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The Renaissance of Security Studies

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This article examines the evolution of security studies, focusing on recent developments in the field. It provides a survey of the field, a guide to the current research agenda, and some practical lessons for managing the field in the years ahead. Security studies remains an interdisciplinary enterprise, but its earlier preoccupation with nuclear issues has broadened to include topics such as grand strategy, conventional warfare, and the domestic sources of international conflict, among others. Work in the field is increasingly rigorous and theoretically inclined, which reflects the marriage between security studies and social science and its improved standing within the academic world. Because national security will remain a problem for states and because an independent scholarly community contributes to effective public policy in this area, the renaissance of security studies is an important positive development for the field of international relations.

In the mid-1970s, the field of security studies began a dramatic resurgence. In addition to a noticeable increase in professional activity and published work on security-related topics, security studies became more rigorous, methodologically sophisticated, and theoretically inclined. Scholars continued to differ on specific policy issues, but competing views were increasingly based on systematic social scientific

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research rather than on unverified assertions or arguments by authority. These developments help explain the recent prominence of the subfield and its growing acceptance within the academic world, and they establish a firm foundation for future work. Given the continued need for independent analyses of security issues, the resurgence of security studies is an important positive development for the field of international relations.

This article examines this recent renaissance with several aims in mind. First, I seek to provide a survey of the field and a guide to the current research agenda.¹ Second, by examining the evolution of a particular subfield, I hope to offer some basic insights into the sociology of knowledge in international relations. What determines the prominence of different fields, the attention paid to specific topics within them, and their ability to generate cumulative knowledge? Finally, by tracing the rise, fall, and recovery of security studies, I seek to identify some practical lessons for managing the field in the years ahead.

This article is divided into five sections. Part I offers a definition of security studies and describes its place within the broader field of international relations. Part II outlines the central features of the so-called Golden Age (1955–1965) and discusses why the field declined in the late 1960s. Part III describes the recent renaissance, examining both how the field has changed and why this rebirth occurred. Part IV summarizes the current research agenda and considers some potential pitfalls; Part V offers several lessons and guidelines for enhancing future progress.

What is “Security Studies?”

The boundaries of intellectual disciplines are permeable; as a result, any effort to delineate the precise scope of security studies is somewhat arbitrary. The main focus of security studies is easy to identify, however: it is the phenomenon of war. Security studies assumes that conflict between states is always a possibility and that the use of military force has far-reaching effects on states and societies (Bull, 1968; Martin, 1980). Accordingly, security studies may be defined as *the study of the threat, use, and control of military force* (Nye and Lynn-Jones, 1988). It explores the conditions that make the use of force more likely, the ways that the use of force affects individuals, states, and societies, and the specific policies that states adopt in order to prepare for, prevent, or engage in war.

The security studies literature often overlaps with more general works on international relations, and most of it fits comfortably within the familiar realist paradigm. In general, however, the research program of security studies is usually informed by debates over central policy problems and tends to address phenomena that can be controlled by national leaders (Smoke, 1975:259).² As a result, scholarship tends to concentrate on *manipulable variables*, on relationships that can be altered by deliberate acts of policy. Given the military power is the central focus of the field and is subject to political control, this tendency is appropriate.³

¹ This article is a personal and therefore subjective assessment of the field. I regret the inevitable omissions; given limited space, I have been forced to omit many important works. For other recent surveys of security studies, see Smoke (1975), Gray (1982), Jervis, Lederberg, North, Rosen, Steinbruner, and Zinnes (1986), Social Science Research Council (1986), Nye and Lynn-Jones (1988), and Freedman (1989).

² Works of “pure” theory reflect changing political concerns as well, but the connections are often less direct and the policy implications less evident.

³ Many international relations theories contain concepts that are not easily controlled, such as “polarity,” “lateral pressure,” or “status inconsistency.” By contrast, works in security studies tend to employ concepts that are controllable by national leaders, such as military doctrine and strategy, the tools of statecraft (e.g., deterrence) or the size and character of armaments.

Military power is not the only source of national security, and military threats are not the only dangers that states face (though they are usually the most serious). As a result security studies also includes what is sometimes termed “statecraft”—arms control, diplomacy, crisis management, for example. These issues are clearly relevant to the main focus of the field, because they bear directly on the likelihood and character of war.

Because nonmilitary phenomena can also threaten states and individuals, some writers have suggested broadening the concept of “security” to include topics such as poverty, AIDS, environmental hazards, drug abuse, and the like (Buzan, 1983; N. Brown, 1989). Such proposals remind us that nonmilitary issues deserve sustained attention from scholars and policymakers, and that military power does not guarantee well-being. But this prescription runs the risk of expanding “security studies” excessively; by this logic, issues such as pollution, disease, child abuse, or economic recessions could all be viewed as threats to “security.” Defining the field in this way would destroy its intellectual coherence and make it more difficult to devise solutions to any of these important problems.

Moreover, the fact that other hazards exist does not mean that the danger of war has been eliminated. However much we may regret it, organized violence has been a central part of human existence for millenia and is likely to remain so for the foreseeable future. Not surprisingly, therefore, preparations for war have preoccupied organized polities throughout history (McNeill, 1982). Any attempt to understand the evolution of human society, let alone the prospects for peace, must take account of the role of military force. Indeed, given the cost of military forces and the risks of modern war, it would be irresponsible for the scholarly community to ignore the central questions that form the heart of the security studies field.⁴

Throughout this essay, I concentrate primarily on works that meet the standards of logic and evidence in the social sciences. It is important to recognize, however, that much of the published work on security topics does not meet these standards.⁵ Because national security issues are highly politicized and the resources at stake are enormous, work on these topics is often written for political rather than scientific goals (Walt, 1987a). This tendency is exacerbated by classification procedures that limit public access to relevant information and is compounded further by the extensive network of consultants and “think tanks” supported by defense contractors or the Defense Department itself. Although some of this work meets basic scholarly standards, much of it should be viewed as propaganda rather than as serious scholarship. This is not true of all “policy analysis,” which often employs sophisticated theoretical concepts and careful empirical research. But there is a difference between the scholarly side of security studies and works that are largely political advocacy, just as there is a difference between scholarship in criminology and the public debate on gun control.

The “Golden Age” of International Security Studies

The field of security studies is a relatively recent creation. Prior to World War II, interest in strategy and military affairs was primarily limited to the professional military, and scholarship on military issues was confined to military and diplomatic history.⁶ Civilian contributions to the study of strategy were discouraged, although

⁴ I am indebted to Michael Desch for discussion on these points.

⁵ Quasi-scholarly work on security topics may be found in journals such as *Strategic Review*, *Armed Forces Journal International*, or *International Defense Review*, or in books such as Graham (1983), Caldicott (1984), or Tyroler (1984).

⁶ Exceptions include studies of arms races (Richardson, 1960), the causes of war (Wright, 1942), and the geopolitics of U.S. grand strategy (Spykman, 1942).

the horrifying costs of World War I demonstrated that war was “too important to be left to the generals.” Civilians became extensively involved in military planning for the first time during World War II, setting the stage for the “Golden Age” or “first wave” of security studies (Jervis, 1979; Gray 1982:45–58).

As one would expect, the nuclear revolution cast a large shadow over the field, and analysts in the “Golden Age” devoted most of their efforts to understanding its implications.⁷ The revolutionary impact of nuclear weapons was recognized immediately (Borden, 1946; Brodie, 1946) and the issues raised by the new technology preoccupied strategists throughout this period (Kaplan, 1983; Trachtenberg, 1989). The central question was straightforward: how could states use weapons of mass destruction as instruments of policy, given the risk of any nuclear exchange? The effort to grapple with this problem produced seminal works on deterrence, coercion, and escalation, along with numerous prescriptive works on alternative strategies (Kaufmann, 1956; Kissinger 1957; Brodie, 1959, 1966; Kahn, 1960, 1965; Schelling, 1960, 1966; Ellsberg, 1961; Snyder, 1961). Other works explored the causes of stability (Wohlstetter, 1959; Hoag, 1961), alternative targeting policies (Knorr and Read, 1962), the potential value of arms control (Brennan, 1961; Bull, 1961; Schelling and Halperin, 1961; Singer, 1962), and the role of conventional forces and limited war in the nuclear age (Osgood, 1957).

Significantly, these works were the product of an eclectic and interdisciplinary approach to specific real-world problems.⁸ Given the youth of the field and the novel problems it faced, this is hardly surprising. Strategic thought from the prenuclear era was only partly relevant to the issues raised by the nuclear revolution, and the lack of established scholars allowed analysts with very diverse backgrounds to enter the field (Trachtenberg, 1989:309–11).⁹ Although security studies has generally been centered in political science, it has always been an interdisciplinary enterprise.

Much of the seminal research during the Golden Age was conducted at think tanks like the RAND Corporation (Smith, 1966; Kaplan, 1983). Access to information and a supportive institutional setting were obvious assets, but their close relationship with the Defense Department may have encouraged these scholars to view national security problems from an excessively military perspective (Green, 1966, 1968). In particular, this factor may explain some of the limitations of the first wave of scholarship.

Limitations and Lacunae in the Golden Age

First, as many critics have noted, the early works in security studies offered little empirical support for their conclusions and prescriptions (Green, 1966; George and Smoke, 1974; Jervis, 1979). In general, these works contained little reliable information about the subjects they addressed and no systematic evidence supporting the authors’ hypotheses or recommendations. Even a richly historical work like Bernard Brodie’s *Strategy in the Missile Age* (1959) became vague and speculative when discussing contemporary issues. As a result, readers seeking to assess the adequacy of existing forces or the feasibility of a given strategy received little guidance from these works.

⁷ According to Laurence Martin, “the problem of [nuclear] deterrence did more than anything else to create the modern academic field of strategic studies” (1980:93).

⁸ For example, the concept of the “manipulation of risk” emerged from the application of game theory to the problem of deterrence, while “stability theory” emerged from an empirical study of the vulnerability of U.S. bomber bases. See Trachtenberg (1989:311–16) and Achen and Snidal (1989:153).

⁹ Major figures in the Golden Age included economists (Thomas Schelling, Henry Rowen, Andrew Marshall, James Schlesinger), physicists (Herman Kahn, Leo Szilard), sociologists (Morris Janowitz, Hans Speier), psychologists (Paul Kecskemeti), mathematicians (Donald Brennan, Albert Wohlstetter), and political scientists (Bernard Brodie, William Kaufmann, Henry Kissinger, and Glenn Snyder).

This problem was somewhat unavoidable, of course. Much of the relevant information was classified, and apart from the bombing of Japan, there was no historical record of nuclear warfare upon which to base conclusions or recommendations. Scholars were forced to rely heavily on deductive techniques such as game theory, illustrated by analogies or evocative historical anecdotes (Schelling, 1960; Kahn, 1960).

The early literature in security studies also employed a rather narrow definition of politics. The field tended to ignore nonmilitary sources of international tension and to focus solely on military balances. For example, deterrence theory assumed the existence of a hostile “aggressor” (the Soviet Union) and concentrated on how one made retaliatory threats credible, without asking why an opponent would want to challenge the status quo in the first place. Although Schelling and others recognized that beliefs and perceptions (such as the perceived risk of preemption) could affect the likelihood of war, the early works focused primarily on how different force postures could alter the incentive to strike first. Thus, the early literature tended to slight the political sources of international conflict or the potential role of accommodative diplomacy (Trachtenberg, 1989:317–18).

This limitation was partly a methodological artifact; the “rational actor” assumption that lay at the core of deterrence theory directed attention away from the organizational, psychological, and domestic political factors that also shape state behavior. The Cold War played a role here as well: because the Soviet desire to expand was taken for granted, more attention was paid to deterring it than to verifying the assumption or explaining its origins. And, as Defense Department employees, it is not surprising that the “civilian” strategists concentrated on military problems and downplayed domestic politics, misperception, and diplomacy.

This tendency may have also encouraged the separation of security studies from other scholarly work on war and peace. For example, warnings about the danger of “conflict spirals” and the value of accommodation emerged primarily from academic scholars in the field of “peace research” rather than from the “civilian strategists” (Etzioni, 1962; Osgood, 1962; Jervis, 1976:109). Similarly, the most important general critiques of deterrence theory and much of the early interest in arms control came from academics as well, although some of them had close ties to government agencies.¹⁰ Finally, the “behavioral revolution” in the social sciences helped spawn a diverse array of research programs on international conflict, such as the Correlates of War Project at the University of Michigan.¹¹ These programs made a major contribution to the methodological self-consciousness of the international relations profession (Singer, 1972; Vasquez, 1987; Gochman and Sabrosky, 1990), but they were not designed to address questions of direct concern to national leaders.¹² In sharp contrast to the early works in security studies, therefore, these projects had little impact on public policy and were often dismissed as irrelevant by the security studies field.

The End of the Golden Age

The first wave of security studies ended in the mid-1960s, and the field entered a period of decline. Several different causes were at work.

First, the research program of security studies had reached something of a dead

¹⁰ On deterrence theory, see Green (1966) and George and Smoke (1974). On arms control, see the list of contributors in Brennan (1961).

¹¹ Other examples include the Dimensionality of Nations (DON) project and the Stanford Studies in International Conflict and Integration. See Rosenau (1976).

¹² Some of these scholars did work on policy issues, and some components of the larger projects had policy relevance. See Singer (1962) and Holsti (1972).

end by this time. The central questions identified by the rational deterrence paradigm were now well understood if not yet fully resolved, and the remaining issues, such as the tradeoff between the alleged need for first-strike options to make extended deterrence credible and the increased risk of war that these capabilities created, seemed beyond resolution within the existing theoretical framework (Trachtenberg, 1989:332). Although doctrines and weapons programs could still be and were debated, further advances would require new conceptual approaches or more advanced analytical tools.

A second problem was the failure of the first wave of scholars to produce a significant group of Ph.D. students. Although individuals like Albert Wohlstetter at the University of Chicago and William Kaufmann at MIT did train a number of protégés, they were more likely to become consultants or government officials than to enter academic departments. As a result, a large “successor generation” did not emerge until relatively recently.

A third reason for decline was the Vietnam War. Not only did the debacle in Indochina cast doubt on some of the early work in the field (such as the techniques of “systems analysis” and the application of bargaining theory to international conflict), it also made the study of security affairs unfashionable in many universities. The latter effect was both ironic and unfortunate, because the debate on the war was first and foremost a debate about basic security issues. Was the “domino theory” accurate? Was U.S. credibility really at stake? Would using military force in Indochina in fact make the U.S. more secure? By neglecting the serious study of security affairs, opponents of the war could not effectively challenge the official rationales for U.S. involvement.¹³ The persistent belief that opponents of war should not study national security is like trying to find a cure for cancer by refusing to study medicine while allowing research on the disease to be conducted solely by tobacco companies.

External events undermined the field in other ways as well. The emergence of U.S.–Soviet détente made the study of war seem less important, and the United States’s declining economic position led to greater interest in the issues of international political economy. Accordingly, scholars began to question the utility of military force and to emphasize the role of economic issues. “Transnational relations” and “interdependence” became the new watchwords, as part of an explicit challenge to the realist paradigm (Cooper 1968; Morse, 1970; Keohane and Nye, 1972, 1977).

At roughly the same time, however, scholars in security studies began to abandon the relatively simple assumptions that had guided the first wave. The main accomplishment of this period was the application of organization theory to national security issues, primarily in studies of weapons procurement and foreign policy decision-making (Art, 1968; Allison, 1971; Halperin, 1974; Steinbruner, 1974; Beard, 1976). Despite its obvious policy relevance, however, this literature focused on questions of implementation rather than the central questions of strategy, force requirements, or the likelihood of war. As such, these advances did not reverse the decline of the field as a whole.

The Renaissance

The renaissance of security studies began in the mid-1970s, signaled by the end of the Vietnam War, the Ford Foundation’s decision to sponsor several academic centers in security affairs, and the founding of *International Security*, which became the

¹³ An exception was the Committee of Concerned Asian Scholars, which produced a variety of publications outlining the case against U.S. involvement in Indochina. Among scholars of international politics, important statements against the war include Waltz (1967) and Morgenthau (1969).

main scholarly forum for the field. Although the field retained its interdisciplinary character and a close connection to real-world issues, the “new wave” differed from the Golden Age in several important respects.

New Developments in Security Studies

The Use of History. Among the most important developments in security studies was greater reliance on history. Aided by increased access to relevant archival material, historians conducted increasingly detailed investigations of national security policy. These efforts led to revised interpretations of important historical events and a growing partnership between historians and political scientists (Gaddis, 1987, 1990). Even more important, scholars began to rely more heavily on historical cases as a means of generating, testing, and refining theories. Although case studies had been used for these purposes in the past, the method of “structured, focused comparison” refined by Alexander George and his associates encouraged scholars to use the historical record in a more disciplined fashion (George and Smoke, 1974; Smoke 1977; George 1979).¹⁴

The comparative case study method was explicitly designed to counter the ahistorical approaches that had characterized the first wave. By focusing on concrete historical events, the method sought a more nuanced, “policy-relevant” theory (George and Smoke, 1974:616–42). Comparative case studies were not a panacea, of course; case selection remained a crucial issue, causal inferences were difficult to make with confidence, and these studies often produced rather modest “contingent generalizations” instead of powerful general theories. But as even its critics admit, the comparative case method expanded the set of relevant hypotheses and helped expose the limitations of existing theories (Achen and Snidal, 1989).

The Challenge to Rational Deterrence Theory. The use of history was especially evident in the wide-ranging assault on deterrence theory. Drawing upon psychology, organization theory, and a host of historical studies, these works questioned the assumptions of perfect information and rational calculation that lay at the heart of the rational deterrence paradigm.¹⁵ The result has been a lively debate on the requirements of deterrence, the utility of the rational deterrence framework, and the appropriate strategies for evaluating it (George and Smoke, 1974, 1989; Steinbruner, 1976; Jervis, 1979, 1989; Lebow, 1981; Jervis, Lebow, and Stein, 1985; Orme, 1987; Huth, 1988; Achen and Snidal, 1989; Downs, 1989; Lebow and Stein, 1989, 1990; Huth and Russett, 1990).

Nuclear Weapons Policy. New ideas and information transformed the analysis of nuclear weapons policy as well. As the recurring debates over strategic nuclear policy revealed, civilian analysts were increasingly capable of analyzing complex technical issues because the necessary data and analytical methods were now widely available (Davis and Schilling, 1973). The debate over nuclear strategy and arms control was closely linked to concerns about alleged Soviet superiority, the progress of various arms control negotiations, or new weapons proposals such as the M-X missile and the Strategic Defense Initiative (Gray, 1979, Jervis, 1984; Miller and Van Evera, 1986).

¹⁴ Works that employed the comparative case-study method include Snyder and Diesing (1977), Lebow (1981), Betts (1982, 1987), Mearsheimer (1983), Posen (1984), Snyder (1984, forthcoming), Levite (1987), Walt (1987b), Evangelista (1988), George, Farley, Dallin (1988), Huth (1988), and Shimshoni (1988).

¹⁵ Advances in game theory have enabled scholars to relax the assumptions of the original deterrence model, although these new techniques require other restrictions to achieve formal solutions. See O'Neill (1989) and Powell (1990).

These disputes have become increasingly sophisticated and well informed, reflecting the greater expertise and information available outside official circles (Epstein, 1987; May, Bing and Steinbruner, 1988; Eden and Miller, 1989; Glaser, 1990).¹⁶

As noted, the combination of organization theory and careful empirical research also produced significant innovations in deterrence theory. Where early work on nuclear strategy had assumed that each side's military forces would respond obediently to the commands of national authorities, scholars began to question this comfortable belief through sophisticated analyses of nuclear command and control systems (Steinbruner, 1978; Ball, 1981; Bracken, 1983; Blair, 1985; Carter, Steinbruner, and Zraket, 1987) and careful historical studies of past nuclear crises (Sagan, 1985; Betts, 1987; Bundy, 1988). These analyses suggested that civilian authorities had limited knowledge of and control over U.S. nuclear operations, and that the precise state of the strategic nuclear balance had little direct effect on international politics in general or crisis behavior in particular.

Finally, increased access to the documentary record enabled historians to demolish a variety of myths about the history of nuclear weapons policy. These studies revealed the strong counterforce bias of U.S. strategic doctrine and reinforced the conclusion that limited nuclear exchanges would be difficult if not impossible to control (Rosenberg, 1979, 1983; Friedberg, 1980; Ball, 1981; Schilling, 1981; Ball and Richelson, 1986; Sagan, 1989). In short, where scholarship in the Golden Age was necessarily abstract and "data-free," the study of nuclear weapons policy during the renaissance rested on a much firmer base of empirical support.

Conventional Warfare. Apart from the "limited war" debate in the 1950s and a flurry of interest in counterinsurgency during the Vietnam War, conventional warfare was downplayed during the Golden Age. This tendency was reversed during the renaissance; the emergence of strategic parity and a concern over the conventional balance in Europe after Vietnam sparked renewed interest in the role of conventional military power. Although some of this work was straightforward policy analysis, many of these studies were based on new theoretical approaches and empirically tested propositions about conventional warfare. Even when flawed, such works laid the foundation for subsequent refinements. In addition to a lively debate on the conventional balance and the appropriate techniques for measuring it (Fischer, 1976; Mako, 1983; Posen, 1984–85, 1989; Biddle, 1988; Cohen, 1988; Thomson, 1988; Epstein, 1989; Kupchan, 1989b; Mearsheimer, 1989), scholars explored the requirements of conventional deterrence (Mearsheimer, 1983; Betts, 1985; Shimsoni, 1988), the lessons of Vietnam (Thies, 1980; Rosen, 1982; Krepinevich, 1986; Shafer, 1988; Clodfelter, 1989; Pape, 1990), the danger of surprise attack (Betts, 1982; Levite, 1987; Kam, 1988), and the merits of alternative force postures and doctrines (Luttwak, 1980–81; Mearsheimer, 1981–82; Betts, 1983). Other studies debated strategies for the Rapid Deployment Force (Waltz, 1981; Epstein, 1981) and the U.S. Navy (Posen, 1982; Epstein, 1983–84; Brooks, 1986; Mearsheimer, 1986). In short, although nuclear weapons policy continued to receive attention, the study of conventional warfare figured prominently in the renaissance.

U.S. Grand Strategy. Grand strategy is a state's "theory" for creating security through military and diplomatic means (Posen, 1984:13). Increased interest in the subject was especially evident in the United States, sparked by a growing sense that the United States was over-committed and needed to rethink its strategic priorities. In

¹⁶ For example, it is now commonplace to use dynamic campaign models to measure the effectiveness of alternative strategic force postures.

addition to several historical studies (Gaddis, 1982; Leffler, 1984), a host of books and articles debated the scope of U.S. interests, the utility of military force for defending them, and the likely responses of other states to alternative U.S. policies (Huntington, 1982; Calleo, 1987; Posen and Van Evera, 1987; Gray 1988; David, 1989; Desch, 1989; Walt, 1989; Van Evera, 1990). Although a consensus on U.S. grand strategy remained elusive, the debate illustrated the growing tendency for scholars to base their recommendations on testable empirical and theoretical claims.

Security Studies and International Relations Theory. The renaissance of security studies also saw the return of national security issues to the scholarly agenda among theorists of international politics. At the most general level, Kenneth Waltz's *Theory of International Politics* (1979) presented a powerful reformulation of the realists perspective, aided by spirited defenses of realism within international political economy (Gilpin, 1975; Lake, 1987; Grieco, 1990).¹⁷ In contrast to the liberal theories popular during the era of détente, these works emphasized the enduring importance of anarchy and war as constraints on state behavior. Not surprisingly, renewed interest in the causes of war was evident as well. Important studies by Robert Jervis and George Quester sparked a lively exchange on the effects of offensive and defensive advantages (Quester, 1977; Jervis, 1978; Levy, 1984; Posen, 1984; Snyder, 1984; Van Evera, 1984; Sagan, 1986; Shimshoni, 1990–91), while Bruce Bueno de Mesquita's controversial "expected utility" theory of war prompted an equally intense debate (Bueno de Mesquita, 1981; Majeski and Sylvan, 1984; Wagner, 1984). Scholars also explored the effect of domestic politics, misperception, and system structure on the likelihood of war (for an excellent survey of this literature, see Levy, 1990). Other theoretical works with direct relevance for security affairs included studies of alliances (G. Snyder, 1984; Walt, 1987b), détente (Lynn-Jones, 1986), and the strategies for cooperation between adversaries (Oye, 1986; George, Farley, and Dallin, 1988; Rock, 1989). In short, the renaissance of security studies was not limited to narrow policy research. Explicit theoretical studies formed a large part of the field, and scholars consciously sought to apply these results to specific policy problems.

The Role of the Ivory Tower. The final characteristic separating the Golden Age from the recent renaissance is the growth of security studies within the academic world. Although several of the major figures of the first wave held university positions, they did their most influential work at think tanks like RAND. Although analysts outside the ivory tower remain important, the center of gravity has clearly shifted back toward academe.¹⁸ Since 1980, for example, membership in the International Security section of the International Studies Association has grown nearly twice as fast as overall membership. The creation of the International Security and Arms Control section within the American Political Science Association in 1988 reflects a similar trend.¹⁹

¹⁷ By the 1980s, the perspective of some liberal theorists had moved substantially closer to the realist position, without embracing it entirely. Compare Keohane and Nye (1972, 1977) with Keohane (1984) and Nye (1988). For a summary of this trend, see Grieco (1990).

¹⁸ Since 1982, the Mershon Center at Ohio State University has awarded an annual prize for the best first book in national security affairs. As of this writing, four of the recipients are university professors (John Mearsheimer, Barry Posen, the present author, and Aaron Friedberg), and two of the other winners (Bruce Blair and Andrew Krepinevich) received the award for books based on Ph.D. dissertations.

¹⁹ Membership in the International Security section of the ISA increased from 316 to 683 between 1980 and 1990 (an increase of 116 percent), while total ISA membership increased from 1892 to roughly 3000 over the same period (a 59 percent increase).

Explaining the Renaissance

As this discussion suggests, the emergence of new policy problems and specific theoretical and empirical puzzles played the primary role in sparking the renaissance of security studies. The resurgence of the field was reinforced by several other developments as well.

The End of the Vietnam War. The U.S. withdrawal from Indochina made it easier for students to study national security issues without being ostracized, and it may have reduced the suspicion with which scholars in security studies were viewed by academic departments. The defect also encouraged a reassessment of U.S. foreign and defense policy; as a result, younger scholars were inspired to study security issues in order to learn from past mistakes. If the Vietnam War undermined security studies in the 1960s, it helped revive it once the war was over.

The Collapse of Détente. Interest in security affairs was also revived by the deterioration of U.S.–Soviet relations in the late 1970s and early 1980s. Although public concern for America's international position was exaggerated, the Iranian and Nicaraguan revolutions, the SALT II treaty, and Soviet intervention in Africa and Afghanistan helped place national security issues back on the public and academic agenda. The Reagan Administration's dramatic defense build-up reinforced this trend by provoking fears that U.S. national security policy was extravagant at best and provocative at worst. Just as the Cold War launched the Golden Age and détente caused security studies to languish, increased international tensions helped reinvigorate the field.

Increased Access to Data. Another major cause of the renaissance was the increased quality and quantity of information available to scholars working outside the official national security establishment. In addition to the growing partnership between historians and political scientists and the increased use of archival material, security studies profited from greater access to data on contemporary security issues. This development was part of the general campaign against governmental secrecy inspired by Vietnam and Watergate, and by the mid-1970s authoritative data on defense policy were available from organizations such as the Congressional Budget Office and the Office of Technology Assessment, along with the *Annual Report of the Secretary of Defense* and a variety of Congressional hearings and committee reports.²⁰ These official sources were supplemented by publications from private organizations such as the International Institute for Strategic Studies (IISS), the Brookings Institution, the Institute for Defense and Disarmament Studies (IDDS), and the Stockholm International Peace Research Institute (SIPRI).²¹ Like the official sources upon which they were based, some of these publications contained inevitable errors and biases (Brzoska, 1981; Blackaby and Ohlson, 1982). Exposure to public scrutiny helped correct these problems over time, however, and civilian analysts became increasingly adept in analyzing contemporary defense issues. In short, improved access to information was a necessary condition for the growth of the field.

Increased Outlets for Publishing. New outlets for scholarly publishing were a boon to the field as well. In the past, the lack of refereed journals in security affairs was an obstacle to younger scholars in the academic world and may have contributed to the

²⁰ For examples, see Congressional Budget Office (1977) and Carter (1984).

²¹ For examples of these publications, see Cochran, Arkin and Hoenig (1984), Epstein (1987), the IISS *Military Balance* and the SIPRI *World Armaments and Disarmament Yearbook*.

partial isolation of security studies from the university community.²² But the creation of *International Security* in 1976 and the *Journal of Strategic Studies* in 1978, the improved quality of publications such as *Survival* and *The Adelphi Papers*, and the founding of the Cornell Studies in Security Affairs in the early 1980s encouraged more ambitious and rigorous works on security topics.²³ Even *International Organization*, the premier journal in the field of international political economy, issued an explicit invitation for articles on security affairs during this period, which further underscored the enhanced legitimacy of the field.

Financial Support. Like its medieval namesake, the renaissance of security studies was fueled by wealth. The Ford Foundation's early commitment to establishing research centers at Harvard, MIT, Stanford, Cornell and UCLA had an exceptionally strong impact, together with its long-standing commitment to the IISS. Increased public concern about national security issues encouraged generous support from institutions such as the Carnegie Corporation of New York, the Ford Foundation, the MacArthur Foundation, the John M. Olin Foundation, the National Academy of Sciences, the Pew Charitable Trusts, the U.S. Institute of Peace, and the Smith-Richardson Foundation, among others.²⁴ The programs these funds supported enable scholars to conduct research free from official pressures, helped younger scholars complete their training, and allowed new members of the field to forge valuable professional networks within the diverse intellectual atmosphere of a university. It is not surprising, therefore, that many prominent younger scholars in security studies spent part of their careers in one of these programs.

Security Studies and Social Science. Last but not least, the resurrection of security studies was facilitated by its adoption of the norms and objectives of social science. As a social science, security studies seeks to develop general explanatory propositions about the use of force in international politics, and to apply this knowledge to important contemporary issues. Like other social scientists, scholars in security affairs engage in three main activities: 1) *theory creation*, the development of logically related causal propositions explaining a particular phenomenon of interest; 2) *theory testing*, attempts to verify, falsify, and refine competing theories by testing their predictions against a scientifically selected body of evidence; and 3) *theory application*, the use of existing knowledge to illuminate a specific policy problem. The first two categories are often linked—the creation of new theories is usually accompanied by efforts to test them—while “policy analysis” in security affairs consists primarily of the third.²⁵

²² Prior to 1976, scholarly articles on security affairs were confined primarily to *World Politics*, *International Studies Quarterly*, or the *Journal of Conflict Resolution*. These journals devoted much of their space to other topics, however, which reduced opportunities for scholars conducting serious analytical work in security studies. *Foreign Affairs* and *Foreign Policy* published important scholarly articles on occasion, but they were more likely to publish undocumented policy advocacy by government officials, journalists, or academics.

²³ This development was obviously both cause and effect; the growth of publishing opportunities itself reflected growing interest in the field. Journals such as *International Security* helped improve the quality of scholarship by emphasizing cumulative research, careful documentation, policy relevance, and theoretical or historical originality.

²⁴ The MacArthur Foundation alone allocated \$65 million for research on peace and security issues between 1984 and 1992, and it funded over 350 graduate students and 140 faculty members between 1985 and 1988 alone. See Benedict (1989).

²⁵ As in other areas of public policy, policy recommendations in security affairs rest on vague notions about the impact of alternative policies. Making these “folk theories,” e.g., the “domino theory,” the “window of vulnerability,” explicit and testing them is a key part of academic research in the field.

Security studies seeks *cumulative knowledge* about the role of military force. To obtain it, the field must follow the standard canons of scientific research: careful and consistent use of terms, unbiased measurement of critical concepts, and public documentation of theoretical and empirical claims. Although no research enterprise ever lives up to these standards completely, they are the principles that make cumulative research possible. The increased sophistication of the security studies field and its growing prominence within the scholarly community is due in large part to the endorsement of these principles by most members of the field.

To summarize: The renaissance of security studies was over-determined. It reflected a political climate in which the importance of national security problems was increasingly appreciated and in which academic institutions became more receptive to work in this area. Although the renaissance began before the recent surge in financial support, these grants sustained its growth. Increased access to information and civilian expertise were both cause and effect, along with the marriage between security studies and social science. The result was the reemergence of an important subfield of international relations in a new and improved form.

Problems and Prospects for Security Studies

What lies ahead for security studies? On the one hand, the widespread belief that the end of the Cold War has decreased the risk of war may temporarily divert financial support and research energies in other directions. On the other hand, a permanent decline is unlikely for at least three reasons. First, as the war in the Persian Gulf reminds us, military power remains a central element of international politics, and failure to appreciate its importance invariably leads to costly reminders. Second, security studies has been institutionalized within many university departments; indeed, a graduate program lacking qualified experts in this area must now be considered incomplete. Thus, new Ph.D.s will emerge in due course and will enjoy adequate professional opportunities. Most important of all, the collapse of the Cold War order will create new policy problems and new research puzzles. In short, the scholarly agenda in security studies is expanding, not shrinking, and security studies will remain an active-sub-field for some time to come.

Potential Problems

Despite these grounds for optimism, several dangers could undermine the future development of the field. As noted earlier, the resources at stake in debates over defense and foreign policy create a strong temptation to focus on short-term policy analysis. Moreover, as Hans Morgenthau once warned, active involvement in policy debates inevitably tempts participants to sacrifice scholarly integrity for the sake of personal gain or political effectiveness (Morgenthau, 1970; Walt, 1987a:146–60). At the very least, there are powerful incentives to concentrate on consulting work and policy analysis rather than on cumulative scholarly research. If security studies neglects long-term research questions and focuses solely on immediate policy issues, a decline in rigor and quality will be difficult to avoid.

Yet the opposite tendency may pose an even greater danger. On the whole, security studies have profited from its connection to real-world issues; the main advances of the past four decades have emerged from efforts to solve important practical questions. If security studies succumbs to the tendency for academic disciplines to pursue “the trivial, the formal, the methodological, the purely theoretical, the remotely historical—in short, the politically irrelevant” (Morgenthau, 1966:73), its theoretical progress and its practical value will inevitably decline.

In short, security studies must steer between the Scylla of political opportunism and the Charybdis of academic irrelevance. What does this mean in practice? Among other things, it means that security studies should remain wary of the counterproductive tangents that have seduced other areas of international studies, most notably the “post-modern” approach to international affairs (Ashley, 1984; Der Derian and Shapiro, 1989; Lapid, 1989). Contrary to their proponents’ claims, post-modern approaches have yet to demonstrate much value for comprehending world politics; to date, these works are mostly criticism and not much theory.²⁶ As Robert Keohane has noted, until these writers “have delineated . . . a research program and shown . . . that it can illuminate important issues in world politics, they will remain on the margins of the field” (Keohane, 1988:392). In particular, issues of war and peace are too important for the field to be diverted into a prolix and self-indulgent discourse that is divorced from the real world.

The use of formal models should also be viewed with some caution, though their potential value is greater. Formal methods possess obvious virtues: analytic assumptions tend to be stated more explicitly, gaps in evidence can be handled through systematic sensitivity analyses, and advanced mathematical techniques can identify deductive solutions to previously intractable problems (for recent examples, see O’Neill, 1989; Downs and Rocke, 1990; Powell, 1990). Formal analysis can also depict a theory’s logical structure with precision, generating counterintuitive propositions and identifying inconsistencies.

Yet despite these strengths, recent formal applications have had relatively little impact on other work in the field. This situation stands in sharp contrast to earlier formal works (Schelling, 1960; Olson and Zeckhauser, 1966), which had a broad and lasting influence. One reason is the tendency for recent works to rely on increasingly heroic assumptions, which render these models both impossible to test and less applicable to important real-world problems. The danger, as Schelling warned, is “the willingness of social scientists to treat the subject [of strategy] as though it were, or should be, solely a branch of mathematics” (1960:10).

Obviously, scholarship in social science need not have immediate “policy relevance.” But tolerance for diverse approaches is not a license to pursue a technique regardless of its ultimate payoff; the value of any social science tool lies in what it can tell us about real human behavior. Formal models are useful when they do this, but they should not be viewed as ends in themselves. Unfortunately, despite the impressive technical firepower displayed in many recent formal works, their ability to illuminate important national security problems has been disappointing.

Because scientific disciplines advance through competition, we should not try to impose a single methodological monolith upon the field. To insist that a single method constitutes the only proper approach is like saying that a hammer is the only proper tool for building a house. The above strictures are no more than a warning, therefore; progress will be best served by increased dialogue between different methodological approaches (Downs, 1989).²⁷

²⁶ Although Yosef Lapid cites Imre Lakatos’s critique of naive positivism approvingly (Lapid, 1989:239, 245), he neglects Lakatos’s key argument: theories are only overturned by the development of a superior alternative (Lakatos, 1970).

²⁷ In the past, for example, security studies tended to dismiss quantitative research on conflict as irrelevant, while the latter tended to view security studies as unscientific “policy analysis.” Both charges are undoubtedly true in some cases, but a blanket dismissal is increasingly inappropriate. Instead, encouraging both groups to become more familiar with alternative approaches would improve both enterprises. For example, whenever these literatures reach different conclusions—such as on the impact of domestic conflict or regime type on the likelihood of war—there is an obvious opportunity for further work.

A Research Agenda for Security Studies

Any attempt to define a research agenda will invariably omit important or unforeseen possibilities. Nevertheless, several subjects clearly merit further attention.

The Role of Domestic Politics. Some of the most interesting advances in security studies have come from scholars focusing on different aspects of domestic politics. What unites these disparate theories is the belief that domestic politics is a powerful determinant of national security policy. For example, several prominent studies have argued that liberal democracies do not fight each other (Small and Singer, 1976; Chan, 1984; Weede, 1984; Doyle, 1986; Maoz and Abdolali, 1989); given the importance of this claim, further research is needed to resolve the remaining theoretical and empirical puzzles.²⁸ Similarly, the long-standing debate over the military's role as a cause of war remains unresolved (Huntington, 1957; Vagts, 1959; Betts, 1977; Snyder, 1984; Van Evera, 1984), along with the validity of the so-called scapegoat and diversionary theories of war (Levy, 1988, 1990). Other recent works suggest that regime change or revolution is a potent cause of conflict as well (Maoz, 1989; Walt, 1990), but further research to measure and explain this effect is still needed. Students of arms races have long stressed the role of domestic factors (York, 1970; Kurth, 1971; Senghaas, 1972; Evangelista, 1988), and Jack Snyder's recent work (1991) on empires argues that the internal politics of rapidly industrializing societies encourages "log-rolled" domestic coalitions to unite behind highly expansionist foreign policies. Given the recent shifts in the domestic politics of the Soviet Union and its Eastern European allies, further work on these different approaches is clearly in order.

The Causes of Peace and Cooperation. Another potential growth area is in greater attention to the causes of peace and cooperation. To be sure, most theories about the causes of war are also theories about peace (Van Evera, 1984; Blainey, 1988), and exploring ways to reduce the risk of war has been part of the field since its inception.²⁹ In the past, however, security studies tended to view explicit research on peace as utopian or naive, perhaps based on a belief that realists should not be diverted into such idealistic pursuits. For their part, peace researchers tended to assume that the use of force was always irrational, that arms races were a powerful cause of conflict rather than a symptom, and that war was always the result of misperception. The tendency for some peace researchers to view capitalism as a powerful engine of conflict (despite the abundant evidence against this belief) divided the two fields even further.³⁰

Over time, however, the two perspectives have begun to converge. As discussed above, scholars in security studies have devoted considerable attention to misperception and domestic politics as causes of war, while some peace researchers have begun to address issues of military strategy and defense policy in a more sophisticated and well-informed way. This trend is perhaps most evident in the literature on "nonoffensive" defense: many of these writings acknowledge the need for military power while investigating alternative force structures that could ameliorate the security

²⁸ In addition to problems of definition (were England and Germany liberal states in 1812 and 1914 respectively?) and the lack of independence between cases (many liberal states were formerly united in the British empire), these studies have yet to offer a persuasive explanation for the "liberal peace."

²⁹ For example, deterrence theory identifies the conditions that make decisions for war irrational, surely a worthy goal for opponents of war.

³⁰ For surveys of peace research from a variety of perspectives, see Singer (1976), Boulding (1978), Wiberg (1981), and Quester (1989).

dilemma between states (Ahfeldt, 1983; Alternative Defense Commission, 1983; Agrell, 1987; Gates, 1987; Saperstein, 1987; Flanagan, 1988). Although primarily a product of the peace research community, these works bear a strong resemblance to the offense/defense literature in security studies.

Increased interest in peace and cooperation is evident in other ways as well. For example, scholars of security affairs have been understandably skeptical of “security regimes” in the past (Jervis, 1983), but more recent studies suggest that international regimes can have modest positive effects on the ability of states to cooperate on specific security issues (Lynn-Jones, 1985; Nye, 1987; George, Farley, and Dallin, 1988). Although self-help remains the primary imperative in international politics, institutional arrangements could still contribute to peace, particularly if they directly address the primary controllable causes of war identified by previous scholarly work.³¹

Far from being a utopian ideal, efforts to reduce the danger of war are consistent with the central focus of security studies and with realism’s traditional pessimism about the prospects for a durable peace. Moreover, preserving peace contributes directly to national security, at least for most states most of the time. Given their belief that war is always a possibility, realists should be especially interested in devising ways to ensure that it does not occur. In short, well-informed research on peace is a realistic response to anarchy and should be part of security studies.

The Power of Ideas. Finally, interest in the “autonomous power of ideas” has also grown in recent years. The role of “strategic beliefs” in foreign and military policy has been stressed by historians (Howard, 1984), by scholars drawing upon psychology (Jervis, 1976; Kull, 1988), and by studies of military organizations and domestic politics (Snyder, 1984, 1991; Van Evera, 1984; Thomson, 1990). More generally, John Mueller (1989) and James L. Ray (1990) have argued that war is a fading institution among advanced industrial societies, just as dueling and slavery become obsolete in the 19th century. Significantly, their arguments are not based on the dangers posed by nuclear weapons. Instead, they claim that the horrors of conventional war have discredited the earlier belief that it was a noble or heroic activity. This argument remains incomplete, however, for we lack a theory to account for the observed change in attitudes (Kaysen, 1990). Mueller attributes the shift to the dehumanizing experience of World War I, but this does not explain why earlier wars failed to produce a similar result. Without a theory of attitude change, we cannot estimate the durability of current antiwar attitudes or devise a workable strategy for reinforcing them. And as Mueller admits, the outbreak of World War II shows that if most *but not all* states believe war is too horrible to contemplate, those that do not share this view will be more likely to use force precisely because they expect opponents to acquiesce rather than fight. Unless popular revulsion against war becomes universal and permanent, it provides no guarantee that inter-state violence would end. Despite these limitations, the impact of changing attitudes on warfare remains a fascinating question, as part of the general subject of how states learn.

The End of the Cold War. For the past forty years, the two superpowers defined their security policies primarily in response to each other, and the rivalry between them shaped the conduct of most other states as well. Accordingly, the waning of U.S.–Soviet rivalry will have a significant impact on security studies.

³¹ Examples include offensive military imbalances, territorial disputes, xenophobia, and hypernationalism. The U.S.–Soviet arms control negotiations helped stabilize their deterrent relationship by limiting anti-ballistic missile systems, and the United Nations Educational, Scientific and Cultural Organization (UNESCO) led a largely successful campaign to eliminate national biases within European textbooks (Dance, 1960).

First, the study of grand strategy will be increasingly important. As discussed earlier, interest in U.S. grand strategy revived during the renaissance of security studies, but there are still no theoretical or comparative works on grand strategy and relatively few studies of other cases.³² Because both great and lesser powers will need new security arrangements once the Cold War is over, research on alternative grand strategies will be of obvious interest. Under what conditions should states employ military force and for what purposes? With the waning of the Soviet threat, what interests will the other great powers seek to defend? Can the United States and its allies now reduce their military forces, or should they be configured for other contingencies? These issues are certain to receive considerable attention, and some of it should come from experts without a professional interest in the outcome.

Second, the end of the Cold War raises basic issues about the prospects for peace. Will the waning of U.S.–Soviet rivalry reduce the danger of war or allow familiar sources of conflict to reemerge? Will regional powers take more aggressive actions to improve their positions—as Iraq sought to do by invading Kuwait—or will they behave more cautiously in the absence of superpower support? Attempts to answer these and other questions will necessarily build on the existing knowledge base in the field, but will also stimulate new empirical studies and theoretical innovations.

These concerns are already evident in the scholarly debate over the future of Europe. At least four main views can be identified. “Third-image pessimists”³³ argue that the re-emergence of a multipolar Europe will restore the conditions that fueled war in Europe in the past; for this reason, the end of the Cold War will increase the danger of war. They recommend that U.S. military forces remain in Europe to dampen these effects and favor the managed spread of nuclear weapons (to Germany in particular) to alleviate the security fears they believe will accompany the superpowers’ withdrawal from Europe (Mearsheimer, 1990). “Second-image pessimists” downplay systemic causes and emphasize the dangers arising from the weak democratic institutions in Eastern Europe and the Soviet Union. They fear that competing interest groups will use foreign policy to enhance their domestic positions; in the worst case, several factions would unite in a coalition combining their separate expansionist agendas, as occurred in Germany and Japan before the two world wars. The recommended antidote is Western assistance to support the new democracies in Eastern Europe, and the rapid integration of these states into the European Community (EC) (Snyder, 1990).

Rejecting these pessimistic views, “second-image optimists” argue that the leveling of European societies, the dampening of militarism, and the extensive rewriting of nationalist history in Europe have removed the main causes of earlier wars. This view sees the possible dissolution of the Soviet Union as the main threat to peace, and favors Western efforts to encourage a peaceful transition and to prevent the re-emergence of the domestic forces that fueled aggression in the past (Van Evera, 1990–91). Finally, “institutional optimists” suggest that economic integration and international institutions (such as NATO, the EC, or the Conference on Security and Cooperation in Europe) will be strong enough to safeguard peace in Europe. A full scholarly presentation of this view is not yet available—though Snyder (1990) presents elements of one—but it implies using existing institutions to facilitate arms control and to manage economic and political tensions in an independent and increasingly united Europe (Hoffmann, 1990; Keohane, 1990).

³² Studies of grand strategy for non-U.S. cases include Handel (1973), Luttwak (1976), Ben-Horin and Posen (1981), Friedberg (1988), and Mandelbaum (1988).

³³ “Third-image” theories view war as a result of the anarchic international system, “second-image” theories focus on the internal character of states, and “first-image” theories address causes found in human nature. See Waltz (1959).

A brief summary cannot do justice to the subtlety and power of these competing views. It is worth noting, however, that all of them rely on scholarship developed or refined during the renaissance of security studies: the scholarly debate on the future of Europe is very much a contest between rival theoretical visions. It is also an issue with far-reaching implications for defense budgets, alliance commitments, and the likelihood of war. Far from signaling a declining role for security studies, in short, the end of the Cold War will keep security issues on the front burner for some time to come.

Economics and Security. The relationship between economics and security is of growing interest as well. One obvious dimension is the connection between military spending and economic performance; the debate sparked by Paul Kennedy's *The Rise and Fall of the Great Powers* illustrates the continued dissensus on this question (Kennedy, 1987; Adams and Gold, 1987; Huntington, 1988–89; Friedberg, 1989; Kupchan, 1989a; Nye, 1990). Second, despite the attention that resource issues received after the 1973 oil shocks, disputes persist on the strategic importance of economic resources and their role as potential causes of international conflict (Shafer, 1982; Maull, 1984; Finlayson and Haglund, 1987; Johnson, 1989). The recent war in the Persian Gulf highlights the continued relevance of this issue, as well as the potential effectiveness of economic sanctions as a diplomatic instrument.

A third issue linking economics and security is the political influence of the military-industrial complex (MIC). Although several recent works have analyzed the procurement process in detail (Gansler, 1982, 1989; Stubbing, 1986; McNaugher, 1989), there has been little research on the MIC's *political* role in shaping national policy. Even our historical knowledge is deficient; there is still no adequate successor to Huntington's *The Common Defense* (1961), Schilling, Hammond, and Snyder's *Strategy, Politics, and Defense Budgets* (1962), and Enthoven and Smith's *How Much is Enough?* (1971). Indeed, there is no authoritative scholarly analysis of the U.S. defense buildup in the 1980s.³⁴ Cross-national comparisons would be valuable as well, to supplement the few studies now available (Evangelista, 1988). Given the resources at stake, investigating how such decisions are made seems well worth the effort of economists and security experts alike.

Refining Existing Theories. The discussion in this section underscores how new theories and approaches have sparked lively scholarly exchanges throughout the renaissance of security studies, on topics such as the impact of offensive and defensive advantages, the effect of domestic politics on war, the causes and consequences of arms races, the requirements of extended deterrence, the sources of military innovation, and the prospects for security cooperation. In most cases, however, competing hypotheses have not been subjected to systematic empirical tests. In addition to the usual efforts to devise new theories, therefore, refining and testing existing hypotheses through well-designed empirical studies should form a central part of future work.

Protecting the Data Base. As noted earlier, the renaissance of security studies was facilitated by greater access to relevant information. Unfortunately, several recent developments suggest that the information so necessary for scholarship and for an informed public debate is being seriously curtailed. The *Annual Reports* produced by the Defense Department during the Reagan Administration were less informative

³⁴ Instead, most recent writings on U.S. defense policy are journalistic, polemical, or narrowly focused (Fallows, 1982; Stubbing, 1986; or Kotz, 1988).

than earlier versions, and this trend has continued under President Bush.³⁵ The Reagan Administration was also more aggressive in prosecuting alleged leaks and in manipulating media coverage, thereby inhibiting journalists from investigative reporting and reducing the raw data available for use by scholars (Hertsgaard, 1988).³⁶ Even more worrisome, a recent volume of the *Foreign Relations of the United States*, the State Department's official record of U.S. diplomacy, contained such serious distortions that the Chairman of its Advisory Committee resigned in protest, accompanied by widespread condemnation from the Historical profession (Cohen, 1990; Kuniholm, 1990; Society of Historians of American Foreign Relations, 1990).³⁷

Efforts to shield government policy from outside evaluation pose a grave threat to scholarship in the field. No doubt some government officials would like to deny ordinary citizens the opportunity to scrutinize their conduct; as a central part of that evaluative process, the scholarly profession should resist this effort wholeheartedly. The danger goes beyond the interests of any particular subfield; restricting information threatens the public debate that is central to democracy and essential to sound policy. Events as diverse as the Bay of Pigs invasion, the Iran/contra affair, and the troubled development of the B-2 bomber remind us that excessive secrecy allows ill-conceived programs to survive uncorrected. Instead of limiting the study of security issues to a select group of official "experts," therefore, open debate on national security matters must be preserved. Such a debate *requires* that scholars retain access to a reliable and complete data base.

Conclusion: Some Lessons for the Future

The rise, fall, and recovery of security studies offer several guidelines for the future conduct of the field. To encourage continued progress, I conclude this essay by considering (1) the evolution of knowledge in the field, (2) the place of security studies in the academic world, (3) the role of research support, and (4) the norms and ethos of the field.

The Evolution of Knowledge

The history of security studies reveals several features about the evolution of social science. First, it illustrates how external events influence the scholarly agenda: as noted throughout this essay, research in security studies has been heavily shaped by changing international conditions. Obviously, an excessive focus on immediate policy issues can stifle long-term progress and increase the danger that research support will be subject to a "feast or famine" cycle as international tensions rise and fall (Jervis et al., 1986:60; Nye & Lynn-Jones, 1988:21). On the whole, however, the attention paid to policy issues has positive effects: it is the main source of new research questions and discourages any drift toward academic irrelevance.

³⁵ The Defense Department seems proud of its failure to inform us: its 1990 *Annual Report* boasts that it saved \$121,800 by "tailoring the report directly to statutory requirements . . . and eliminating unnecessary no-charge distribution." In other words, Secretary Cheney's staff included only what was absolutely required by law and reduced public access to its report!

³⁶ The Bush Administration's handling of the Panama invasion and the Gulf War suggests that it is following a similar approach, aided by a compliant media (Cook and Cohen, 1990).

³⁷ Specifically, Volume X in the 1952–1954 series, covering U.S. policy in Iran, makes no mention of Operation AJAX, the U.S.-backed coup that ousted the Mossadegh government in 1953. According to Bruce Kuniholm, an historian of U.S.–Iranian relations and former State Department employee with access to the complete account: "the misleading impression of U.S. non-involvement conveyed in the pages of this volume constitutes a gross misrepresentation of the historical record sufficient to deserve the label of fraud" (Kuniholm, 1990:12).

Second, the history of security studies also illustrates the mechanisms by which social science advances. One avenue is borrowing from other disciplines: like the rest of international relations, security studies has profited by drawing upon other bodies of knowledge. The other source of progress is competition between rival theories. Competition encourages contending approaches to refine their arguments and to seek better empirical support, and it usually leads them to incorporate each other's ideas as well. As noted earlier, the past decade has seen a partial convergence between the subfields of security studies, peace research, and international political economy, a development that is likely to benefit all three. The end of the Cold War will reinforce this trend by removing some of the substantive divisions between these subfields.³⁸ The lesson, of course, is that while competition is essential for scientific progress, scholars with different theoretical perspectives can learn a great deal from each other.³⁹

Security Studies and the Ivory Tower

A recurring theme of this essay has been the twin dangers of separating the study of security affairs from the academic world or of shifting the focus of academic scholarship too far from real-world issues. The danger of war will be with us for some time to come, and states will continue to acquire military forces for a variety of purposes. Unless one believes that ignorance is preferable to expertise, the value of independent national security scholars should be apparent. Indeed, history suggests that countries that suppress debate on national security matters are more likely to blunder into disaster, because misguided policies cannot be evaluated and stopped in time.⁴⁰

As in other areas of public policy, academic experts in security studies can help in several ways. In the short term, academics are well placed to evaluate current programs, because they face less pressure to support official policy.⁴¹ The long-term effects of academic involvement may be even more significant: academic research can help states learn from past mistakes and can provide the theoretical innovations that produce better policy choices in the future. Furthermore, their role in training the new generation of experts gives academics an additional avenue of influence. Assuming they perform these tasks responsibly, academics will have a positive—albeit gradual—impact on how states deal with the problem of war in the future.

The Role of Research Support

The renaissance of security studies was facilitated by increased financial support from several sources, especially private foundations. Managing the allocation of research support is an imposing challenge: among other things, there are no per-

³⁸ In the past, security studies tended to focus primarily on East-West issues, while international political economy concentrated on West-West and, to a lesser extent, North-South issues. The end of the Cold War will lead both subfields to address similar issues, such as the future of Europe; the result will be a fruitful competition between contending theories. I am indebted to John Mearsheimer for discussion on this point.

³⁹ Scholars in security studies would also profit from greater attention to some of the findings of peace researchers, while the latter could learn much from the former about identifying important theoretical and practical issues.

⁴⁰ Examples include Germany's Schlieffen Plan, the Japanese campaign of expansion in Asia in the 1930s, the Argentine junta's attempt to seize the Falkland Islands in 1981, and Israel's ill-fated invasion of Lebanon in 1982. In each of these cases, public discussion of strategy and foreign policy was suppressed, and the basic flaws in each strategy were not discussed openly. On this general problem, see Van Evera (1984, 1987) and Walt (1987a: 146–54).

⁴¹ Recent examples are Mearsheimer (1986) and Brown (1989).

factly reliable criteria for assessing the merits of competing proposals or the benefits of earlier decisions. Outside review committees can help, but any attempt to evaluate the role of outside support should acknowledge the inherent problems donors face and the laudable aims that many of them seek.

The renaissance of security studies suggests several lessons in this regard. Support for “scholarly infrastructure” has been the most effective way for private foundations to contribute to long-term progress. As discussed above, the Ford Foundation’s support for several academic research centers was essential in resurrecting the field. Since then, the MacArthur Foundation’s support for *International Security*, its institutional grants to a number of academic and research organizations, and the Ford Foundation’s continued support for the IISS have provided forums for the publication of serious academic research and for the airing of scholarly disputes, along with direct support for individual scholars.⁴² The Pew Charitable Trusts and the John M. Olin Foundation have also provided extensive support for research programs in security studies. If groups such as these continue to offer adequate financial backing, the momentum gained during the renaissance should continue.

The recent effort to enrich security studies by drawing scholars from other disciplines into the field (for example, through the MacArthur/SSRC “dual expertise” program) has had more mixed results. Although some “retooling” programs were valuable (especially for language training and to encourage natural scientists to learn about security issues), the belief that security studies was intellectually impoverished paid insufficient attention to the interdisciplinary approach that had characterized the field since its inception. Furthermore, making a major contribution to any discipline requires serious and time-consuming preparation; the necessary expertise cannot be acquired in a year or two. Security studies is no exception to this rule, and it is therefore not surprising that progress during the renaissance has come primarily from scholars within the field who drew upon other disciplines rather than from experts from other fields who suddenly turned their attention to security issues.

Foundations should take risks on occasion, of course, and the ultimate benefit of a particular initiative cannot be known in advance. Over the longer term, however, it is most important to maintain an active group of experts whose primary interest is security studies itself (Lebow, 1988:515). Because these individuals will have the greatest impact on future debates over national security, foundations that hope to influence these debates should ensure that these experts are adequately supported. Accordingly, importing scholars from other fields should be no more than a supplement to support for those with a demonstrated interest and a solid background in the field.⁴³

A more serious danger is the politicization of research support. In the past decade, a number of foundations with distinct ideological positions have entered the field.⁴⁴ If access to research support becomes contingent on “correct” political views, the integrity of security studies will be gravely threatened. In the short term, the presence of several ideologically varied foundations and the existence of many separate research centers has ensured that no single set of views has come to dominate the

⁴² In this respect, the MacArthur Foundation’s recent decision to end its financial support for *International Security* threatens the health of the field, unless alternative sources of support can be found.

⁴³ It should be noted that “dual expertise” programs have been a relatively small part of foundation activity; the bulk of recent funding in security studies has gone to established programs and topics.

⁴⁴ Within the field, the MacArthur Foundation and the Carnegie Corporation are usually seen as left-wing in orientation, the Ford Foundation is centrist, and the Olin Foundation, Pew Charitable Trusts, Scaife Foundation, and Smith-Richardson Foundation are seen as right-wing. For background on the second group, see Morgan (1981) and Blumenthal (1986).

field. But science is not a contest that the loudest or most lavishly funded forces should win; the quality of scholarship should be the dominant consideration. Over the longer term, foundations should support scholarship that follows the basic norms of science rather than research that conforms to particular political preferences. The goal is to encourage talented scholars to attack important questions, regardless of their ultimate conclusions. In short, keeping ideological litmus tests out of the funding process is essential to preserving the legitimacy of security studies as a scholarly enterprise.

Norms and Ethos of the Security Studies Community

The final set of lessons concerns the role of several informal norms within the field. These norms are neither unique to security studies nor universal among academic disciplines. Although a number of senior scholars provided important role models, these norms have been especially evident among younger members. Three principles merit special mention.

First, security studies has profited from a collaborative ethos. Members of the field are encouraged to exchange ideas, evidence, and criticism freely despite significant substantive disagreements. Admittedly, this norm is more an aspiration than a universally-accepted reality, but the spirit of cooperative criticism has helped individual scholars be more productive and enabled the field to advance more quickly.

This achievement is all the more remarkable given the range of opinion within the field. Far from being a clique of like-minded cronies, security studies has displayed extraordinary diversity over the past decade, punctuated by episodes of intense debate. One need only be familiar with the disputes already discussed (on deterrence theory, U.S. grand strategy, naval strategy, strategic weapons policy, surprise attack, the origins of World War I, and so on) to realize that the security studies field is not a unified group either methodologically, substantively, or politically. Yet with but a few notable exceptions, the field has avoided destructive professional rivalries.

What unites the field is a desire to increase our understanding of the role of force in international politics. Recurring debates testify to the strength of the field and are a major engine for its continued progress. As one participant has noted: "Scholarship on national security matters is, like all scholarship, a collective enterprise . . . [Individuals] publish their findings; this invites criticism from their colleagues, [and] provides foundations and inputs for the work of others. . . . By this method the community as a whole advances our understanding of specific methodological and substantive issues and of the field as a whole" (Posen, 1989:145).

A second norm is relevance, a belief that even highly abstract lines of inquiry should be guided by the goal of solving real-world problems. Because the value of a given approach may not be apparent at the beginning—game theory is an obvious example—we cannot insist that a new approach be immediately applicable to a specific research puzzle. On the whole, however, the belief that scholarship in security affairs should be linked to real-world issues has prevented the field from degenerating into self-indulgent intellectualizing. And from the Golden Age to the present, security studies has probably had more real-world impact, for good or ill, than most areas of social science.

Finally, the renaissance of security studies has been guided by a commitment to democratic discourse. Rather than confining discussion of security issues to an elite group of the best and brightest, scholars in the renaissance have generally welcomed a more fully informed debate. To paraphrase Clemenceau, issues of war and peace are too important to be left solely to insiders with a vested interest in the outcome. The growth of security studies within universities is one sign of broader participation, along with increased availability of information and more accessible publica-

tions for interested citizens. Although this view is by no means universal, the renaissance of security studies has been shaped by the belief that a well-informed debate is the best way to avoid the disasters that are likely when national policy is monopolized by a few self-interested parties.

Viewed as a whole, therefore, the renaissance of security studies has been a valuable development for international relations. As we are entering an era where new security problems will arise and new strategies for dealing with them will be required, the importance of security studies is manifest. If participants observe the norms that have guided the field in recent years and if adequate research support remains available, prospects for continued advances are good. And if future work both builds upon and challenges the results of the recent renaissance, that will be convincing evidence of the continued health of the field.

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