or two-way causality and are mutually reinforcing to encourage positive spirals of peaceful and cooperative interactions. Over time, then, and barring serious breakdowns, Kantian systems should become less Hobbesian and more stable.

Democracies are more peaceful than authoritarian regimes and especially unlikely to fight with other democracies. At the same time, democracies are more likely to trade with each other and to join international organizations. Trade and investment create incentives to avoid disrupting relations with other states. The economic growth that is concomitant with increasing interdependence can also encourage and sustain democratization by contributing to the deconcentration of wealth with societies. International organizations also work in multiple ways to ameliorate or suppress interstate conflicts. They also serve to facilitate the reductions of frictions that emerge as states become more economically interdependent and can support democratization as well.

Yet all three processes (democratization, interdependence, and international organizations) function more effectively in periods of limited conflict than in periods of intense tensions and full-fledged warfare. Democratic rights and procedures are more difficult to sustain in the face of high levels of external threat. Traders and investors tend to avoid situations in which future violence might negate the possibility of growth and profitable economic interactions. International organizations are predicated on the expectation of more pacific interactions and tend to be ignored as relationships become increasingly conflictual.

Russett and Oneal are stronger on discussing the multiple ways democratization, interdependence, and international organizations interact theoretically than they are in explaining how these processes have emerged historically. On the one hand, they acknowledge (Russett and Oneal, 2001: 20) that some semblance of global economic interdependence began to emerge in the sixteenth century, thanks to the activities of "British, Dutch, Portuguese and other traders, backed by their countries' naval power . . . ." However, a genuinely global network impacting most of the world's population awaited significant changes in transportation and communication speeds and costs that only took place in the second half of the nineteenth century. More or less paralleling this globalization was the extension of the Westphalian/European interstate system that enshrined independent state sovereignty as a basic organizing principle to the rest of the world.

Within this historical context, the expanding European-centric system had managed to survive the Napoleonic and German challenges to eliminate the independence of European states by creating alliances to defeat the hegemonic aspirations on the battlefield. Each time, some efforts were made to reform the postwar system in order to prevent a recurrence of intensive warfare. Between 1815 and 1945, these postwar reform efforts increasingly took the form of encouraging democratization, eliminating barriers to trade and investment, and facilitating international cooperation via the creation of international institutions. Relapses into Hobbesian strife clearly were not guaranteed by these intermittent attempts at systemic reform. But each attempt improved on the efforts of the previous iteration (for instance, increases in the proportion of states that are democratic, greater levels of economic interdependence, and more international organizations) holding out hope for continuing evolutionary progress along Kantian system dynamic lines. In particular, regional pockets of increased liberalism and cooperation (western Europe, North America) appear to be leading the way to a more comprehensive suppression of Hobbesian tendencies.

The irony of this argument is twofold. One, despite its emphasis on states and their institutions, it is vague about agency. There is clearly a reluctance to bestow any credit on systemic leadership (see chapter eight) that, we have argued, is critical to the development of the forces/processes (economic interdependence, democratization, and international organizations) given credit for constraining Hobbesian propensities. The other irony is that Russett and Oneal prefer to stress the modeling of the constraints on Hobbesian propensities (albeit, with a Hobbesian dependent variable)—as opposed to modeling the tug-of-war between Hobbesian and Kantian influences. In this respect, we think our own modified trading state theory, while certainly not incompatible with Kantian factors, is potentially a more comprehensive approach in the sense that we try to theorize about both Hobbesian and Kantian forces simultaneously.

A Democratic Peace Conditioned by Economic Development and Liberal Values

Michael Mousseau's (2000, 2002a, 2002b, 2003) work on the origins of the democratic peace is distinguished by his very strong emphasis on the role of economic development.[2] One of his arguments is that previous research on the democratic peace controlled for wealth but not economic development. Once one does control for economic development, it turns out that the pacifying effects of joint democratic institutions are twice as robust in dyads that are both economically developed in comparison to other dyads. Moreover, dyads that are democratic, but not economically developed, do not exhibit significant tendencies toward peaceful relations. These findings do not mean, at least for Mousseau, that the democratic peace is spurious and that selective dyadic peace should be attributed entirely to economic

development. Mousseau argues instead that development, democratization, and liberalism come in a package, with development leading to the others and, once in place, all three are characterized by substantial mutual reinforcement.

Mousseau's argument is informed by an anthropological theory known as cultural materialism. Factors and processes are divided into three basic societal components in this perspective: infrastructure, structure, and superstructure. According to Harris (1979), infrastructure refers to modes of production and reproduction and their interaction with the environment, structure encompasses social and political institutions including the domestic political economy, while superstructure includes values, beliefs, and culture. The basic argument of cultural materialism is that innovations in the infrastructure have primacy in the sense that the structure and superstructure are more likely to adjust in compatible ways to infrastructural changes than the other way around. Structural and superstructural innovations are capable of influencing the infrastructure but this direction of causality flow is both less probable and much slower moving.

This simple but powerful argument is applied to international relations by interpreting the European movement toward a market economy after 1450 as a break from the multiple, hierarchical imperial systems that predominated prior to the sixteenth century. Imperial states impose centralized control on the flow of goods and, in particular, extract tribute from their peripheries. Market economies, in contrast, can only function if contractual cooperation governs the exchange of commodities, thereby creating more space for individual choice and the rational pursuit of self-interest. In turn, engaging in such behavior is said to encourage the emergence of values emphasizing individualism and human rights, freedom, tolerance, compromise, trust, legal equity, and respect for law and property. These superstructural values, to the extent that they prevail, reinforce the stability of the market's functioning and the democratic political institutions that best reflect market values and culture.

The consequent emergence of a democratic peace is then due in part to popular constraints on foreign policy that accompany the development of political institutions that reflect liberal market values. Decision-makers seeking reelection are likely to heed these popular constraints. But it is also argued that democratic decision-makers are more likely to settle their differences through negotiation because that is their natural inclination coming from a liberal culture. They are also likely to value highly collective security arrangements, international law, and international organizations. To the extent that developed democracies predominate in the international system, one should also expect them to attempt to bring some semblance of law and order to the rest of the nondemocratic world as well.

Of course, many of the specifics of this democratic peace explanation are quite conventional to democratic peace arguments. The difference lies mainly in Mousseau's emphasis on the development of economic markets as the source of democratic institutions, international organizations, and

trade. The more traditional liberal interpretation is to view these three pacifying agents of the liberal canon as separate sources of constraint.

Mousseau succeeds in providing a theoretical "glue" that appears capable of bringing together a number of hitherto separate ingredients. The primacy of the infrastructure thesis may be totally wrong empirically but it is certainly a plausible ordering that deserves to be taken seriously. One problem, however, is that Mousseau waffles on whether markets and exchange are infrastructure or structure. Cultural materialism clearly places domestic political economy and political institutions in the structural category. Mousseau sometimes acknowledges this conceptualization and at other times treats markets and exchange as infrastructure. They cannot be both. If they belong to the structural category, something genuinely infrastructural is presumably missing from the Mousseau argument.

Another problem with the cultural materialism interpretation is its principal emphasis on domestic society. This emphasis probably can be traced to one of the motivations for the theory's initial development. Societies with similar infrastructures are predicted to share similar structures and superstructures because they have similar infrastructures. But this ignores the possibility of diffusion, long derided by anthropologists, or intersocietal influences. The exceptions are war and trade that are also considered to be structural institutions. Since war and trade require opponents and partners, some element of external contact is allowed but not given much causal weight. Mousseau deviates from this tendency by emphasizing market exchange but ignores the influence of war altogether and treats political institutions as derivatives of market exchange that are strongly reinforced by superstructural values. Indeed, it is not clear whether it is the nature of domestic political economy or the superstructural values and attitudes that principally drive the developed-democratic peace in Mousseau's perspective.

A third and perhaps related problem is Mousseau's Eurocentric and underdeveloped historical script. He has northwest European Protestants revolutionizing first trade by making it less hierarchical and then changing the norms of the international system between 1450 and 1648. From these roots, advanced industrial societies emerged with democratic institutions and liberal ideologies. But the initial attempts in the fifteenth and sixteenth centuries on the part of some Europeans to break into non-European exchange markets that had been in place for some time were led by non-Protestant Portugal and Spain. Portugal was absorbed by Spain and the ultimate mid-seventeenth-century humbling of Spain in European regional politics was brought about by a combination of Dutch, Swedish, and non-Protestant French arms. Subsequent technological innovations accelerated industrialization initially in the commercial leaders who were located in northwestern Europe by the late sixteenth–early seventeenth centuries. A number of later European developers, however, evaded liberal institutions by instead developing more centralized approaches to economic development and trade and had to be defeated before liberal constitutions and values could be imposed by the coalitions that won the global wars of the

past four centuries (and to a considerable extent, the Cold War). Something is definitely missing from the Mousseau interpretation if participation in market exchange is not sufficient to bring about democracy and liberalism in the advanced industrial societies of the late twentieth century. What seems most obviously missing is the timing of development and associated institutional tendencies, as well as the wars fought among aristocrats, fascists, communists, and liberals to see whose ideas would predominate in organizing the world economy.

A Capitalist Peace

Erich Weede (1999) views the democratic peace as a sub-component of a capitalist peace engineered by U.S. grand strategy after World War II. If the United States had simply imposed democratic institutions on Germany and Japan in the aftermath of their defeat, Weede argues, it is unlikely that the postwar settlement would have succeeded as well as it did. The more critical intervention was the concession of relative economic gains to poorer economies in Europe and Japan along the way to creating the possibility of economic growth, prosperity, and, ultimately, free trade among most of the more technologically advanced economies.

The commitment to free trade and continuing expectations of expanding trade decreases the incentives for war among trading states. The economic growth of initially poor states also reduces the likelihood of their resorting to more coercive approaches to advancing their interests. To the extent that prosperity accompanies capitalistic growth and trade, an important ingredient for democratization is established—thereby further reducing the chances of war between rich and poor democracies.

The success of this capitalistic order in what is described as a North American–western European–Japanese triangle eventually seduced China and the Soviet Union away from their belligerent hostility of the Cold War era. China began experimenting with various forms of capitalism under Deng Xiaoping. The Soviet Union developed glasnost policies under Gorbachev that led fairly quickly to the collapse of the Soviet state and its threat to the capitalist West. Presumably then, peace among at least the great powers thus hinges on the successful penetration and establishment of capitalism, prosperity, free trade, and democratization in China and Russia.

Weede's liberal explanation clearly overlaps with the views put forward by Russett and Oneal and Mousseau. His interpretation, however, differs somewhat from Russett and Oneal's trinity approach. Whereas Russett and Oneal accept the likely reciprocal influences between trading, interdependence, and democratization, Weede is suggesting that trade indirectly facilitates the establishment of democratization. This effective subordination of democracy as a consequence of interdependence seems more closely compatible with Mousseau's argument that liberal values are the main drivers of trade and democracy. Yet Weede also parts company with Mousseau's

explanation by emphasizing the agency role of U.S. hegemony in creating a capitalist order in the North American–West European–East Asian triangle.

Weede's argument is also quite compatible with Rosecrance's (1986) trading state model and, therefore, our own modification of the trading state model that insists on a significant role for systemic leadership in establishing a liberal order. Nonetheless, two important elements are missing in the Weede perspective. Weede's historical script virtually begins in 1945 and neglects the earlier antecedents of the post-1945 order in the West.[3] He also ignores any dynamics associated with perceptions about the use and cost of coercive force. Like Russett and Oneal, he chooses to stress only the constraints on the use of force in his capitalist peace interpretation.

Synthetic Interactive Discontinuity

Robert Jervis argues that great power warfare, or preparations for great power warfare, has been the principal driver of international politics in the past but that this motor will no longer function similarly in the future. Developed great powers no longer even contemplate war, at least within their elite circle. If so, one of the main factors shaping internal and external politics has ceased to operate as before. Jervis (2002: 1) regards this development as "perhaps the single most striking discontinuity that the history of international politics has anywhere provided."

How can this discontinuity be explained? Jervis's approach is to synthesize six factors found in overlapping realist (American hegemony and nuclear weapons), liberal (democracy and economic interdependence), and constructivist (norms of nonviolence and shared identities) interpretations. The first factor and the only one labeled as necessary are the high costs of warfare and the difficulties associated with conquest. Given the very low probability of achieving anything tangible in a war between two developed great powers, without also incurring nuclear war costs that are too terrible to imagine, war ceases to exist as a viable option. If this factor is combined with an unusually high level of satisfaction with the status quo and the superiority of trading networks over conquest as a route for obtaining material goods, one has an overdetermined and exceedingly low expected utility of war. The overdetermination is expanded further by a high degree of value similarity and compatibility within the Deutschian pluralistic security community that has emerged. To the extent that the main political values revolve around democracy, compromise, tolerance, respect for law, and the need to avoid coercion, there is little to be gained by attempting to convert another country's value system.

A fifth factor is the decline in territorial disputes. Historically, conflicts over space have been the most common single factor motivating international tensions. For a variety of reasons, including the delinking of national prosperity and territorial control, these types of conflicts rarely arise anymore within the community organized around the developed great powers (western Europe, North America, and Japan). A final factor in Jervis's

explanation is the Cold War. Externally induced threat led to U.S. protection and the construction of a coalition to meet the threat. In turn, coalition construction and maintenance meant that its members needed to worry about the other members' stability and economic welfare. They also needed to be careful not to push disagreements too far. Once established as the community norm, cooperation became relatively self-sustaining, barring some major environmental shock such as a world depression.

All six factors interact and reinforce one another. Jervis's argument is avowedly eclectic and should be applauded for its ontological pluralism. Yet the liability of such an approach is that it is similar to making stew without an explicit recipe. One takes a pinch of liberal this and a dab of realism that, season with some constructivist spice, and voila, one has an eclectic explanation that is pleasing to the palate. But what happens if some or all of the factors are themselves interrelated? Is U.S. hegemony unrelated to the economic interdependence of the advanced industrial states or the advent of nuclear weapons? Do democratic institutions have anything to do with norms of nonviolence and cooperation, or shared identities? The answers to such questions is probably yes. Why are territorial disputes less likely in the West? To the extent that is the case, a great deal more theoretical specification and elaboration is in order before we can be reasonably confident that we understand the putative causal mechanisms at work.

Still, the form of this argument, by now, should sound familiar. Jervis's most important (necessary) factor is the cost of warfare. Systemic leadership is prominently displayed in the form of U.S. hegemony and coalition building. The other four factors pertain to trading interdependence, democratization, similar values, and diminished disputes over territory. We feel quite theoretically comfortable with these soup ingredients. We simply prefer to use a different modified trading state bowl to encompass them.

Summarizing this quick review of alternative interpretations, table 10.1 lists the main emphases of the four perspectives discussed above and compares them to our own modified trading state argument. In the abstract, we contend that there is an unusual amount of substantive compatibility. All five arguments stress the significance of cooperation and trading interdependence. None rule out some explanatory role for democracy and democratization but four of the five subordinate democracy as the product of other more central processes and developments. Three acknowledge explicitly a major role for systemic leadership. While only two (ours and Jervis's) emphasize the increasing costs of warfare, all of the other factors appear to be derivatives of some of the other more central ones. Liberal values, a status quo orientation, and diminished territorial disputes presumably are tied closely to the success of a liberal economic growth/trading order.[4] Politically, international organizations are rather difficult to separate from the impetus of postwar settlements and the creation of new political organizations by system leaders. Economically, a number of international organizations stem from attempts to regulate and facilitate the spread of new transportation and communication technologies (Murphy, 1994).

Table 10.1 Eight theoretical emphases on bigger pictures

Emphases	*Russett/Oneal*	*Mousseau*	*Weede*	*Jervis*	*Ours*
Costs of warfare				Emphasized	Emphasized
Benefits of trade	Emphasized	Emphasized	Emphasized	Emphasized	Emphasized
Systemic leadership			Emphasized	Emphasized	Emphasized
Democracy	Emphasized	Subordinated	Subordinated	Subordinated	Subordinated
Liberal values	Emphasized	Emphasized	Emphasized	Emphasized	Compatible
Territorial disputes				Emphasized	Compatible
Status quo orientation				Emphasized	Compatible
International organizations	Emphasized				Compatible

Thus, it would seem that many of us are operating on a similar theoretical wavelength even though we may disagree about how precisely to package it. The transformation of world politics is underway and has been underway for some time. Exactly how long this transformation process has been ongoing and which of its dimensions should be stressed as fundamental motors are not yet subjects of consensus. That may come in time if we realize just how similar and/or overlapping these interpretations really are. Moreover, most of us would agree that the ongoing transformation still has a considerable way to go before we can declare world politics as fully transformed. If one argues, as we do, that the problem reduces to calculations about the costs of warfare and the attractions of economic development/trade, circumstances can always change in such a way that war becomes more, and economic development/trade less, attractive. The world depression of the 1930s is a not-so-distant case in point. Even if we manage to evade an acute world depression in the twenty-first century, there can be no doubt that the comparative calculations about war and trade will be made differently in various parts of the world. Thus, the ongoing transformation will proceed haltingly and unevenly. It may take another century of conducive environmental change before we will be able to tell just how much world politics has been transformed.

APPENDIX

Major Powers

In several analyses (chapter two is an exception), we adopt as catholic an approach to identifying major powers as possible by accepting the Correlates of War definition: Britain (1816–present), France (1816–1940, 1945–present), Russia/the Soviet Union (1816–present), Austria/Austria-Hungary (1816–1918), Prussia/Germany (1816–1945), Italy (1860–1943), Japan (1895–1945), the United States (1899–present), and China (1950–present). We balk only at restoring German and Japanese major power status in the early 1990s.

Democracy

Regime type information is taken from the Polity III (Jagger and Gurr, 1995) data set (http://www.colorado.edu/IBS/GAD/spacetime/data/Polity.html). The most common strategy is to focus on two additive scales, one for democracy and one for autocracy. The democratic score awards up to 10 points on the following categories: competitiveness of political participation (0–3 points), competitiveness of executive recruitment (0–2 points), openness of executive recruitment (0–1 points), and constraints on chief executive (0–4 points). The autocracy scale focuses on the same categories plus one other (regulation of political participation) but is weighted slightly differently: political participation competitiveness (0– 2 points), political participation regulation (0–2 points), executive recruitment competitiveness (0 or 2 points), executive recruitment (0–1 points), chief executive constraints (0–3 points). The nature of the scales are such that a high democracy scale score means a low autocracy score and vice versa but given the differential use of the 4–5 categories, the varying weights, and the distinctions between mass participation and chief executive categories, it is possible for a political system to be awarded points on both scales simultaneously. The solution is to construct a net democracy score by subtracting the Polity III 0–10 autocratic scale from the 0–10 democratic scale. States that score 6 or higher on this net score are treated as democracies; all other states are autocracies. However, the precise threshold for democracy status is not a major concern in a project focused on major powers as this one is. For the most part, the major powers often focused upon in this study have not hovered in the 5–7 point, net democracy score range that is frequently employed as a threshold. Major powers tend to be either above 7 or below 5 on the net democracy scale.

We make no claims that these data are problem-free. As one critical evaluation (Gleditsch and Ward, 1997: 365) notes, the scales are biased toward authority relations and chief executive attributes but they remain "the most complete and up to date as well as the most historically extensive data on authority characteristics, encompassing all independent polities since 1800." For major power samples, the United States is coded as democratic for its entire inclusion in the sample, Britain from 1880 on, France between 1848 and 1850, 1877 and 1939, 1946 and 1957, and from 1969 on, Germany between 1919 and 1932, with Russia qualifying very briefly only at the very end of the analyses in 1991. All other major powers are coded as autocracies.

Conflict

Conflict is measured in terms of the frequency of militarized interstate disputes (MIDs) and/or wars. For MIDs, the 2.1 data set (http://pss.la.psu.edu/Mid_data.html) was current at the time of our analyses. However, we employed the dyadically revised version developed by Zeev Maoz (http://spirit.tau.ac.il/~zeevmaoz). MIDs encompass activities involving the "threat, display or use of military force short of war" by one state "explicitly directed towards the government, official representatives, official forces, property, or territory of another state" (Jones, Bremer, and Singer, 1996: 163). Examples of the use of force include blockades, territorial occupations, personnel seizures, clashes, raids, and declarations of war. The different ways in which force can be displayed runs the gamut from alerts, through troop and weapons mobilizations, to border fortification and violations. As in the case of the Polity data, there is room for improvement in these data but the data set itself remains fairly unique in its spatial and temporal coverage. As a consequence, it has become a standard measurement of conflict.

NOTES

Chapter One

1. Representative summaries of this literature are Russett (1993), Ray (1995), Chan (1997), Maoz (1998), and Russett and Oneal (2001).
2. See, for instance, Deutsch et al. (1957), Maoz and Russett (1993), Russett (1993), Ray (1995), and Weart (1998).
3. Constructivist explanations of the democratic peace (see Risse, 1995, Kahl, 1998/99) fit approximately into this cluster as well.
4. See, on this question, Bueno de Mesquita, Siverson, and Woller (1992); Bueno de Mesquita and Siverson (1995), and Thompson (1995b).
5. As is argued by Siverson and Emmons (1991) and Gaubatz (1996), Oneal and Russett (1997, 1999), Russett (1998), Russett and Oneal (2001).
6. See Seeley (1886), Hintze (1975), Finer (1975), Tilly (1975), Almond (1990), Midlarsky (1995), Thompson (1996), Reuveny and Li (2003), and Colaresi and Thompson (2003). See, however, Reiter (2001) for empirical evidence that does not support this idea.
7. See Huntington (1991) and Przeworski et al. (2000).
8. Lemke and Reed (1996) and Tammen et al. (2000).
9. Various efforts along these lines are found in Modelski and Perry (1991), Modelski (1999), and Thompson (2000).
10. Several caveats and warnings need mentioning. The cases are treated fairly casually in the 1957 study. Reference is made by 1957 authors to samples of 10 or 13 cases when, in fact, 18 to 21 different cases are noted at various points in the discussion. The timing of the emergence of a security community is often given fairly loosely and it is not always clear whether the authors are simply guessing or whether the authors actually had specific evidence related to their two indicators for crossing the integrated threshold. In any event, no specific evidence is provided to the reader. In one case (Britain–Denmark), two different beginning dates are offered in different parts of the study. Another case is described as the only community failure discovered (Austria-Prussia before 1879) even though it was ruled out explicitly earlier as a pluralistic security community. Late in the 1957 book, it is noted that the BENELUX states are at the same level of integration as Scandinavia, which suggests that we might introduce a few more dyads (Luxembourg–Netherlands, Belgium–Luxembourg) to table 1.2. Moreover, we really have no idea whether some possible dyads were considered initially for inclusion and then rejected for a lack of evidence. Nor do we have much if any information on unsuccessful attempts at security community formation—and therefore no variation on the dependent variable. In sum, there is no reason to assume that these cases are somehow carved in stone as pluralistic security communities.
11. The 1870–1913 major power analyses are reported in Choucri and North (1975). See also Choucri, North, and Yamakage (1992).
12. Norway was not an independent actor in the nineteenth century.
13. The Scandinavian transformations are reviewed in chapter three.
14. A democratic outcome need not be the case. Autocratic decision-makers have chosen to emphasize economic development over international conflict temporarily in order to improve their resource base for subsequent competition.
15. Obviously, this tendency is influenced by countervailing tendencies to rely on autocratic allies in controlling peripheral areas, especially during periods of ideological struggle as in the Cold War.

16. At the same time, we make no effort to deal with all of the puzzles associated with the democratic peace literature. One in particular that we evade in this volume, because we are not totally convinced that it is a genuine puzzle, is the argument that new democracies are especially at risk for becoming engaged in warfare. See Mansfield and Snyder (1995, 2002), Wolf (1996), Enterline (1996), Thompson and Tucker (1997a, 1997b), and Gleditsch and Ward (2000).

Chapter Two

1. See Russett (1993), Ray (1995), Chan (1997), and Russett and Starr (2000).
2. See Layne (1994), Midlarsky (1995), Gates, Knutson, and Moses (1996), Thompson (1996), and Chan (1997).
3. For discussions of critical examinations of the empirical challenges to the democratic peace, see Thompson and Tucker (1997a, 1997b), Maoz (1998), and Russett and Starr (2000).
4. The discussion of the war making–state making literature is based on Hintze (1975), Finer (1975), Tilly (1975, 1990), Gurr (1988), Rasler and Thompson (1989), Almond (1990), and Mann (1993).
5. A variation on this argument appears as chapter three in the book.
6. Modelski and Gardner (1991, 2002), Midlarsky (1995), Mitchell, Gates, and Hegre (1999), James, Solberg, and Wolfson (1999), Crescenzi and Enterline (1999), Mousseau and Shi (1999), and Oneal and Russett (2000).
7. Oneal and Russett (2000) respond specifically to James, Solberg, and Wolfson (1999).
8. These studies focused on here do not exhaust the pertinent quantitative literature by any means. See, for instance, Reiter (2001), Li and Reuveny (2003), and Colaresi and Thompson (2003). None of these studies, however, preclude the need for the type of examination to be carried out in this chapter.
9. See Rasler and Thompson (1983, 1985, 1989) and Thompson and Rasler (1999).
10. Among other, see Lipset (1959, 1994), Moore (1966), Rustow (1970), Dahl (1971), Huntington (1991), Hadenius (1992), Rueschemeyer, Stephens, and Stephens (1992), and Vanhanen (1997).
11. See Cutright (1963), Cutright and Wiley (1969), Jackman (1973), Arat (1988), Gasiorowski (1988, 1995), Remmer (1990), Burkhart and Lewis-Beck (1994), Huntington (1996), Londregan and Poole (1996), Przeworksi, Alvarez, Cheibub, and Limongi (1996), Leblang (1997), Prezeworski and Limongi (1997), and Gasiorowski and Power (1998).
12. The democratic peace literature is actually divided on the question of monadic versus dyadic effects, as highlighted in chapter six. There had been a consensus through the mid-1990s that democracies were as belligerent as autocracies on a monadic level. That consensus seems to be eroding with a reemergence of an emphasis on monadic effects, based on more recent empirical findings, that refer to more specific types of behavior than simply general conflict frequencies. See Russett and Starr (2000) for a discussion of this swing in attitudes.
13. For specific major power dating, see the appendix.
14. On democratization waves, see, for instance, Huntington (1991) and Markoff (1996).
15. The rivalry database utilized in this study is described in Thompson (2001). Rivals are identified as competitive, threatening enemies as perceived by state decision-makers. Data for this conceptualization were gleaned from a comprehensive survey of pertinent material reported in political histories addressing who decision-makers perceived to be their principal rivals and when they were perceived as such.
16. For instance, Huntington (1991), Starr (1991), Maoz (1996) Gleditsch and Hegre (1997), and Gleditsch and Ward (2000).
17. Military personnel data were taken from the Correlates of War Project's National Capabilities data set.
18. The construction of the external threat index was inspired by Doyle's (1997) illustrative analysis of balance of power politics. After completing our analyses, we discovered that Bennett (1998) and Bennett and Nordstrom (2000) also does something similar although he includes information on relative capabilities of the threatener and threatened and their alliances. His rivalry measure is also based on militarized dispute frequency instead of decision-maker perceptions (as is ours).
19. Each observation takes the form of aggregation and averaging for the period 1850–59, 1860–69, through 1970–79.
20. These data are described in the appendix.
21. The MIDs data are described in the appendix.

22. (1) Domestic Resource Concentration$_t$ = α + β_1 (Changes in Threat)$_t$ + β_2 (Domestic Resource Concentration)$_{t-1}$ + error$_t$; (2) Democracy$_t$ = α + β_1 (Threat)$_t$ + β_2 (Changes in Domestic Resource Concentration)$_t$ + β_3 (War)$_t$ + β_4 (Democracy)$_{t-1}$ + error$_t$; (3) Disputes$_t$ = α + β_1 (Threat)$_t$ + β_2 (Domestic Resource Concentration)$_t$ + β_3 (Democracy)$_t$ + β_4 (War)$_t$ + β_5 (Disputes)$_{t-1}$ + error$_t$. We rely on a pooled, cross-sectional time-series (PCTS) design. Although there are several approaches to modeling PCTS data in order to reduce heteroskedascity and autocorrelation (e.g., feasible generalized least squares, or White's (1980) robust regression), we use ordinary least squares with panel corrected standard errors (Beck and Katz, 1995) to estimate our results. Beck and Katz (1995, 1996) demonstrate that standard errors estimated with a variation of White's (1980) robust-standard errors technique for panel data (or more commonly called panel corrected standard errors) are more accurate and efficient than standard errors calculated by generalized least squares approaches that estimate the error process with an AR1 model. In their 1996 study they also find that panel corrected standard errors are more accurate and efficient than White's heteroskedasticity in PCTS designs. Beck and Katz (1996) maintain that White's robust regression produces standard errors that are almost identical to panel corrected standard errors in the presence of heteroskedasticity but are markedly inferior in the presence of correlated errors. Even so, we estimated our models with both robust regression and the panel corrected standard error approach and found that the results were very similar. Although the standard errors were significantly smaller with the Beck and Katz variation of White's technique, the statistical significance of the coefficients were the same. In addition, we introduce a lagged dependent variable in our models to control for the effects of autocorrelation. Stationarity problems force us, occasionally, to substitute first difference scores for levels.
23. See Beck and Katz (1996).
24. (4) Change in Democracy$_t$ = α + β_1 (Change in Threat)$_t$ + β_2 (Threat)$_{t-1}$ + β_3 (Democracy)$_{t-1}$ + β_4 (War)$_t$ + error$_t$. Prior to estimating our regression model, we conducted unit root tests of stationarity on democracy and threat. The results show that these variables are stationary and do not require cointegration-based error correction models. Thus, we employ the same PCTS design used for the first three equations (ordinary least squares with panel corrected standard errors and lagged dependent variables to control for autocorrelation). Outliers were identified by added-variable plots as well as DFBETAs. The following dummy variables were introduced to control for specific cases: France, 1816; Germany, 1816; Russia, 1816, and China, 1816. These cases emerged as outliers when threat was first differenced.
25. The fifth model [log (Dispute)$_t$ = α + β_1 (Threat)$_t$ + β_2 (Democracy)$_t$ + β_3 (Peace Years)$_t$ + β_4 (War)$_t$ + β_5 (Disputes)$_{t-1}$ + error$_t$] uses time series, cross-sectional data with a binary dependent variable using logit. To deal with some of the possible statistical problems related to the nature of the design, we incorporate two analytical checks suggested by Beck, Katz, and Tucker (1998): (1) a duration variable—the number of peaceful years between disputes—and (2) an estimation of Huber's robust standard errors for the coefficients in order to reduce the effects of any heteroskedasticity. The peace years duration variable and the three cubic spline segments were derived from Richard Tucker's (1999) program. A spline variable is introduced in order to control for the possibility that binary dependent variables can exhibit temporal dependence. The correlation between disputes and the independent variables (threat and democracy) could be influenced by the extent to which the duration of periods with nondisputes is time dependent. One solution is to introduce temporal dummy variables via splines to control for this influence. Beck, Katz, and Tucker (1998) provide a more comprehensive treatment of this remedy.
26. Dummy variables were introduced for the following outliers in model 1 only: Austria, 1930–39 and Germany, 1930–39.
27. Residuals estimated from the regression models were examined for any remaining effects of autocorrelation, and the tests (e.g., the Ljung-Box Q statistic and the Breusch-Godfrey and Arch F-statistics) indicated that there is no significant residual autocorrelation.
28. Outliers were identified by *L–R* (leverage-versus-squared-residual) plots and dummy variables were introduced for specific cases: United States, 1960; Russia, 1910, 1960; and Japan, 1930 for models 1–3 in table 2.4. The sample size variation between models 1 and models 2/3 is the result of missing data values for Germany prior to 1870 and Italy prior to 1860 for the Vanhanen democracy index and the power concentration variable.
29. We looked at the reverse relationship for table 2.4 (the pooled cross-sectional time series of disputes on threat, democracy, and domestic power concentration). The time series regressions indicate

disputes as the independent variable fails to predict significantly to the two indicators of democracy or to domestic power concentration. We also conducted Granger causality tests between militarized disputes and threat in order to establish whether this relationship was unidirectional or bidirectional. The Granger tests showed that lagged values of threat predicted disputes significantly, but not the other way around. We have too few observations to employ Granger tests for the modeling involving domestic resource concentration.

30. It is possible that other lag structures could characterize the data analyzed for table 2.5's reported findings. We estimated the equation varying the lag structures for the short-term variables while maintaining the present lag structure for the levels variables. We find that the short-term relationships are significant at 0, 1, or 3 lags, while the long-term variables remain statistically significant. We also varied the lag structure of the long-term or change variables. Again, we find that long-term threat is significant at 0, 1, and 3 lags. Our results appear quite robust.
31. Colaresi and Thompson (2003) note that arguments and models about democratization, at best, restrict their external components to the idea that the external environment is conducive to the extent that international institutions and existing democracies encourage the development of more democracies. Our argument, of course, is that conducive external environments encompass much more than sympathetic institutions.

Chapter Three

1. See Hintze (1975), Finer (1975), Rasler and Thompson (1989), Tilly (1990), and Mann (1993). For a compatible argument, see, as well Gurr (1988).
2. The strong major power bias in European warfare is clearly suggested in Wright (1942/65), appendix 19.
3. See, among others, Moore (1966), Anderson (1974), Huntington (1991), and Reuschmeyer, Stephens, and Stephens (1992).
4. The years of warfare have been culled from Hovde (1943), Arneson (1949), Lisk (1967), Dupuy and Dupuy (1977), Barton (1986), and Fitzmaurice (1992).
5. However, Dovring (1978: 15) notes that Denmark was characterized by the most unequal distribution of wealth in Scandinavia, not unlike the rest of Europe, until reforms were initiated in the 1780s to improve agrarian productivity.
6. The nonlinear oscillations of Danish and Swedish internal and external politics between 1300 and 1900 suggest that Downing's (1992) thesis that democracy was dependent on medieval constitutions surviving the state-making distortions brought about by seventeenth-century war making overlooks a great deal of behavior with explanatory significance.
7. For a brief summary of the major explanatory approaches, see Mann (1993: 167).
8. About 24 percent of all taxes raised reached the national government according to Mann (1993: 180).
9. See Skocpol and Kestnbaum (1990: 17) and Tilly (1993: 167).
10. The long French decline is discussed and measured in Rasler and Thompson (1994, chapter 2).
11. Theda Skocpol (1979: 181) argues that this is what many wealthy French elites had in mind.
12. See Blanning (1986: 77–79, 89), Bosher (1988: 184), Black (1994: 42).
13. This argument is promoted by Doyle (1989: 424) and Black (1994: 518).
14. Other motivations (Blanning, 1986: 105–07; Bosher, 1988: 176–81; Porter, 1994: 127) are said to have included a chance to eliminate the monarchy, demonstrating the permanence of the new regime, gaining external credibility for France's paper money, restoring the exchange rate to earlier levels, and, at a slightly later point, to settle accounts with Austria, which was blamed for much of France's decline since 1756.
15. On this point, see Blanning (1986: 107; Skocpol and Kestnbaum, 1990: 19; Bosher, 1988: 78).
16. This focus on France and Austria risks ignoring the developments in other great powers that made some type of major power war likely. See Blanning (1986), Doyle (1989: 198), and Black (1994: 532).
17. Geoffrey Best (1982: 92) refers to this development as a form of war addiction.
18. This position is advanced in Skocpol (1979: 185), Best (1982: 97), and Bosher (1988: 239). Goldstone (1991: 479–80) generalizes that when absolutist regimes are overthrown, they are most likely to be replaced by more efficient and more populist types of dictatorships unless the state is also defeated in war.

19. In the late eighteenth and early nineteenth centuries, United States decision-makers, after some spirited debate, chose not to build a competitive navy for fear that it might invite a British preemptive strike.
20. This process is discussed in greater detail in Rasler and Thompson (1994: chapters one–five).
21. This shift was also facilitated by the realization on the part of the English that the French were a greater threat than the Dutch.
22. On the continuity of this theme, see Samuels (1994).
23. However, the 1927 Shantung military intervention was authorized by a prime minister who was also head of the majority party at the time (see Choucri, North, and Yamakage, 1992: 126). After 1932 and the end of party control, both of the major parties announced their support for military expansion in China (Berger, 1977: 354).
24. Reischauer (1964: 156) exaggerated when he called the Taisho democratization period as a "runaway liberal movement of the urban middle classes."
25. See Crowley (1966), Kato (1970: 220), Berger (1977), and Hata (1988: 274).
26. The navy compounded this problem by also overextending their defense perimeter in the western Pacific (Willmott, 1982: 435–60).
27. See as well, Crowley (1966), Iriye (1970), and Silberman (1970).
28. Alliance with Britain had been viewed as part of the grand strategic solution to the problem of being vulnerable to an attack from the sea while pursuing continental interests (Crowley, 1966: 4).
29. This topic is discussed at some length in Crowley (1966), Duus (1976: 252), and Barnhart (1987).
30. Berger (1977: 52) emphasizes that party politics were greatly in disarray. The identity of the majority party had just changed and there was considerable disagreement within it as to who should lead the party.
31. See Duus (1968: 250) and Kato (1970: 217, 236).
32. On this point, see Berger (1977: 4). Scalapino's (1962: 216) critique emphasizes the role of the Zaibatsu's close dependence on the government as a function of Japan's late development.
33. There is of course some irony in the fact that the attacks on the government in power were led by the major opposition party seeking to displace it.

Chapter Four

1. The war initiation information is revised and updated through 1997 at http://cow2.la.psu.edu. See as well Sarkees, Wayman, and Singer (2003).
2. As described in the appendix, we subtract the Polity III autocracy score from the democracy score and utilize 6 as a threshold for a democratic political system.
3. In a few war cases, no single side is credited with initiation. These cases have been deleted from the analysis.
4. Since domestic processes certainly can encourage the maintenance of rivalry relationships, strategic rivalry can also be viewed as an internal path-dependency.
5. See Albrecht–Carrie (1970), Bullen (1974), and Pinkey (1986).
6. For an early international relations effort to examine some of the implications of French decline, see Doran (1971).
7. French pursuits of more aggressive foreign policies in Spain (1823) and Italy (1832) were undertaken with little significant great power opposition. Spain was conceded as France's sole sphere of influence within Europe and the Austrians were not in a position to oppose the 1832 Ancona expedition.
8. In 1849 France sent an expeditionary force to Rome to restore Pius IX who had been overthrown by the new Roman republic. This war may represent the closest two democracies or liberal republics have come to actually fighting one another. One of the reasons it is not counted as an inter-democratic affair is that the Roman Republic did not last long. There is also an irony implicit in the usual explanation offered for the French intervention: Louis Napoleon was paying back an electoral debt to clerical supporters stemming from his December 1848 election.
9. The initial instincts of the Orleans monarchy was to withdraw the Algerian operation that it had inherited from its predecessors. The combat in North Africa represented a drain on troops available for the defense of France in Europe and also increased British suspicions about French intentions. However, as noted in Pinkney (1986: 141), in 1830 it would have been politically imprudent to

relinquish the first French conquest since the Napoleonic era. It also helped that it did not seem to raise the probability of a war in Europe.

10. French diplomatic feelers were extended to the conservative monarchies in 1836 and 1848 in search of an alternative to British support. In 1848, there was some possibility of a French-Austrian intervention in the Swiss civil war on behalf of the conservative, Catholic cantons as discussed in Bullen (1974) and Woolf (1979: 295). If the French had initiated this intervention, it might have constituted another approximation of a democratic–democratic clash. However, Switzerland had to fight a civil war in part to become democratic and Britain successfully discouraged the intervention of the other great powers.
11. British decision-makers preferred a situation in which French hegemonic ambitions could be contained while not leaving France so weak that Russian ambitions for European expansion might be encouraged. Nevertheless, British and French preferences collided in all sorts of places between Spain and Tahiti.
12. It is not clear how far the French were prepared to push the Belgian crisis, but Bullen (1974: 6) indicates that there was some expectation of war on the British side.
13. For instance, Magraw (1986: 71) and Pinkney (1986: 131).
14. Pinkney (1986: 132) suggests that the 1840 crisis had one byproduct in arousing German fear of the possibility of French expansion in Europe, thereby renewing aspirations for German unification.
15. Ray (1994) gives greater credence to the potential for war in the Fashoda crisis than does the present analysis. However, he too views the crisis outcome as having more to do with capability calculations than democratic regimes. See, as well, Russett's (1993: 7–9) treatment of the crisis in which he downplays the role of democratic norms in the crisis bargaining but emphasizes the postcrisis invocation of shared norms.
16. See Clogg (1979: 70) and Papacosma (1977: 11).
17. Italy had the misfortune of joining the great power ranks just as the entry costs, thanks to industrialization, were escalating the costs of participation in the system's elite ranks. The extraordinary dualism of the Italian economy hardly helped matters either.
18. Both Piedmont and the Kingdom of the Two Sicilies are reported to have had early interests in Libyan and Ethiopian colonial efforts (Bosworth, 1979: 135).
19. Doyle (1986: 1156) argues that liberal republics will tend to align against authoritarian opponents in world wars. More arguments or disagreements on the relationship between regime type and alliance formation include Siverson and Emmons (1991), Simon and Gartzke (1996), Thompson and Tucker (1997a), and Reiter and Stam (2002). We return to this question in chapter nine.
20. Although Lyttleton (1987: 6) does not utilize path-dependency conceptualization, his discussion of the initial development of the Italian state matches the conceptualization closely: ". . . the early years of the new state created a pattern of institutions and practices which were hard to change, and which often acted as a brake on further development."
21. See Barclay (1973: 130–31), Kershaw (1989), and Griffin (1993).
22. A more detailed look at the fascism issue is best left to a discussion of Weimar Germany and Central Europe in the interwar years, with Italy leading the way.
23. In the late 1920s, Italy made a conscious effort to work within the international economic status quo to encourage the inflow of foreign capital. The policy changed only after the external environment was changed radically by the advent of depression (Lyttleton, 1986: 441).
24. According to Magraw (1986: 210), Bismarck preferred a moderate republic in France. A more radical regime would constitute a revolutionary threat. A Catholic monarchy might lead to an anti-German alliance with Austria. For a more systematic study of the relationships between wars and regime change, see Bueno de Mesquita, Siverson, and Woller (1992: 638–46).
25. There may be some analytical tension here between long-term (external pressures and path-dependencies) and short-term (decision-making) perspectives. Yet institutional constraints and democratic norms presumably are also long-term factors except, perhaps, in the case of intermittent democracies in which certainly constraints and possibly norms could fluctuate with changes in regime type. The problem is that the constraints and the norms (along with external pressures and path-dependencies) remain hypothetical as systematic influences.
26. Russett (1995: 167) suggests that "Neither an unfavorable strategic cost-benefit evaluation nor shared democracy is a necessary condition for avoiding war. But, allowing for some possibility of irrationality or misconception, either may well constitute a virtually sufficient condition."

Our point is that while he may be right, we do not yet know enough about how these variables work together to speak of necessary and sufficient conditions. Unfortunately, we also currently lack the theoretical foundation that would encourage us to assess their combined effects on war avoidance or initiation.

Chapter Five

1. Analysts are beginning to undertake such analyses. See, for example, Oneal, Oneal, Maoz, and Russett (1996), Oneal and Russett (1997), Russett, Oneal, and Davis (1998), and Russett and Oneal (2001). So far, though, these studies, and earlier ones, have focused on seeing whether the statistical significance of the regime type variable survives the examination of a number of control variables or whether other dimensions of the Kantian package add explanatory power. Oneal and Russett (1999) is an interesting systemic exception to this generalization in the sense that they integrate several "hegemonic" variables with their Kantian focus. Somewhat along similar lines, albeit with a focus on different variables and different level of analysis, we want to see how regime type does in the context of an alternative explanation of pacific tendencies. We return to the question of the comparison to "hegemonic" variables in chapter nine.
2. See, for instance, Cohen (1994), Oren and Hays (1997), and Kacowicz (1997).
3. See, as well, Gates, Knutsen, and Moses (1996).
4. See Cohen (1994), Farber and Gowa (1995, 1997a, 1997b), and Gowa (1999).
5. See Thompson and Tucker (1997a, 1997b) and Maoz (1998).
6. See Modelski (1999), Modelski and Thompson (1999), and Thompson (2000).
7. Some analysts use contiguity as an alternative filter to reduce noise but such an approach is not appropriate for major powers that are less bound by geographic constraints.
8. To deal with possible statistical problems related to the nature of the design, we incorporate two analytical checks suggested by Beck, Katz, and Tucker (1998): (1) a duration variable (the number of peaceful years between disputes) with three cubic spline segments to mitigate the influence of autocorrelation and (2) an estimation of Huber's robust standard errors for the coefficients in order to reduce the effects of any heteroskedasticity. The peace years duration variable and the three cubic spline segments were derived from Richard Tucker's (1999) program. The general model regressing dispute occurrence on regime, rivalry, and peace years is:

$$\text{Log (Dispute)} = a + B_1\,(\text{Regime Type}) + B_2\,(\text{Rivalry}) + B_3\,(\text{Peace Years}) + B_4\,(\text{Spline 1}) + B_5\,(\text{Spline 2}) + B_6\,(\text{Spline 3}).$$

9. 2.13 versus 1.72; 3.45 versus 2.93; 2.46 versus 1.75 in the 1816–1992 era and 1.82 versus 1.22; 3.41 versus 1.82; and 2.15 versus 1.21 in the 1816–1945 period.
10. See, for instance, Small and Singer (1976), Rummel (1983, 1985), Weede (1984), Chan (1984), Maoz and Abdolai (1989), Benoit (1996), Gleditsch and Hegre (1997), Gledtisch and Ward (1998), and Leeds and Davis (1999).
11. We look more closely at this question in chapter nine.
12. See Maoz and Abdolai (1989), Crescenzi and Enterline (1999).
13. Specifically, the resumption of rivalries between Russia and China, Russia and Japan, and China and Japan are certainly conceivable, as are a resumption of strategic rivalries between the United States and some, if not all, of the China–Russia–Japan triad. The Sino-Indian rivalry persists and could be upgraded in status if India's minor power status is altered in the twenty-first century.
14. This index continues the practice of subtracting the 0–10 autocracy scale from the 0–10 democracy scale but without worrying about a threshold value.
15. This aggregation problem is similar with other variables such as alliances, wars, and international organizations. How many Arab Leagues equal a NATO? How many Schleswig–Holstein wars approximate a World War II? How many Red Crosses does it take to match the impact of the United Nations? It probably also applies to democracies, as well, if one accepts the argument that the regime type of the system leader is critical in fostering the diffusion of that regime type throughout the system. Then, too, a democracy with a population of 250 million is not quite the same entity as a democracy with a population of 10 million. If nothing else, aggregation involves some risk of subsequent problems of interpretation.
16. The same could be said about regime type.

17. Many explanations for why some major power rivalries are more peaceful than others can be found in the articles in Kegley (1991) and especially Singer (1991) who offers some 14. See, as well, Thompson (1995), Vasquez (1993, 1996), Geller and Singer (1998: 68–96) on war-prone dyads, and many of the rivalry papers found in Diehl (1997) and Thompson (1999).

Chapter Six

1. Bueno de Mesquita (1981), Small and Singer (1982), Wang and Ray (1994), Gartner and Siverson (1996).
2. Lake (1992), Bueno de Mesquita and Siverson (1995), Stam (1996), Bennett and Stam (1996).
3. Small and Singer (1970, 1982), Rosen (1972), Blainey (1973), Canizzo (1980), Organski and Kugler (1978), Wayman et al. (1983), Arquila (1992), Wang and Ray (1994), Stam (1996), Reiter and Stam (1997).
4. For different perspectives on this controversy, see Bueno de Mesquita, Morrow, Siverson, and Smith (1999), Reed and Clark (2000), Desch (2002, 2003), Reiter and Stam (2002, 2003), Choi (2003), Lake (2003), and Schulz and Weingast (2003).
5. Preferences aside, we check empirically to see whether this design decision influences our outcomes unduly.
6. On electoral costs, see Mueller (1973), Stein (1980), Stein and Russett (1980), Cotton (1986), Russett (1990), Bueno de Mesquita, Siverson, and Woller (1992), Thompson (1995), and Bueno de Mesquita and Siverson (1995).
7. See Small and Singer (1982) and Singer (1991) for details. The COW war list has undergone some revision in recent years. We have chosen to focus on the earlier published lists of interstate wars, and other codings as in the case of initiation information, on the presumption that the published information constitutes a relatively stable core of information on the outbreak and conclusion of warfare. There may be some room for transferring one or two extra-systemic wars to the interstate column and/or altering the dates of a few cases of ongoing warfare and national participation. Nevertheless, we assume that the changes are unlikely to be extensive enough to alter our findings.
8. Small and Singer (1982: 182) coded the Korean War (1950–53) and the Egyptian–Israeli War of Attrition (1969–70) as cases without obvious winners. We have added two more cases—the Iran–Iraq War (1980–88) and the Sino-Vietnamese War (1985–87).
9. Initiators are identified for all cases except the Sino-Vietnamese War (1985–87) on which we could find little useful information.
10. The 1956 Sinai War is one obvious exception to this rule.
11. Nine wars involved mixed coalitions of different political systems on one or both sides: Roman Republic (1849), Boxer Rebellion (1900), First Balkan (1912–13), Second Balkan (1913), World War I (1914–18), Hungarian Allies (1919), World War II (1939–45), Korean War (1950–53), and the Vietnamese War (1965–75). In addition, we intervened twice to alter the Polity III information in situations involving changes of government that precipitated wars but were not reflected in the Polity III database. In the 1849 Roman Republic case, it was clear that the insurgents were more democratic than the threatened Papal system, but we were unsure what score to assign in terms of the two Polity institutional scales and/or whether the net score would surpass the democratic threshold of 6. Therefore, we treated the losing side's regime type in this case essentially as missing data. In the 1974 Turkish Cypriot case, the democratic Cypriot regime was overthrown by a coup that led to the Turkish attack. We coded the Cypriot losing side's regime type as an autocracy and assigned it an arbitrary −5 score for a later analysis involving interval regime scores.
12. The COW composite capability index aggregates military personnel, military expenditures, energy consumption, iron/steel production, total population, and urban population by first calculating war participant shares for each type of capability and then aggregating and averaging the share scores. In cases of missing data on specific types of capability, we simply relied on the data that were available in the COW national capabilities data set that contains information through 1985.
13. There is already some disagreement in the literature on war initiation, discussed in Wang and Ray (1994), as to whether the initiator's edge in winning is consistent over time.
14. None of our independent variables are intercorrelated at a level higher than 0.5 for any of the various time periods that are examined with the predictable exception of the nominal measure of regime type and an interval measure of regime type that is introduced at a later point in the analysis.

15. The four major cases are the Russo-Hungarian (1956), Sinai (1956), Sino-Indian (1962), and the Sino-Vietnamese (1979) wars. Other wars involving major power participation have been excluded either because the outcome was ambiguous or because a coalition mixed different types of political systems (as in the case of the Korean and Vietnamese wars).
16. In the Duval and Thompson (1981) study, the question was whether small states were more or less likely to become involved in conflict than large states. Israel was found to account for approximately one half of the small state conflict in the sample. With Israel in the sample, small states were more likely to engage in conflict than large states. Without Israel in the sample, there was no conflict propensity. There are several ways to interpret this type of problem but, minimally, if the presence or absence of one state changes the outcome, the findings are hardly robust.
17. This question is actually more complicated. See, among others, Gardner and Siverson (1996) and Rasler and Thompson (1999) on the predatory advantages of initiating wars against isolated targets. Regime type simply does not matter much in terms of who initiates and who is attacked.

Chapter Seven

1. Russett and Starr (2000) advance several other reasons for the disagreement. They suggest that the monadic effect is probably weaker than the dyadic effect and undoubtedly masked to some extent by selection effects. They also criticize previous analysts for failing to think in multivariate terms and stressing conflict initiation over the identity of conflict recipients. While we tend to agree with these observations, only the last issue will be pursued explicitly in our analysis.
2. However, see Gleditsch and Hegre (1997).
3. See, as well, Mazower (1998) who describes the 1917–89 European experience as one of convulsive political transformation focused around an era of ideological rivalries.
4. Logically, if democracies do not fight other democracies and they are equally belligerent as autocracies, democracies must fight autocracies disproportionately. But, we hope to say a bit more than just this logical conclusion.
5. Booker (1995) surveys some of this variety.
6. See Organski and Kugler (1980), Lemke and Reed (1996), Rousseau, Gelpi, Reiter, and Huth (1996), and Kacowicz (1998).
7. See Maoz (1996, 1998), Thompson (1996), and Gleditsch and Ward (2000).
8. Examples include Gleditsch and Hegre (1997), Oneal and Ray (1997), Oneal and Russett (1997), and Werner and Lemke (1997).
9. The latest trend in realist studies, as reviewed in Rasler and Thompson (2001b), is to move away from the neorealist stress on general insecurity and to return to an emphasis on the threats posed by challengers of the status quo.
10. Although the Sino-Russian rivalry is quite old, it was renewed in the Cold War third wave after a brief period of Chinese–Russian alignment against the United States.
11. A good example of evolutionary tendencies is found in Cederman (2001).

Chapter Eight

1. For more on this topic, see Thompson (1992) and Rasler and Thompson (1994, 2000, 2001b).
2. An example is Crescenzi and Enterline's (1999) examination even though their findings are certainly more complex than the straightforward, bivariate argument might anticipate.
3. This observation is not quite the same as Quincy Wright's comment about increased sheep noted in chapter one, but it is not incompatible with the older line of thought.
4. Doyle (1999) provides support for this argument.
5. Bueno de Mesquita and Lalman (1988) argued for the superiority of dyadic analyses over systemic examinations some time ago.
6. Militarized interstate disputes have been used before in examinations of systemic arguments with mixed outcomes. See Cupitt, Whitlock, and Whitlock (1993) and Pollins (1996).
7. Russett once agreed with this observation. In Russett (1985: 211), he argued that ". . . the United Kingdom was *never*, even at its peak in the 19th century, the dominant power as measured by either GNP or military expenditures. The wealth provided by its industrial strength was always overwhelmed in terms of GNP by the demographic base of its sometimes less wealthy but more

populous chief competitors, its military expenditures were always markedly below one or more of its continental rivals. Only in manufacturing production, and then only rather briefly, did it lead the world." We would add that it also led in sea power. More generally, problems with relying on the COW composite capability measure for questions involving structural change are discussed in Thompson (1983). However, the greatest structural "distortion" associated with this measure occurs in the nineteenth century than the period examined by Oneal and Russett.

8. The dissynchronization model represents a fusion of leadership long cycle and Dehio (1962) arguments. See Thompson (1992) and Rasler and Thompson (1994, 2000, 2001b).
9. Rasler and Thompson (2001b) examine the relationship between the extent of initial war aims and global–regional structural dissynchronization for the 1494–1945 period. Initial war aims were more likely to involve regional hegemony when power concentration in the primary region was greater than the level of global concentration. The implication is that system structure encourages more extensive war aims.
10. Earlier examinations of global–regional dissynchronization (Thompson, 1992; Rasler and Thompson, 1994, 2001) have used different indexes. The basic choices have revolved around measuring concentration, either in terms of the leader's share or an index that uses information on all actors' shares, and whether global and regional concentration are examined separately or in a "gap" measure that involves subtracting regional concentration from global concentration.
11. We employ the Maoz version (http://www.spirit.tau.ac.il/~zeevmaoz) of the Correlates of War MIDs data (version 2.1) in which all dispute cases (sometimes with multiple participants) have been converted to focus on the dyadic participants. For a discussion of militarized disputes, see Jones, Bremer, and Singer (1996).
12. As in other chapters, we use the conventional Correlates of War major power conventions although we continue to balk at bestowing that status on Japan and Germany in the early 1990s.
13. The Ray and Singer concentration index is based on the following algorithm. One first calculates the sum of each actor's percentage share squared, subtracts $1/N$, then divides by $1-1/N$, and finally calculates the square root of the product of the first three operations.
14. Global powers must demonstrate a 10 percent share of global reach, naval capabilities, and naval activity in more than one regional sea.
15. The nature of the sea power index for the 1816–1992 period is complicated by a series of technological changes. Between 1816 and 1860, shares of first class ships-of-the line (60 + guns) are counted and averaged with shares of naval expenditures. Between 1861 and 1879, shares of capital ships are counted and averaged with shares of naval expenditures. Between 1880 and 1945, shares of first class battleships are counted and averaged with shares of naval expenditures. From 1946 to 1959, only the distribution of heavy attack aircraft carriers is counted. After 1960, aircraft carriers are counted in addition to nuclear attack submarines and indices for sea-based missile destructiveness (EMT) and accuracy (CEP). The shares of each of the four categories are averaged on an annual basis.
16. This observation does not rule out the expansion of German war aims during the war. The most sophisticated interpretation of initial German war aims in World War I is Levy (1990–91).
17. The non–western European resources of the United States and the Soviet Union decided the contest at the expense of the Axis Powers.
18. One way to look at the most recent changes in Eurasian concentration is that Russian, British, and French army sizes are being downsized faster than the Chinese army size.
19. The Polity III data on regime type are described in Jagger and Gurr (1995) and our appendix.
20. We exclude a consideration of other variables such as economic interdependence and international organizations from this analysis for several reasons. We suspect that at the systemic level in particular it is important to distinguish between dyadic and systemic sources of interdependence. Pursuing this tangent at this time theoretically and empirically would require some additional space and distract attention from our central focus. It makes more sense to treat this question separately in a future analysis. Oneal and Russett's (1999) outcome for the influence of international organizations was decidedly mixed. One problem may be that while the number of international organizations can certainly be aggregated, the contemporary exponential increase in the number of organizations does not exactly accord with their substantive importance for systemic conflict. This also is a problem that deserves more scrutiny. Oneal and Russett (1999) also looked at a measure of status quo dissatisfaction based on convergence in alliance memberships. They did not find this variable statistically significant but it also possesses interpretation problems. At the very least, the major power inclination to join alliances has not been constant over the nineteenth and twentieth centuries.

One measure that Oneal and Russett did find to be significant was a tension index based on the size of hegemonic defense budgets. We examined this measure in our own equations and found it to be highly correlated with our controls for World Wars I and II. Therefore, we excluded it from the analysis to avoid collinearity problems and because it did not address an issue that was not already being addressed by the dissynchronization indices.

21. The results summarized in table 8.3 are based on a nonlinear time series regression. The variables exhibit long term trend (1816–1992 era only) and autocorrelation problems. The significant autocorrelation coefficients suggest that current values of *Y* are influenced by its past (3 years in 1816–1992 and first and third years in the 1816–1945 period). The significant trend variable in the 1816–1992 era indicated that the *X* and *Y* variables are characterized by a weak tendency toward positive incremental change. The immediate question for our analysis is whether democratization and the concentration variables predict significantly to disputes after controlling for the effects of these time-dependent processes.
22. There has been some debate over whether the democratic peace is genuine or attributable to a spurious relationship. Part of this debate centers on whether democratic dyads predict less conflict equally well before and after 1945. See, for instance, Cohen (1994), Farber and Gowa (1995, 1997), Thompson and Tucker (1997), and Gowa (1999). Almost everyone agrees that democratic variables do better in the second half of the twentieth century than at earlier times. We agree with Cederman (2001) that these findings are best viewed as the increasing strength of an emergent phenomenon—as opposed to assuming that key relationships should necessarily be of equal strength in all time periods.
23. One other threat to the validity of these findings is the argument (Ray, 2000: 312) that summarizing conflict at the systemic (or subsystemic) level lumps together too many different kinds of conflict (e.g., democratic, mixed, and autocratic) to be meaningful. We do not think that is the case. We considered rerunning our equations to focus only on autocratic–democratic conflict but discovered that the frequency of mixed conflict was too highly correlated with total conflict ($r = 0.93$) to expect any different outcomes.

Chapter Nine

1. Note Wright emphasized long wars—presumably with world wars in mind—as opposed to the wars in general examined in chapter six.
2. These propensities have taken time to emerge. The last three global wars (the French Revolutionary–Napoleonic Wars and World Wars I and II) have had increasingly strong ideological flavors focused on regime type. The preference for free trade began to appear in the Dutch seventeenth century but was not put into practice before the mid-nineteenth century.
3. The opening of the Spanish colonies in the Americas (or at least access to them) was one of the major issues fought over by the French and British from the early eighteenth through the early nineteenth centuries.
4. See the empirical evidence demonstrated in Reuveny and Thompson (2001), which shows that world economic growth is driven by system leader growth and that southern democracies are encouraged by world economic growth.
5. These war initiation numbers were first discussed in chapter four. The source for assigning initiator status to war participants is Small and Singer (1982: 196–97) as revised and updated at http://www.cow2.la.psu.edu
6. This was the case at least through Polity III. Britain's democratic status, apparently, has been moved back several decades in subsequent revisions to the Polity data.
7. Figure 9.1 is somewhat stylized. To take advantage of the most recent information on democratization, we use Modelski and Gardner's (2002) data on the number of democratic states at decadal intervals through 2000, based on Polity IV information. For the number of states in the system, we use information on state system membership (v2002.1) that is available through 2002 at the Correlates of War 2 Project (http://cow2.la.psu.edu). The former series is decadal in form while the latter is annual. To make these series compatible, states that enter the system between the decadal intervals are recorded as members of the system only at the next decadal interval (for example, a state independent in 1963 would be treated for figure 9.1 purposes as entering the system in 1970).
8. Some of the new additions are rather small islands in the south Pacific.

9. These data are based on Polity III information as discussed in the appendix.
10. Economic growth is also of some importance. Not surprisingly, Reuveny and Thompson (2004) find that systemic leadership, conflict, and world economic growth are significant predictors of southern (less developed states) democratization between 1870 and 1992.
11. We are aware that this conclusion begs the question of precisely how much credit should go to systemic leadership in comparison to strictly local considerations. Pursuing the complexities of this question would take us too far afield from our current preoccupations. Thus, we must defer more precise statements to future research.
12. Siverson and Emmons approached the question of whether democratic dyads are more likely to ally by contrasting how many allied democratic dyads existed in comparison to how many democratic dyads existed in any given year. We think this is a legitimate way to investigate questions about democratic states' alliance propensities but it is not the only way to look at the problem. Different approaches will yield different answers as is evidenced in Simon and Gartzke (1996), Thompson and Tucker (1997a), Lai and Reiter (2001), and Reiter and Stam (2002).
13. By doing so, Siverson and Emmons reintroduce the U.S. hegemony variable that they earlier had rejected.
14. See as well the harsh criticisms of the democratic peace literature advanced by Spiro (1994) who, nevertheless, accepts the special alliance affinity of democratic states as something that needs to be explained.
15. Bueno de Mesquita and Lalman (1992: 151–52) argue that democratic and nondemocratic states are almost exactly identical in their propensity to form alliances with states of similar orientations. However, their calculations are based on a static 1815–1970 analysis of European data. Presumably, the pre–World War II behavior masks the different propensity demonstrated after 1945.
16. The post 1815 major power concert to resist the possibility of a resumption of French expansion could be said to serve as a precedent of sorts for NATO but the nineteenth century concert was less explicit, less for alliance-oriented, and less the creation of a system leader. With hindsight, one could also say that the French hegemonic threat was largely exhausted in the first half of the nineteenth century.
17. Newly admitted NATO members have been required to demonstrate commitments to democracy, human rights, a free-market economy, civilian control of the military, and a disavowal of territorial disputes.
18. See chapters one and four, for arguments that various types of regional peace are also important to subsequent possibilities for democratization. Scandinavia and North America represent regions in which local wars became increasingly less likely thanks respectively to the demotion of Sweden and Denmark from the Baltic great power ranks and the mutual deterrence conducted between Britain and the United States after 1815. In this sense, both systemic and regional pax deserve some of the credit for reduced probabilities of warfare between states.

Chapter Ten

1. Portugal may seem an exception to the consistency of the linkages between global system leaders and democratization. However, see Modelski's (1999) discussion of this problem.
2. See Mousseau, Hegre, and Oneal (2003), Hegre (2003), and Senese (1999).
3. Actually, Weede compares the post–World War II settlement to the post–World War I settlement, noting that what transpired after 1945 was missing the post-1919 order.
4. The decline in territorial disputes among wealthy northern states probably also reflects the fact that many of these states have had some time to work out their territorial disagreements, in addition to realizing the futility of continuing to fight over places such as Belgium or Alsace-Lorraine. Another way of looking at this is to note that a large number of rivalries stem from territorial disputes (Rasler and Thompson, 2003). Fewer territorial disputes implies fewer interstate rivalries.

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