COMMENTARY

Policy and Politics in Security Studies

JACK S. LEVY

One of the central themes in Robert Jervis’s insightful review of security studies is that the field is profoundly political. Jervis focuses on the United States and argues that the security studies field has been significantly shaped by recent events and by American policy agendas. At the paradigmatic level, the interwar period led to idealist and liberal approaches, World War II to realism, the Vietnam War to critical orientations, and the post–Cold War period to multiple paradigms. There was an enormous emphasis on nuclear weapons and deterrence theory during the Cold War but not afterwards, an explosion of research on ethnonationalism and civil wars after the end of the Cold War, and growing interest in terrorism after the 9/11 attacks. One can extend this argument to diplomatic history as well as international relations theory (Levy 2001). As Croce (1960) suggested, “all history is contemporary history,” driven by contemporary problems and mindsets.

While I do not deny the basic argument, my aim here is to suggest that we need to balance this perspective with the recognition that many influential research programs in the field are driven more by autonomous theoretical or analytical developments or by evidentiary support than by recent events or policy agendas. Jervis concedes that there are some “internally-generated agendas and research programs,” but his emphasis is clearly on the role of normative considerations. A strong implication is that there is a significant gap between normative conceptions of scientific progress and the way that many research programs in the field actually develop.

In suggesting a useful corrective to this view, I do not address the question of which of these patterns is the more common. I also acknowledge that research programs are macro-level phenomena that represent the aggregation of many individuals’ decisions as to where to focus their intellectual energies, and this makes it very difficult to make general statements about the sources of or motivations for a general research program involving many scholars. While many were attracted to the democratic peace research program (Russett 1993; Ray 1995) because they believed that democracies were more peaceful (toward each other or more generally) and because they sought to promote a Wilsonian American foreign policy agenda, others engaged the debate because they questioned the scientific validity of the basic finding (that democracies rarely if ever go to
war with each other) and expected that they could falsify or at least qualify it by correcting flaws in existing research designs.

I focus here on the causes of war and international conflict more generally, which has been both a central and near-constant theme in the literature on international politics for two millennia, and at the same time one that has fluctuated to some extent with changing patterns of world politics and national policy agendas. In terms of contemporary research, one very important and influential line of argument, initiated by Fearon’s (1995) essay on rationalist theories of war, is that because war is an inefficient means of settling disputes, the question scholars ought to ask is what precludes states from reaching a negotiated settlement that avoids the mutual costs of conflict. This fundamental idea generated a major research program on bargaining and war (Gartzke 1999; Powell 1999, 2002; Wagner 2000; Filson and Werner 2002); focused particular attention on the role of commitment problems, private information and incentives to misrepresent that information, and the “costly signaling” of information; affected the study of ethnonational conflict (Lake and Rothchild 1998), economic interdependence (Morrow 1999), and other phenomena as well as interstate war; and influenced the qualitative as well as formal study of peace, war, and security.

The research program on bargaining and war grew out of a larger research program that incorporated uncertainty, or “incomplete information,” into a formal rational-choice framework. Over the last decade or so rational-choice modeling has become one of the most influential approaches to the study of peace, war, and security, and models incorporating incomplete information are central to nearly all of those efforts. Scholars have applied these models to the study of deterrence (Powell 1990; Fearon 1994), alliances (Smith 1995), diversionary behavior (Downs and Rocke 1994), the impact of regime type on crisis bargaining (Schultz 1998; Bueno de Mesquita et al. 1999), and numerous other substantive problems. This is a theory-driven research program that applies models from economics to particular substantive problems in international relations.

While these bargaining models have implications for policy, they did not arise from real-world developments or policy agendas. Rather, they were made possible by (and in fact were not feasible before) economists invented certain analytic techniques that permitted the analysis of games of incomplete information. The research program evolved through the common pattern of individual scholars attempting to resolve empirical and (primarily) theoretical anomalies.

Examples of research programs that have been propelled by theoretical or technical developments that are independent of recent events, U.S. policy, or particular ideological predispositions are not confined to rational choice. The concept of bounded rationality (Simon 1957) has had an enormous impact on the study of international relations and on political science more generally.
COMMENTARY ON JERVIS

(Jones 1999), and the development of prospect theory in social psychology (Kahneman and Tversky 1979) has led to numerous applications in the international relations field (Farnham 1994; Levy 1997). While applications of prospect theory to political science have been theoretically driven, I should note that the development of prospect theory itself was basically inductive rather than deductive, an attempt to integrate a diverse set of observed behavioral patterns into a theory that could explain them.6

This point about the inductive origins of some research programs brings me back to the democratic peace. While Jervis correctly notes that the democratic peace has an important policy component, and while others have developed this argument more fully (Lawrence 2002; Oren 2003), my own view is that the inter-democratic peace (which was introduced to the field by Doyle [1983a] and then by a special issue of *Journal of Conflict Resolution* [1984]), was driven primarily by the empirical evidence, at least in its early stages. It was the near-lawlike character of the inter-democratic peace proposition, in a field characterized by relatively few empirical regularities of even modest strength, that energized scholars, regardless of their normative biases or policy preferences.7 By the mid-1980s scholars had attempted to replicate the basic finding for the post-1815 system; explore its validity over additional spatial and temporal domains; determine whether it was a statistical anomaly or perhaps the spurious result of the influence of other variables; establish the direction of causality; explore potentially anomalous cases; devise and test alternative theoretical explanations for the observed patterns; and explore other testable implications of the democratic peace at the monadic and dyadic levels.

While the significant normative and policy implications of the democratic peace undoubtedly contributed to the expansion, persistence, and broader influence of this research program, particularly after the end of the Cold War, my argument, which requires much more thorough documentation than space permits here, is that the initial scholarly interest in the democratic peace in the 1980s can be explained by the natural tendency of scholars to investigate theoretical or (in this case) empirical anomalies in their research programs.

Let me end by noting that while the influence of normative/policy concerns on research programs is an important question for investigation, in many respects the more important question is not whether normative and policy concerns influence research programs (which is inevitable), but how. It is one thing if such concerns shape (1) the questions that scholars ask, or perhaps even their initial theoretical conjectures. It is quite another thing, however, if such concerns have a significant impact on (2) how scholars define their concepts, translate their conjectures into rigorously formulated theories, and construct research designs to test them. It is the impact of ideological biases and policy preferences on the latter—particularly if that impact is significant, unacknowledged, and not
compensated for in one’s research design (by considering alternative explanations and constructing hard tests, for example)—that poses the far greater threat to the integrity of social science.

Notes

1. I thank Katherine Barbieri, Ronald Krebs, and Carmela Lutmar for their helpful comments on this essay.

2. Jervis correctly notes that the American focus is substantively significant because the study of international relations in different countries varies with their governments’ different policy agendas and with their academic communities’ distinctive intellectual styles (Waever 1998).

3. I define “security studies” broadly to include both (1) theoretical and empirical studies of variations in militarized conflict over time and space, as well as studies of state strategies and actions affecting such conflictual outcomes, and (2) more policy-oriented research on national, international, or human security. These two research traditions overlap but are not perfectly congruent. While Jervis’s statement about the scholarly emphasis on nuclear weapons throughout the Cold War is undoubtedly correct, it is striking that most researchers associated with the “Correlates of War” project (Singer 1980), for many years the most influential large-N project in the field, did not usually incorporate nuclear weapons into their analyses. More recent large-N research projects commonly included a nuclear/non-nuclear variable.

4. A few years earlier, Bueno de Mesquita and Lalman (1992) had demonstrated that war is not an equilibrium outcome for two rational and unitary actors with complete information.

5. The key developments included the treatment of games of incomplete information (about adversary preferences) as games of imperfect information (about prior moves in the game) (Harsanyi 1967–68) and the refinement of key equilibrium concepts—including perfect equilibrium (Selton 1975) and sequential equilibrium (Kreps and Wilson 1982)—that permitted the solution of these games. The potential utility of these analytic tools for international relations was illustrated by an influential article by Alt, Calvert, and Humes (1988). Particularly influential in the study of bargaining was Rubenstein (1982). Note that Jervis (1970) anticipated much of the conceptual logic underlying signaling games. It is true, of course, that the Cold War provided an impetus for the early development of game theory.

6. Because prospect theory and behavioral decision theory more generally began as a response to observed anomalies in expected utility theory, and have progressed by identifying additional theoretical and empirical anomalies and generating new theoretical ways of dealing with them, they can be interpreted as fitting Popper’s (1962) conception of science as an alternating sequence of conjectures and refutations.

7. Another example of the discovery of strong empirical relationships leading to a significant line of research in an attempt to validate (or invalidate) the observed patterns and to explain them is the research program on territory and war (Vasquez 1993; Huth 1996; Hensel 2000). This was part of a larger shift in large-N research from the systemic to dyadic level of analysis, driven by the hope of finding other regularities comparable in strength to the dyadic democratic peace.
COMMENTARY

Nuclear-Deterrence Theory:
Where We Left Off When the Berlin Wall Came Down

ROBERT POWELL

As Robert Jervis observes in his interesting discussion of the intellectual development of security studies, “Nuclear weapons are central to the story” (Jervis, p. 108 in this volume). He then describes some of the most important ideas that shaped our thinking about nuclear deterrence during the Cold War. This commentary focuses in a little more detail on one part of the story.

In the late 1980s, an effort to reexamine some of the central ideas in nuclear-deterrence theory in light of some new developments in game theory was just getting underway. This work largely stopped when the Cold War ended, and it remains to be seen whether renewed concern about the spread of weapons of mass destruction and missile proliferation will revive it. In what follows, I briefly summarize where this work stood when the Soviet Union collapsed and then highlight one of the contributions that work made that helps to illuminate current debates about nuclear policies. Limitations of space force me to be very selective.

Nuclear-Deterrence Theory and Game Theory in the Late 1980s

The advent of secure, second-strike forces created a technological condition of mutually assured destruction (MAD) which rendered defense impossible and marked the Nuclear Revolution. But the nuclear revolution did not eliminate political conflicts of interest; it only changed the strategic setting in which these conflicts would take place. How, then, would these conflicts play out in this new strategic arena? How and to what extent could states exert coercive pressure on each other in order to further their interests—be those interests to protect what they already have or acquire more? A fundamental credibility problem lies at the heart of these questions, and we can see much of nuclear-deterrence theory as an effort to resolve this problem.

A state’s assured-destruction capability gives it the ability to make the cost an adversary has to bear in any conflict outweigh any possible gains. If, therefore, a state’s threat to impose these costs were sufficiently credible, an adversary would prefer backing down to pressing on in a crisis. This suggests that the ability to
exert coercive pressure turns on the credibility of the threat. But how can a state credibly threaten to impose a sanction that if imposed would subsequently result in its own destruction? Indeed, since both states have second-strike capabilities and can therefore make the costs outweigh any gains, why would either state be any more able to exert coercive pressure on its adversary than its adversary would be able to exert on it? Why do these capabilities not simply cancel each other out?

Schelling devised brinkmanship as a solution to the credibility problem. He argued that because neither state can physically protect itself from an adversary’s attack, the prospect of escalating to catastrophic levels of mutual destruction is always present in any crisis. Indeed, “it is the essence of a crisis that the participants are not fully in control of events . . .” (Schelling 1966, 97). The risk of accidental or inadvertent escalation to nuclear war was, in turn, the key to solving the credibility problem and to understanding the dynamics of coercion.

In a condition of mutually assured destruction, states cannot credibly threaten to launch a massive nuclear attack deliberately. But they may be able to make credible “threats that leave something to chance” (Schelling 1960, 187–203). That is, a state may be able to credibly threaten and actually engage in a process—a crisis or a limited war—that raises the risk that the confrontation will spiral out of control and escalate to a catastrophic nuclear exchange. How much risk a state could credibly threaten to run would depend on what was at stake in the political conflict. The higher the stakes, the more risk a state could credibly threaten to run.

Crisis in this view become a kind of brinkmanship or competition in risk taking. During a crisis states exert coercive pressure on each other by taking steps that raise the risk that events will go out of control. Each state, therefore, faces a series of terrible choices throughout a crisis. It can quit or it can decide to hang on a little longer and accept a somewhat greater risk in the hope that its adversary will find the situation too dangerous and back down. If neither state backs down, the crisis goes on with each state bidding up the risk until one of the states eventually finds the risk too high and backs down or until events actually do spiral out of control.

Brinkmanship is in effect a kind of auction (Powell 1990, 154–55). In a typical (English) auction, the bidders bid up the price until no one is willing to bid more than the last bid. At that point, the auction ends, the highest bidder gets the item on offer for the price she bid, and no one else pays anything. This, however, is only one of many different types of auction. In a second-price auction, for example, the highest bidder wins but pays the second-highest bid. In yet another type of auction called an “all-pay auction,” the highest bidder still wins but everyone pays the price that he or she bid.

Brinkmanship can be seen as a variant of an all-pay, second-price auction in
which bids are measured in terms of the risk that events will go out of control and not in terms of money. During a crisis, the states engage in military activities that in effect bid up the risk until one of the states finds the risk too high and quits or the crisis actually does go out of control. The state that prevails is the one that is willing to hang on longer, i.e., makes the highest bid. But the amount of risk that the states actually run during the crisis is determined by the state that backs down first. Thus, the “price” that each state must pay, i.e., the risk of disaster they must run, is determined not by the highest bid but by the second-highest bid. This makes brinkmanship an all-pay, second-price auction. By the end of the Cold War, the general view among academic deterrence theorists was that brinkmanship was the best model we had for thinking through the dynamics of nuclear crises (Posen 2000, 160 n. 7).

Where does game theory come into this story? Oddly enough, only a little bit and then only at the beginning and the end. Game theory did play some role in the early development of nuclear-deterrence theory, most notably in Schelling’s *Strategy of Conflict* (1960). But as O’Neill (1994) shows, this role was much less important than is generally believed. Indeed, game theory’s potential contribution was quite limited until the early 1980s, because we did not have the game-theoretic tools needed to formalize the two essential elements of brinkmanship, namely, credibility and the states’ uncertainty about each other’s resolve or willingness to run risks.

In the late 1960s, John Harsanyi (1967–68) showed how to formalize situations in which the players have asymmetric information, e.g., situations in which each player knows his or her own payoffs or willingness to run risks but does not know the other player’s. A decade later, game theorists began to develop a set of formal tools for analyzing credibility issues. When these two developments were combined in the early 1980s, they triggered an explosion of work that transformed economics over the next decade. By the mid-1980s these tools were beginning to be applied to nuclear-deterrence theory.

The end of the Cold War brought an abrupt halt to this work as it did to so much else in security studies. This left us with precisely two models of brinkmanship (Nalebuff 1986; Powell 1987, 1990) along with a few formal analyses of other aspects of nuclear-deterrence theory (e.g., Brams and Kilgour 1987a, 1987b, 1988; Kilgour and Zagare 1991; O’Neill 1986, 1987; Powell 1989a, 1989b; Wagner 1991; and Zagare 1990). This corpus was too limited and too inaccessible to most of the scholars in the field for it to have had much of an effect on academic thinking about nuclear deterrence, much less on the usually polarized policy debates about nuclear policies. Nevertheless, even this very limited amount of work suggested that we knew much less about nuclear deterrence than we thought we knew. It also was beginning to help clarify a set of issues that continue to be relevant to today’s policy debates.
Does Nuclear-Deterrence Theory Still Work?

One of the more important examples of this continuing relevance is that the formalization of brinkmanship as a kind of auction makes it especially clear that there is a critical distinction between the two questions “How does nuclear deterrence work?” and “Can the United States successfully use nuclear deterrence to secure its interests as it arguably did during the Cold War against the Soviet Union?” This distinction is often lost in policy debates, and this leads to some rather muddled thinking.

The first question is fundamentally theoretical. It asks if brinkmanship usefully describes important aspects of the basic mechanism through which political conflicts of interest play out in the shadow of nuclear weapons. A detailed answer to this question is beyond the scope of this discussion. But the thankfully limited empirical experience we have with nuclear crises suggests that the risk of escalation to nuclear war casts a long shadow. Brinkmanship is at least capturing an essential part of the story.

The second question is at the center of some current debates. Some analysts argue that even though deterrence worked during the Cold War, it is likely to fail in the post–Cold War world where the United States faces states that are willing to run much larger risks than the Soviet Union was. This, therefore, casts doubt on the United States’ ability to secure its interests through nuclear deterrence and, the argument continues, makes nuclear-deterrence theory irrelevant or dangerous when applied to the post–Cold War world. By contrast, some argue that if deterrence worked against the Soviet Union, it should also work against other states, including rogues (e.g., Keeney 1998; Lewis, Gronlund, and Wright 1999–2000; Mearsheimer and Walt 2003).

Both sides of this debate seem to be conflating the two questions above and to be confusing a theory with predictions derived from it. The current policy debate is fundamentally an argument about who would prevail in any future confrontation. More abstractly, it is a dispute about what a theory predicts about who would prevail.

There is, however, a critical distinction between a theory and the predictions derived from it. The latter depend not only on the theory but on the initial conditions as well. Who wins an auction and at what price depend not only on the rules of the auction, e.g., first-price, second-price, all-pay, etc., but also on how much the bidders value what is being auctioned off and on their uncertainty about the other bidders’ valuations. The risk of escalation to nuclear use and the fear of this suggests that the brinkmanship mechanism still provides a good model (or, at least, the best one we have) for crises. This, however, does not mean that the United States will be able to deter its adversaries as well as it ar-
guably did during the Cold War. Who prevails and at what risk in brinkmanship depends on the balance of resolve and the clarity of that balance. Should these factors differ in post–Cold War conflicts from what they were during the Cold War—and there is good reason to believe that they will (Glaser and Fetter 2001, 69; Powell 2003)—then the Cold-War experience will provide a poor guide to the future. In short, the theory may still apply but the initial conditions are likely to differ. The sharper theoretical formulations that emerged from the nascent formal work on deterrence theory held the potential of helping us see critical distinctions more clearly.

Conclusion

Sadly, nuclear issues are back in the headlines after being largely absent for a decade. There is renewed and acute concern about the spread of nuclear weapons to “rogue” states and, especially after the attacks of September 11, 2001, to terrorist groups. As a result, the United States is deploying a national missile defense and went to war against Iraq. Too often the newly nuclear-armed India and Pakistan have been on the threshold of war over Kashmir with the concomitant risk that “things will go out of control.” China is modernizing its nuclear forces while tensions across the Taiwan Straits ebb and flow. Nuclear deterrence plays a central role in all of these issues, and our understanding of nuclear-deterrence theory plays a central role in our analysis of them. All of this suggests a renewed interest in deterrence theory.

Notes

1. For discussions of the Nuclear Revolution, see Jervis (1984, 1989) and Schelling (1966).
2. During the Golden Age of nuclear deterrence theory—roughly the decade from the mid-1950s through the mid-1960s—theorists devised two basic ways of dealing with the credibility problem, brinkmanship and the strategy of limited retaliation. Reasons of space limit my discussion to brinkmanship. For elaborations of brinkmanship, see Jervis (1984, 1989) and Schelling (1966). On limited retaliation, see Knorr and Read (1962). Powell (1985, 1990) compares these two approaches to the credibility problem.
3. See Paul Milgrom (1989) for an introduction to auction theory.
4. The 1994 Nobel Prize in economics recognized this work along with that of John Nash. Kreps (1990) provides an accessible and nontechnical survey of some of these developments.
5. As Betts observes, the relatively sanguine view that some take about the way deterrence worked during the Cold War is largely based on the second half of the Cold War. But international politics seemed much more dangerous and fraught with peril during the first part of the Cold War when the balance of resolve over various issues was much less clear.
“The early phase of the Cold War, before the crises over Berlin and Cuba worked out the limits to probes and provocations, is a less reassuring model. Only with hindsight is it easy to assume that because the superpowers did not go over the edge, it was foreordained by deterrence that they could not” (Betts 2000, 71).

6. See, for example, Bundy (1988) and Betts (1987). Powell (2003) elaborates the argument that brinkmanship often applies after the Cold War and then uses this model to analyze the effects of NMD and rogue states on stability.

7. This argument is developed in detail in Payne (2001).
COMMENTARY

Academics, Decision Makers, and Security Policy during the Cold War: A Comment on Jervis

THOMAS C. SCHELLING

Jervis states that “seeing security studies as being created at the end of World War II is very much a simplification, but one with some validity.” I would have said, “not very much a simplification.”

He mentions the centers and institutes set up at universities after the war; he mentions the journals *World Politics* and *International Security*, and he mentions RAND. I think there is more to be said. There was a wholly unprecedented “demand” for the results of theoretical work; scholars had an audience and scholars had access to classified information. Unlike any other country, with the possible exception of Israel, the United States had a government permeable not only by academic ideas but by academic people, especially under Democratic Party administrations.

Harvard, MIT, and RAND alone provided the new Kennedy administration with the White House national security adviser and two of his deputies, the president’s scientific adviser, two assistant secretaries of defense and a deputy assistant secretary and the director of the Advanced Research Projects Agency (ARPA), two assistant secretaries of state, and numerous other slightly less conspicuous academics. Academics populated the Defense Science Board and the Scientific Advisory Board of the air force and populated and chaired numerous advisory committees on strategic weaponry and arms control. Academics lectured at the several war colleges, which themselves adopted “academic” curricula. The Institute for Strategic Studies in London, like Harvard, MIT, the Council on Foreign Relations, and numerous other centers and institutes, hosted military officers at the rank of colonel for an academic year. (Captain Elmo Zumwalt spent his year at the ISS and later became Chief of Naval Operations.) The services sent younger officers to universities for Ph.D.s. (Brent Scowcroft has a Ph.D. in international relations.)

Foundations played an unprecedented role in financing whole centers, professorships, research projects, conferences, and travel; and the navy and the air force had their own external research programs in security studies. One of the lasting influences of the McNamara regime at Defense was the stimulation of the military services to train themselves to compete with, not object to, the highly educated civilians, and eventually to cooperate.
There was nothing remotely like this before World War II.

Referring to the “vulnerability” of the Strategic Air Command’s weapons, Jervis says, “This Wohlstetter made clear to the general public in 1959 when, in response to the Air Force’s refusal to act on his arguments, he published a declassified version of his argument.” The Wohlstetter analysis was made available to the Gaither committee in 1957; in 1958, in anticipation of the “surprise attack” negotiations scheduled for late fall, an assistant-secretary level committee considered the problem of surprise and identified it as the vulnerability problem; and when the British, Canadians, French, and Germans joined the Americans in Washington to prepare for the negotiations, Wohlstetter’s manuscript, “The Delicate Balance of Terror,” was circulated.

The significance of this analysis can hardly be overstated. It was the death sentence for Atlas and Titan, it was the raison d’être for Polaris and Minuteman, and it became the infrastructure that supported the ABM treaty a decade later.

A small point, but one worth making in relation to Jervis’s noting the “conventionalizers’” belief only in deterrence by denial, is that Herman Kahn was never so simplistic as to share their ideology. His fervent advocacy of civil defense notwithstanding, he was among the most subtle in his appreciation of holding enemy populations hostage.

Jervis remarks that “to label a study or a topic one of national security gives it standing, legitimacy, and a claim on national resources and priorities.” And how! We have “environmental security” and public health as national security, education as part of national security; and at a meeting with the Soviet Academy of Sciences in 1990 I learned that identifying health or environment with “security” had budgetary advantages even in that society.

What I’ve said above is a few amendments (or endorsements) to Jervis’s masterly survey of security studies. What has he omitted that needs to be recognized? I think most notably he has neglected one of my favorite strategic theorists, one whom I take pride in having recognized early in his career, namely, Robert Jervis. Jervis gets a couple of citations, but no extended treatment. Jervis’s treatment of “perceptions” is a landmark contribution that must have been the victim of modesty in this essay.

I think Barry O’Neill’s recent Honor, Symbols, and War (1999) introduces a dimension to the understanding of security issues that was largely unrecognized before, that has not yet influenced strategic thinking but almost certainly will in future. Jervis’s study is retrospective; if it were more prospective it might include O’Neill’s contribution.

Finally, I’m happy that Jervis made no mention of game theory. Recall Anatol Rapoport’s Fights, Games, and Debates (1960) that blamed game theory for giving a zero-sum flavor to security thinking. At the time I thought he didn’t know what—or rather, whom—he was talking about. I believe that rudimentary
game theory has been to some security theorists a help in formulating ideas; it has provided some useful terminology; but I do not believe that any theoretical contributions to security studies has been the least dependent on “game theory.” Some of the work may fall within the domain of game theory; but, as far as I can tell, none that does has been dependent on game theory. I believe game theory got, in Jervis’s survey, just the recognition it deserves.
COMMENTARY

Taking Stock of Security Studies

MARC TRACHTENBERG

Robert Jervis’s goal in this paper is to take stock of the field of security studies—to look at how the field developed, especially in the United States during the Cold War period; to bring out what was distinctive about it; and to survey its strengths and weaknesses. Perhaps his most important point is that security studies was always a highly politicized field. Policy concerns in large part set the academic agenda. Scholars sought to influence policy in very direct ways, and their arguments were often rooted in their own personal political beliefs.

For Jervis, this situation had its good points and its bad points. On the one hand, this emphasis on the problems of the real world—above all, on the dramatically new and extraordinarily important problems having to do with nuclear weapons—was a major source of intellectual vitality. But this type of approach also had a downside. The field was not quite as scholarly, as serious, and as free of political bias as it might have been. And there was a special problem, Jervis points out, stemming from the fact that the field was neither fish nor fowl: the policy agenda and the more purely academic agenda kept getting in each other’s way. A realist theory of international politics, for example, implies that states do not need instruction in policy matters; criticisms of policy are thus somewhat inconsistent with the basic thrust of that theory. But people rarely faced up to difficulties of this sort, and scholars instead saw no problem with trying to do a little of everything.

Still, Jervis tends to take all of this philosophically. A field of this sort, in his view, could never really be divorced from policy concerns, and a degree of politicization was more or less inevitable. Taking that into account, the pluses and minuses pretty much balance each other out. And there is the related issue of the shape of the field—what defines it, and what should define it. For Jervis, this set of issues is not of central importance. Field definitions are more or less arbitrary, and the fields are defined simply by the practical need to “carve out a manageable area of study.” Good work is good work, and bad work is bad work, no matter how it is labeled.

These are the issues I want to comment on here. The basic point I want to make is that politicization (to use a single word to refer to the cluster of characteristics Jervis talks about) is not to be viewed in relatively neutral terms. I think the effect has been quite negative, and that the really important work was done when people were able to see beyond their own personal political predilections and grapple
with these issues on a more abstract, more purely intellective level. I also think that the definition of the field matters more than Jervis suggests, and that some of the problems with security studies as that field took shape in America in the 1950s and 1960s had to do with the particular way it came to be defined.

But let me begin by emphasizing the one main point here where I think Jervis is absolutely right, and that has to do with the extraordinary emphasis people in the field traditionally placed on practical issues of policy. No less a figure than Bernard Brodie once wrote that strategy was concerned above all with what he called “how to do it” problems. The assumption was that it was preeminently a policy science, and was not concerned primarily with simply understanding the world.

I was somewhat taken aback by the fact that academic work in this area had that kind of flavor when I first became interested in the field in the early 1980s. My goal at that time was to understand international politics in the Cold War period. Nuclear weapons obviously loomed large in the story, and the general question of what impact nuclearization had on international politics was clearly of fundamental importance. All I knew at that point was that I needed some real guidance in this area from people who had thought seriously about this problem, and the most important books in the field were not hard to identify.

One of the first books I read was Henry Kissinger’s *Nuclear Weapons and Foreign Policy* (1957a), which had been originally published in 1957. This was clearly an important work in the field, and the title seemed to promise that this book would give me what I was looking for. That book, I naively thought, would be a serious, academic study of the relationship between nuclear weapons and foreign policy—a study of how nuclear weapons influenced the conduct of foreign policy, an analysis perhaps of how the nuclear revolution affected the way international politics worked. It turned out that Kissinger was not fundamentally concerned with those issues at all, and that the main goal of the book was to make the case for a particular strategy that he wanted the U.S. government to adopt, a strategy of limited nuclear war. The book itself was not really scholarly in the strict sense. If it was a major work in the field, then that in itself suggested that the field as a whole was a little odd by the usual academic standards.

This is not to imply that it was wrong for scholars to concern themselves with issues of policy. The problem was one of balance. The focus on policy tended to a certain extent to crowd out the more scholarly kind of analysis. It was as though the really fundamental issues were not worthy of study—that at the fundamental level, the answers were so obvious that those central problems did not really need to be studied. The basic assumptions, in fact, tended to be a little simplistic. On the right, the assumption was that deterrence was good, so the goal was to get as much deterrence as you could. Deterrence, in the sense of what dissuades a potential enemy considering whether to start a war, thus became the be-all and end-all of strategy. On the left, it was taken for granted that arms races were a basic source of
international tension, that war could result from “strategic instability,” and that arms control was therefore a key to peace. The central questions here—whether those assumptions were in fact valid, and whether it made sense to focus so heavily on issues of that sort—were more or less ignored. To take the argument about arms control, for example, it did not make sense (as Jervis notes) for people who believed that the nuclear stalemate was rock-solid, and that real strategic superiority was therefore beyond reach, to argue that “halting the arms race” was of such fundamental importance. If the strategic balance was never really at risk, where was the problem? If there was no real incentive to strike first in a crisis—if there was no real risk, that is, of “crisis instability”—why was the need to prevent “crisis instability” taken as a compelling argument for arms control?

You can see the problem when you look at the discussion of ballistic missile defense, especially in the 1960s and the 1980s. Opponents of BMD argued, often in the same breath, that such systems would be both ineffective and terribly destabilizing, as though they could possibly be both things at the same time. And yet since the focus was on policy, it made sense to use any argument that served one’s immediate political purposes. Inconsistent arguments could easily coexist in the same political space; politicization thus tended to lower the intellectual standards of the field.

What I have said here applies to much that was written on security issues during the Cold War period. It applies with particular force, I think, to much of the work done on policy issues at that time. But the last thing I want to do is to imply that the field as a whole is to be understood in such terms. There were those who (as Brodie put it) understood from the start that the problems they were dealing with were problems of “great intellectual difficulty, as well as other kinds of difficulty,” and the work they produced was the glory of the field. But that work is of permanent value precisely because it was not a mere projection of their personal political views.

Consider, for example, the origins of the stability doctrine—the idea that the best of all possible nuclear worlds is one where no nuclear power has an incentive to be the first to launch a nuclear attack. Jervis notes the importance of the idea and says that while “many minds contributed to the development” of this concept, it was Thomas Schelling “who expressed it most clearly as ‘the reciprocal fear of surprise attack.’” But where in fact did Schelling get the idea from? Schelling told me that he first saw it in a draft of Albert Wohlstetter’s famous “Delicate Balance of Terror” article, which was being circulated among the experts preparing for the 1958 Geneva conference on preventing surprise attack; and in fact a passage making that argument appears in the final version of that article published in Foreign Affairs in 1959. Wohlstetter, as Jervis says, was on the hawkish end of the spectrum in the security studies world, but it was the hawkish Wohlstetter who evidently played the key role in giving birth to a con-
cept that would provide the basis for a relatively dovish approach to policy issues, a concept that in particular justified attaching great importance to arms control. But this could happen only because Wohlstetter was trying to do more than simply make a case for a particular policy.

As for Schelling himself, his thought is scarcely to be understood in narrow political terms. He was concerned both with how nuclear forces could be made more usable for political purposes (with his idea of the “threat that leaves something to chance”) and also with how nuclear weapons could be made less usable (by taking measures, based on an understanding of the “dynamics of mutual alarm,” that would increase “crisis stability”). Schelling was of course concerned with policy problems. His analysis of those issues, however, was not simply ad hoc, but was rather rooted in something deeper—that is, in insights that took shape as he grappled with these problems at a more general level. It was the thinking that took place at that level that led to work of enduring value.

The same general point can be made about Brodie. Brodie was of course concerned with basic issues of policy, and above all with the central questions of nuclear strategy. But when you read his work, you never get the sense that he was pushing a particular political agenda. Instead, you get the sense that he was grappling with problems of the utmost importance in a very serious way. He was too intelligent to think that those problems had easy answers; he was distrustful, on temperamental even more than on intellective grounds, I think, of the sort of particular answers that one side or the other was pushing in the public debate, and he always tended to lean against the prevailing winds. But it was that critical stance, that ability to step outside the political debate, that for me at least makes Brodie such an attractive figure.

This issue of what the field should be—of what sorts of approaches it should value and of what kind of work it should try to keep at arm’s length—is therefore, I think, of fundamental importance. In other words, the question of how the field is to be defined really matters, because the basic question here is what sort of work is to be encouraged. If the goal is to assess the field of security studies in the form it took in America during the Cold War period, it makes sense to think about the kind of intellectual personality the field came to have. Was that particular personality a source of strength, or of weakness?

There are many things that can be said in this context, and I have to admit that I, as a historian and thus as something of an outsider in the field, am particularly sensitive to this whole set of issues. But let me focus here on just one of them, the sort of field definition suggested by the very term “security studies,” a term that suggests the field is concerned above all with military issues—with weaponry, with strategy, with arms control, and so on.

Now, once again, there was nothing wrong in itself with studying military issues, just as there was nothing wrong with writing on current issues of policy. But
that emphasis on the military side of the story had to my mind one very unfortu-
nate consequence. People tended, I think, to lose sight of the political core—to
lose sight, that is, of what international politics is about. The question was what
would “strengthen deterrence” or “improve crisis stability,” and not, for example,
what would affect the course of the political process that could give rise to a cri-
sis in the first place. Kenneth Waltz at one point criticized the liberal institution-
alis for wanting to “take the politics out of international politics”—a fair point,
I think, but that kind of criticism could be made of the security studies field more
generally. In work on war causation especially, the purely political side of the
story did not, as a general rule, receive anything like the attention it deserved.

Let me again give the example of ballistic missile defense. In the two rounds
of debate on this issue—first ABM and then SDI—the central question was
whether strategic defense, in and of itself, was a good idea. The supporters of
BMD said that of course America should try to protect its own population from
enemy attack; the opponents said that missile defense was unworkable and
could undermine stability. What was wrong with that debate was that neither
side really framed the issue in political terms. Neither side took it for granted
that the basic goal was to influence the course of U.S.-Soviet relations—to in-
fuence a political process that could conceivably lead to war or to a relatively
stable international system. But from a Clausewitzian point of view, war is the
outcome of political conflict, and so the goal of policy is to influence an adver-
sary’s political behavior by structuring the incentives within which he operates.
If we are unhappy with what an adversary is doing, we reach for strategic ad-
antage. The aim is to get an adversary to see that he will pay a price—perhaps
simply an economic price, if he chooses to take countermeasures—for engaging
in that kind of behavior. On the other hand, arms control—in this case, in the
form of an agreement not to deploy missile defense systems—would be the re-
ward for moderate political behavior. Arms control would not be an end in itself,
but would go hand in hand with political détente. But given the culture of the
field, this type of thinking did not come naturally, to say the least.

Security studies thus came in that sense to have a certain apolitical flavor,
and that, I think, was one of its major weaknesses. War was not viewed in es-
tentially political terms—that is, as the outcome of a political process, with the
aim of policy being to influence how that process ran its course. War was instead
generally viewed either as a kind of accident, resulting from such things as the
“reciprocal fear of surprise attack,” or as the product of aggression resulting
from a “failure of deterrence.” To a historian, both types of approach, or even
the two together, seemed terribly inadequate. They did not come close to captur-
ing the phenomenon of war causation as we historians understand it.

So much for the past. What about the present, and the future? I personally
very much like the way security studies has been developing in recent years. I
think it is more intellectually serious now. People ask important questions and they are able to analyze them more rigorously. The best people in security studies know how to use historical material as well as any professional historian, and this in itself is a very important development. People are still interested in current issues of policy, but the field has, I think, become much less politicized and more scholarly, and that to my mind is a very positive development.