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# SHOULD STRATEGIC STUDIES SURVIVE?

By RICHARD K. BETTS\*

A specter is haunting strategic studies—the specter of peace. This sounds odd so long after the burst of euphoria at the end of the cold war, which dissipated into so many nasty little wars. Political science, however, has been less interested in war per se than in cataclysmic war among great powers, war that can visit not just benighted people far away, but people like us. Half a century of world war and cold war provided that impetus for strategic studies. After the cold war, however, universities face other demands as resources shrink. Has the warrant for feeding this field expired? Certainly not.

First, one interest alone fully justifies keeping the flame burning: to have expertise on the shelf in case great-power conflict arises again, which is more likely to happen than not. For whatever reason, the United States finds itself in a war or crisis in almost every generation.

Second, confusion continues about what U.S. foreign policy should expect military power to do for less vital interests. What force can accomplish in a specific situation does not follow directly from standard international relations theories or rational choice models; the answer depends on military technology, organization, and doctrine, and how they fit with local political and geographic circumstances. After the cold war, liberals, on the one hand, who spent the last thirty years trying to reduce American military power, demanded that Washington “do something” with the armed forces to suppress atrocities, promote democracy, and keep peace in places like Bosnia, Somalia, and Haiti. Conservatives, on the other hand, insisted on buying hefty forces but not using them. Vague notions that military power can impose political solutions at a reasonable cost, or that outside military power is useless for doing so, were subjected to little analytical discipline after 1990. If capacity for informed strategic analysis—integrating political, eco-

\* Thanks to Robert Art, David Baldwin, Michael Desch, Peter Feaver, Stephan Haggard, Michael Handel, Samuel Huntington, Robert Jervis, Miles Kahler, David Lake, Michael Mandelbaum, John Mearsheimer, Barry Posen, Cynthia Roberts, Gideon Rose, John Ruggie, Warner Schilling, Jack Snyder, Barry Steiner, Marc Trachtenberg, and Stephen Walt. The value of their criticisms exceeded my ability to incorporate them within length restrictions, which also limited bibliographical footnotes to illustrative examples rather than recognition of the full range of important works.

conomic, and military judgment—is not preserved and applied, decisions on the use of force will be uninformed and, therefore, irresponsible.

Third, the size and composition of the U.S. defense budget are crucial, affecting fiscal and social policy as well as foreign affairs. Who can rationally recommend whether the budget should be higher or lower, or what it should buy, without any expertise on the nature of military forces and what combinations of them are necessary to achieve objectives set by elected officials? If civilian strategists are not to decide along with the professional military, either ignorant civilians will do it, disjoining political and military logic, or the military will do it alone.

Fourth, U.S. civil-military relations are problematic. The armed forces were reformed and rejuvenated over the same time that political leadership loosened oversight. Reagan's romantic nationalism made for laissez-faire civilian control, and Clinton's impaired moral authority, owing to his own draft evasion, precluded vigorous guidance as commander in chief. After Vietnam, the military became more popular with the mass public as the elite distanced itself from it. Fewer civilian policymakers have experienced military service themselves, while the military institution as it shrinks is growing apart from society after a half century of closeness enforced by the mass mobilization of world war and cold war. There is no danger of direct insubordination, but a larger proportion of military officers now feels more competent and more moral than the rest of their country and less respectful of their government. Education in strategy will not solve problems in civil-military relations and might even aggravate conflict if it emboldens civilians to question military judgments. But if checks and balances matter, it can only help.

Strategic studies is both necessary and contested because it focuses on the essential Clausewitzian problem: how to make force a rational instrument of policy rather than mindless murder—how to integrate politics and war. This requires the interdisciplinary joining of military grammar and political logic, in Clausewitz's terms, a marriage that gets lip service in principle but is often subverted in practice by those who identify more with one half of the union than the other. Soldiers often object to politics permeating war because it gives civilians the right to meddle in operations, while many intellectuals object to dignifying war as an instrument of policy or an academic priority. For all these reasons, political science became the main academic home for the field, and the place of military affairs within it is periodically challenged.

Within a field of international relations constantly riven by sectarian debates about overarching frameworks like realism, liberalism, and their

“neo” variants, the murky boundaries of strategy fuel controversy. To clarify where strategic studies *should* fit, think of a subfield of three concentric circles: at the core is *military science* (how technology, organization, and tactics combine to win battles); the outer, most inclusive ring is *security studies* (everything that bears on the safety of a polity); and in the *middle* lies *strategic studies* (how political ends and military means interact under social, economic, and other constraints).

The distinctions are relevant in principle, because they illustrate why strategic studies should be the most important part of the subfield—broader in scope than strictly military problems, but more focused than security studies, which is potentially boundless. In practice, however, the distinctions solve few problems because the dividing lines between strategic studies and the other two layers can never be clear, and the distinctions are not recognized institutionally. Only security studies has academic standing, so the place of strategic studies emerges through debates about defining security. Most scholars of security identify it with strategic studies, but much of what they do strikes some in other subfields as too close to military science for comfort. Critics then argue for reorienting the security subfield to so many other issues that the military core may become a pea lost in an amorphous ball of wax. The intellectual coherence of strategic studies increases with linkage to the military core, but institutional status and legitimacy grow with distance from it.

One danger in strategic studies is missing the political forest for the military trees. That danger was greater during the cold war than now. The opposite danger—that defining security broadly will squeeze out work on the military aspects—is greater now. There is no consensus that attention to military matters remains an important responsibility for social science, or even that knowledge of military systems is as vital for studying security as knowledge of economic systems is for studying political economy.

### THE CASE FOR SCIENTIFIC STRATEGY

The case for strategic studies had to be made a half century ago as well. Bernard Brodie’s 1949 article, “Strategy as Science,” was a brief for developing strategy as a systematic field of analysis because it was “not receiving the scientific treatment it deserve[d] either in the armed services or, certainly, outside of them.”<sup>1</sup> The only scholars who had paid much attention to the subject up to that point were historians. The

<sup>1</sup> Brodie, “Strategy as a Science,” *World Politics* 1 (July 1949), 468.

methodological model that Brodie endorsed was the one represented by the discipline of economics.

Perhaps Brodie should have heeded the warning to be careful what you wish for, lest you get it. Much of what he recommended came to pass, but with results that did not entirely please him or critics who had little use for his aim from the beginning. Brodie had in mind an instrumental science for solving practical problems. This evoked skepticism on two fronts. Although the services sometimes welcomed analysis by civilian scholars, many military professionals regarded outsiders' work on strategy as impertinent interference. Although scholars of strategy established lodgments in universities and think tanks, many intellectuals saw them as unprofessional or immoral, considering instrumental science inferior to loftier theoretical work, or, when applied to managing violence, the work of the devil.

Most scholars of international relations recognize that war is an important problem but are interested only in the before and after, not in war itself—in war's causes and consequences, but not its conduct, which is considered somehow epiphenomenal or intellectually puerile. Strategic studies is concerned with all three phases of war because they are interdependent; conduct becomes cause, as mechanisms of violence shape decisions about its political application. It is impossible to understand impulses and choices in the political dimension of war or peace without understanding constraints and opportunities in the military dimension. Options for *how* to make war affect *whether* war is made, who wins or would win, and thereby the shape of the postwar world (or the peacetime world, if anticipated results of combat affect diplomatic deals). For example, it is not possible to understand how Germany managed to rule Europe for half of the 1940s without understanding how it overcame the opposing might of France and Britain as it had not been able to do in 1914. This cannot be explained by indices of power (GNP, population, the size of armed forces) that are accessible to nonspecialists but only by grasping innovations in the process of combat—how the Wehrmacht adapted the technology and doctrine of armored warfare to revolutionize operations. Similarly, one cannot understand why Germany ultimately failed by looking at military science, but only by looking to wider dimensions of strategy—the ideological and psychological reasons for Hitler's miscalculations in invading the Soviet Union and declaring war on the United States.

Intellectual support for strategic studies parallels cycles of international conflict and calm. When the danger of war obtrudes in the real world, the study of war prospers, because the academy considers it un-

avoidable. When danger slackens, academic interest or tolerance falter. Two decades after "Strategy as a Science" was published, as Vietnam was destroying the cold war consensus, Hedley Bull noted that the professional strategist's status was tenuous due to controversy over the legitimacy of the very question at issue: "What shall the state do with its military force? . . . [T]here will not be general agreement about the worth and utility of students of strategy, in the way that there is . . . about that of students of medicine, architecture, or economics."<sup>2</sup> Nearly half a century after Brodie's article, in the happy wake of the cold war, David Baldwin argued that "perhaps the time has come to abolish the subfield of security studies."<sup>3</sup>

The intellectual advances Brodie sought in 1949 did not solve all the problems he saw, and created some new ones. In the enthusiasm for science, strategic studies developed a scientific strain and overreached. Nevertheless, with later leavening of the scientism by better comparative historical analysis in the second half of the cold war, Brodie's brief yielded progress. If Baldwin's advice prevails, the problems that motivated Brodie—the superficial quality of analysis available to support public decisions about war and peace, and the absence of civilian analytical checks on preferences of the professional military—will grow again.

Brodie spoke as the Clemenceau of the academy: strategy was too important to be left to the generals. As one who knew military history and moved among those in uniform as a wartime officer and peacetime consultant, he was frankly cynical about the cultural and organizational constraints that inhibited serious strategic analysis by soldiers themselves. He considered professional officers unattuned to strategy because the complexity of military operations made them preoccupied with tactics and technology. He believed that regular officers view strategy in terms of the hallowed "Principles of War" (maxims about "the objective," "economy of force," "unity of command," and so forth that appear in manuals of most Western armies), that they have difficulty grasping the real meaning of Clausewitz's insight on the relation between war and politics, and that anti-intellectualism and hierarchy prevent trenchant thought. In Brodie's view, "political scientists . . . are concerned with the *context* of military operations," whereas "to the military, the means available, rather than the object, are what determine the character of a war" (pp. 467–68, 473, 486).<sup>4</sup>

<sup>2</sup> Hedley Bull, "Strategic Studies and Its Critics," *World Politics* 20 (July 1968), 596.

<sup>3</sup> David Baldwin, "Security Studies and the End of the Cold War," *World Politics* 48 (October 1995), 135.

<sup>4</sup> See also Bernard Brodie, *Strategy in the Missile Age* (Princeton: Princeton University Press, 1965), 11, 13; idem, *War and Politics* (New York: Macmillan, 1973), 9–11; idem, "Scientific Progress and Political Science," *Scientific Monthly* 85 (December 1957), 317.

Since military authors are tied to their services, it is hard for anyone but a civilian to proffer analysis independent of service doctrine. (There are exceptions. Perry Smith published an unflattering account of his service's strategic planning, yet survived, through the support of a patron, and reached two-star rank himself. Andrew Krepinevich savaged his service's doctrine in the Vietnam War, but finished his career as a lieutenant colonel working in the civilian reaches of the Pentagon.<sup>5</sup> Most officers who challenge their services wait until retirement.) Moreover, the nuclear revolution put the dominant level of warfare beyond experience, which is the main teacher in the military ethos. Thus when strategic studies burgeoned in the 1950s, most of the writing was by civilians.

As Brodie noted in 1949, "The military profession is by no means alone in its frequent recourse to the slogan as a substitute for analysis—certain scholarly disciplines, not excluding political science, have been more than a little untidy in this regard" (p. 471). He saw economics, the most developed social science, as the model because strategy is about "problems involving economy of means, *i.e.*, the most efficient utilization of potential and available resources" (p. 475). Choices in weapon procurement, for example, should not be governed by slogan-like concepts like "balanced force," but by marginal utility (pp. 478–81).

All of this anticipated currents that would dominate the development of strategic studies in the first half of the cold war. Brodie wrote his article while at Yale, but at the same time that he was beginning his affiliation with the fledgling RAND Corporation. Established by the Air Force, RAND became a magnet for those who wrestled intellectually with the strategic challenge of the nuclear revolution. Some like Brodie, William Kaufmann, and Alexander George were political scientists versed in history, but most were mathematicians, physicists, or economists like Albert Wohlstetter, Herman Kahn, Thomas Schelling, James Schlesinger, Andrew Marshall, Henry Rowen, Malcolm Hoag, Carl Kaysen, and Daniel Ellsberg. This group spawned much of the theoretical corpus that undergirded academic study of strategy during the cold war.<sup>6</sup>

<sup>5</sup> Smith, *The Air Force Plans for Peace, 1943–1945* (Baltimore: Johns Hopkins Press, 1970); Krepinevich, *The Army and Vietnam* (Baltimore: Johns Hopkins University Press, 1986).

<sup>6</sup> Fred Kaplan, *The Wizards of Armageddon* (New York: Simon and Schuster, 1983); Lawrence Freedman, *The Evolution of Nuclear Strategy*, 2d ed. (New York: St. Martin's Press, 1989); Barry Steiner, *Bernard Brodie and the Foundations of American Nuclear Strategy* (Lawrence: University Press of Kansas, 1991).

## THE FIRST CYCLE OF COLD WAR STRATEGIC STUDIES

The year after Brodie's article appeared the Korean War confirmed the militarization of the East-West conflict, U.S. defense spending tripled, NATO became the centerpiece of foreign policy, and strategy became big business. In universities, realist theory and security policy took over the field of international relations, eclipsing the subfields of international law and organization that had dominated in the interwar years. In the 1950s and 1960s the Social Science Research Council's (SSRC) Committee on National Security Research under William T. R. Fox built a network of academics. University programs sprang up at: Princeton's Center of International Studies, where Klaus Knorr theorized about war potential, economic mobilization, and NATO strategy, and which produced works on deterrence by Glenn Snyder, William Kaufmann, and Herman Kahn; Columbia's Institute of War and Peace Studies, which sponsored research by Kenneth Waltz, Samuel Huntington, Paul Hammond, Warner Schilling, and others on causes of war and defense policy-making; Ohio State's Mershon Center, which supported not only mainstream research on security, but critics as well, such as Philip Green; Harvard's Center for International Affairs, where Henry Kissinger continued to make his mark after the publication of his Woodrow Wilson Award-winning book on nuclear strategy for the Council on Foreign Relations; and MIT's Center of International Studies (and later its Defense and Arms Control Studies Program). In London, the International Institute for Strategic Studies was established and has since provided a steady stream of analytical publications and unclassified data compilations.<sup>7</sup>

Professors jumped into policy prescription, beginning with *The Absolute Weapon*, edited by Brodie.<sup>8</sup> Strategy might not have developed academically outside of military history if not for the nuclear revolu-

<sup>7</sup> Knorr, *The War Potential of Nations* (Princeton: Princeton University Press, 1956); Knorr, ed., *NATO and American Security* (Princeton: Princeton University Press, 1959); Snyder, *Deterrence and Defense* (Princeton: Princeton University Press, 1961); Kaufmann, ed., *Military Policy and National Security* (Princeton: Princeton University Press, 1956); Kahn, *On Thermonuclear War* (Princeton: Princeton University Press, 1960); Waltz, *Man, the State, and War* (New York: Columbia University Press, 1959); Huntington, *The Common Defense* (New York: Columbia University Press, 1962); Schilling, Hammond, and Snyder, *Strategy, Politics, and Defense Budgets* (New York: Columbia University Press, 1962); Demetrios Caraley, *The Politics of Military Unification* (New York: Columbia University Press, 1966); Michael Armacost, *The Politics of Weapons Innovation* (New York: Columbia University Press, 1969); Green, *Deadly Logic* (Columbus: Ohio State University Press, 1966); Kissinger, *Nuclear Weapons and Foreign Policy* (New York: Harper, 1957). IJSS publications include the journal *Survival*, the Adelphi Papers, and the annuals *Military Balance* and *Strategic Survey*.

<sup>8</sup> Brodie, ed., *The Absolute Weapon* (New York: Harcourt, Brace, 1946).



tion. Nuclear war spurred theorizing because it was inherently more theoretical than empirical: none had ever occurred. Except for Hiroshima and Nagasaki, where there was no question of retaliation, there was no messy store of historical evidence to complicate elegant abstractions. Available empirical data were technical—the physics of fission, fusion, and ballistics—and the implications appeared simple: for the first time, great powers would have the option to annihilate enemy societies overnight. Since no one had experience, intellectuals felt less inhibited by military expertise. Alain Enthoven, the prototypical Pentagon “whiz kid,” was notorious for his arrogant comment in a dispute over strategic plans: “General, I have fought just as many nuclear wars as you have.”<sup>9</sup>

With scant empirical grounds for testing propositions, nuclear strategy and deterrence seemed perfectly suited to deductive logic and game theory. A few simple ideas, based on a small number of assumptions and variables, seemed extremely powerful. By the 1960s theorists had highly developed ideas about how to organize nuclear capabilities to stabilize U.S.-Soviet deterrence. Arguments among strategists from the ivory tower about logical effects of “invulnerable second strike capability,” “reciprocal fear of surprise attack,” “counterforce options,” “mutual assured destruction,” “graduated escalation,” and “crisis stability” had a profound influence on civilian leaders.<sup>10</sup>

As long as nuclear weapons remained leashed and strategy seemed successful, strategic studies prospered. At the opposite end of the spectrum from nuclear war, however, strategy did not prosper. After the Cuban missile crisis, the focus of East-West competition shifted to the Third World. Many strategists turned their attention to problems of counterinsurgency. In this realm, in contrast to nuclear abstraction, theories *were* mercilessly subjected to testing. Most nonspecialists saw Vietnam (rather than successful cases of counterinsurgency in Greece, Malaya, and the Philippines) as the test and as evidence that theories failed when applied. Most of the work on counterinsurgency by professional analysts, however, was case-study research, and most of the theories came from practitioners.<sup>11</sup> Theoretical breakthroughs in the first

<sup>9</sup> Quoted in Kaplan (fn. 6), 254.

<sup>10</sup> Marc Trachtenberg, *History and Strategy* (Princeton: Princeton University Press, 1991), chap. 1; Patrick Morgan, *Deterrence*, 2d ed. (Beverly Hills, Calif.: Sage, 1983); Richard Betts, “Nuclear Weapons,” in Joseph Nye, ed., *The Making of America's Soviet Policy* (New Haven: Yale University Press, 1984); Robert Jervis, *The Illogic of American Nuclear Strategy* (Ithaca, N.Y.: Cornell University Press, 1984); Colin Gray, *Nuclear Strategy and National Style* (Lanham, Md.: Hamilton Press, 1986); Lynn Eden and Steven Miller, eds., *Nuclear Arguments* (Ithaca, N.Y.: Cornell University Press, 1989).

<sup>11</sup> *Selected Works of Mao Tse-tung*, vol. 2 (Peking: Foreign Languages Press, 1965); Jay Mallin, ed., *“Che” Guevara on Revolution* (Coral Gables, Fla.: University of Miami Press, 1969). Academics

cycle of strategic studies had been more about deterrence, nuclear strategy, and escalation than about revolution, intervention, and subconventional war. Apart from whatever credit it might claim for helping to prevent World War III, the field's weakness in the first cycle was the overwhelming attention given to the least likely type of war and the late consideration of the most likely. Given the utilitarian rationales for the field, it is hardly surprising that critics saw the Vietnam disaster as a reflection on it.

The other area in which analysts became influential in policy was defense program management. RAND provided not only deterrence theorists but cost-effectiveness experts to McNamara's Pentagon. Along with the unprecedented supervision of military operations in the air war over Vietnam, the managerial revolution was a prime precipitant of civil-military friction. To some, the military reaction to the civilian analysts evinced the anti-intellectualism that Brodie complained about in 1949, with military vested interests resisting dispossession as new players sought to rationalize the allocation of marginal resources.<sup>12</sup> In other respects, proponents of cost-effectiveness criteria overplayed their hand, blithely overruled traditional military judgment, and revealed the limits of economic analysis as a basis for military decision.<sup>13</sup>

In the 1960s Brodie made a midcourse correction. He rethought his enthusiasm for economic conceptualization of strategy, worrying that the approaches he had recommended in 1949 had been taken much

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developed limited war theories mostly about Korea and NATO, not subconventional war. Robert Osgood, *Limited War* (Chicago: University of Chicago Press, 1957); Henry Kissinger, *The Necessity for Choice* (New York: Harper, 1961); Morton Halperin, *Limited War in the Nuclear Age* (New York: Wiley, 1963). On unconventional war in the third World, French and British colonial veterans wrote theoretical statements: David Galula, *Counterinsurgency Warfare* (New York: Praeger, 1964); Robert Thompson, *Defeating Communist Insurgency* (New York: Praeger, 1966). One of the few theoretical works by academics that holds up is Samuel Huntington, "Patterns of Violence in World Politics," in Huntington, ed., *Changing Patterns of Military Politics* (New York: Free Press of Glencoe, 1962). Examples of case studies include Lucian Pye, *Guerrilla Communism in Malaya* (Princeton: Princeton University Press, 1956); Douglas Pike, *Viet Cong* (Cambridge: MIT Press, 1966); Jeffrey Race, *War Comes to Long An* (Berkeley: University of California Press, 1972). For postmortems, see Douglas Blaufarb, *The Counterinsurgency Era* (New York: Free Press, 1977); Larry Cable, *Conflict of Myths: The Development of American Counterinsurgency Doctrine and the Vietnam War* (New York: New York University Press, 1986); D. Michael Shafer, *Deadly Paradigms* (Princeton: Princeton University Press, 1988); Timothy Lomperis, *From People's War to People's Rule* (Chapel Hill: University of North Carolina Press, 1996).

<sup>12</sup> The seminal cost-effectiveness work on defense management is Charles Hitch et al., *The Economics of Defense in the Nuclear Age* (Cambridge: Harvard University Press, 1960). See also E. S. Quade and W. I. Boucher, eds., *Systems Analysis and Policy Planning Applications in Defense* (New York: American Elsevier, 1968).

<sup>13</sup> Robert Art, *The TFX Decision* (Boston: Little, Brown, 1968); Robert Coulam, *Illusions of Choice* (Princeton: Princeton University Press, 1977). McNamara's apostles claimed not to claim too much. See Charles Hitch, *Decision-Making for Defense* (Berkeley: University of California Press, 1965), 76; but as correctives, J. A. Stockfish, *Plowshares into Swords* (New York: Mason and Lipscomb, 1973), 197; and James Schlesinger, "Uses and Abuses of Analysis," in U.S. Senate, Committee on Government Operations, *Planning Programming Budgeting* (Washington, D.C.: GPO, 1970).

farther than he had expected, and that tools that were useful for limited purposes had been abused to answer questions beyond their applicability. Leaving RAND for UCLA in 1966, he was appalled by the “astonishing lack of political sense” and the ignorance of diplomatic and military history that he saw among economists who had become eminent strategists. “It is not that they have no time for history but rather that the devotees of any highly developed science . . . tend to develop a certain disdain and even arrogance concerning other fields.” In 1949 he had seen professional soldiers as too limited by soft intuition and folklore; in the 1960s he believed economics could do no better without incorporating more of the knowledge that scientists often consider soft.<sup>14</sup> By the 1970s, however, he need not have worried. Having played a central role in development of deterrence theory, economists were by then found hardly anywhere in the academic study of military affairs. RAND had also evolved into a bureaucratized contract research organization as much as a think tank and was no longer the hothouse of theoretical ferment it had been in the 1950s.

For a time no one took up the slack. Vietnam poisoned the academic well, and détente removed the urgency about deterrence. For a decade after the late 1960s, little serious work on military affairs was undertaken in universities, apart from arms control studies. The 1970s produced ample work on U.S.-Soviet negotiations, much of it a valuable extension of ideas developed earlier,<sup>15</sup> but most of which was technical and ahistorical. The Ford Foundation established research centers that concentrated on arms control at Harvard, Stanford, MIT, and Cornell. Systems analytic techniques were applied to defense program issues in monographs put out by the Brookings Institution, which influenced Washington policy debates of the 1970s but were not designed to advance theoretical debates (subsequent Brookings studies moved in that direction).<sup>16</sup> Later, the MacArthur Foundation dispensed numerous grants but emphasized nonmilitary subjects.

<sup>14</sup> Trachtenberg (fn. 10), 13n; Brodie, quoted in Steiner (fn. 6), 196–97; Brodie, “Why Were We So (Strategically) Wrong?” *Foreign Policy*, no. 5 (Winter 1971–72), 154. One of the principals who imposed economic analysis in the Pentagon foresaw the problem; Charles Hitch, “National Security Policy as a Field for Economics Research,” *World Politics* 12 (April 1960), 448.

<sup>15</sup> Donald Brennan, ed., *Daedalus* 89, special issue (Fall 1960); Thomas Schelling and Morton Halperin, with the assistance of Donald Brennan, *Strategy and Arms Control* (New York: Twentieth Century Fund, 1961).

<sup>16</sup> Examples of Studies in Defense Policy published by the Brookings Institution in Washington, D.C., in the trough between the first and second cycles include Martin Binkin, *Support Costs in the Defense Budget* (1972); William White, *U.S. Tactical Airpower* (1974); Barry Blechman, *The Control of Naval Armaments* (1975). More academic Brookings publications in the second cycle include Joshua Epstein, *The Calculus of Conventional War* (1985); Richard Betts, *Nuclear Blackmail and Nuclear Balance* (1987); Thomas McNaugher, *New Weapons, Old Politics* (1989); Bruce Blair, *The Logic of Accidental Nuclear War* (1993).

There was also a counteroffensive against the dominance of strategic studies over the field of international relations. Scholars advocated shifting the focus to interdependence and political economy because the importance of states and the utility of force had declined.<sup>17</sup> By the end of the 1970s, however, the tide turned. Klaus Knorr—an early colleague of Brodie, editor of *World Politics*, one of the few economists who kept working on questions of national security after the 1960s, and one of the few strategists to integrate political, military, and economic analysis—had been among the first to argue the declining utility of force. Like Brodie, however, Knorr became alarmed by those who took his argument too far and within a decade was publishing reconsiderations.<sup>18</sup> Within a few years of publishing *Power and Interdependence*, moreover, Joseph Nye turned his own interests toward security issues.

### THE SECOND CYCLE AND AFTER

The hiatus in strategic studies ended with the revival of the cold war at the close of the Carter administration. The logistical base for the field grew. In the first cycle, *World Politics* was the main outlet for academic articles on strategy. In the second cycle, specialized journals came to the fore, especially *International Security*.<sup>19</sup> In the first cycle, ideas revolved around basic concepts (deterrence, stability, credibility). In the second cycle, debate was about the elaboration of concepts, variations on old themes, and how specific configurations of capability would buttress or undermine peace.<sup>20</sup> In the second cycle, the most novel research and theoretical development took an empirical turn.

One area that opened up at the end of the 1970s was strategic intelligence. A few excellent works on the subject had appeared early in the cold war because political pressure to account for disasters eased re-

<sup>17</sup> Robert Keohane and Joseph Nye, *Power and Interdependence* (Boston: Little, Brown, 1977), chap. 2.

<sup>18</sup> Knorr, *On the Uses of Military Power in the Nuclear Age* (Princeton: Princeton University Press, 1966); idem, "On the International Uses of Military Force in the Contemporary World," *Orbis* 20 (Spring 1977); Richard Betts, Michael Doyle, and John Ikenberry, "An Intellectual Remembrance of Klaus Knorr," in Henry Bienen, ed., *Power, Economics, and Security* (Boulder, Colo.: Westview Press, 1992), 17–19.

<sup>19</sup> See also *The Journal of Strategic Studies*, *Survival*, *Defense Analysis*, *Comparative Strategy*, *Arms Control*, and *Small Wars and Insurgencies*. One of the better journals, *Security Studies*, began publishing after the cold war ended. Official journals include *Naval War College Review*, *Parameters*, and *Joint Force Quarterly*.

<sup>20</sup> Creative revisitations included Michael Mandelbaum, *The Nuclear Revolution* (New York: Cambridge University Press, 1981); Richard Ned Lebow, *Nuclear Crisis Management* (Ithaca, N.Y.: Cornell University Press, 1987); Robert Jervis, *The Meaning of the Nuclear Revolution* (Ithaca, N.Y.: Cornell University Press, 1989). See also Michael Howard, "The Forgotten Dimensions of Strategy," *Foreign Affairs* 57 (Summer 1979).

strictions on information about a few cases. Roberta Wohlstetter's classic book, *Pearl Harbor*, was based on thirty-nine volumes of congressional hearings, and Klaus Knorr's article, "Failures in National Intelligence Estimates," drew on his involvement in the postmortem of the Cuban missile crisis by the intelligence community's Board of National Estimates.<sup>21</sup> Declassification surged in the 1970s. The revelation of secrets from World War II (such as "Ultra" code breaking) produced a spate of historical studies.<sup>22</sup> More theoretical works capitalized on these and on information about cold war intelligence activities that started to become available with the congressional investigations of 1975–76, as well as on ideas from psychology and organizational sociology.<sup>23</sup> The subject sustained two new journals: *Intelligence and National Security* and *The International Journal of Intelligence and Counterintelligence*.

The bulk of research in the second cycle remained preoccupied with how to prevent World War III. (Lessons were often sought by revisiting World War I.)<sup>24</sup> New empiricism corrected prevalent assumptions about policy that had been inferred from deductive theories of deterrence. Scholars who burrowed into declassified documents and interviews revealed that much conventional wisdom among civilians about nuclear targeting did not in fact reflect strategy in practice—the doctrine embodied in the military's Single Integrated Operational Plan for nuclear war. ("Counterforce" targeting, which mainstream theory and political leaders' rhetoric had rejected as destabilizing, had never been abandoned.)<sup>25</sup> Others showed that much of the fundamental logic of

<sup>21</sup> Wohlstetter, *Pearl Harbor* (Stanford: Stanford University Press, 1962); Knorr, "Failures in National Intelligence Estimates," *World Politics* 16 (April 1964).

<sup>22</sup> J. C. Masterman, *The Double-Cross System* (New Haven: Yale University Press, 1972); R. V. Jones, *The Wizard War* (New York: Coward, McCann and Geoghehan, 1978); F. H. Hinsley et al., *British Intelligence in the Second World War*, 5 vols. (New York: Cambridge University Press, 1979–90); Ernest May, ed., *Knowing One's Enemies* (Princeton: Princeton University Press, 1984); Wesley Wark, *The Ultimate Enemy* (Ithaca, N.Y.: Cornell University Press, 1985); Michael Handel, ed., *Strategic and Operational Deception in the Second World War* (London: Cass, 1987); Barry Katz, *Foreign Intelligence* (Cambridge: Harvard University Press, 1989).

<sup>23</sup> Harold Wilensky, *Organizational Intelligence* (New York: Basic Books, 1967); Michael Handel, "The Yom Kippur War and the Inevitability of Surprise," *International Studies Quarterly* 21 (September 1977); Richard Betts, "Analysis, War, and Decision," *World Politics* 31 (October 1978); Lawrence Freedman, *U.S. Intelligence and the Soviet Strategic Threat*, 2d ed. (Princeton: Princeton University Press, 1986); Ariel Levite, *Intelligence and Strategic Surprises* (New York: Columbia University Press, 1987); Richard K. Betts, "Surprise, Scholasticism, and Strategy," *International Studies Quarterly* 33 (September 1989); Ephraim Kam, *Surprise Attack* (Cambridge: Harvard University Press, 1988); Loch Johnson, *America's Secret Power* (New York: Oxford University Press, 1989); Arthur Darling, *The Central Intelligence Agency* (University Park: Pennsylvania State University Press, 1990); James Wirtz, *The Tet Offensive* (Ithaca, N.Y.: Cornell University Press, 1991).

<sup>24</sup> *International Security* 19, special issue (Summer 1984).

<sup>25</sup> Aaron Friedberg, "A History of U.S. Strategic 'Doctrine'—1945–1980," *Journal of Strategic Studies* 3 (December 1980); Desmond Ball, "U.S. Strategic Forces," *International Security* 7 (Winter

canonical theories about nuclear "stability" that academics and civilian policymakers had come to take for granted was utterly confounded by the realistic operational limits of command and control systems.<sup>26</sup>

The other main strand of empirical work was in conventional strategy. This shift in attention was prompted entirely by the nuclear impasse. None of the convoluted theorizing about how to use or threaten to use nuclear weapons had managed to discover a consistently rational solution to the contradiction between the aims of stabilizing mutual nuclear deterrence between the superpowers and deterring a Soviet conventional attack against NATO. The former required that any nuclear first strike would be suicidal and therefore unthinkable; the latter required that an attack by enemy conventional forces could be blocked without nuclear escalation. Conventional wisdom in the West held that NATO's nonnuclear defenses were too weak and required reliance on the threat of nuclear first use—which meant that it must not be unthinkable. This in turn prevented Washington and Moscow from accepting any hint of inferiority in their respective nuclear forces. If nuclear competition was to be dampened, more confidence in conventional alternatives would be the price.

A new generation of analysts focused on assessing whether, why, and how NATO could achieve more such confidence, by examining in detail the data and assumptions behind standard estimates of the balance of forces and strategic alternatives in Europe. Questioning official assumptions, models, and calculations, and applying new conceptual frameworks, they took up where McNamara's whiz kids had left off in the mid-1960s but approached the problem in more depth. Theoretically, they transposed the concepts and categories of nuclear deterrence theory, whereby particular configurations of forces and emphases in operational doctrine were alleged to foster stability.<sup>27</sup>

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1982–83); David Rosenberg, "The Origins of Overkill," *International Security* 17 (Spring 1983). Scott Sagan corrects both the "expert's myth" that nothing changed at all and the "layman's myth" that mutual assured destruction represented U.S. strategy. Sagan, *Moving Targets* (Princeton: Princeton University Press, 1989).

<sup>26</sup> The definitive work is Bruce Blair, *Strategic Command and Control* (Washington, D.C.: Brookings Institution, 1985). See John Steinbruner, "Beyond Rational Deterrence," *World Politics* 28 (January 1976); idem, "National Security and the Concept of Strategic Stability," *Journal of Conflict Resolution* 22 (September 1978); Desmond Ball, *Can Nuclear War Be Controlled?* Adelphi Paper 169 (London: IISS, Autumn 1981); Paul Bracken, *The Command and Control of Nuclear Forces* (New Haven: Yale University Press, 1983); Peter Feaver, *Guarding the Guardians* (Ithaca, N.Y.: Cornell University Press, 1992).

<sup>27</sup> Jack Snyder, *The Ideology of the Offensive* (Ithaca, N.Y.: Cornell University Press, 1984); Stephen Van Evera, "The Cult of the Offensive and the Origins of the First World War," *International Security* 9 (Summer 1984); George Quester, *Offense and Defense in the International System*, 2d ed. (New Brunswick, N.J.: Transaction, 1988); Steven Miller, ed., *Conventional Forces and American Defense Policy* (Princeton: Princeton University Press, 1986); Richard Betts, "Conventional Deterrence," *World*

This wave of attention to conventional forces brought new emphasis on comparative analysis of historical cases. Writing primarily in *International Security* and *Studies in Security Affairs*, a series published by Cornell University Press, scholars sought additional analytic leverage on questions of relative capability to supplement debates about quantitative models of the military balance. New literature investigated political, economic, social, technological, organizational, and doctrinal issues that determined military effectiveness, and thereby focused the academic consideration of the essence of strategy: how to integrate political ends and military means.<sup>28</sup> A Clausewitz revival ensued in the same period, beginning with a new translation of *On War* (to which Brodie contributed a commentary). The classic *Makers of Modern Strategy* was also updated.<sup>29</sup>

Scholars who did this work prospered in the 1980s. Political science departments that had grown blasé about strategy in the period of détente scrambled to build their staffs again as superpower competition reheated, the Vietnam hangover dissipated, and realist conceptions of world politics rebounded. Opposition to identifying security with strategic studies existed all along,<sup>30</sup> but the identification prevailed in academic hiring in this period. A generous supply of fellowships (espe-

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*Politics* 37 (January 1985); William Kaufmann, "Nonnuclear Deterrence" and "The Arithmetic of Force Planning," in John Steinbruner and Leon Sigal, eds., *Alliance Security* (Washington, D.C.: Brookings Institution, 1983); John Leppingwell, "The Laws of Combat?" *International Security* 12 (Summer 1987); Eliot Cohen, "Toward Better Net Assessment," *International Security* 13 (Summer 1988); John Mearsheimer, "Assessing the Conventional Balance," *International Security* 13 (Spring 1989); Joshua Epstein, "The 3:1 Rule, the Adaptive Dynamic Model, and the Future of Security Studies," *International Security* 13 (Spring 1989); Charles Kupchan, "Setting Conventional Force Requirements," *World Politics* 41 (July 1989); Stephen Biddle, "The European Conventional Balance," *Survival* 30 (March–April 1988).

<sup>28</sup> See, for example, Cornell University Press books such as John Mearsheimer, *Conventional Deterrence* (1983); idem, *Liddell Hart and the Weight of History* (1988); Eliot Cohen, *Citizens and Soldiers* (1985); Barry Posen, *The Sources of Military Doctrine* (1984); Stephen Rosen, *Winning the Next War* (1991). Examples of historically oriented analysis included John Keegan, *The Face of Battle* (New York: Viking, 1976); Martin van Creveld, *Supplying War* (New York: Cambridge University Press, 1977); idem, *Command in War* (Cambridge: Harvard University Press, 1985); Richard Betts, *Surprise Attack* (Washington, D.C.: Brookings Institution, 1982); Allan Millett and Williamson Murray, eds., *Military Effectiveness*, 3 vols. (Boston: Allen and Unwin, 1988); Edward Luttwak, *Strategy* (Cambridge: Harvard University Press, 1987); Michael Handel, *War, Strategy and Intelligence* (London: Cass, 1989); Joseph Bouchard, *Command in Crisis* (New York: Columbia University Press, 1991).

<sup>29</sup> Carl von Clausewitz, *On War*, ed. and trans. Michael Howard and Peter Paret (Princeton: Princeton University Press, 1976); Peter Paret, ed., *Makers of Modern Strategy: From Machiavelli to the Nuclear Age* (Princeton: Princeton University Press, 1986); Edward Mead Earle, ed., *Makers of Modern Strategy: Military Thought from Machiavelli to Hitler* (Princeton: Princeton University Press, 1943). See Harry Summers, *On Strategy* (Novato, Calif.: Presidio Press, 1982), which set off an unprecedented wave of attention to Clausewitz in the U.S. Army; and Michael Handel, ed., *Clausewitz and Modern Strategy* (London: Cass, 1986).

<sup>30</sup> Richard Ullman, "Redefining Security," *International Security* 8 (Summer 1983); Joseph Nye and Sean Lynn-Jones, "International Security Studies," *International Security* 12 (Spring 1988); Jessica Mathews, "Redefining Security," *Foreign Affairs* 68 (Spring 1989).

cially through Harvard's Olin Institute and Center for Science and International Affairs, the Brookings Institution, and arms control centers sponsored by the Ford Foundation) kept up the corps of researchers in politico-military affairs. This renaissance lasted as long as avoiding World War III remained at the top of the real-world agenda.<sup>31</sup>

The end of the cold war turned security studies back to basics: questions about causes of war and peace, effects of the general distribution of power in international relations, economic and ideological influences on patterns of conflict and cooperation, nationalism, and so forth. Academic research on the operational and technical questions that dominated the 1980s stopped almost completely, but more general work on military institutions, history, and strategic issues thrived.<sup>32</sup> In what we may call either the third cycle of post-World War II strategic studies or the first post-cold war phase, research is advancing on civil-military relations, organization theory, arms control, strategic culture, coercion, grand strategy, and other subjects.<sup>33</sup> In contrast to the cold war, when analysis revolved around deterrence and the East-West military balance, no one policy problem dominates the agenda. This makes the enterprise richer than ever. But without the danger of apocalyptic war at the center, the force of the claim to relevance that overrode intellectual skepticism about the field during the cold war has weakened.

<sup>31</sup> Stephen Walt, "The Renaissance of Security Studies," *International Studies Quarterly* 35 (June 1991). For other reviews, see Colin Gray, *Strategic Studies and Public Policy* (Lexington: University Press of Kentucky, 1982); Helga Haftendorn, "The Security Puzzle," *International Studies Quarterly* 35 (March 1991); Neta Crawford, "Once and Future Security Studies," *Security Studies* 1 (Winter 1991).

<sup>32</sup> Brian Downing, *The Military Revolution and Political Change* (Princeton: Princeton University Press, 1992); Charles Kupchan, *The Vulnerability of Empire* (Ithaca, N.Y.: Cornell University Press, 1994); Kimberly Martin Zisk, *Engaging the Enemy* (Princeton: Princeton University Press, 1993); Bruce Porter, *War and the Rise of the State* (New York: Free Press, 1994); Williamson Murray, MacGregor Knox, and Alvin Bernstein, eds., *The Making of Strategy* (New York: Cambridge University Press, 1994); Jeffrey Legro, *Cooperation under Fire* (Ithaca, N.Y.: Cornell University Press, 1995); Richard K. Betts, *Military Readiness* (Washington, D.C.: Brookings Institution, 1995). Two books on crucial military topics would have been blockbusters had they appeared during the cold war, but were little appreciated after it: Barry Posen, *Inadvertent Escalation* (Ithaca, N.Y.: Cornell University Press, 1991); Paul Stares, *Command Performance* (Washington, D.C.: Brookings Institution, 1991).

<sup>33</sup> Chris Demchak, *Military Organizations, Complex Machines* (Ithaca, N.Y.: Cornell University Press, 1991); Scott Sagan, *The Limits of Safety* (Princeton: Princeton University Press, 1993); Robert Kaufman, *Arms Control during the Prenuclear Era* (New York: Columbia University Press, 1990); Emily Goldman, *Sunken Treaties* (University Park: Pennsylvania State University Press, 1994); Elizabeth Kier, "Culture and Military Doctrine," *International Security* 19 (Spring 1995); Alastair Iain Johnston, *Cultural Realism* (Princeton: Princeton University Press, 1996); Robert Pape, *Bombing to Win* (Ithaca, N.Y.: Cornell University Press, 1996); Peter Liberman, *Does Conquest Pay?* (Princeton: Princeton University Press, 1996); Stephen Rosen, *Societies and Military Power* (Ithaca, N.Y.: Cornell University Press, 1996); Robert Art, "A Defensible Defense," *International Security* 15 (Spring 1991); Michael Desch, *When the Third World Matters* (Baltimore: Johns Hopkins University Press, 1993); Peter Feaver, "The Civil-Military Problematique," *Armed Forces and Society* 23 (Winter 1996); Jonathan Mercer, *Reputation and International Politics* (Ithaca, N.Y.: Cornell University Press, 1995).



Where should strategic studies go? The current trajectory, on which a wider array of research topics rides than during the first two cycles, is a good one. Weak spots in the earlier cycles could still stand more work today: the political dimension of internal or civil war, and the operational dimension of irregular or subconventional war. Since 1945 scholars focused most on interstate war and nuclear or conventional strategy, but most of the conflicts that actually occur are of the other sorts. The comparative politics field in political science attends to internal conflict, and there is plenty of atheoretical policy literature on "low intensity" conflict, but not yet enough academic attention within international relations and strategic studies.<sup>34</sup> Another topic that merits special attention is the evolution of Chinese forces, doctrine, and strategy, and whether China's military development can match its economic surge. The cold war spawned an impressive corps of analysts of the Soviet military (Christopher Donnelly, John Erickson, Mary Fitzgerald, Raymond Garthoff, David Holloway, Arnold Horelick, Roman Kolkowicz, Stephen Meyer, Michael MccGwire, William Odom, Thomas Wolfe, and many others); there are counterparts on China (such as June Dreyer, Paul Godwin, John Lewis, Jonathan Pollack, David Shambaugh, and Arthur Waldron), but the list is shorter.

Despite the widening ambit after the cold war, skeptics who never liked the ascendancy of strategic studies see less reason to indulge it and demand that "security" studies be broadened.<sup>35</sup> The effect of accepting these arguments would be to slash attention to military strategy in universities. The best solution to intellectual controversy is to let a hundred flowers bloom, but departments do not have a hundred flower pots. Few, as it is, have found room for more than one expert on military affairs, and some have none. Broad definitions of security would allow departments to hire specialists in areas far afield from war and strategy and still claim that they cover the security slot.

### THE MISSING DISCIPLINE

As Thomas Schelling argued in 1960, strategy's theoretical development has been retarded because "the military services, in contrast to al-

<sup>34</sup> Stephen R. David, "Explaining Third World Alignment," *World Politics* 43 (January 1991); Barry Posen, "The Security Dilemma and Ethnic Conflict," *Survival* 35 (Spring 1993); Chaim Kaufmann, "Possible and Impossible Solutions to Ethnic Civil Wars," *International Security* 20 (Spring 1996).

<sup>35</sup> Edward Kolodziej, "Renaissance in Security Studies?" *International Studies Quarterly* 36 (December 1992); Ronnie Lipschutz, ed., *On Security* (New York: Columbia University Press, 1995); Peter Katzenstein, "Conclusion," in Katzenstein, ed., *The Culture of National Security* (New York: Columbia University Press, 1996); Keith Krause and Michael Williams, "Broadening the Agenda of Security Studies," *Mershon International Studies Review* 40 (October 1996).

most any other sizable and respectable profession, have no identifiable academic counterpart."<sup>36</sup> Strategic studies has piggybacked on other disciplines—mainly history and political science—instead of securing an autonomous institutional home. There are no *departments* of strategy or war studies in U.S. universities (in contrast to Britain). This in itself is not damning; not all interdisciplinary fields have departmental status. But there is still a disjunction between intellectual and institutional logics. The essence of strategy should be the integration of two disciplines—military science and political science—but one of them is missing. Interdisciplinary strategy suffers from the lack of an established academic *discipline* of military science to anchor it.

First, there is no institutional redoubt to fall back on when support for interdisciplinary work declines. To understand the causes, conduct, and consequences of war, one should know something of politics, economics, psychology, sociology, geography, technology, force structure, and tactics. When world developments favor strategic studies, the interdisciplinary character is an advantage, since it exploits strengths of several fields. Otherwise it is a vulnerability, since enthusiasm for interdisciplinary research falters when making room for it encroaches on one's own department.

Second, specialists in strategy are spread thinner. Unlike political scientists in international political economy (IPE), they have no analogue to economics as an allied field to draw on. They must develop the military science aspects of their work themselves, as autodidacts. (Then they smuggle military science into political science, where colleagues sometimes wonder whether what they are doing belongs there.) IPE does not focus on the technical functioning of markets but can assume that serious students have at least taken a basic course in economics. Strategists must cram the relevant military science into their own teaching, since students do not get it anywhere else. Economics is assumed to be fundamental for education in international affairs, but elementary military science is not. (In my own university's master's program, all students must take three economics courses; none but the few specializing in security policy are required to take any course on military matters.) In a world of limited resources that keeps many claims at bay, none of this means that military science should be a full-

<sup>36</sup> Schelling, *The Strategy of Conflict* (Cambridge: Harvard University Press, 1960), 8. There is also constant debate (as among students of politics) over whether strategy is science or art. Clausewitz and Sun Tzu are usually identified with art and Jomini with science, but Michael Handel shows that there is more agreement among the three than generally assumed. Handel, *Masters of War*, 2d ed. (London: Cass, 1996).

fledged discipline in the arts and sciences. But without an institutionally established discipline at the core, strategy must either be welcomed into other disciplines as a sideline or exiled from universities.

Academic work on strategy is sponsored elsewhere, primarily the service war colleges and the National Defense University. In "Strategy as a Science," Brodie wrote, "We need to make of our war colleges genuine graduate schools" (p. 487). This aim has been best approximated in the Naval War College, which has a strategy department dominated by civilian historians. These islands within military organizations, however, will never sustain strategic studies on their own, nor should they. Understanding of military affairs should not become a closed system, where none outside the uniformed establishment can claim expertise. If serious strategic studies is to survive, it needs a niche in real universities. Given the interdisciplinary and policy-oriented nature of strategic studies, graduate schools of public policy and international affairs should be a logical locus. But although such schools now give Ph.D.'s, they do not have an autonomous underpinning. Scholars staffing them still come mostly from the arts and sciences, which is where a viable academic enterprise must have roots.

Most social sciences have dealt with military subjects. For example, sociologists such as Morris Janowitz and Charles Moskos built the Inter-University Seminar on Armed Forces and Society and its journal *Armed Forces and Society*. Few of the social sciences, however, have generated a critical mass of scholars fully conversant with strategy. Economists got involved in strategic work as consultants, or by moving to RAND or Washington, not by establishing it as a field within their parent discipline. In practice, history and political science are the homes for strategic studies.

Military history is essential knowledge for anyone prescribing strategy, but it does not fare well within the history profession. Few major departments beyond Yale, Duke, and Ohio State have kept even a single military historian on their rosters. Strategic studies has been more welcome in political science. In part this is because political science has been an eclectic and permissive discipline, without a rigidly autonomous agenda, method, or qualifications. (Several major political science departments even appointed faculty with other degrees to cover strategy—mathematician Albert Wohlstetter at the University of Chicago, chemist George Rathjens at MIT, and economist Robert Powell at Berkeley. It is hard to imagine departments in any of those other disciplines hiring a political scientist for anything.) Most researchers in international relations exploit other disciplines for much of their work.

Empirically oriented ones look to history, those interested in deductive theory look to economics and philosophy. The most zealous social scientists, however, see methodological eclecticism as flabby indiscipline. Strategy will not fare well if scientific impulses achieve hegemony in the political science guild. Strategic studies can and should be as rigorous as any discipline, but it has a natural interest in a permissive writ for political science, if that discipline is to be its main home.<sup>37</sup>

Lack of a military science discipline also limits institutional links between military and academic cultures. Both camps have come a long way since "Strategy as a Science." Officers have become civilianized, getting M.B.A.'s or social science Ph.D.'s, complementing the traditional military orientation to engineering; and civilians have become militarized, serving in the Defense Department or getting more rounded educational backgrounds in military operations than McNamara's systems analysts had. Blurry boundaries among the realms of policy, strategy, and operations, however, keep the proper balance of civil-military power uncertain. Most accord civilians the right to make policy, and the military the right to run operations, but strategy is what links the two. Pulled in two directions, strategic choices are inevitably seen by some as primarily political and civilian and by others as primarily operational and military.<sup>38</sup>

Brodie did not consider the professional military equipped to accomplish the integration of policy and operations that is the essence of strategy, but American society would really not have it otherwise. Strategy sucks the military into high politics. Professional soldiers usually prefer a division of labor, segregating policy and operations into neat compartments, assuming that strategy will be their own mechanistic translation of policy guidance into military programs and plans that they can execute to the stipulated ends. Civilian strategists worry that military tunnel vision may yield dangerous and unrecognized political consequences—for example, building incentives for preemptive attack into the configuration of capabilities.

*If strategy is to integrate policy and operations, it must be devised not just by politically sensitive soldiers but by militarily sensitive civilians. Either of these types makes third parties in politics or academia uncomfortable. Ironically, many academics who endorse strong civilian control of the mili-*

<sup>37</sup> A dean of the profession's brief for eclecticism is Gabriel Almond, "Separate Tables," *PS* 23 (Fall 1988), 829–30, 839–40.

<sup>38</sup> Samuel Huntington, *The Soldier and the State* (Cambridge: Harvard University Press, 1957); Samuel Finer, *The Man on Horseback* (New York: Praeger, 1962); Amos Perlmutter, *The Military and Politics in Modern Times* (New Haven: Yale University Press, 1977); Richard K. Betts, *Soldiers, Statesmen, and Cold War Crises*, 2d ed. (New York: Columbia University Press, 1991).

tary prove reluctant to support it by promoting civilian strategic studies. Amateurs should not control what they do not understand, especially in a business that puts legions of lives at stake. Yet many academic critics share military skepticism (albeit for different reasons) about intellectual attention to details of military operations.

The main problem is not the pacifist or radical fringes of the academic world, despite the distaste they evince for a field they associate with support for U.S. policy. Neither group has as much clout in political science as elsewhere in academia. The problem is that many in the liberal mainstream concede that strategic studies is legitimate, but when major war appears to recede as a prospect in the real world—as it did in the 1970s and again after the cold war—they resist ranking the subject highly when their own fields' priorities are at stake. Seen as legitimate in principle, strategic studies faces marginalization in practice when departments see it as a second-rate claim on their discipline.

#### STRATEGIC STUDIES AND SECURITY STUDIES

The intellectual and institutional status of strategy is confused by persistent lack of consensus on how much attention military aspects of security should get and where lines should be drawn between narrow military science, integrative strategic studies, and all-encompassing security studies. In "Strategy as a Science" Brodie noted that military strategy was subordinate to the larger problem of how

to increase one's advantage without unduly jeopardizing the maintenance of peace or the pursuit of other values. This broader enterprise, which might be called "security policy," can be construed to cover the total preparation for war as well as the waging of it. It would thus deal . . . with political, social, and economic as well as military matters in both domestic and foreign contexts. (p. 477)

Brodie's "security policy" was closer to what I have called strategic studies, as his discussion of "strategy" was closer to military science, perhaps because he did not foresee vigorous arguments that security involves far more than preparation for war. His later frustration with economists' approach to strategy was their inattention to factors he lumped with "security" in 1949. Today it is fair to distinguish strategic and security studies in order to recognize that security includes things besides military concerns, as long as no doubt is left that security policy requires careful attention to war and strategy. Security studies today embraces many related topics such as diplomacy, policy formation, social and

economic mobilization, scientific innovation, arms control, and terrorism.<sup>39</sup> Some, however, regard even this breadth as inadequate.

As semantic commentary on the *term* "security," arguments that security studies should consider problems ranging from economic performance to environmental damage are quite fair. They do not help to organize the field of international relations, however, because they do not delimit a subfield. A subfield must be broad enough to encompass a significant range of problems, but narrow enough to be a coherent area of inquiry, distinguishable from other subfields and the parent field. Expansive definitions of security quickly become synonymous with "interest" or "well-being," do not exclude anything in international relations or foreign policy, and thus become indistinguishable from those fields or other subfields. Recognition of this boundary problem led Baldwin to suggest that security studies be abolished as a subfield and "reintegrated" into international relations. If the point was to reverse fragmentation and encourage the reintegration of all specializations, this argument would be reasonable, but he denies that the other main subfield of international relations, IPE, should be reintegrated as well.

First, Baldwin argues, no other subfield but security is "defined in terms of techniques of statecraft."<sup>40</sup> Even if this is true, the difference is less significant than the similarities. IPE is as much or as little about economic phenomena as security studies is about military phenomena. Both trade and war involve conflict and cooperation, negotiation, and ultimate media of exchange and settlement (cash payment and combat). Both combat and commerce are modes of interaction in which purposes, constraints, instruments, and procedural dynamics produce outcomes and overlap with other realms of interaction.

Second, Baldwin suggests, "the rationale for subfields is to ensure that important subtopics are not neglected,"<sup>41</sup> and security topics are established at the core of the parent field of international relations where realism is the dominant paradigm. Specialization, however, is at least as much for *deepening* knowledge on important subjects as for

<sup>39</sup> Examples of breadth beyond strictly military aspects include Paul Kennedy, *The Rise and Fall of the Great Powers* (New York: Random House, 1987); Aaron Friedberg, *The Weary Titan* (Princeton: Princeton University Press, 1988); Richard Ullman, *Securing Europe* (Princeton: Princeton University Press, 1991); Jack Snyder, *Myths of Empire* (Ithaca, N.Y.: Cornell University Press, 1991); Douglas Macdonald, *Adventures in Chaos* (Cambridge: Harvard University Press, 1992); Stephen Walt, *The Origins of Alliances* (Ithaca, N.Y.: Cornell University Press, 1987); idem, *Revolution and War* (Ithaca, N.Y.: Cornell University Press, 1995).

<sup>40</sup> Baldwin (fn. 3), 140. See also Baldwin, "The Concept of Security," *Review of International Studies* 23 (1997).

<sup>41</sup> Baldwin (fn. 3), 140.

guarding against neglect. Moreover, it has been twenty years since one could worry that IPE might be neglected, and realism has been on the defensive again since the cold war ended. Considering that international relations has more or less broken down into two main subfields, it hardly seems necessary to drop to one. If anything, more subfields should be strengthened (for example, environmental studies, which covers subjects ultimately as important as the regnant subfields and is more neglected than either security or IPE).

Clarity and claims might best be served by renaming the security subfield "IPM" (international politico-military studies). This would confirm the focus on strategic integration of ends and means, highlight the parallel to IPE, and circumvent the dispute over "security" that mixes legitimate semantic claims with objectionable attacks on strategic studies. The deal would concede the case for identifying the scope of security with international relations in general, in exchange for recognition of an "IPM" subfield (strategic studies) on a par with any other. Practically, however, there is no constituency on either side for such recategorization, so strategy's academic status will continue to be set through arguments about security studies.

As consensus on standards remains elusive, students of strategy regularly encounter criticisms of the field's quality, occasionally in print but most often in professional badinage. One objection is that mainstream strategic work is theoretically weak or has not advanced since the deterrence theory of the early cold war.<sup>42</sup> John Ruggie laments failures to consider possible transformations of international politics: "the worst offender by far is the American field of security studies," because "no epochal thought has been expressed by any serious specialist in that field since 1957, when John Herz published 'Rise and Demise of the Territorial State.'"<sup>43</sup> This confuses disagreement with closed minds: there is no evidence that those who disbelieve in transformation have refused to consider it, any more than that those Ruggie admires have refused to consider the case for continuity.

Have other subfields done much better in producing knowledge? Not by standards of cumulation or cross-fertilization. Work on deterrence and arms control represented as much cumulation as found in most of political science. Indeed, if work in the later cold war amounted to refinements of earlier breakthroughs rather than new ones, this represented progress based on cumulation. Debates on war causation and civil-military relations have filtered into other subfields via levels-of-

<sup>42</sup> Nye and Lynn-Jones (fn.30), 12, 21-22, 26.

<sup>43</sup> Ruggie, "Territoriality and Beyond," *International Organization* 47 (Winter 1993), 143.

analysis and bureaucratic politics arguments, and security studies adapted cognitive theory and organization theory before IPE did.<sup>44</sup>

Even if it were true that theoretical innovation in strategic studies has been less paradigm-shattering than in other fields, this would not ipso facto demonstrate weakness rather than strength. Critics would have to demonstrate that more recent and numerous theories in other fields are *better* theories—more useful for understanding the world—than the fewer and older ones of strategy. Theories may endure because they prove durable, or may change constantly because each new one proves wanting. One Clausewitz is still worth a busload of most other theorists.

Are technical discussions about weaponry or operational doctrine evidence of strategists' atheoretical fixation on particulars? Such criticism has some merit in regard to technically denominated literature of the cold war (though most of it was not in political science) and is understandable when provoked by hardware fetishists often taken for representatives of strategic studies. Otherwise, it is no more reasonable than it would be to denigrate political economy for attention to specific commodities, financial instruments, or trade agreements.

Some critics such as rational choice theorists who deride traditional empirical work as "just telling stories," or quantitative researchers who criticize it as "anecdotal," see emphasis on comparative case studies as generically weak compared to deductive theorizing or "large-N" studies. These other approaches thrive and compete effectively in universities with mainstream strategic studies as practiced in Brodie's tradition. Such work appears mainly in *The Journal of Conflict Resolution*, *American Political Science Review*, and *International Studies Quarterly*.<sup>45</sup>

Distaste for military studies sometimes comes from moral suspicions that it embraces war rather than attending to how to abolish it. American strategic research, however, is mainly about how to avoid war. Most work in strategic studies is profoundly *conservative*, in the literal sense, because it is concerned with *stability*, a value that privileges peace

<sup>44</sup> Thanks to Peter Feaver for this point. See John Steinbruner, *The Cybernetic Theory of Decision* (Princeton: Princeton University Press, 1974); Robert Jervis, *Perception and Misperception in International Politics* (Princeton: Princeton University Press, 1976); Graham Allison, *Essence of Decision* (Boston: Little, Brown, 1971); Harvey Sapolsky, *The Polaris System Development* (Cambridge: Harvard University Press, 1972).

<sup>45</sup> See also Bruce Bueno de Mesquita, *The War Trap* (New Haven: Yale University Press, 1981); Bueno de Mesquita and David Lalman, *War and Reason* (New Haven: Yale University Press, 1992); Jack Levy, *War and the Modern Great Power System, 1495-1975* (Lexington: University Press of Kentucky, 1983); Paul Huth, *Extended Deterrence and the Prevention of War* (New Haven: Yale University Press, 1988); Robert Powell, *Nuclear Deterrence Theory* (New York: Cambridge University Press, 1990); George W. Downs and David M. Rocke, *Tacit Bargaining, Arms Races, and Arms Control* (Ann Arbor: University of Michigan Press, 1990).



over revisionism. In this respect, liberals interested in arms control have been the most conservative. Few academic works promote schemes for using force to change the status quo. Rather, they focus on deterrence or defense, to discourage the resort to violence to effect political change.

Focusing intently on how to manipulate the threat of deadly force, for whatever benign purpose, strikes some as fatalistic, selling short the search for cooperative strategies. Why waste time and foundation grants on finding better ways to do a bad thing when we might apply our talents to making it unnecessary? But accepting the occurrence of war and considering how to cope with it more effectively are no more fatalistic than accepting liberal capitalism, and considering how to optimize trade within it, would seem to a Marxist. Realist assumptions about group conflict that underlie most strategic studies require no more and no less validation than those of optimists who believe in the obsolescence of war. Debate over these assumptions lies at the heart of political theory and has been recycled and unresolved for centuries. It would be foolhardy to bet that social science can resolve it and arrogant for either side to deny an academic place to the other.<sup>46</sup>

#### STRATEGY FOR WHAT?

Are scholars of strategy too policy-oriented (not sufficiently theoretical) or too involved in government consulting to keep straight the conflicting demands of truth and power? (At different times, critics have given it both ways—denigrating the field for being too relevant in the era when there were huge security problems and dismissing it now for not being relevant enough.) At high points of the cold war, analysis often did fixate on the U.S.-Soviet balance of military power and the relative merits of particular weapons programs. It is also true that few strategists apologize for wanting to affect prospects for war and peace in the outside world. Apart from aesthetic fascination with the elegance of theory itself—theory for theory's sake—the rationale for valuing theoretical over policy analysis in the intellectual pecking order is that the former can subsume and inspire a wider range of analysis, and thus reveals more and lasts longer than work on a transient issue. This utilitarian rationale means that one good theory can illuminate many policy questions—but also that some link between theory and practice is ultimately the test of a theory's value. *Neither theory nor policy can be optimized apart from each other.* Central theoretical insights often flow from

<sup>46</sup> See Christopher Davis, "War and Peace in a Multipolar World," *Journal of Strategic Studies* 19 (March 1996).

grappling with concrete questions rather than a priori constructs. For example, Albert Wohlstetter drew basic precepts about strategic instability from his work on a RAND study of choices in bomber deployment patterns.<sup>47</sup>

Two academic pathologies should raise the stock of policy studies. One is that the professional premium on theorizing tends to proliferate theories, promote constant revision of theories, and encourage production of second-rate theories over first-rate applications. Albert Hirschman, with impeccable credentials as a theorist, long ago indicted “the tendency toward *compulsive and mindless theorizing*.”<sup>48</sup> One sure sign of intellectual degeneration in a field is when the logical relationship between generalization and specification is inverted, theories threaten to outnumber their applications, and the shelf life of theoretical work turns out to be hardly longer than that of policy analysis. Some social scientists are untroubled that professional incentives encourage such imbalance, because never having had to meet a payroll in the policy world, they overestimate the ease with which an effective application can be derived from a theoretical insight. Every intellectual would rather be an Einstein than an engineer, but useful knowledge is not advanced if the academy generates a horde of would-be Einsteins but few competent engineers. Strategists are not just engineers, but they consider empiricism and application no less important than the theoretical part of their work.

The other pathology is when theorization becomes a closed system, with no connection through which insights can be applied to the outside world—when theorists communicate effectively with no one but each other. When this happens, a theory may remain beautiful but it loses the claim to utility. It is the widespread perception in the outside world that theorization is a closed system that makes “academic” a pejorative adjective in normal parlance. A system can be closed in two senses: lack of feedback from policy application, or lack of interest in testing theories against evidence. Both problems are addressed in typical strategic studies research programs that proceed from policy issues, to theoretical formulation, to empirical testing, to policy application.

Intellectuals who spend much time in Washington sometimes worry that much theoretical work in contemporary political science reflects both pathologies and has not proved much less ephemeral or more useful than good applications of old theory. Unless academics themselves

<sup>47</sup> Wohlstetter, “The Delicate Balance of Terror,” *Foreign Affairs* 37 (January 1959).

<sup>48</sup> Hirschman, “The Search for Paradigms as a Hindrance to Understanding,” *World Politics* 22 (April 1970), emphasis in original.

become involved on the periphery of policy-making, the only way that their work can have effect outside the closed system in universities is if practitioners read it. Few high-level staff in the U.S. government read anything more academic than *Foreign Affairs*, and high-level policy-makers seldom have time to read any unofficial material but op-ed pieces. One academic journal that is read occasionally in Washington is *International Security*, because it melds policy analysis and theory. This is one reason it has had a circulation 50 to 80 percent higher than its IPE counterpart *International Organization* and that academics in other fields sometimes denigrate its academic quality.

Some academics may value the aesthetic qualities of theory as much as the utilitarian. Strategists can get as excited as anyone over the elegance of an idea, but see elegance without empirical confirmation and applicability as no more science than art. As Brodie suggested, any criterion for strategy but a utilitarian one is a contradiction in terms: "The question that matters in strategy is: Will the idea work? . . . *Strategy is a field where truth is sought in the pursuit of viable solutions.*"<sup>49</sup>

In the first half of the cold war, academic strategists played a visible role in U.S. defense policy. There have been many officials with Ph.D.'s since. For better or worse, however, few practicing academics in strategic studies have been directly influential since the 1960s, except for Henry Kissinger. Most scholars who have held high national security offices have been generalists (McGeorge Bundy, Walt Rostow, Zbigniew Brzezinski) or ones from strategic studies who left academia early in their careers (Michael Armacost, Paul Wolfowitz, Arnold Kanter, Dennis Ross, Lynn Davis). Some academic strategists are consultants to foreign affairs agencies, but few are reputed to be powers behind any throne. Ironically, in the past quarter century, policy experience has enriched academic research more than the reverse, since many prominent scholars of strategy spent brief periods early in their careers working at middle levels in the government (usually thanks to fellowships from the Council on Foreign Relations).

The direct effect of strategic studies on the outside world may be greater than that of much other social science. It remains quite limited, however, perhaps because scholarship in the field became more academic after the first cycle. Thus the field is more like others than it is different, in the sense that the influence of education is hard to pinpoint. It percolates through students who go into the outside world, through the few policymakers who read research, or through other channels dif-

<sup>49</sup> Brodie, *War and Politics* (fn.4), 452-53, emphasis in original.

ficult to trace. In any case, to whatever extent strategic studies is not a closed system, it is cause for celebration, not criticism.

Brodie's disappointment with the first cycle reflected the failure of strategists then most prominent to integrate the analytic rigor of economics with the broader expertise in military science, politics, and history that he himself had. Strategic studies improved in those terms in the second cycle. Now the question is whether strategic studies, larded as it is with military science, will remain at the center of security studies or will wither as academic guilds drive the focus of research to other subjects.

Strategy is not the whole of security and need not be anointed as the first priority of international relations. This defense of strategic studies is not a special pleading to return the field to a dominant position, but simply a case for keeping its status equal to any other subfield. Whatever resources are available for hiring, faculties should decide what to cover on the basis of long-term evidence of what has mattered in world politics rather than recent events, intellectual fads, or moral hopes. A department that can afford only one professor of international relations needs a generalist and cannot demand that she know much military science. A department that can hire in separate subfields, however, should ensure that coverage of "security" includes as much emphasis on strategic studies as if the slot were defined as in "IPM."

War has always been an essential phenomenon in world politics. There is nothing wrong with asserting that it is waning as long as such propositions (which have been popularized and discredited three times before in the past century) are not allowed to strike the issue from the agenda of highest priority problems. If war does become obsolete, the wasted intellectual effort in continuing to study it will have been a small price. If it does not, and if research ever has any useful impact at all, future generations may be glad that we kept our intellectual powder dry.